RAISING THE ALARM: THE CULTURAL ORIGINS OF CLIMATE ‘DENIALISM’ IN AMERICA, 1970-1988

By

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ABSTRACT

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At the height of the environmental movement between the late 1960s and early 1970s, a cadre of elite American scientists sought to establish what they believed to be a middle ground between opposing perspectives of future environmental threats. On the one hand were those within the environmental movement who appeared to depict the future in exaggerated doom-and-gloom terms, when the existence of humanity itself seemed threatened by environmental degradation and abuse. On the other hand were pro-growth interest groups that dismissed environmental concerns and regulations because they appeared to indicate the government's encroachment on private markets. This political back and forth, at least according to those who perceived themselves as more rational and dispassionate within the scientific establishment, seemed overly adversarial and disruptive to the business of understanding and managing environmental threats. An examination of how and why they sought to position themselves between these two poles within the environmental movement serves as the basis for this dissertation.

Considered scientific experts, these individuals -- S. Fred Singer, Helmut Landsberg, Philip Handler, Philip Abelson, Frank Press -- strongly believed in the importance of what sociologists of science call "organized skepticism," whereby scientific claims are validated by scientists prior to being released to the public. To protect the integrity of their position, they frequently leveled two kinds of criticism against those with whom they disagreed. The first criticism was direct toward those scientists whom they felt engaged in advocacy in that they used

their scientific credentials to advocate for policy changes based on their own non-scientific value judgments. Advocacy, they argued, threatened the credibility and authority of the scientific community. Second, they reserved their most potent criticism for those whom they called prophets of doom -- individuals who appeared overly emotional and irrational in their imaginings of future catastrophe. This latter criticism was more serious because it suggested that scientists used the language of science to buttress their public claims while simultaneously avoiding or overlooking the technical uncertainties that could undermine their own authority. Additionally, the claims of so-called prophets of doom appeared to resemble the kinds of sensationalized claims that appeared in the media; without a proper appreciation for the uncertainties within one's public claims, some scientists could risk appearing as unprofessional headline seekers. Whether perceived as an advocate or a prophet of doom, scientists -- as argued by the aforementioned experts -- had an obligation to avoid inflaming pre-existing public anxieties about the future. Doing so, they believed, could minimize what they saw as a gradual undermining of public confidence in science to solve the nation's environmental problems.

In sum, this dissertation does three things. First, it examines the disagreements scientists had regarding the assessment and public communication of environmental threats. Second, it explains what appears to be their underlying motivations. Third, it seeks to provide a possible foundation on which to understand why present debates over climate change often orbit around issues of proper scientific communication and public engagement. Ultimately, this dissertation examines how and why scientists spent a great deal of time negotiating the proper manner in which to 'go public' about the nature and urgency of environmental threats during the 1970s and early 1980s.

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Looking back over the last five years, historian of science James Fleming has been a friend and mentor in my evolution as an early-scholar. Since 2009, when he invited me to attend my first history of climate science workshop at Colby College, Jim has been a wonderful source of both professional and personal advice and support. His stoic patience and understanding, combined with an earnest concern for my progress as a scholar, continues to provide the courage to overcome my own frustrations and insecurities. A source of inspiration, Jim has provided a model of what it means to guide others. For that, I am eternally grateful.

Without archivists, the great project of historical research is lost to the abyss of time. Elizabeth Novara, an archivist at the University of Maryland Archive, was a tremendous source of assistance and guidance during my visits to the Helmut Landsberg Papers early in my research. Kate Legg, archivist at the National Center for Atmospheric Research, has become a friend and collaborator over the last two years, particularly when it came to deciphering the world of Boulder. David Hays, archivist at the University of Colorado-Boulder, provided a helpful hand and wonderful conversation. His willingness to talk with me over lunch on multiple occasions made my experience in Boulder that much more pleasant. Invaluable assistance has also been provided by Lisa Kemplin (University of Illinois at Urbana-Champaign), Janice Goldblum (National Academy of Sciences Archive), Hui Hua Chua (Michigan State University Library), James Herring (Jimmy Carter Library), Rodney Ross (National Archives), as well as archivists at the Smithsonian Air and Space Museum. I also want to acknowledge the incredible generosity of Gerald Barney in allowing me to use his personal collection of documents pertaining the Global 2000 Report. I can only imagine the significant amount of time and energy it took for him to organize and scan the thousands of documents necessary for my research.

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One of the most fortuitous occasions was meeting Matthew Stanley. Having known him since 2004, when we both entered Iowa State University's History Department, his initial course on the boundaries between science and pseudo-science in the 19th century provided the initial spurt of interest in how science is defined and practiced in different cultural contexts. I cannot watch Ghostbusters the same way again.

Graduate school has been a voyage unlike any other. A strange blend of fanciful dreams and the perils of uncertainty, I have only become more appreciative of those who have made my transition into academic life a personally fulfilling one. My wife, Laura, is a partner in the truest sense of the word -- as a friend, collaborator, and most of all the love of my life. Her strength is beyond measure, especially in those quiet moments at the end of day when the gravity of professional and personal troubles seemed unbearable. My parents have been instrumental. Their love and dedication to my success has been a fountain of inspiration. Without their ongoing encouragement I truly could not have remained optimistic about my decision to pursue a Ph.D. My grandparents have also been a wonderful connection to the outside world. Whether in the form of hand-written letters about family, conversations about their own experiences with

hardship, or even a birthday card, I am in constant awe of their love and tenderness even when I occasionally lose sight of things outside of writing and researching. In times of apathy, I frequently think of my sister. Her work ethic is marvelously inspiring, and her success keeps me hopeful that hard work can truly pay off. I will also treasure my many travel experiences with Bruce, all of which took me out of my zone of comfort but nonetheless made me more appreciated of life's possibilities. In light of the many friends -- Bre, Josh, Matt, and Jen, I will miss our time together -- and colleagues I acquired over the years, I have learned lessons that will resonate for the rest of my life.

PREFACE

This dissertation examines how and why American scientists disagreed over how to assess, conceptualize and publicly communicate the scale of future environmental risks and dangers during the late 1960s, 1970s, and 1980s. By examining these disagreements, I argue that historians are in a better position to understand why such disagreements became increasingly politicized during the 1980s during the Reagan administration. These are important questions because societies throughout the 20th century have relied on scientists to adjudicate whether dangers justify national policies to either ameliorate any negative effects or prevent the perceived dangers from occurring at all. While certainly justified for a variety of reasons (i.e. scientists have the training and expertise to evaluate natural phenomena, their methods tend to minimize falsehoods and maximize reliable conclusions), scientists themselves are frequently unwilling to raise the public alarm due to fears that their involvement could jeopardize their public cultural authority and standing among their peers.

For scientists, raising the public alarm is a highly contingent and problematic endeavor because they must find ways to balance any pre-existing cultural norms both within society and within the scientific profession itself. Too much public engagement may lead to a suspicion that they are acting in a way that makes then indistinguishable from those who appear driven by other motivations (e.g. political). Too little alarm and scientists could be perceived as excessively conservative and cautious, too complacent in their responsibilities to protect the welfare of humanity given their unique understanding of the natural world. Questions about how scientists balance their own personal value systems with their social responsibilities to the general public, as well as how the scientific community negotiates its own cultural and social norms regarding public communication, serves to guide this dissertation.

Indeed, one may begin to briefly illustrate the problematic nature of raising public alarm within a variety of historical contexts -- in France during World War I, in the United States in the 1970s, and in Italy during the first decade of the 21st century. By briefly showcasing how public alarm can be perceived as both a social threat as well as a social benefit, my dissertation contributes to scholarship on the social and cultural factors that often shape how and why scientists engage with society regarding perceived future threats. Scientists, as illustrated below, have an important role to play not only in monitoring and understanding what can best serve the public but also in regulating those elements that allow societies to remain relatively stable over the long term.

The events of World War I provides a salient opportunity to understand the role of scientists in determining how much alarm is appropriate. In 1914, Dr. Charles Vallon, Chief physician with the Paris Asylum of St. Anne, was introduced to a patient whose town was bombarded during the early months of the war. The unnamed man publicly reported to his fellow countrymen about what he envisioned to be the future defeat of the French forces. He was arrested and sent to the asylum for observation to determine whether he was criminally responsible for inducing public fear in a state of war, or should be treated for what also appeared to be signs of psychological distress induced by the devastation he witnessed. While both signified intent behind the man's actions, they resulted in very different consequences: imprisonment or treatment. Vallon, having attended the trial where the man's fate would be settled, used his judgment as a trained psychologist to determine an appropriate course of action given what many believed to be the risks of the man's actions to society. After a series of observations, the weight of evidence led to one conclusion: the man was *not* criminally responsible for his actions, but was psychologically distressed and, as such, Vallon advocated treatment within an asylum.

At least this was how Vallon's experiences were recounted four years later in a short piece within the *British Medical Journal* by an anonymous author.[1] After four long years of war, the author used Vallon's observations to illuminate the importance of minimizing undue public fear. According to the author, "gloomy prophets" or the "vaticinations of Cassandra -- a reference to the Greek myth about Cassandra being given the power to prophesize by Apollo but tragically never being believed by others -- deserved isolation from society regardless of whether they were criminals or merely psychologically distressed.[2] Vallon himself, according to the British author, also believed that all those responsible for spreading unnecessary public alarm -- the so-called "alarmists" -- should be regarded with suspicion because they posed a danger to society by weakening public confidence and morale to fight. The judgment was that in times of crisis, what were deemed alarmist claims indicated a deeper social despair that -- if spread -- could undermine a nation's motivation to protect itself.

The central point of describing Vallon's experiences was not just in determining whether the man's activities were criminal or not, but why raising the alarm was perceived negatively as alarm*ism*. The court's involvement, Vallon's observations and judgment about the man's intent and frame of mind, and his subsequent incarceration within an asylum suggests a rich undercurrent of thought about proper communication in the public sphere. The fact that the man was pessimistic within his own mind was not the problem, however; the problem was that he broadcasted his fears within a public setting. This suggested irresponsibility in that he played off

[1] "The Psychology of the Alarmist," *The British Medical Journal* 1, 2997 (June 8, 1918): pp. 651-652. While Charles Vallon (1853-1924) was a physician at the Paris Asylum of St. Anne, a brief search of his life or work yielded little way to determine the truth of these experiences.
[2] For a discussion of the mythic figure of Cassandra, see Schapira, Laurie. *The Cassandra Complex: Living with Disbelief* (Toronto: Inner City Books, 1988)

of the latent fears of a susceptible public, not -- as might be presupposed -- by inducing an entirely new fear. Humans feel fear within, but the *public* display of pessimism, of "alarmism," was a threat to what others believed to be the importance of maintaining *public* optimism. Vallon's determination that he was not a criminal was in part based on notions of the social good during wartime. Indeed, history reveals other instances when public claims are perceived as dangerous and threatening -- even in peacetime.

In his 1973 book, *False Prophets of Pollution,* American horticulturalist R. Milton Carleton noticed what he believed to be evidence of what he called "alarmist" rhetoric used by the environmental movement and claims of some American ecologists -- who he pejoratively labeled "fake ecologists."[3] In the wake of the maturing field of ecology as an environmentally-conscious discipline during the 1950s and 1960s, Carleton believed that some ecologists -- however well intentioned -- allowed their passion for advocacy to overtake their reason in the face of environmental challenges.

While not trained as a psychologist, he believed the social threat of public alarm was rooted in a deep psychological malady. “Let us try to look inside their minds to see what motivates them,” Carleton adventurously declared. Inside what he called this “dark pit of unfathomable mysteries" was what he imagined to be a deep-seeded struggle between emotion and reason.[4] While not all fear was irrational, Carleton argued, alarmists were unique in that they appeared excessively and disproportionately driven by emotion due to a desire to avoid divine punishment for defiling the earth through human activities -- what he termed

[3] Carleton, Milton. *False Prophets of Pollution: How Fake Ecologists Sidetrack America's Progress* (Trend Publications, 1973). Carleton was the cofounder of the American Horticultural Society (founded in 1922), past president of the Chicago Horticultural Society and the Men’s Garden Clubs of America, and served as editor of the science and garden section of *Better Homes and Gardens*. See, "Rites for R. Milton Carleton, Horticulturalist," *Chicago Tribune*, 5 September 1986.
[4] *Ibid*, p. 141

"supernatural punishment."[5] By bringing attention to what they believed to be potentially catastrophic environmental threats, so-called alarmists could overcome humanity's complacency and facilitate the return of humankind to an imaginary "halcyon age when all was serene and peaceful; when the ills of modern civilization did not exist."[6]

For Carleton, those whom he called alarmists were also threatening because they appeared to be organized into what he called a "disaster lobby." Not only were so-called alarmists projecting their psychological fears of the future into the public sphere but they also appeared to organize around what appeared to be their own naive self-interests. Furthermore, those whom Carleton called alarmists appeared blind to their own role in facilitating the kinds of environmental problems that they felt necessitated a fundamental restructuring of industrial society. "In the final analysis," he lamented, "it is *your* demands for the products and services of industry which result in pollution." Those environmentalists who appeared to be prophets of doom, he argued, sought to induce a disproportionate fear of the wrong things and unwittingly allowed what he considered the real environmental threats to fester underneath everyday life. "The sophistry of those who weep because bird eggs do not hatch, yet cry for rigid controls that cause human misery is typical of the dichotomy which makes pollution control so difficult," he noted.[7] What Carleton saw was, in effect, a conspiracy of gloom promulgated not only by unguided environmentalists but also some scientists themselves (i.e. "fake" ecologists).

While the experiences of Vallon and Carleton illustrate how raising the public alarm may be perceived as alarmism, history also reveals instances when raising public alarm was perceived -- in hindsight -- as the most appropriate course of action. In Italy in 2012, six scientists and a government official were successfully prosecuted for manslaughter because of how they assessed

[5] *Ibid*, p. 142
[6] *Ibid*, p. 56
[7] *Ibid*, p. 21

and communicated risk before a major earthquake hit the Italian city of L'Aquila on April 6, 2009. According to the prosecutor, the defendants not only failed to properly warn state authorities of an impending earthquake they also appeared to marginalize the dangers by suggesting little need for a response. The result was a significant loss of life and property. According to the judgment of the judge, Marco Billi, the scientists' assessments of the risks "turned out to be completely vague, generic and ineffective."[8] Their sentence: a prison term and financial compensation to each of the hundreds of victims and the city itself.

Suffice it to say, a unique precedent had been established regarding the responsibilities of scientists to the state. Rather than advocate for what seemed to be the best science available regarding the likelihood of a future earthquake, scientists were informed of their responsibility to warn the general public in spite of great scientific uncertainty. Of course, this carries its own risks. Marcello Melandri, one of the defense attorneys, claimed that the decision would likely incentivize scientists to lower their standards of what constituted real risk and compel scientists to prematurely and excessively react to even low-probability events. As he said, scientists would likely produce "many more false alarms in such situations, because experts will choose to cry wolf when in doubt. In the end, they will become less and less credible."[9] For Melandri, the precedent was not just about scientists being wrong potentially but about the broader desire to protect the credibility of scientists enough to warn the public in times of genuine distress.

And this brings me to the central thesis of this dissertation: how do scientists negotiate how to interpret the available scientific evidence in a way that is respectful of the uncertainties while inducing enough public anxiety to warrant what many believe to be a justified

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[8] Davies, Lizzy, "L'Aquila Quake: Italian Judge Explains why He Jailed Scientists Over Disaster," *The Guardian*, 18 January 2013, http://www.theguardian.com/world/2013/jan/18/italian-scientists-jailed-laquila-quake, last accessed June 17, 2014

[9] Cartlidge, Edwin, "Prison Terms for L'Aquila Experts Shock Scientists," *Science* 338, 6106 (26 October 2012): pp. 451-452. Also, Goodwin, Jean, ed. *Between Scientists and Citizens: Proceedings of a Conference at Iowa State University, June 1-2, 2012* (Great Plains Society for the Study of Argumentation, 2012): pp. 10-15

precautionary response to future threats? As revealed by the experiences of scientists in different historical contexts -- Vallon during World War I, Carleton during the 1970s, and Italian seismologists during the early 21st century -- one person's alarmism may be another's proper vigilance and there is little way to gauge in times of high scientific uncertainty which is preferred. As historians, this space of ambiguity provides an opportunity to explore how scientists negotiate their behaviors and roles within society. Toward this end, this dissertation focuses on how American scientists disagreed over how to assess and publicly communicate future environmental threats to the general public during the 1960s and 1970s, and examines why these kinds of issues were felt to be integral to discussions regarding the nature of expertise and scientific credibility. As examined in this dissertation, American scientists frequently found it difficult to navigate how best to engage the public in a way that balanced their own professional ideals and scientific understanding with what they deemed to be their commitments to the welfare of society.

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INTRODUCTION

> Many specialists, sincerely alarmed over what man is doing to his world -- and what he is capable of doing as his numbers grow -- feel they are caught in a dilemma: Warn reasonably or talk doom? -- *Tuscaloosa News*, 1971[1]

> 'So hot? my little Sir,' said Emerson, warning us not to mistake the sound of a popgun for the crack of doom. -- Arthur Schlesinger, Jr., Foreword, *The Cycles of American History*

This dissertation examines how and why a small group of elite American scientists -- climatologist Helmut Landsberg, geophysicist Philip Abelson, geophysicist Frank Press, physicist S. Fred Singer, and biochemist Philip Handler -- sought to establish what they called a middle ground within the highly-charged environmental politics of the 1960s and 1970s. On the one hand, they argued, were those in the environmental movement and the far-left who appeared to exaggerate the severity of future environmental threats in the hope to create an ecologically-minded American society. On the other were those who they interpreted as pro-growth interest groups that appeared to hastily dismiss environmentalists' concerns out of an attempt to maintain the status quo of environmental abuse and exploitation. Each side employed a rhetoric that appeared designed to promote their specific interests while preventing genuine understanding of environmental risks as these individuals understood them.

To minimize what they interpreted as the increasing politicization of environmental science within the context of these disputes, they sought to identify and communicate what they believed to be a more accurate assessment of future risks in three specific ways. First, they

[1] Hornig, Roberta and James Welsh, "Environmental Catastrophe in Future," *The Tuscaloosa News*, 31 January 1971, p. D5

believed that acknowledging and communicating existing scientific uncertainties to both policy makers and the general public was the most responsible course of action until scientists could acquire a more robust understanding of the nature and scope of environmental threats. Second, they embraced a more reticent approach to public engagement until more sufficient and reliable evidence was acquired. Third, they sought to use what they considered moderate and less heated rhetoric that reflected what they believed to be the true risks of environmental threats given the significant scientific uncertainties that existed at the time. Taken together, these three goals -- emphasize existing uncertainties, minimize public engagement, avoidance of what they saw as exaggerated rhetoric -- provided a way to situate themselves within environmental politics without contributing to what they saw as unproductive and overly emotional rhetoric.

While the heated climate change debates of the last twenty years may lead historians to cynically evaluate and dismiss what may appear to be an anti-environmental and politically conservative agenda, this dissertation provides room for a more nuanced interpretation of their views and goals. While their public and private correspondence does, in fact, yield ample evidence of their antipathy toward some sectors of the environmental movement, their broader motives were guided by a desire for scientists to be seen in a particular way by the general public. First, their effort to pave what they considered a responsible middle ground was motivated by a desire to restore public trust and confidence in elite scientists such as themselves. Without what they considered to be reasoned and dispassionate analyses of environmental problems, they believed that political brinksmanship would soon supplement scientific understanding of and responses to environmental problems -- and allow their voices to be increasingly relegated to the shadows of public discussion. Second, they wanted to maintain what they considered an array of professional ideals and norms of behavior that they believed distinguished professional scientists from amateurs, pseudo-scientists, and those they often referred to pejoratively as alarmists and prophets of doom. By delineating themselves and their activities in these ways, they believed that they could not only influence the course of environmental discussions but could also restore public confidence in the statements of elite scientists such as themselves.

Using three case studies of perceived global environmental threats during the late 1960s and 1970s -- the threat that the atmosphere was running out of oxygen; the prospect that supersonic transport (SST) emissions would cause atmospheric changes that could potentially and directly harm human beings; the risk that industrial activities may change the global climate system -- this dissertation examines how they responded to what appeared to be a threat to their public credibility in lieu of broader skepticism toward scientists. Before describing in more depth the underlying factors that led to their efforts to construct a middle ground, a brief biographical description of these individuals seems appropriate.

BIOGRAPHIES OF EXPERTISE

All of the experts discussed in this dissertation -- Philip Handler, Philip Abelson, Helmut Landsberg, S. Fred Singer, and Frank Press -- were white, male, highly credentialed, situated at the highest echelons of the American scientific community, and maintained strong networks of influential individuals like themselves. Indeed, their professional trajectories reveal both important similarities and differences that contributed to their standing within the American scientific community, as well as their views about the proper way that scientists should have behaved in light of environmental risks.

Helmut Erich Landsberg was born in Frankfurt, Germany in 1906. In 1930, at the age of 24, he received his Ph.D. in seismography from the Frankfurt Institute of Meteorology and

Geophysics under the guidance seismologist, Beno Gutenberg.[2] While Gutenberg left Frankfurt in 1929 for a professorship at the California Institute of Technology, where he would soon establish himself as one of the leading seismologists in the world, Landsberg stayed behind to supervise the Frankfurt-based Taunus Observatory until 1934. That year, Landsberg emigrated to Pennsylvania State College as the college's first meteorologist.[3] After serving in World War II as a meteorological analyst, he served as Director of the Air Force Geophysics Directorate from 1951 to 1954, followed by the Directorship of the Department of Climatology within the U.S. Weather Bureau from 1954 to 1967. Soon thereafter, he was offered a professorship at the Institute for Fluid Dynamics and Applied Mathematics at the University of Maryland, where he served until his retirement in the mid-1970s.

Landsberg also served in various administrative roles within the American and international geophysics community. He served as the President of the American Geophysical Union from 1968 to 1970, President of the American Institute of Medical Climatology from 1969 to 1980, as well as serving on the World Meteorological Organization (WMO) Commission for Climatology from 1970 to 1978. As the associate editor of the *Journal of Meteorology* during the 1950s, his editorship of the *Advances in Geophysics* from 1952 and 1977, editor-in-chief of the *World Survey of Climatology* from 1964 to 1985, as well as chairman of the publication committee for the International Society of Biometeorology from 1960 until his death in 1985, he was instrumental in strengthening the discipline of climatology during the post-World War II period.

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[2] "Beno Gutenberg," *Complete Dictionary of Scientific Biography*, Vol. 5; "Beno Gutenberg," *Science in the Early Twentieth Century: An Encyclopedia*, ABC-CLIO

[3] Unfortunately, evidence is insufficient to establish the specific reasons he left Frankfurt. However, one may be justified in suspecting that he left due to the cultural and political changes taking place in Germany during the 1930s. For further discussion of scientific research in Nazi Germany, see Macrakis, Kristie. *Surviving the Swastika: Scientific Research in Nazi Germany* (Oxford: Oxford University Press, 1993); Walker, Mark. *Nazi Science: Myth, Truth, and the German Atomic Bomb* (Cambridge; Perseus Books Group, 1995). For a historical account of the emigration of German scientists, see Deichmann, Ute. *Biologists Under Hitler* (Cambridge: Cambridge University Press, 1996); Medawar, Jean and David Pyke. *Hitler's Gift: The True Story of the Scientists Expelled by the Nazi Regime* (New York: Arcade Publshing, 2013);

For his efforts, Landsberg was praised frequently by his colleagues and the American government. He received the Department of Commerce's Gold Medal for accomplishments in the field of meteorology and climatology,[4] was esteemed as the "Foremost Climatologist of the World" in April 1978 by the World Meteorological Organization, and finally awarded the prestigious National Medal of Science by President Reagan in 1985.[5] Two months after his death in December 1985, Landsberg's contributions to climatology were memorialized at the University of Maryland, College Park, the contents of which were later published.[6] For those who knew him, he appeared to embody the highest ideals within the climatological community. "There has been at least one person who has played a fundamental role in the development of climatology in this century, namely Helmut Landsberg," stated Swedish meteorologist, C.C. Wallen.[7] Joseph Smagorinsky, one of the most respected modelers of the 1970s and 80s, also noted that Landsberg "played an important spiritual role as a leader and as a scientist."[8]

While younger by almost twenty years, and therefore had not acquired a comparable reputation as Landsberg, physicist S. Fred Singer's academic credentials were strong. While pursuing his Ph.D. in physics at Princeton University during the late 1940s, he worked as a physicist at Johns Hopkins University until landing a position as the scientific liaison in London

[4] The Department of Commerce Gold Medal is considered the highest honor presented by the U.S. Secretary of Commerce, and was started under Secretary Charles Sawyer in 1949.

[5] He was a fellow of the following scientific organizations: Royal Meteorological Society, American Academy of Arts and Sciences, American Association for the Advancement of Science, American Geophysical Union, American Meteorological Society (AMS), and the Washington Academy of Sciences.

[6] Baer, Ferdinand, "Symposium on Climate in Memory of Helmut E. Landsberg 10 February 1986, College Park, Maryland," *Bulletin of the American Meteorological Society* 67 (December 1986): 1493-1500; Baer, F., Catfield, N.L., Mitchell, J.M., eds. *Climate in Human Perspective: A Tribute to Helmut E. Landsberg* (Boston: Kluwer Academic Publishers, 1991); also see a book review by Rudulf Brazdil and William Morgan, *GeoJournal* 27 (July 1992): 319-320. Further insights into his life can be gained by referring to the following: Baer, Ferdinand, "Helmut E. Landsberg," *Memorial Tributes: National Academy of Engineering* 5 (1992): 152-57; Munn, R.E., "Obituary Professor Helmut Landsberg," *Archives for Meteorology, Geophysics, and Bioclimatology* 36 (September 1986): 381-82; Murray, Mitchell, "Helmut Landsberg: Climatologist Extraordinary," *Weatherwise* 39 (October 1986): 254-61; Schneider, Stephen, "Editorial: Three Essays of Rememberance on the Life of Helmut Landsberg," *Climatic Change* 9 (December 1986): 259; "The Life of Helmut E. Landsberg," *Bulletin of the American Meteorological Society* (February 2006): 236-37

[7] Carl Christian Wallen, Interview 1995 by Gordon Cartwright, p. 29

[8] Smagorinsky, Joseph, "Climatology's Scientific Maturity," within *Climate in Human Perspective: A Tribute to Helmut E. Landsberg* (Kluwer, 1991)

under the auspices of the Office of Naval Research in 1950. After three years, he became a professor of physics at the University of Maryland while serving as the Director of the Center for Atmospheric and Space Physics until 1964. Meanwhile, he was involved in early meetings that ultimately resulted in one of the most ambitious scientific endeavors of the 20th century -- the International Geophysical Year, which took place between 1957 and 1958.[9] Later, he became the first director of the U.S. Weather Bureau's National Weather Satellite Center from 1962 to 1964, followed by a three-year stint as Dean of the School of Environmental and Planetary Sciences at the University of Miami.

Like Landsberg, Singer was also given ample experience within the federal scientific apparatus. Given his emerging reputation within political circles in Washington during the late 1950s and 1960s, he took a leave of absence in 1967 from the University of Miami to serve as the Deputy Assistant Secretary for Scientific Programs and Water Pollution Control within the U.S. Department of Interior. Like "conservative conservationist" Russell Train, who served as Under Secretary of Interior between 1969 and 1970 for Secretary of Interiors Stewart Udall (1961-1969) and Joseph Hickel (1969-1970), Singer witnessed the dramatic tensions that were beginning to envelop environmental politics; he believed that the conservation of the environment needed to be balanced with an acute appreciation for the growing needs of an industrialized nation like the United States.[10]

While Singer was never positioned at the highest levels of political authority within Washington bureaucracy, many of his peers considered him a capable administrator within the scientific community. Orson Anderson, editor of the *Journal for Geophysical Research*, noted

[9] Singer was present at the first dinner party in 1950 where discussions of the IGY began. Others included James Van Allen and geophysicist Lloyd Berkner. For further discussion of Singer, see Needell, Allan. *Science, Cold War and the American State* (Washington, D.C.: Smithsonian Institution, 2000): pp. 325-328

[10] Flippen, J. Brooks. *Conservative Conservationist: Russell E. Train and the Emergence of American Environmentalism* (Baton Rouge, LA: Louisiana State University Press, 2006). Also, see Train, Russel. *Politics, Pollution and Pandas: An Environmental Memoir* (Washington, D.C.: Island Press, 2003)

Singer's "rare quality of being able to produce rapid changes in a complicated administrative structure."[11] Willard Libby, an American physicist and chemist, also described Singer as a "first rate scientist," a "world renowned geophysicist," and one who had "considerable experience in scientific administration."[12] Like many of his peers, his professional life encompassed both experience within the federal government as well as within the academic scientific community.

Born in 1913, geophysicist Philip Hauge Abelson also served as one of the most credentialed American scientists of the post-World War II period. He received both baccalaureate and master's degrees in chemical engineering and solid state physics from Washington State College in 1933 and 1935, respectively, followed by a Ph.D. in nuclear physics at the University of California-Berkeley in 1939. After working with some of the most brilliant minds in nuclear physics during the 1930s at Berkeley, including J. Robert Oppenheimer, the leader of the Manhattan Project during World War II, Abelson received a position at the Department of Terrestrial Magnetism (DTM) of the Carnegie Institution of Washington. There, he developed a robust theoretical knowledge of nuclear physics and chemistry. By 1953, he was appointed director of the Carnegie Institution's Geophysical Laboratory. Alongside his professional research activities, his administrative efforts were significant. While he was editor of *Science* between 1962 and 1984, he became President of the Carnegie Institution in 1978.[13] As a testament to his influence within the American scientific community, President Reagan awarded Abelson the National Medal of Science in 1987.[14]

Not affiliated with the geophysics community, biochemist Philip Handler was an influential statesman of science after World War II. He received his B.S. from the City College

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[11] Anderson to Maurice Stans, 25 September 1970, Box 27, S. Fred Singer Papers
[12] Libby to Maurice Stans, 5 October 1970, Box 27, S. Fred Singer Papers
[13] "Carnegie Institution Names Abelson," *Science* 168, 3933 (15 May 1970): p. 803
[14] Teramani, Jeffrey, "Abelson Receives National Medal of Science," *Science* 237, 4815 (7 August 1987): p. 661

of New York in 1936, and received his Ph.D. from the University of Illinois in 1939. Subsequently, he landed a tenure-track position within the Duke University School of Medicine, and at the age of thirty five became chairman of the Department of Biochemistry. Elected to the National Academy of Sciences in 1964, he later became its president in 1968.[15] During the 1950s through the 1970s, he was a member of the President's Science Advisory Committee (PSAC), served on various committees within the National Institutes of Health, the National Science Foundation, and National Academy of Sciences, and served on various boards with the Rockefeller University, the Hebrew University of Jerusalem and the Weizmann Institute of Science. He was a member of many scientific organizations, including the American Philosophical Society, and -- like Abelson and Landsberg -- received the National Medal of Science in 1981.[16]

Born the same year as Singer, in 1924, Frank Press received his B.S. from the City College of New York in 1944 (eight years after Handler), followed by an M.A. and Ph.D. in Geophysics from Columbia University in 1946 and 1949, respectively. Following professorships at Columbia and the California Institute of technology from the late 1940s to the mid-1960s, he transferred to the Massachusetts Institute of Technology in 1965. After serving as chair of the Department of Earth and Planetary Sciences at MIT from 1965 to 1977, he was asked by President Carter to serve as science advisor and Director of the Office of Science and Technology Policy. From the 1950s to the 1970s, he was a member or consultant on a number of scientific committees and official delegations pertaining to nuclear power and weaponry, served in the Presidential Science Advisory committee (PSAC) from 1961 to 1964, and served as

[15] Nelson, Bryce, "Philip Handler: National Academy Nominates a Worldly 'High Priest'," *Science* 162, 3857 (29 November 1968): pp. 981-984;
[16] Holden, Constance, "Handler Receives Medal of Science," *Science* 214, 4520 (30 October 1981): p. 526

President of the American Geophysical Union (1972-74) and the Seismological Society of America (1962-1963).

All told, Abelson, Press, Handler, Landsberg, and Singer established themselves after World War II as very talented scientific administrators and researchers. By virtue of their standing, they may be considered members of what Michael Polanyi calls a "republic of science" in that they also attempted to use their influence to define the norms and values to which scientists should ideally adhere.[17] Indeed, they had the means and reputation to do so. They all had close connections within Washington, D.C. elites, they tended to be respected as science administrators as well as professional scientists, they were members of the most prestigious scientific institutions within the United States, and they tended to have a broad perspective of the future direction of American science. Beginning in the 1960s and 70s, however, each had their own reasons to suspect that discussions of the environment were being guided by those who they believed had a less vested interest in the reputation and credibility of the American scientific community. Their concerns were in part a reflection of the extent of scientific uncertainties that pervaded environmental discussions.

UNCERTAINTIES OF GLOBAL ENVIRONMENTAL SCIENCE

The range of environmental problems discussed publicly during the late 1960s and early 70s was quite wide -- the prospect that the climate was changing, increased incidence of lung cancer due to mining practices, ground water contamination, the adverse effects of detergents on crops, the biological effects of nitrogen fertilizers, among many others.[18] One of the most important reasons for this increased attention to the environmental impacts of industrial activity

[17] See, Polanyi, Michael, "The Republic of Science: Its Political and Economic Theory," *Minerva* 1, 1 (1962): pp. 54-73
[18] For an extensive list of environmental problems as perceived in the late 1960s, see Lieber, Harvey, "Public Administration and Environmental Quality," *Public Administration Review* 30, 3 (May-June 1970): pp. 277-286.

was the rise of an environmental movement in the early 1960s.[19] Popular literature also revealed to the American public the destructive and variegated effects of industrial growth on ecosystems around the country, and served as an effective way to mobilize popular protests around these concerns.[20] The increased attention to the environment provided ample opportunity for scientists to disagree about the extent and nature of environmental risks, and whether they warranted public anxiety and fear.

There were two kinds of environmental problems that frequently spurred media attention. The first were the easily observable consequences of industrial activity, such as presence of pollution in the nation's waterways and urban centers.[21] For environmentalists, these were the key issues that deserved a forceful response from the federal government; corporate abuses of the environment could not go unchallenged. As a consequence of what appeared to be the blatant destruction of the natural beauty, environmental advocates began to act on behalf of the environment and pave a new path for the betterment of humanity.

Other environmental problems were those that only scientists -- through their expertise, knowledge, and utilization of very sensitive monitoring equipment -- could hope to identify and understand. On the one hand, there was an assortment of *micro*-environmental problems that

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[19] For historical accounts of the environmental movement, see Hays, Samuel, "The Environmental Movement," *Journal of Forest History* 25, 4 (October 1981): pp. 219-221; Rome, Adam. *The Bulldozer and the Countryside: Suburban Sprawl and the Rise of American Environmentalism* (Cambridge: Cambridge University Press, 2001); Shabecoff, Philip. *A Fierce Green Fire: The American Environmental* Movement (Washington, D.C.: Island Press, 2003); Dunlap, Thomas. *DDT: Scientists, Citizens, and Public Policy* (Princeton: Princeton University Press, 1981); Gottlieb, Robert. *Forcing the Spring: The Transformation of the American Environmental Movement* (Washington, D.C.: Island Press, 2005); Rome, Adam, "'Give Earth a Chance': The Environmental Movement and the Sixties," *The Journal of American History* (September 2003): pp. 525-554.

[20] For a contemporary account of tactics used by environmentalists to stall or prevent environmentally destructive activities, see Mitchell, John and Constance Stallings, eds. *Ecotactics: The Sierra Club Handbook for Environmental Activists* (New York, 1970)

[21] For accounts of polluted waterways, for instance, see Laycock, George. *The Diligent Destroyers* (New York: Ballantine Books, 1970); Graves, John. *Goodbye to a River* (New York: Ballantine Book, 1960); Fuller, John. *We Almost Lost Detroit* (New York: Readers Digest Press, 1975); Teal, John and Mildred Teal. *Life and Death of the Salt Marsh* (New York: Ballantine Books, 1969). "Halt Pollution in Fox River: Attempt to Stop Pollution," *Chicago Tribune* (20 February 1966): p. E1; "Ohio River Pollution," *Wall Street Journal* (1 April 1969): p. 10; "State River Pollution Problem hits Upper Reaches of Charles," *Boston Globe* (19 August 1962): p. 9; "Industry Gets Much of Blame in Cuyahoga River Pollution: But it Contends Others Might Be At Fault," *Chicago Tribune* (28 August 1967): p. 1. For a historical account of the Cuyahoga River, see Stradling, David and Richard Stradling, "Perceptions of the Burning River: Deindustrialization and Cleveland's Cuyahoga River," *Environmental History* 13, 3 (July 2008): pp. 515-535.

manifested in increased cancer rates, for instance; they were hidden because Americans could not see directly observe with their own eyes the consequences of complex and interrelated ecological threats on a local level. One of the most vibrant national discussions emerged when trained biologist Rachel Carson wrote her 1962 book, *Silent Spring*, in which she described the ecological and biological threats of an indiscriminate use of the pesticide DDT.[22] Additionally, the immediate years after World War II led to increased scientific and public attention to the perceived environmental hazards of nuclear fallout.[23]

On the other hand, a range of atmospheric hazards appeared to threaten not just human health on a local scale but also entire global ecosystems necessary to sustain life itself. They were highly complex scientific problems and their long-term implications on human welfare were uncertain -- issues such as the effects of industrial output on the global atmosphere. While it was clear that the efforts of the environmental movement yielded many legislative success stories regarding local and easily observable pollution, including the groundbreaking National Environmental Policy Act (NEPA) signed by President Nixon in 1970, global problems appeared more fraught with difficulties regarding their proper framing within the public sphere.[24] As environmental historian Samuel Hays attests more broadly about the nature of environmental science and uncertainty,

> The overriding experience of environmental science was the degree to which much of what one wished to understand was not yet known. Each advance in knowledge seemed to expand what was not known more rapidly than it did what was known; society seemed

[22] Carson, Rachel. *Silent Spring* (New York: Houghton Mifflin Company, 1962)

[23] Rothschild, Rachel, "Environmental Awareness in the Atomic Age: Radioecologists and Nuclear Technology," *Historical Studies in the Natural Sciences* 43, 4 (September 2013): pp. 492-530; Bruno, Laura, "The Bequest of the Nuclear Battlefield: Science, Nature, and the Atom during the First Decade of the Cold War," *Historical Studies in the Physical and Biological Sciences* 33, 2 (2003): pp. 237-260; Lutts, Ralph, "Chemical Fallout: Rachel Carson's Silent Spring, Radioactive Fallout, and the Environmental Movement," *Environmental Review* 9, 3 (Autumn, 1985): pp. 210-225; Hacker, Barton, "Radiation Safety, the AEC, and Nuclear Weapons Testing," *The Public Historian* 14, 1 (Winter, 1992): pp. 31-53; Kott, Carolyn, "The Origins of the American Scientific Debate over Fallout Hazards," *Social Studies of Science* 9, 4 (November 1979): pp. 403-422

[24] Clark, Ray and Larry Canter, eds. *Environmental Policy and NEPA: Past, Present, and Future* (Boca Raton, FL: St. Lucie Press, 1997)

faced with escalating ignorance, adding a constant frontier aspect to almost every facet of environmental science.[25]

Unlike locally observed pollution, the uncertainties of global environmental problems provided a fertile soil in which genuine disagreements took place over the proper way to publicly communicate with the general public about future dangers.[26] These global problems were especially disconcerting because they seemed to represent the gravest threats to humankind, and therefore provided ample material on which the public and the media could speculate about future catastrophe -- what historian of science Jacob Hamblin called a "catastrophic brand of environmental thinking."[27]

Among the three problems listed above, the prospect of a human-induced climate change was complex in that it entailed many important uncertainties and yet posed what appeared to be the greatest risks to humankind.[28] For this reason, scientists sought to understand and explain the global climate system in more detail, and whether greenhouse emissions were influencing the heat balance of the earth. By understanding the mechanisms of climatic change, scientists would be in a better position to speak credibly and publicly about the implications of such changes for societies around the world. From changes in the Earth's orbit, to changes in oceanic circulation, to the role of volcanic dust, scientists from a variety of disciplines since the 19th century promoted theories that seemed to explain what they considered the available evidence, or at least some of the evidence. However, no one theory was robust enough during the 1960s and 1970s to

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[25] Hays, Samuel. *Beauty, Health, and Permanence: Environmental Politics in the United States, 1955-1985* (Cambridge: Cambridge University Press, 1987): p. 252

[26] While risk perception appeared to start with the seminal work of Mary Douglas during the late 1970s and 80s, research of cultural biases conducted in the early 1990s seems most relevant to later scholarship of climate change risks. See, Dake, Karl, "Orienting Dispositions in the Perception of Risk: An Analysis of Contemporary Worldviews and Cultural Biases," *Journal of Cross-Cultural Psychology* 22, 1 (1991): pp. 61-82; Wildavsky, Aaron and Karl Dake, "Theories of Risk Perception: Who Fears What and Why," *Daedalus* 119, 4 (Fall 1990): pp. 41-60; Thompson, M, Ellis, R., and Wildavsky, A. *Cultural Theory* (Boulder, CO: Westview, 1990); Plough, Alonzo and Sheldon Krimsky, "The Emergence of Risk Communication Studies: Social and Political Context," *Science, Technology and Human Values* 12 (Summer-Autumn, 1987): pp. 4-10

[27] Hamblin, Jacob. *Arming Mother Nature: The Birth of Catastrophic Environmentalism* (Oxford: Oxford University Press, 2013): p. 8.

[28] Hulme, Mike, "Reducing the Future to Climate: A Story of Climate Determinism and Reductionism," *Osiris* 26, 1 (2011): pp. 245-266

become the principal explanation for changes in the global climate system, and therefore scientists were never in agreement about the implications of such changes.[29]

By the 1970s and 1980s, discussions over climate change evolved to more concerted efforts to use computer-based models to understand whether the warming effects of carbon dioxide and other greenhouse gases would overwhelm any cooling effects of aerosol particulates in the atmosphere, or vice versa.[30] As noted by climatologist Helmut Landsberg in 1969: "It is as yet uncertain which effect is gaining the upper hand. If suspensions prevail, the globe will cool; if the carbon dioxide accumulates, it would heat up."[31] Nonetheless, computer-based models seemed to offer the only credible solution to understand the future climatic system; if scientists could forecast the future by incorporating various physical parameters into their models, they could credibly speak about the role of human activity on changing the global climate. However, even after models used for meteorology were adapted to the needs of climate science in the 1960s, these earliest model results were still riddled with uncertainties.[32] Accounting for the role of ocean and cloud dynamics was particularly problematic for the earliest models.[33] Given the myriad of uncertainties in climate forecasting -- what climate modeler W.W. Kellogg called the "cascade of uncertainty" -- scientists were ambivalent over how best to engage the general public regarding what appeared to be the potential hazards of climate.[34]

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[29] For a chart detailing all of the various theories offered to explain climatic changes since the 19th century, see Fleming, James. *Historical Perspectives on Climate Change* (Oxford: Oxford University Press, 1998): p. 109

[30] For a brief discussion of the reasons why the analogy of the earth's climate system to a greenhouse is problematic, see Trenberth, Kevin, ed. *Climate System Modeling* (Cambridge: Cambridge University Press, 1992): pp. 28-29

[31] Landsberg to Russell Keim, 31 January 1969, Series 2.1, Box 1, Papers of Helmut Landsberg, University of Maryland, College Park, MD (hereafter known as PHL)

[32] For a discussion of computer modeling in meteorology during and after World War II, see Harper, Kristine. *Weather by the Numbers: The Genesis of Modern Meteorology* (Cambridge: MIT Press, 2008)

[33] Mearns, Linda, "Quantification of Uncertainties of Future Climate Change: Challenges and Applications," *Philosophy of Science* 77 (December 2010): pp. 988-1011

[34] Kellogg, W.W. and Robert Schware. *Climate Change and Society: Consequences of Increasing Atmospheric Carbon Dioxide* (Boulder, CO: Westview Press, 1981). For general discussions of scientific uncertainties within climate discussions during the 1970s, see Singer, S. Fred, ed. *Global Effects of Environmental Pollution* (New York: Springer-Verlag, 1970)

Discussions over other uncertain environmental threats contributed to tensions within the scientific community regarding how best to engage the public. Beginning in the mid-1960s, some prominent geophysicists -- Lloyd Berkner and L.C. Marshall -- began to suspect that industrial waste could cause a reduction in the amount of oxygen in the atmosphere. Based on their broader studies of the evolution of planetary atmospheres, they reasoned that pollutants could potentially interrupt the ability of marine microorganisms to convert sunlight into oxygen and therefore produce enough oxygen to breathe. While the basic chemistry seemed sound in theory, an empirical assessment of global oxygen levels was unavailable and, therefore, most scientists were reluctant to engage the public about a threat that may never materialize. As such, an exploratory research program was initiated to measure global oxygen levels to adjudicate whether suspicions about the scope and urgency of the issue had any scientific merit. As it turned out, those who attempted to raise public awareness of the threat level turned out to have been premature; the threat was plausible but did not warrant public engagement. Another matter that prompted discussions within the scientific community was whether the implementation of a fleet of supersonic transports during the early 1970s would cause changes in the ozone layer, as well as in the global climate system. Congressional hearings were held regarding its potential environmental risks, and the issue -- primarily due to a lack of sufficient empirical data on upper atmospheric chemistry -- caused divisions within the scientific community over how best to publicly communicate the risks to humanity. Ultimately, these case studies offers an opportunity to understand why scientists disagreed over the proper manner to engage the public given existing uncertainties, and how these environmental discussions reflected broader changes in the reputation of American science.

AMERICAN SCIENCE UNDER ATTACK

Beginning in the late 1960s, many well-credentialed scientists grew concerned that claims of future environmental disaster -- the proponents of which appeared to overlook important uncertainties -- reinforced what many perceived to be a broader political and cultural malaise sweeping the country. Given public hostility to America's involvement in the Vietnam War, Watergate, and what appeared to be a science-driven defense industry, the general public grew suspicious of the scientific establishment because it appeared that they were not accountable to the needs and desires of the general public.[35] In 1975, Congressman William Hungate (D-MO) solemnly reflected upon retirement that politics “has gone from an age of ‘Camelot’ when all things were possible to the age of ‘Watergate’ when all things are suspect.”[36] During his term as president in the late 1970s, Jimmy Carter also predicted that an “erosion of our confidence in the future is threatening to destroy the social and political fabric of America.”[37] One historian went so far as to claim that “the mood of the late 1970s was in important ways the gloomiest in late twentieth-century American history.”[38] For some American scientists, this state of affairs influenced the reputation and authority of the scientific community. Ultimately, critics perceived the "scientific estate" as uncontrolled in its ability to maneuver through the halls of power to influence decisions important for the general welfare of the population.[39] Some scientists believed that this gloomy malaise regarding America's future appeared entirely unique and unprecedented given prior decades.

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[35] The physical sciences were not the only ones to fall within the relationship between Cold War defense and science. Rohde, Joy. *Armed with Expertise: The Militarization of American Social Research during the Cold War* (Cornell: Cornell University Press, 2013); Martini, Edwin. *Agent Orange: History, Science, and the Politics of Uncertainty* (Amherst, MA: University of Massachusetts Press, 2012); Solovey, Mark. *Shaky Foundations: the Politics-Patronage-Social Science Nexus in Cold War America* (New Brunswick, N.J.: Rutgers University Press, 2013)
[36] *Washington Post*, 7 July 1975; quoted in Mieczkowski (2005), p. 3.
[37] Quoted in Connelly, Matthew, “Future Shock: The End of the World as They Knew It,” within Niall Ferguson, Charles Maier, Erez Manela, and Daniel Sargent, eds. *The Shock of the Global: The 1970s in Perspective* (Cambridge: Belknap Press of Harvard University Press, 2010), p.346
[38] Patterson, James. *Restless Giant: The United States from Watergate to Bush v. Gore* (Oxford: Oxford University Press, 2005), p. 15. Patterson’s sentiments are matched within Mieczkowski, Yanek. *Gerald Ford and the Challenges of the 1970s* (The University of Kentucky Press, 2005)
[39] Price, Don. *The Scientific Estate* (Cambridge: Harvard University Press, 1965). Also, Lapp, Ralph. *The New Priesthood: The Scientific Elite and the Uses of Power* (New York: Harper & Row, 1965)

From the 1930s well into the 1960s, America was perceived as a place of grand expectations fueled by the wisdom of elites and experts.[40] Despite the cultural anxieties that lay at the heart of the Cold War and the inauguration of an atomic age in the mid-1940s, a "technological exuberance" and a faith in the power of science were instrumental in guiding policy and reassuring an already confident public.[41] Academic institutions were building sizeable research budgets, and the federal government had become a source of scientific research and funding for experts from a wide range of disciplines within the social, environmental, and physical sciences.[42] Heated competition with the Soviet Union increased federal investment for scientific research significantly, and federal agencies like NASA had become one of the leading recipients of the federal bounty.[43] Contrary to the skepticism and hostility experienced in the 1960s and 1970s, one historian simply detailed in his account of polio in mid-20th century America that science and technology "were riding high."[44]

By the 1970s, a faith in a better tomorrow began to crumble under the weight of these grand expectations. Historian Howard Segal, for instance, argues that 1) an endless stream of technology-related environmental crises, 2) repeated disappointments with nuclear power and other alleged technological panaceas, and 3) an emergent distrust of both public officials and

[40] Patterson, James. *Grand Expectations: The United States, 1945-1974* (New York: Oxford University Press, 1996); Halberstam, David. *The Best and the Brightest* (Ballantine Books, 1993)
[41] York, Herbert. *The Advisors: Oppenheimer, Teller, and the Superbomb* (San Francisco: W.H. Freeman and Company, 1976), p. ix.; Mackenzie, G. Calvin and Robert Weisbrot. *The Liberal Hour: Washington and the Politics of Change in the 1960s* (New York: The Penguin Press, 2008)
[42] Graham, Hugh. *The Rise of American Research Universities: Elites and Challengers in the Postwar Era* (Baltimore: Johns Hopkins University Press, 1997); Geiger, Roger. *To Advance Knowledge: The Growth of American Research Universities, 1900-1940* (Oxford: Oxford University Press, 1986); Kleinman, Daniel. *Politics on the Endless Frontier: Postwar Research Policy in the United States* (Durham: Duke University Press, 1995); Wilson, John. *Academic Science, Higher Education, and the Federal Government, 1950-1983* (Chicago: University of Chicago Press, 1983); England, James. *A Patron for Pure Science: The National Science Foundation's Formative Years, 1945-57* (Washington, D.C.: National Science Foundation, 1983). For historical account of federal research policy prior to 1940, see Dupree, Hunter. *Science in the Federal Government: A History of Policies and Activities to 1940* (Cambridge: Belknap Press of Harvard University Press, 1957); Westwick, Peter. *The National Labs: Science in an American System* (Cambridge: Harvard University Press, 2003); Leslie, Stuart. *The Cold War and American Science: The Military-Industrial-Academic Complex at MIT and Stanford* (New York: Columbia University Press, 1993); Smith, Bruce. *American Science Policy Since World War II* (Washington, D.C.: Brookings Institution, 1990)
[43] Conway, Erik. *Atmospheric Science at NASA: A History* (Baltimore: Johns Hopkins University Press, 2008); McDougall, Walter. . . . *the Heavens and the Earth: A Political History of the Space Age* (New York: Basic Books, 1985)
[44] Oshinsky, David. *Polio: An American Story* (Oxford: Oxford University Press, 2005), p. 91

technical experts combined to undermine Americans' historically optimistic outlook.[45] Not immune from this wave of cynicism and suspicion, scientists were no longer widely idealized: "whereas scientists generally had good press through the 1940s, 1950s, and the 1960s, their image as beneficial and omniscient was marred in later decades."[46] The experiences of nuclear physicist J. Robert Oppenheimer, for instance, contrasted sharply with the emerging everyday realities of the 1950s, 1960s, and especially the 1970s.[47] Historian P.D. Smith put it more dramatically: "In the twentieth century, scientists were raised up to be gods only to be cast down as devils."[48] Highlighting the importance of Eisenhower's 1961 farewell message that cautioned America's embrace of what he called the "scientific-technological elite," ecologist and environmental activist Barry Commoner fearfully warned that granting scientists a special competence in society would have ended up shielding scientists from public judgment. Therefore, Commoner insisted, scientists posed a threat to democracy if they did not consider the public in their scientific activities.[49]

For some within the highest echelons of the American science establishment, living through this dramatic shift was jarring. One of the most illustrative examples of this transition resides in the experiences of Philip Abelson, who served as editor of the most respected American journal of science published by the American Association for the Advancement of

[45] Segal, Howard. *Technological Utopianism in American Culture* (Syracuse: Syracuse University Press, 2005), p. 165.
[46] Badash, Lawrence. *A Nuclear Winter's Tale: Science and Politics in the 1980s* (Cambridge: The MIT Press, 2009), p. 92
[47] Bird, Kai and Martin Sherwin. *American Prometheus: The Triumph and Tragedy of J. Robert Oppenheimer* (New York: Vintage Books, 2006); Thorpe, Charles. *Oppenheimer: Tragic Intellect* (Chicago: University of Chicago Press, 2006)
[48] Smith, P.D. *Doomsday Men* (New York: St. Martin's Press, 2007): p. xxi
[49] For a transcript of Eisenhower's farewell address, see http://mcadams.posc.mu.edu/ike.htm (last accessed June 15, 2014). Commoner, Barry. *Science and Survival* (New York: Viking Press, 1966), p. 108, and quoted in Egan, Michael. *Barry Commoner and the Science of Survival: The Remaking of American Environmentalism* (Cambridge: The MIT Press, 2007), p. 86Also, see Bury, J.B. *The Idea of Progress* (New York: Macmillan, 1932), xxii, xx; Beard, Charles. *A Century of Progress* (New York: Harper and Brothers, 1933), 3-19. Even broader, there exists an inherent historical tension between experts and democracy; see, Shapin, Steven, "Foreward to the 1999 Edition," within Greenberg, Daniel. *The Politics of Pure Science* (1999), pp. xv-xxii; Turner, Stephen, 'What is the Problem with Experts?', *Social Studies of Science* 31, 1 (February 2001): pp. 123-149; Brown, Mark. *Science in Democracy: Expertise, Institutions, and Representation* (Cambridge: MIT Press, 2009)

Science (AAAS).[50] In 1984, after twenty two years as editor since 1962, some scientists praised what they saw as Abelson's administrative acumen and strong leadership during this tumultuous time, as well as his adept coverage of, and interest in, an entire range of scientific findings and discoveries. According to his successor, biochemist Daniel Koshland, Jr., Abelson had the "rare gift" to discern the significant scientific discoveries from the trivial, and appropriately discriminate between major shifts and the "ephemeral fashions of science." In particular, he "harmonized in the same magazine two potentially discordant goals, journalism and scholarship, so that the magazine has never succumbed to the meretriciousness of sensational journalism or the desiccation of overspecialized scholarship."[51]

The observation that Abelson balanced what appeared to be two extremes -- overspecialized science and meretricious journalism -- was not only represented in the kinds of articles he allowed to be published in *Science*. Abelson's editorship also appeared balanced in that he exemplified what some scientists believed to be the appropriate role of scientists in American society. As Landsberg wrote in 1984, Abelson's "admirable scientific statesmanship has been an inspiration to me and many of my colleagues. Your balanced editorials in 'Science' have been a steadying influence in an era when advocacy has become a bad habit in some parts of the scientific community."[52] As later discussed, Landsberg believed that Abelson's leadership prevented what he believed to be the influence of scientific experts who appeared to expand their professional activities into political advocacy.

The editorship of Abelson appeared all the more significant given what William Carey, AAAS officer, called a significant "transition" within American science during the late 1960s

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[50] "Reception and Prize Honors Abelson, "*Science* 228 4705 (14 June 1985): p. 1340
[51] Koshland Jr., Daniel, "Philip Hauge Abelson," *Science* 227, 4682 (4 January 1985): p. 13.
[52] Landsberg to Abelson, 2 May 1984, Series 2.2: Box 1, Papers of Helmut Landsberg, University of Maryland, College Park, MD. Hereafter, Landsberg's papers will be referred to as the acronym, PHL.

and 1970s. This was an era, he argued, when national budget cuts to scientific research appeared symptomatic of a broader cultural antipathy toward science, when what he called "ethical issues of baffling complexity" troubled the "scientific conscience."[53] In this light, the reason Landsberg and Koshland praised Abelson's "balanced" editorship was not merely an opportunity to recognize his achievements; they also used it as an opportunity to reflect on what appeared to be the rapid degradation of established science in American society.

For Abelson, the transformation was all too clear. As early as the late 1960s, he implored members of the scientific community to take the reigns over their professional reputations and prevent what appeared to be the impulsive destabilization of American science. "Scientists can stop the mindless dismantling of American science," he once argued. "They have the wit and energy to develop the political clout necessary to do the job, and they should get about that business."[54] In 1970, he was even more concerned with what appeared to be a major backlash against the privileged position of scientists in American society. The time had come, he believed, for academic scientists especially to regain their prestige, re-reduce the amount of "glamour" within American science by focusing on the hard, behind-the-scenes labor necessary to produce reliable results, and remind policy makers and the general public how "dependent" they were on what he considered to be sound and dispassionate science over the long-term.[55]

These changes in the reputation of the American scientific establishment reflected a much larger history about the ambivalent role of expertise in American society. Relying on expert knowledge from above may be conceived as inherently undemocratic; how can one reasonably subordinate their own reason and logic for someone else who claims to be an expert on a particular set of issues? As the noted observer of early American culture, Alexis de Tocqueville,

[53] Carey, William, "A Transition for Science," *Science* 221, 4605 (1 July 1983): p. 7
[54] Abelson, Philip, "Political Action in Behalf of Science," *Science* 165, 3900 (26 September 1969): p. 1315
[55] Abelson, Philip, "Troubled Times for Academic Science," *Science* 168, 3931 (1 May 1970): p. 525

observed in the early 19th century: "Each American has but recourse to the individual effort of his own reason. . . Each man thus retreats into himself from where he claims to judge the world." As he remarked further, each American citizen, "perceiving in not a single person in their midst any signs of undeniable greatness or superiority, constantly return to their own rationality as to the most obvious and immediate source of truth."[56]

To make matters appear more difficult for elite scientists who believed that their views were vital, the public appeared to demand evidence of social responsibility apart from scientific research. It appeared as if the importance of acknowledging and even incorporating personal values into decision making were supplanting an earlier vision of a distant and dispassionate expert. No longer willing to stand idly by while irresponsible and authoritarian science and technology threatened humanity's very existence – the use and potential devastation of nuclear war was a persistent image -- the *values* of scientists became a focal point of discussion, all under the belief that values were a potential hedge against unhindered and potentially catastrophic progress.[57] It seemed as if the public was retreating from an earlier idolization of expertise, and it was up to the elite few to uphold what they deemed to be the values and norms that marked a professional scientist.

DIVERGING NORMS OF PUBLIC COMMUNICATION

One of the most important causes of divisions within the American science community was disagreements over the manner in which scientists engaged with the general public. Within

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[56] See, Bevan, Gerald, trans. *Democracy in America and Two Essays on America* by Alexis de Tocqueville (Penguin Books, 2003), p. 494

[57] Not coincidentally, scholarship began to emerge throughout the 1980s and into the 21st century regarding the interconnections between values and science, immediately following my period of interest. See, Graham, Loren. *Between Science and Values* (New York: Columbia University Press, 1981); Lowrance, William. *Modern Science and Values* (Oxford: Oxford University Press, 1985); Ruse, Michael, "Biological Science and Feminist Values," *Proceedings of the Biennial Meeting of the Philosophy of Science Association* (1984): 525-542; Rooney, Phyllis, "On Values in Science: Is the Epistemic/Non-Epistemic Distinction Useful?" *Proceedings of the Biennial Meeting of the Philosophy of Science Association* (1992): 13-22; Koertge, Noretta, "Science, Values, and the Value of Science," *Philosophy of Science* 67 (September 2000): 1-27; Elliott, Kevin, "Direct and Indirect Roles for Values in Science," *Philosophy of Science* 78, 2 (April 2011): 303-324

her 1977 book, *The Visible Scientists*, Rae Goodell argued that social ills and increased public skepticism toward scientists "put pressure on science to update its antiquated concepts of how much to tell the public, when, and how. In short, dramatic changes in science and in communication are forcing changes in science communication, in the process, the kind of scientist who communicates."[58] These scientists were visible because they dealt publicly with topics that were often highly controversial, and they often sought ways to engage with television personalities and news reporters to influence how the general public imagined the seriousness of environmental concerns. As one reviewer of Goodell's book noted, these individuals were "prone to take controversial stands and use the personal traits that have made them popular to advance their side off the argument."[59]

Like Goodell, sociologist of science Dorothy Nelkin also began to publish articles on what she believed to be changing norms of social responsibility within American science.[60] On the one hand, she argues, were those experts who sought political neutrality and autonomy by restricting their energies only to scientific research. These were the individuals like Landsberg and Abelson, those members of the elite scientific establishment who believed that they had the wherewithal to guide popular discussions of the environment. Other credentialed scientists departed from what they interpreted to be this rather narrow and constrained vision, and began to look elsewhere for opportunities to respond to what were deemed to be serious environmental threats. They sought to change how scientists understood their roles within society. As Nelkin

[58] Goodell named the following as the "visible scientists": Paul Ehrlich, Linus Pauling, Margaret Mead, B.F. Skinner, Carl Sagan, Barry Commoner, and William Shockley. Goodell, Rae. "The Visible Scientists," *The Sciences* 17, 1 (January-February 1977): pp. 6-9. Also, see *The Visible Scientists* (Boston: Little & Brown, 1977)
[59] Basalla, George, "Review: The Visible Scientists," *Science* 197, 4307 (September 2, 1977): pp. 977-978
[60] Nelkin, Dorothy, "Changing Images of Science: New Pressures on Old Stereotypes," *Newsletter on the Program on Public Conceptions of Science* 14 (January 1976): pp. 21-31

argued, "the greatest pressure on reshaping the internal organization of science comes from the increased involvement of scientists in political and policy-making activities."[61]

Some ecologists, especially, believed that their responsibilities extended beyond the pursuance of basic research insulated from the public stage. Given what appeared to be the fervor of the environmental movement, Nelkin documents how the Ecological Society of America, for instance, outlined a course of action wherein ecologists would no longer be "comfortably aloof from public responsibility."[62] While many members of the ecological community did not accept this idea wholesale, and indeed this idea created significant tensions among professional ecologists, Nelkin's work -- in addition to Goodell -- provides much justification to suspect that these tensions within American science were real to those who participated.[63]

Vehement believers in what sociologists of scientists call "organized skepticism," defined by sociologist Robert Merton as the process by which scientific claims-making is checked by rigorous and structured scrutiny by other experts, many experts within the scientific establishment challenged those whom they felt engaged the public prematurely as advocates.[64] Those experts addressed in this dissertation -- Landsberg, Singer, Handler, and Abelson, and Press -- grew particularly frustrated when bypassing the scientific vetting process fueled what they saw as the media's fixation on perpetuating doomsday ("alarmist") prophecies, thus rendering the task of establishing what they deemed to be credible scientific claims that much

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[61] Nelkin (1976): p. 24.

[62] Nelkin, Dorothy, "Ecologists and the Public Interest," *The Heritage Hastings Report* 6, 1 (February 1976): pp. 38-44. Also, see a summary of the ESA report within Miller, Richard and John Reed, "Summary Report of the Ecology Study Committee with Recommendations for the Future of Ecology and the Ecological Society of America," *Bulletin of the Ecological Society of America* 46, 2 (July 1965): pp. 61-82

[63] For other articles by Nelkin, see Nelkin, Dorothy, "Scientists in an Environmental Controversy," *Science Studies* 1, 3/4 (October 1971): pp. 245-261; Nelkin, "Scientists and Professional Responsibility: the Experience of American Ecologists," *Social Studies of Science* 7, 1 (February 1977): pp. 75-95; Nelkin, "Scientists in an Adversary Culture: The 1970s," *Newsletter on Science, Technology, and Human Values* 24, (June 1978): pp. 33-39

[64] For a discussion of "organized skepticism" as a scientific norm, see Zuckerman, Harriet, "The Sociology of Science," within *Handbook of Sociology*, ed. Neil J. Smelser (Newbury Park: Sage, 1988): pp. 511-74

more difficult.[65] Public reticence to them was conceived as an insulating practice, a professional norm that could elicit better knowledge for the pursuance of ameliorative environmental policies and protecting one's identity as an expert over the long-term.

While not really technocrats, the experts discussed in this dissertation do reveal an attempt to position themselves as authoritative voices in popular understandings of environmental issues.[66] Instead of viewing their activities as acting on behalf of the American public (i.e. they were not elected officials), they viewed their activities as an attempt to restore the prestige of expertise. Without a sound scientific understanding of global environmental threats insulated from what they saw as hyperbolic rhetoric used within environmental politics by interest groups, the institutions and agencies responsible for decision making could erroneously implement policies that could be worse than the initial problem. For some experts discussed in this dissertation, their duty was to re-establish the credibility of scientists and counter if not entirely isolate those whom they believed to be irresponsible members of the scientific community. For them, establishing a middle ground meant restoring an older vision of expert scientist.

Indeed, for some within the scientific establishment, it appeared that this older vision of dispassionate inquiry was all but dead. Lewis Branscomb, Director of the National Bureau of

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[65] For a brief historical discussion of "prophets of doom" within the broader global environmental movement, see chapter four within McCormick, John. *The Global Environmental Movement: Reclaiming Paradise* (London: Belhaven Press, 1989). For more contemporary criticisms, see Grayson, Melvin and Thomas Shephard, Jr. *The Disaster Lobby: Prophets of Ecological Doom and Other Absurdities* (Chicago: Follett, 1973); Beckmann, Petr. *Eco-hysterics and the Technophobes* (Boulder, CO: Golem Press, 1973); Maddox, John. *The Doomsday Syndrome* (London: Macmillan, 1972)

[66] While the literature on the history of technocratic thought is too varied to include in a single footnote, historians may benefit from returning to scholarship that appeared in the 1970s and 80s. See Gunnell, John, "The Technocratic Image and the Theory of Technocracy," *Technology and Culture* 23, 3 (July 1982): pp. 392-416; Akin, William. *Technocracy and the American Dream: The Technocratic Movement, 1900-1941* (Berkeley: University of California Press, 1977); Segal, Howard. *Technological Utopianism in American* Culture (Syracuse: Syracuse University Press, 2005); Carlisle, Robert, "The Birth of Technocracy: Science, Society, and Saint Simonians," *Journal of the History of Ideas* 35, 3 (July-September 1974): pp. 445-464; McDougall, Walter, "Technocracy and Statecraft in the Space Age -- Toward the History of Saltation," *The American Historical Review* 87, 4 (October 1982): pp. 1010-1040; Maier, Charles, "Between Taylorism and Technocracy: European Ideologies and the Vision of Industrial Productivity in the 1920s," *Journal of Contemporary History* 5 (1970): pp. 27-61; Taylor, Peter, "Technocratic Optimism, H.T. Odum, and the Partial Transformation of Ecological Metaphor after World War II," *Journal of the History of Biology* 21, 2 (Summer 1988): pp. 213-244

Standards (NBS), gave a distinguished lecture to the American Association for the Advancement of Science (AAAS) on December 27, 1970. He explained the roots of what he perceived to be public frustration with technology and science. The public, he reasoned, had become isolated from opaque realities of technological change, and science -- "father of technology" -- had become a source of anxiety rather than a source of reassurance. Additionally, one vision of a scientific expert gave way to a new brand of expert, one that could integrate their subjective and personal values into their professional advisory activities to meet the needs of an anxious society. The "era of the expert advisor -- aloof from the nontechnical consideration and immune to challenge -- is over," he crisply observed.

> There was a time for the expert, perhaps a time when scientific vanity showed most. But no one is free of social bias, and, indeed, one man's social bias is another man's ethical principles. The scientist's principles must be involved, and his technical arguments must be made clear and persuasive. The emergence of the scientist as an active, responsible, if biased, citizen was a relatively radical idea a few years ago; this role is now more widely accepted.[67]

As environmental historian Samuel Hays similarly argues, "the real world of scientific choice gradually led some to think of scientists as a particular group of people, expressing particular values and views as a result of their training, experience, and personal predilections, rather than as neutral investigators, observers, and advisors."[68] For some, this was a problem that needed a solution.

ESTABLISHING A PARTISAN MIDDLE GROUND

My dissertation is partly informed by the work of historian Samuel Hays, and what he termed in his 1989 seminal work, *Beauty, Health, and Permanence,* to be a "middle ground" within environmental politics. He argues that it consisted of two groups: 1) scientists, technical

[67] Branscomb, Lewis, "Taming Technology," *Science* 171, 3975 (12 March 1971): pp. 972-977, quote on pp. 976-977
[68] Hayes (1987): pp. 329-362, quote from pp. 331-332

experts, and planning authorities and 2) heads of public and private media outlets and leaders of interest groups, who he termed "thought leaders." By the early 1970s, he argues, these two groups came together to position themselves between two antagonistic and adversarial extremes. On one end of the spectrum were environmentalists who tended to press their "case too far and too urgently."[69] From the perspective of technical professionals who sought to "manage" the environment via a consistent reliance on professional skills and specialized knowledge, environmental advocacy appeared too confrontational, impractical, and disruptive. As Hays articulates, some scientists and professional managers "viewed such advocacy with personal distaste and some degree of alarm."[70] On the other end of the spectrum were leaders in agriculture, labor, and industry who appeared to irresponsibly dismiss and ignore the environmental movement for economic motives. Rather than reinforce what they saw as the gridlock between what appeared to be reckless statements within environmental debates, scientists, planning authorities, and "thought leaders" (i.e. the middle ground) sought to occupy "a more neutral position in the spectrum of environmental controversy."[71]

Reminiscent of progressive reformers during the late 19th and early 20th centuries, Hays's technical managers thought that "managing" the environment meant relying on a vast array of experts within government and the private sector, all of whom were far more favorable to resolving these kinds of challenges than "the world of legislative and party politics."[72] With an interest in managing *sustainable* yields instead of the *preservation* of natural systems, he argued, these professionals had a "general belief that environmental advocates were pushing

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[69] Hays, Samuel and Barbara Hays. *Beauty, Health, and Permanence: Environmental Politics in the United States, 1955-1985* (Cambridge: Cambridge University Press, 1987), p. 392
[70] Hays (1987), p. 408
[71] Hays (1987), p. 410
[72] Hays (1987), p. 394. Also, see Hays, Samuel. *Conservation and the Gospel of Efficiency: The Progressive Conservation Movement , 1890-1920* (Cambridge: Harvard University Press, 1959); Andrews, Richard. *Managing the Environment, Managing Ourselves: A History of Environmental Policy* (New Haven, CT: Yale University Press, 1999)

forward their objectives farther and faster than the nation's resources would permit. Those resources would not sustain as much natural beauty, as high levels of health, as much independence and autonomy on behalf of future generations as environmentalists believed." The consequence of this reliance on experts, Hays argues, is that the middle ground gradually evolved to become its "own brand of environmental opposition" during the Reagan administration.[73]

There are both similarities and differences between the scientific experts discussed within this dissertation and the professional environmental managers discussed by Hays. Both groups vehemently objected to "prophets of doom" because they appeared to be too emotional in their characterization of environmental threats.[74] While this is an important similarity, the individuals discussed in this dissertation were members of a scientific elite, not technically-minded managers involved in the everyday decision making to resolve environmental problems. They were relatively unconcerned with directly mediating between environmental advocacy groups (e.g. the Sierra Club), technical professionals within government, and private experts within industry. They conceived of themselves above all else as scientific experts who adhered to a certain array of values and norms that marked them as authoritative and trustworthy. Their concerns were broader, more fixated on the cultural standards of conduct of scientists apart from

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[73] Hays (1987), p. 330

[74] David Sills, a sociologist with the Social Science Research Council, noted how many critics of the environmental movement objected to "alarmist" claims. Sills, David, "The Environmental Movement and Its Critics," Human *Ecology* 3, 1 (January 1975): pp. 1-41; Kaysen, C, "The Computer that Printed Out W*O*L*F," *Foreign Affairs* 50, 4 (1972): pp. 660-668; Maddox, John. *The Doomsday Syndrome* (New York: McGraw-Hill, 1972); Schnaiberg, A, "Politics, Participation, and Pollution: The Environmental Movement," within Walton, J. and Carns, D.E., eds. *Cities in Change: Studies on the Urban Condition* (Boston: Allyn and Bacon, 1973): pp. 605-627; Meier, R.L., "The Other Side of Pollution," within Pohlman, E., ed. *Population: A Clash of Prophets* (New York: New American Library, 1973): pp. 204-223; Neuhaus, R. *In Defense of People: Ecology and the Seduction of Radicalism* (New York: Macmillan, 1971); Burch, W.R., Jr. *Daydreams and Nightmares: A Sociological Essay on the American Environment* (New York: Harper and Row, 1971); Downs, A., "Up and Down with Ecology -- the 'Issue-Attention Cycle,'" *Public Interest* (Summer 1972): pp. 38-50. This tendency to criticize so-called prophets of doom persists in present debates over climate change. See, Bailey, Ronald. *Eco-Scam: The False Prophets of Ecological Apocalypse* (New York: St. Martin's Press, 1993); Davis, David. *Ignoring the Apocalypse: Why Planning to Prevent Environmental Catastrophe Goes Astray* (Westport, CT: Greenwood Publishing Group, 2007); Allitt, Patrick. *A Climate of Crisis: America in the Age of Environmentalism* (New York: The Penguin Press, 2014)

the grind of politics. If anything, they considered themselves mediators between the scientific community and the general public; they were intermediaries who sought to both maximize the reputation of scientists while minimizing anything that detracted from their prestige.

While my incorporation of the term "middle ground" is justified in that the term was used by the historical actors themselves, it comes with certain risks. First, situating oneself between what appear to be two opposing extremes is not, however, inherently virtuous or productive. In 1982, even physicist S. Fred Singer explicitly noted as much in spite of his advocacy of the term since the early 1970s (see Chapter 1). "It is important to point out that the middle view is not necessarily the correct one," he cautioned. "In scientific discussions . . . one cannot simply average the different views of scientists or even seek a consensus. More often than not, it has turned out that an extreme view was the correct one."[75] This was not an attempt to devalue the purpose of a middle ground-perspective within particular situations, particularly given the serious scientific uncertainties that pervaded environmental claims during the 1970s. However, his reflection does acknowledge that a middle ground perspective may be risky when it comes to gauging the urgency of future threats.

Second, portraying one's activities as a middle ground may appear dispassionate, but it may hide underlying motivations and belief systems. Certainly, those scientists who employed a "middle ground" perspective sought to convey their apparent sobriety and reasoned approach to environmental problems apart from politics and what they saw as hyperbolic. Nonetheless, their private and public correspondence is rife with highly passionate remarks about what they saw as the alarming influence of environmental advocates on popular perceptions of environmental challenges. They also frequently expressed their frustrations and antipathies toward some ecologists and strains of environmental thought. In spite of their colorful diatribes, however, one

[75] Singer, S. Fred, et al., "Future Climate," *Foreign Affairs* 61, 1 (Fall 1982): pp. 212-218, quote on p. 213

must not lose sight of how they characterized their efforts to maintain what they saw as a reasonable and legitimate perspective.

For these reasons, those discussed in this dissertation may more aptly be characterized as a "partisan middle ground." By partisan, I do not limit myself to its more common parlance of political or ideological allegiances (i.e. historical actor X is overly partisan in his support of the Republican Party, or to conservative ideology). This is an overly narrow and restrictive way to frame their activities and views, and would make it all too easy to reduce them to caricatures. As such, my employment of the term "middle" means three distinct things. First, their middle-ness was defined by a distinct appreciation for uncertainties within scientific data. Uncertainty, according to them, acted as a bulwark against what they envisioned as hasty policy decisions and appeared to be more consistent with what they saw as the state of science that existed at the time. Without appreciating and communicating uncertainty, one may appear too willing to overlook complexity, and appear as an advocate for a purpose other than the production of what they considered to be robust scientific knowledge. Second, uncertainty mandated a rigid adherence to the collection and analysis of scientific data in an attempt to reduce existing uncertainties. Contrary to what appeared as hasty speculation regarding future environmental threats, the acquisition of more information appeared to be the most reasonable course of action until uncertainties are resolved. Third, and most importantly, they believed that scientists should adhere to a strict code of behavioral conduct that precluded direct engagement with the general public. For them, reticence was crucial to project a certain image of dispassionate authority and expertise.[76]

[76] There is a great deal of literature on what scholars call scientism, or the general belief that science and technology can solve most if not all ills. See, Sorrell, Tom. *Scientism: Philosophy and the Infatuation with Science* (New York: Routledge, 1991); Stenmark, Mikael. *Scientism: Science, Ethics, and Religion* (Aldershot, England: Ashgate, 2001); Bannister, Robert. *Sociology and Scientism: The American Quest for Objectivity, 1880*-1940 (Chapel Hill: University of North Carolina Press, 1987); Schoeck, Helmut. *Scientism and Values* (Princeton: Van Nostrand, 1960); Ezrahi, Yaron. *The Descent of Icarus: Science and the*

ADVOCACY AND PROPHECY

The experts discussed in this dissertation frequently employed two terms to chastise other scientists for breaking prescribe norms of behavior: advocates and prophets of doom. Advocates were those who tended to use their scientific knowledge as a license to engage the public directly about what they believed to be the best direction for society. While scientists were frequently in the best position to know the nature of environmental threats, their advocacy of policies may appear to others as driven less by science than by their personal ethics and values. This was potentially damaging to the professional credibility of the scientific community because the public may not be able to clearly decipher when scientists are merely using science to inform what were ultimately their personal value-systems and socio-political judgments. Indeed, historians have discussed at length the motivation of many within the nuclear science community to steer society away from nuclear weaponry, as well as advocacy by scientists during the 1930s.[77]

While problematic, advocates appeared less threatening than so-called prophets of doom. Many scientists who considered themselves experts accused alleged prophets of doom of being false interlocutors who distorted science for ideological and political reasons. They not only seemed to use their value judgments on science-related matters in public, these kinds of claims appeared to bolster a culture designed to maximize public fear: the media appeared to value fear because it boosted sales, some scientists consciously chose to make what appeared to be

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Transformation of Contemporary Democracy (Cambridge: Harvard University Press, 1990); Levitt, Norman. *Prometheus Bedeviled: Science and the Contradictions of Contemporary Culture* (New Brunswick, N.J.: Rutgers University Press, 1999)

[77] Scientific advocacy is not uncommon in the 20th century, particularly when it came to nuclear weaponry issues and the military. See, Moore, Kelly. *Disrupting Science: Social Movements, American Scientists, and the Politics of the Military, 1945-1975* (Princeton: Princeton University Press, 2008); Kuznick, Peter. *Beyond the Laboratory: Scientists as Political Activists in 1930s America* (Chicago: University of Chicago Press, 1987); Smith, Alice. *A Peril and a Hope: The Scientists' Movement in America, 1945-47* (Chicago: University of Chicago Press, 1965); Strickland, Donald. *Scientists in Politics: The Atomic Scientists' Movement, 1945-46* (Lafayette, IN: Purdue University Studies, 1968); Barnhart, Megan, "'To Secure the Benefits of Science to the General Welfare': The Scientists' Movement and the American Public during the Cold War, 1945-60" (Dissertation: UCLA, 2007)

dramatic claims about the future to encourage public responses, and elements within the environmental movement would then adopt such pessimistic arguments by both the media and scientist-advocates to justify fundamental structural change within American society.[78] While sensationalism within the media was nothing new to American culture, pessimism, fear, and opportunism appeared to be ubiquitous elements within broader environmental politics, and robust scientific knowledge appeared increasingly relegated to the shadows of politics.[79]

Broadly speaking, this response by elite scientists to those whom they saw as advocates and prophets of doom was rooted in a more fundamental competition for the hearts and minds of the American public. When Landsberg praised Abelson's editorship as "balanced," he did so with the knowledge that they were speaking a similar language; they sought to reestablish what they considered to be the rightful place of credentialed and experienced experts to speak on matters important to society, and were increasingly impatient with the slow degradation of the cultural authority of experts such as themselves. As Theodore Brown articulates, cultural authority -- defined as "the measure of the capacity to instill belief; to engender not only understanding, but also assent; to move those affected toward changing attitudes; and to encourage action" -- was a very important matter.[80]

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[78] For more theoretical aspects of media and science, see Bucchi, Massimiano. *Science and the Media: Alternative Routes to Scientific Communication* (New York: Routledge, 1998); Rodder, Simone, Martina Franzen, and Peter Weingart, eds. *The Sciences' Media Connection -- Public Communication and the Its Repercussions* (New York: Springer, 2012); Snyder, Peter, Linda Mayes, and Dennis Spencer, eds. *Science and the Media: Delgado's Brave Bulls and the Ethics of Scientific Disclosure* (London: Elsevier, 2009)

[79] For an account of 19th century sensationalism within American reporting of social issues, see Sachsman, David and David Bulla, eds. *Sensationalism: Murder, Mayhem, Mudslinging, Scandals, and Disasters in 19th-Century Reporting* (Piscataway, NJ: Transaction Publishers, 2013).

[80] Brown, Theodore. *Imperfect Oracle: the Epistemic and Moral Authority of Science* (University Park, PA: The Pennsylvania State University Press, 2009): p. 5. For other reflections of expertise, see Selinger, Evan and Robert Crease, eds. *The Philosophy of Expertise* (New York: Columbia University Press, 2006); Haskell, Thomas. *The Authority of Experts: Studies in History and Theory* (Bloomington, IN: Indiana University Press, 1984); Gieryn, Thomas. *Cultural Boundaries of Science: Credibility on the Line* (Chicago: University of Chicago Press, 1999); Simon, Yves. *A General Theory of Authority* (South Bend, IN: University of Notre Dame Press, 1962); Stehr, Nico. *Experts: The Knowledge and Power of Expertise* (Abington, Oxon: Routledge, 2011)

This is not to say that their objections to what appeared to be prophecies of doom were more noble or thoughtful.[81] In the wake of Rachel Carson's work in the early 1960s, a much larger environmental movement had coalesced around preventing what seemed like an array of entirely plausible environmental crises. Many credentialed scientists during the 1970s -- figures like climatologist Stephen Schneider, physicist James McDonald, and ecologist Lamont Cole -- did not hesitate to engage the public directly about the need for American society to steer a new course -- and thus became advocates according to the standards of others. Their public claims were criticized for resembling what professor of rhetoric Lynda Walsh calls scientist-prophets or those who combined the power of religious prophets with the language and methods of science -- what she calls a "prophetic ethos."[82] These individuals were not necessarily religious millennialists, though it was frequently unclear where one began and the other ended.[83] Similar in kind to what historian of science Steven Shapin calls "charismatic authority," these individuals -- occasionally labeled by critics as advocates or prophets of doom -- appeared to extend their roles beyond what was considered appropriate.[84]

One thing that experts on all sides of the advocacy line had in common was a belief that they were acting on behalf of the public good. The problem, however, was that the public appeared to lack the necessary tools to decipher between different levels of expertise and therefore who was looking out for them. Given the stakes involved, any conflicting messages by

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[81] For a broader discussion of social responsibility of scientists, see LaFollette, Marcel. *Making Science Our Own: Public Images of Science, 1910-1955* (Chicago: Chicago University Press, 1990); Nelkin, Dorothy, "Scientists and Professional Responsibility: The Experience of American Ecologists," *Social Studies of Science* 7, 1 (February 1977): pp. 75-95; Perl, Martin, et al., "Public Interest Science," *Physics Today* 27 (June 1974): p. 23. The popular periodical, *Scientist and Citizen*, suggests an increased intimacy between the general public and scientists. Also, Brown, Martin (ed.). *The Social Responsibility of the Scientist* (New York: The Free Press, 1971); Teich, A., ed. *Scientists and Public Affairs* (Cambridge: The MIT Press, 1974)

[82] Walsh, Lynda. *Scientists as Prophets: A Rhetorical Genealogy* (Oxford: Oxford University Press, 2013).

[83] For apocalyptic visions within Cold War culture, see Lahr, Angela. *Millennial Dreams and Apocalyptic Nightmares: The Cold War Origins of Political Evangelicalism* (Oxford: Oxford University Press, 2007); Wojcik, Daniel. *The End of the World As We Know It: Faith, Fatalism, and Apocalypse in America* (New York: New York University Press, 1997); Chernus, Ira. *Nuclear Madness: Religion and the Psychology of the Nuclear Age* (Albany: State University of New York Press, 1991); Weart, Spencer. *The Rise of Nuclear Fear* (President and Fellows of Harvard College, 2012)

[84] Shapin, Steven. *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008): p. 3

different experts would only add to public confusion over the nature and urgency of environmental threats. The communication of too many visions of future doom was problematic not only because it confused the general public, but also one could not clearly isolate the real but manageable threats. As philosopher Stephen Turner has recently argued: "The public is not very adept at distinguishing the core of expert knowledge from the penumbra: this is a distinction made within the community of experts." While Turner is careful to note that experts are fallible in judging whether claims made by scientists are actually true, he was equally careful to note that "the public is fallible in judging claims of expertise."[85]

More alarming was the public's apparent inability to determine who was using science to cloak their more fundamental political motivations. How was the public to judge whether one's value systems were encroaching on their scientific judgments, and vice versa? Seen in this light, the scientific establishment could use the language of fear and alarm about prophets of doom (and appear political) just as much as prophets of doom may have used fear to motivate political and popular action (and appear equally political). As political theorist Corey Robin has argued, the use of fear itself may be used by anyone as a "political tool, an instrument of elite rule or insurgent advance, created and sustained by political leaders or activists who stand to gain something from it, either because fear helps them pursue a specific political goal, or because it reflects or lends support to their moral and political beliefs -- or both." He calls this "political fear."[86]

Indeed, adhering to a more reticent approach to environmental politics could be perceived as complacent inaction and support of the status quo. Elite experts who advocated for further research may appear disingenuous if only because such caution could stall ameliorative action

[85] Turner (February 2001): pp. 141-42. For a discussion of expertise and lay knowledge, see Pierson, Robert, "The Epistemic Authority of Expertise," *Proceedings of the Biennial Meeting of the Philosophy of Science Association* 1 (1994): pp. 398-405
[86] Robin, Corey. *Fear: The History of a Political Idea* (Oxford: Oxford University Press, 2004): p. 16

and unwittingly reflect anti-environmental interests within society. However, taking an opposite perspective, advocating for further research may actually be a reasonable course of action given what many see as important scientific uncertainties. By voicing hesitation or caution, some scientists could argue that their goal is not to prevent ameliorative action, but rather a genuine concern that scientists could lose their credibility over the long term if scientific uncertainties are not identified and resolved.

As highly credentialed experts, they were especially skeptical toward the general public. Rather than allowing the general public to have a role in scientific deliberation, what scholars traditionally call citizen-science, they were highly resistant to such democratic impulses.[87] For many experts, doom-laden prophecy was synonymous with popular propaganda, and fear was merely the political weapon used to motivate social and political change.[88] As if the cultural authority of science was a zero-sum game, it appeared that fear-based activism by those they called advocates and prophets of doom devalued their own voices of expertise and authority.[89] Rather than continually seeking to "manage" perceptions of impending apocalypse, these experts sought to abolish claims of apocalypse from the public discourse of environmental politics entirely.[90] To establish themselves as voices of expert authority, and highlight the very uncertain

[87] Haddow, Alexander, "The Scientist as Citizen," *Bulletin of the Atomic Scientists* (September 1956): pp. 247-252; Nehemiah (November 1963). For an account of a tension between expertise and democracy, see Lövbrand, Eva, Roger Pielke, Jr. and Silke Beck, "A Democracy Paradox in Studies of Science and Technology," *Science, Technology, and Human Values* 36, 4 (July 2011): pp. 474-496

[88] The scholarship on the intersection between science and propaganda is relatively scarce. Nonetheless, some traces appear occasionally. See, Santesmases, Maria, "Peace Propaganda and Biomedical Experimentation: Influential Uses of Radioisotopes in Endocrinology and Molecular Genetics in Spain (1947-1971)," *Journal of the History of Biology* 39, 4 (Winter, 2006): pp. 765-794; Walker, Mark, "Physics and Propaganda: Werner Heisenberg's Foreign Lectures under National Socialism," *Historical Studies in the Physical and Biological Sciences* 22, 2 (1992): pp. 339-389; Eckert, Michael, "Propaganda in Science: Sommerfeld and the Spread of the Electron Theory of Metals," *Historical Studies in the Physical and Biological Sciences* 17, 2 (1987): pp. 191-233

[89] Sociologist Barry Glassner suggests that Americans are particularly predisposed to fearing the wrong things, and thereby risk overlooking genuine threats to their welfare. Glassner, Barry. *The Culture of Fear: Why Americans are Afraid of the Wrong Things* (New York: Basic Books, 1999)

[90] Scholars have examined in great deal how Eisenhower, for instance, sought to "manage" public perceptions of future apocalypse, though ultimately not very successfully. See, Chernus, Ira. *Apocalypse Management: Eisenhower and the Discourse of National Insecurity* (Stanford: Stanford University Press, 2008); Divine, Robert. *The Sputnik Challenge* (New York: Oxford University Press, 1993)

nature of environmental challenges, they used their positions of authority to draft a coherent rhetorical strategy that could accurately reflect their interest in maintaining an expert image -- what some of them called a middle ground.

DISSERTATION SUMMARY

This dissertation gives a chronological account of the manner in which some elite American sought to remind scientists of their obligations to conduct themselves reticently and dispassionately regarding environmental threats. To explore this dynamic, chapter one explores the establishment of the American Geophysical Union (AGU) Committee of Environmental Quality (CEQ) in 1970. Paralleling the first Earth Day on April 22, 1970, the establishment of the CEQ was an effort by physicist S. Fred Singer and climatologist Helmut Landsberg to influence environmental discussions by inserting what they believed to be a more rational "middle course" into public discussions of the environment.[91] They believed that the time had come for geophysical professionals to understand society-relevant problems, while positioning the CEQ as an arbiter of the truth about future threats. Consistent with many other prominent scientists including the president of the National Academy of Sciences, Philip Handler, and AGU Vice President and Editor of Science, Philip Abelson, they also sought to strategically steer environmental discussions away from what they referred to as advocates and prophets of doom.

By creating an institution specifically designed to act as what Philip Abelson and Singer called a "truth squad," they engaged the scientific community, policy makers, and the general public directly in the hope that such activities would motivate scientists to think twice over how they made scientific claims that could exacerbate what appeared to be public skepticism and

[91] For an account of the first Earth Day on April 22, 1970, see Rome, Adam. *The Genius of Earth Day: How a 1970 Teach-In Unexpectedly Made the First Green Generation* (New York: Hill and Wang, 2013)

hostility toward scientists.[92] Uniquely, the CEQ believed that regulating public rhetoric was the key to renegotiate the role of science -- and scientists -- within American society. The time had come to minimize what they saw as unmitigated pessimism and fear, and restore reticent and dispassionate inquiry in environmental discussions. It was time, they believed, to lower the flag of what they deridingly called alarmism.

Chapter two builds on Landsberg's individual efforts to prevent what he saw as a tendency for the untrained to popularly discuss the future effects of climate change and integrate climate into their research agendas. During the mid-to-late 1970s, figures like physicist Howard Wilcox, astrophysicist John "Jack" Eddy, and science writers Nigel Calder and John Gribbin were especially frustrating for Landsberg. Not only did many of them use climate to further public concern about future catastrophe, the professional boundaries between climatology had been crossed by those who he believed were not credible enough to make public claims about climate at all. By examining Landsberg's frustrations, this chapter explores how he actively embodied the goals of the CEQ while attempting to maintain the credibility of the discipline he helped build for more than four decades.

Chapter three examines in greater depth Singer and Landsberg's philosophical and scientific concerns in how environmental concerns regarding the development of the supersonic transport (SST) were being tainted by what they saw as the rhetoric of catastrophe. The argument by opponents of SST development was that it would emit chemicals into the stratosphere and deplete the ozone layer, and in turn allow more ultraviolet radiation to reach the

[92] The formation of a special committee for such purposes was not unique. See, Wolfe, Audra, "What Does It Mean to Go Public? The American Response to Lysenkoism, Reconsidered," *Historical Studies in the Natural Sciences* 40, 1 (Winter 2010): pp. 48-78. For a philosophical discussion of scientific truth, see Lipton, P., "The Truth about Science," *Philosophical Transactions of the Royal Society of London* 360, 1458 (2005): pp. 1259-1269

surface of the earth. This, many believed, posed a great risk because it could produce a higher incidence of cancer rates and alter the global climate.

This was an important development because political and economic arguments against the development of the SST had evolved into scientific and technical concerns about the atmosphere, and thus provided a point of entry for Singer and Landsberg to enter into the debate. Their mission was not to support advocates for the production of SST, nor support opponents. Instead, and entirely consistent with the goals of the CEQ, Landsberg and Singer believed that if it were to be defunded that it be done for the right reasons. Rather than participating in the politics surrounding the SST, they sought to keep technical and scientific claims out of the public sphere so as to develop a more robust and reliable understanding of the impact of SST emissions on the global atmosphere. Until that was done, any public claims about its potential impact on public health was deemed improper. As Landsberg remarked to the Director of the Environmental Data Service within the ESSA, Woodrow Jacobs, scientific debates about man's role in climate should be kept "in the family" rather than open to "public discussion."[93]

Chapter four focuses on the development of the first national climate policy in American history, the National Climate Program Act of 1978. While not strictly a story about the matter of alarmism in public discussions of environmental threats, I show how geophysicist Frank Press challenged congressional efforts to pass legislation committed to assisting local users as they tried to cope with the effects of climate fluctuations. If the federal government were to go beyond funding additional research, he reasoned, the government would "unduly" raise the expectations of the general public. Like Singer and Landsberg's desire to plow a "middle course" in environmental affairs, Press was determined to support a climate policy that he believed would balance panic and complacency. What this meant in practice was that the

[93] Landsberg to Woodrow Jacobs, 18 March 1969, Series 2.1, Box 1, PHL

federal government would fund a national research program on the global effects of carbon dioxide on the climate, but would not venture into how to apply as-yet uncertain knowledge for the benefit of users. In spite of his own belief that carbon dioxide-induced climate change was a potentially catastrophic problem, he believed that his position as both an administrator (science adviser) and scientist compelled him to pave a course that he believed was appropriately judicious and cautious.

Chapter five continues with an examination of "alarmism" within American culture by exploring the development, dissemination, and reception of the most significant federal effort to model environmental threats -- the Global 2000 Report to the President, published in the summer of 1980. This report was uniquely symbolic in that it served to reinforce the ideas of both environmentalists as well as critics of "alarmist" rhetoric. For environmentalists, the report was indeed an alarmist projection of catastrophe, and reinforced and justified their fears. For the government to take heed of public calls for meaningful policy action represented a step forward for them.

For critics, however, Carter's report served to reinforce their suspicion that prophets of doom were expanding their influence. "Alarmist" rhetoric was no longer confined to environmental advocates and the media; the report signified the first time that the federal government had accepted and codified "alarmism" as a rhetorical strategy to initiate political action. With the rise of the conservative right in American politics, and particularly the election of Reagan in 1980, the cultural divide discussed in prior chapters had finally begun to solidify along political lines. Global 2000, in other words, may be considered the first moment in post-World War II politics where 'liberal' and 'conservative' ideologies began to map onto cultural divisions between "alarmists" and critics like Landsberg, Singer, and Julian Simon.

My conclusion and epilogue will focus on how these issues provide an opportunity for further research into contemporary climate debates. Given what appears to a resurgence of attempts to squelch what appear to be popular claims of doom-and-gloom, and possibly the creation of a new middle ground position, historians may begin to determine whether the middle ground has a purpose in contemporary debates, and whether attempts to pave a middle ground are rooted in the same kinds of motivations described in this dissertation.

CHAPTER 1

PAVING A MIDDLE GROUND -- ESTABLISHING THE AGU COMMITTEE ON ENVIRONMENTAL QUALITY

> "Our chief safeguard against shipwreck is a knowledge that we know little and a rejection of the cocksure illusion that anyone can foreknow with precision all the adventures yet to be encountered. We should be aware of soothsayers, no matter how erudite or self-assured; we want, not soothsayers, but careful mariners." -- Jerome Frank, "The Place of the Expert in a Democratic Society," 1949

> "It is true that our outlook is grim; we face many tough problems. We have to tackle them with determination, and we will do a better job at it if we do not let them get us down -- pitch us into gloom and frantic despair . . . Life can be gay even as it is grim." -- Vannevar Bush, *Pieces of the Action* (1970)

Jerome Frank, a 20th century legal philosopher, believed that societies that maintained what he imagined as flexibility when planning for the future was a judicious and appropriately cautious way to avoid future dangers. While the notion of flexibility seems to be a truism, his broader ambition was more subtle; his metaphorical "careful mariner" contrasted with what he deemed to be self-assured soothsayers. While soothsayers may have a high level of conviction in their claims of the future, it was the careful mariner who would steer the ship through troubled times. In this, Frank was providing a critical appraisal of "soothsayers" as much as he was cautioning against the risks of submitting to authority too easily.[1] There was a problem, however; the general public had little way of distinguishing one from the other and therefore required guidance from those who believed to be the genuine experts.

[1] Frank, Jerome, "The Place of the Expert in a Democratic Society," *Philosophy of Science* 16, 1 (January 1949): pp. 3-24

This chapter focuses primarily on three American scientists -- climatologist Helmut Landsberg, and geophysicists Philip Abelson and S. Fred Singer -- who saw themselves as "careful mariners" in a time of great anxiety about the future effects of environmental threats. They believed that they had the expertise to distinguish those environmental threats firmly rooted in scientific knowledge from those threats frequently communicated to the general public but apparently less founded on settled science. By evaluating environmental risks in what they considered a dispassionate, apolitical way, they believed that their expertise could guide public discussions away from what they saw as hasty and exaggerated rhetoric. Their pursuit stopped short of advocating for a technocracy, however; little evidence suggests that they strived to be the decision makers. Instead, their goal was to make sure that the general public never lost sight of the importance of approaching environmental risks with what they imagined to be cooler heads.

As careful mariners, Landsberg, Abelson, and Singer needed a ship from which they could engage environmental discussions. In 1970, after a decade of considerable popular attention had been paid to environmental problems, they established the American Geophysical Union Committee on Environmental Quality (AGU CEQ). First, their affiliation with the AGU -- the most prestigious institutional body of American geophysics -- invested their goals with a high degree of credibility. Second, that credibility would be useful in their attempt to counter what they saw as the hyperbolic rhetoric communicated by so-called prophets of doom. In doing so, they hoped to clarify the extent and severity of environmental risks. Third, the CEQ allowed geophysicists to compete with other more established fields like chemistry and physics in terms of funding and national recognition. Fourth, and broader in scope, those involved sought to restore the professional reputation of science within American society. By examining the creation of the CEQ, we have an opportunity to see how scientific experts used their positions of authority to establish what they deemed to be a "middle ground" within environmental discussions during the 1970s.

LAMONT COLE AND ENIVIRONMENTAL ADVOCACY

To explain the establishment of the CEQ in 1970, historians must return to environmental debates that arose in response to concerns that the atmosphere was running out of oxygen in mid-to-late 1960s. One of the most active ecologists interested in this issue was an ecologist at Cornell University, Lamont Cole. He believed that the threat was serious enough to warrant further scientific investigation, and also communicated with the general public about the potential consequences should his suspicions prove correct. In doing so, however, he challenged what some geophysicists considered to be traditional norms of scientific behavior. By extending his suspicions into the public sphere, he appeared to prematurely advance a vision of the future that was disconnected from settled science. By examining the rationale behind his concerns regarding the future effects of oxygen depletion, as well as the response by some members of the geophysics community to his public engagement, this chapter argues that his activities prompted figures like Landsberg and Abelson to create the CEQ.

After graduating from the University of Chicago with a degree in physics in 1934, an M.A. in 1939 from the University of Utah, and a Ph.D. in Zoology from the University of Chicago in 1944, Lamont Cole ascended through the ranks of the ecological profession to become chairman of the Cornell Department of Zoology in 1964 and helped shape the Section of Ecology and Systematics within Cornell's Division of Biological Sciences during the mid-1960s. Alongside Cole's academic credentials, his involvement with the Ecological Society of America (ESA) and the American Institute of the Biological Sciences (AIBS) pushed him to become one of the most administratively active ecologists in the United States -- he served as ESA vice president in 1964 and president from 1967 to 1968, as well as the vice president of the AIBS in 1968 and President in 1969. His editorial interests also revealed his interest in understanding other administrative aspects of the profession of ecology; between 1946 and 1963, Cole served as Associate Editor, Review Editor, and Zoological Editor of the journal, *Ecology*, Associate Editor of *Ecological Monographs*, and became Chairman of the ESA Publications Committee in 1965. His training and administrative awareness provided not only a keen understanding of the science of ecology, but also an ability to sense tensions and changes of thought within the ecological community.

By the early 1960s, Cole's professional trajectory shifted in response to what he saw as the seriousness of environmental threats. Amidst internal debates over how ecologists should engage the general public and society in general, he began to think as early as 1962 about how he could benefit society with his knowledge.[2] While Cole's scientific contributions during the 1940s and 50s appear noteworthy -- one obituary stated that he had "tremendous impact on the ecological community and upon all who came into contact with him" -- his evolution from basic research to public advocacy on behalf of humanity and the environment was evident by the early 1960s.[3]

One of the first indications of his changing attitude toward the ecologist's role in American society was his review of Rachel Carson's book *Silent Spring*.[4] While he asserted that Carson's work was a "highly partisan selection of examples" and an "impartial appraisal of all the evidence," he nonetheless argued that the errors were so infrequent, trivial, and irrelevant that

[2] For a historical account of this tension between basic research and advocacy within the environmental profession, see Nelkin, Dorothy, "Scientists and Professional Responsibilities: The Experience of American Ecologists," *Social Studies of Science* 7, 1 (February 1977): pp. 75-95; Hagen, Joel, "Teaching Ecology during the Environmental Age, 1965-1980," *Environmental History* 13, 4 (October 2008): pp. 704-723
[3] For the quote, see "Lamont Cole," *Bulletin of the Ecological Society of America* 59, 4 (December 1978): pp. 171-172
[4] Cole, Lamont, "Book Review of Silent Spring," *Scientific American* 207 (1962): pp. 173-180

any dwelling on them would be "ungallant."[5] Crucially, his broader purpose was not to chastise Carson but rather to praise her for writing a book that was for the most part technically sound, informative, but not overly simplistic. After recounting the fundamental thrust of Carson's concerns about an indiscriminate use of pesticides, as well as what appeared to be efforts by industry and partisan interests to discredit Carson, he concluded that her work presented "enough solidly established facts to justify some alarm."[6]

While this was not the strongest endorsement, Cole was one of the many within the ecological profession who believed that more active social engagement with the public was warranted. A perennial concern among ecologists since the ESA established the Ecology Study Committee in 1958, Carson's work seemed to revitalize discussions about the proper balance between social advocacy and basic research.[7] As noted by the committee in 1965,

> Rachel Carson's book *Silent Spring* created a tide of opinion which will never again allow professional ecologists to remain comfortably aloof from public responsibility. The importance of this book and its effect on public opinion, national scientific policy, and the status of professional societies with respect to public affairs can hardly be overstated.[8]

In the wake of specific concerns among ecologists during the 1950s in regard to the effects of nuclear fallout, ecologists diverged in how best they could be of service to American society -- through better research or more public environmental advocacy. For those who believed that pursuing basic research isolated from societal affairs was the more prudent course, Cole's decision led him to become what historian and anthropologist Gregory Blomquist called

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[5] Cole (1962): p. 173
[6] Cole (1962): p. 173
[7] The ESA Ecology Study Committee was established by ESA President Thomas Park, chaired by Paul Sears, and funded by a $24,000 National Science Foundation Grant in 1958. See, Cantlon, et al. "Proceedings," *Bulletin of the Ecological Society of America* 40, 1 (March 1959): p. 28
[8] Miller, Richard and John Reed, "Summary report of Ecology Study Committee with Recommendations for the Future of Ecology and the Ecological Society of America," *Bulletin of the Ecological Society of America* 46, 2 (June 1965): pp. 61-82.

an "outsider to a changing field."[9] Stirred into a more active social role in the wake of Carson's work, Cole grew particularly concerned about the negative effects of population growth and industrial emissions on global atmospheric oxygen levels.

CASE OF DISAPPEARING OXYGEN

The earliest indications of Cole's interest in oxygen deprivation appeared when he served as a witness in front of the U.S. Senate Committee on Interior and Insular Affairs in 1966.[10] Organized by Senator Gaylord Nelson (D-WI), the hearing was an attempt by policymakers to understand the state and relevance of ecological research within the United States, and draw attention to the importance of ecological understanding in solving the nation's problems. During his testimony, he specifically addressed the question of whether the rate of combustion of fossil fuels would exceed the rate of photosynthesis, and thereby reduce the amount of oxygen in the atmosphere. While he admitted that "satisfactory data of the necessary sort do not exist," thereby revealing the uncertainties within his own "intense" but unsuccessful effort to quantitatively estimate the scope of the problem, Cole nonetheless felt compelled to issue an urgent warning to policy makers that "we are close to the critical point."[11] Like many others who testified, Cole was motivated by what he saw as the "the alarming side effects of the industrialization that is maintaining and expanding our economy even as the population grows."[12]

The following year, in 1967, Lamont Cole appeared to become more vocal about the seriousness of the problem while giving a talk at the 134th Annual Meeting of the American Association for the Advancement of Science (AAAS). While the contents of his speech are

[9] Blombquist, Gregory, "Population Regulation and the Life History Studies of Lamont Cole," *History and Philosophy of the Life Sciences* 29, 4 (2007): pp. 495-516, quote on p. 507

[10] U.S. Senate, Committee on Interior and Insular Affairs. *Ecological Research and Surveys*, Hearing, April 27, 1966 (Serial 65-624). Washington: Government Printing Office, 1966.

[11] Cole, Lamont, Statement to the Senate, Committee on Interior and Insular Affairs. *Ecological Research and Surveys,* Hearing, April 27, 1966 (Serial 65-624). Washington: Government Printing Office, 1966, p. 65. Unfortunately, the nature and extent of Cole's research is unknown.

[12] Cole, Lamont, Statement to the Senate, Committee on Interior and Insular Affairs. *Ecological Research and Surveys,* Hearing, April 27, 1966 (Serial 65-624). Washington: Government Printing Office, 1966, p. 65.

unknown, an analysis of how he engaged the public reveals that he believed that the problem justified a more urgent and dire tone. How the media reported the contents of his talk suggests a great deal about his frame of mind. As reported in the *Washington Post*, Cole speech asserted that the effects of oxygen depletion would be "approximately the same as moving everyone to higher altitudes, a change that might help to alleviate the population crisis by raising death rates."[13] Other media accounts followed, perhaps reflective of an increasing sense of urgency in Cole's mind. "By burning so much gas and oil we're using up the oxygen in the air to such an extent that, in a few generations, we'll have no air left," the *Boston Globe* repeated him as saying a week after his talk.[14] He said that if "we should inadvertently kill enough marine diatoms or the organisms they depend on for fixed nitrogen, we could start running out of oxygen to breathe . . .We are on a collision course with disaster," Cole warned two years later while being interviewed for the *Chicago Tribune*.[15] For Cole, the potentially life-threatening problem of oxygen deprivation to him that humanity was on the wrong course. He was not alone in his fear or his belief that the alarm had to be raised.

In May 1966, two geophysicists from the Dallas-based Southwest Center for Advanced Studies -- Lloyd Berkner and Lauriston C. Marshall -- submitted a request to Donald Hornig, President Lyndon Johnson's science advisor, to look into the possibility that industrial emissions could deplete the atmosphere of oxygen.[16] Entitled "Potential Degradation of Oxygen in the Earth's Atmosphere," their speculation was part of a pre-existing research agenda involving the

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[13] "Catastrophe Seen from Air Pollution," *The Washington Post* (28 December 1967), p. A3

[14] The newspapers reveled in the looming terror proposed by figures like Cole. See, Wiley, John, "Man Burning Out His Oxygen Supply," *Boston Globe*, 28 December 1967, p. 2; "In the Crystal Ball," 1 January 1968, Boston Globe, p. 10A. Articles also appeared in other newspaper outlets immediately following his AAAS talk, including the Chicago Tribune, The Sun, and the Washington Post.

[15] Cole, Lamont, "Just Browsing: Man's Abuse of Air," *Chicago Tribune* (28 September 1969): p. 28

[16] Berkner, L.V. and Marshall, L.C., "Potential Degradation of Oxygen in the Earth's Atmosphere," Memo for File, 26 April 1966. Unfortunately, an original copy of the memo has not been found, and the best information about the provenance of the concern may be found in Box 22 of the Papers of S. Fred Singer, Air and Space Museum, Washington, D.C. For more on Berkner's role in American science during the Cold War, see Needell, Allan. *Science, Cold War, and the American State: Lloyd Berkner adn the Balance of Professional Ideals* (Washington, D.C.: Smithsonian Institution, 2000)

study of the development of planetary atmospheres and the role of oxygen on the evolution of life.[17] If increased levels of oxygen during critical stages in a planet's history afforded opportunities for organisms to evolve and flourish, then it seemed entirely reasonable to speculate that periods of oxygen depletion could limit the potential for life.[18] The implication of their research was clear: if marine organisms' capacity to produce oxygen is affected by industrial activities then humankind may unwittingly asphyxiate itself to extinction.

Given the clear implications for the general public's future welfare, they nonetheless embraced a course of action quite different from Cole's more public activities. They advocated public reticence. As they noted in their memo to Hornig, their choice "has been done deliberately out of fear of public reaction and perhaps improper action concerning a serious problem whose reality has not yet been numerically identified. This is particularly pertinent in light of the world's already serious food shortages."[19]

To be clear, their apprehension about public engagement was not due to disbelief in the problem, nor an attempt to marginalize the problem as if it did not exist. To the contrary, their concerns reflected a genuine dilemma over whether -- as they described in their memo -- the "extent of knowledge so far accessible to them is sufficient to demonstrate whether or not the problem is serious now or in the identifiable future."[20] While human extinction was not out of the realm of possibility, they could not quantify the uncertainties nor could they buttress their concerns with technical knowledge of oxygen levels within the atmosphere and whether they were changing. That space of uncertainty -- that boundary between reasoned speculation and

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[17] Berkner, L.V. and L.C. Marshall, "History of Major Atmospheric Components," *Proceedings of the National Academy of Sciences* 53, 6 (June 15, 1965): pp. 1215-1225. Also, Berkner, L.V. and L.C. Marshall. *On the Growth of Oxygen in the Earth's Atmosphere* (Dallas, TX: Southwest Center for Advanced Studies, 1969)
[18] While little is known about Marshall, one may refer to the following for a biography of Berkner. See, Hales, Anton, "A Biographical Memoir: Lloyd Viel Berkner, 1905-1967" (Washington, D.C.: National Academy of Sciences, 1992)
[19] Berkner, L.V. and Marshall, L.C., "Potential Degradation of Oxygen in the Earth's Atmosphere," Memo for File, 26 April 1966
[20] Berkner and Marshall (1966), pp. 6-7

robust technical knowledge of future consequences -- justified their belief that more needed to be known before they could feel comfortable engaging the general public and policy makers.

Based on Berkner and Marshall's preliminary suspicions of a potential threat, a series of high-level discussions were initiated by Robert White, former Chief of the U.S. Weather Bureau and recently appointed Administrator of the Environmental Science Services Administration (ESSA).[21] After consulting with others in the scientific community, the ESSA and the National Science Foundation (NSF) decided to conduct a joint three-year study to measure current atmospheric oxygen levels and determine whether levels had increased significantly since the early 20th century. By the fall of 1966, within months of Marshall and Berkner's original memo to Hornig, atmospheric scientist Lester Machta from ESSA and Ernest Hughes from the National Bureau of Standards (NBS) were offered the project.[22]

Based on the collection of seventy eight atmospheric samples taken between 1967 and 1970 while aboard the oceanographic ships, USC & GSS *Oceanographer* and the National Science Foundation's USNS *Eltanin*, their conclusion -- in contrast to the seriousness of the inquiry -- was relatively unremarkable.[23] Supplementing previous measurements of global oxygen levels since 1910 with their more precise measurements using modern instrumentation, they concluded that any change has "either been very small or zero."[24] In short, Berkner and

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[21] For a broad contemporary history of the ESSA, see Popkin, Roy. *The Environmental Science Services Administration* (New York: Frederick A. Praeger, 1967). Also, Fleming, James, "The Making of NOAA, 1963-2005: An Evening with Robert White," *History of Meteorology* 5 (2006): pp. 55-64

[22] "No Measurable Change in World's Oxygen Supply, Three-Year Commerce Scientific Study Concludes," *ESSA News* 6, 26 (26 June 1970): p. 1,

[23] The USC&GSS Oceanographer was one of a fleet of research and survey vessels used by the Environmental Science Services Administration (ESSA) to improve scientific understanding of the global environment. A brief online search revealed some interesting ephemera. One may refer to brochure (pdf) entitled, "USC&GSS Oceanographer Proposed Global Expedition," as well as a copy of the brochure detailing the commissioning ceremony on July 13, 1966 in the Washington Navy Yard in Washington, D.C. Likewise, the National Science Foundation-run Eltanin was a deep sea research vessel. See Llano, George, "Biological Oceanology on the USNS Eltanin," *BioScience* 15, 4 (April 1965): pp. 287-289

[24] Machta, L. and E. Hughes, "Atmospheric Oxygen in 1967 to 1970," *Science* 168, 3939 (June 26, 1970): pp. 1582-84. Machta and Hughes expressed some concern about the precision of earlier measurements of the volume of oxygen in the atmosphere, they nonetheless felt confident that any potential errors would not change their overall conclusion.

Marshall's initial concerns -- while certainly praiseworthy because they led to robust and technically sound knowledge of the atmosphere -- were ultimately unjustified.

While the work of Machta and Hughes was scientifically useful in that they provided an empirical basis on which to test the relationship between industrial activity and atmospheric oxygen levels, their conclusions were equally important for falsifying what they believed to be "several 'doomsday' predictions."[25] While they did not address Cole by name, they challenged what they perceived to be a tendency for scientists to engage the public about environmental threats prematurely and induce public fear. For them, it was crucial that scientists not only recognize uncertainty, as Cole had done in his testimony, but also act in a way that was commensurate with those uncertainties, which Cole had apparently not done. Indeed, Hughes and Machta sought to calm what appeared to be a growing sense of public anxiety and stress about the future: "there is now less cause for alarm about the reduction in photosynthetic production of oxygen," they argued.[26]

Why would one of the most renowned ecologists of the 20th century choose to venture so far into the public realm and cast oxygen deprivation in such dire terms without definitive evidence? The answer is not that he was simply a bad scientist; that would be incorrect, anachronistic and would discount his many decades of highly specialized training and experience. Indeed, one could only reasonably dismiss his concerns outright once the results by Machta and Hughes had been published. With results in hand, one could evaluate the nature of his claims in hindsight and establish a precedent for why public declarations of future threats without sufficient evidence were wrong. But, between 1966 and 1970, there was very little

[25] Machta and Hughes (1970): p. 1584
[26] Machta and Hughes (1970): p. 1584

empirical evidence to judge the seriousness of the problem and assess the urgency with which scientists should speak about future threats.

Even so, the lesson seemed clear once the results were published in 1970 Wallace Broecker, Professor of Earth Science at Columbia University and Director of the Geochemistry Laboratory at the Lamont-Doherty Geophysical Laboratory, denounced what he considered to be the kinds of claims that fueled public anxieties. As he attested,

> In almost all grocery lists of man's environmental problems is found an item regarding oxygen supply. Fortunately for mankind, the supply is not vanishing as some have predicted. . . We are faced with so many real environmental crises that there is no need to increase public concern by bringing out bogeymen. Hopefully the popular press will bury the bogeyman it created.[27]

For Broecker, figures like Cole were not the only problem; the popular press was complicit in what appeared to be a system engineered to produce and magnify what he perceived to be imaginary threats -- or what he called "bogeymen." Other geophysicists, like Helmut Landsberg, also regarded Cole's previous pronouncements as indicative of a worrying trend. As he noted at the end of his tenure as President of the AGU in 1970,

> There have been dire predictions of imminent catastrophe by heat death, by another ice age, or by acute oxygen deprivation. The events foreseen in these contradictory prophecies will obviously not all come to pass at the same time, if they come to pass at all.[28]

The response by the geophysical community to Cole's public engagement reveals emerging tensions within the American scientific community. While his concerns were entirely reasonable and aligned with those of many others between 1966 and 1970 (i.e. Berkner and Marshall), his decision to warn the general public -- however noble and ethical on one level -- was considered extremely risky by some within the geophysics establishment. The lack of

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[27] Broecker, Wallace, "Man's Oxygen Reserves," *Science* 168, 3939 (June 26, 1970): pp. 1537-1538
[28] Landsberg, Helmut, "Man-Made Climatic Changes," *Science* 170, 3964 (18 December 1970): pp. 1265-1274

empirical data about changing oxygen levels, the unknown consequences of such a threat on human societies and other natural systems, the reasoned concerns that allowed scientists from distinct disciplines to see oxygen deprivation as a potentially real threat in the mid-1960s -- these were the circumstances faced by scientists that prompted real disagreements about how scientists should communicate and engage with the general public.

In this respect, the publication of results in 1970 were almost irrelevant to these broader concerns; even if Machta and Hughes found data that justified Cole's concerns, the latter's *public* claims would still have been seen as a premature expansion of the knowledge production into the *public* sphere, and many of his colleagues felt that such a course was not only imprudent but counter-productive.[29] His public claims were even more problematic because they appeared to fuel broader popular discussions of future doom; the authority and certainty with which he spoke of future doom seemed to exceed what some scientists believed to be an appropriate level of certainty between 1966 and 1970. Even some within the Department of Interior were struck by the claims of Cole, and sought to understand whether his claims had any merit at all. For a forty four year old geophysicist named S. Fred Singer, who served as the Deputy Assistant Secretary of Water Quality and Research, his experiences with the Cole incident would provide one of the first exposures to the issues of advocacy and the public communication of potential environmental dangers.

U.S. DEPARTMENT OF INTERIOR AND 'SAVING THE WORLD'

In 1968, Lamont Cole wrote an article for the *New York Times Magazine* entitled, "Can the World Be Saved." Unbeknownst to Cole, his article piqued the curiosity of Secretary of the Interior, Stewart Udall. "This article makes me feel uneasy," Udall wrote, a day after the article

[29] For a discussion of the cartography of knowledge production, see Gieryn, Thomas. *Cultural Boundaries of Science: Credibility on the Line* (Chicago: The University of Chicago Press, 1999)

was published.[30] Given the numerous claims discussed in Cole's article, Udall wondered whether the U.S. should initiate what he called a permanent international cooperative program to conserve the atmosphere, and thereby increase the visibility of the DOI in environmental circles. He asked his science advisor, Milner Shaefer, and the DOI Director of the Office of Ecology, John Buckley, to look into the matter for him.[31]

Combining a historical sensibility with discussions of contemporary environmental concerns, Cole described the disruptive effects of population growth on natural global ecological and atmospheric systems -- what he called "delicately balanced cycles" -- that had evolved over hundreds of millions of years. Given the complexity and scope of the problems, he criticized what appeared to him as the most jarring reality of environmental discussions: a lack of communication among distinct disciplines and an unwillingness to engage the public about these potential threats. "Natural scientists, social scientists and political leaders will have to learn to overcome that failure of communication. And all three will have to learn to communicate with the general public," he argued. Taken together -- population growth, atmospheric oxygen depletion, and a warming climate -- Cole believed that a lack of communication contributed to the risk of an "irreversible decline."[32]

That Cole's article prompted a response by Udall is perhaps unsurprising. Since the Kennedy Administration during the early 1960s, Udall had been a strong supporter of conservation efforts by organizations like the Sierra Club, the Wilderness Society, and the Wildlife Management Institute. In 1963, he published his own book about environmental

[30] This paper was originally presented at the 134th meeting of the American Association for the Advancement of Science on December 27, 1967.
[31] Stewart Udall to Schaefer and Buckley, 1 April 1968, Box 22, Singer Papers
[32] Cole, Lamont, "Can the World Be Saved?" *Bioscience* 18, 7 (July 1968): pp. 679-684; "Can the World Be Saved?" *The New York Times Magazine* (March 31, 1968): pp. 34-35, 95-110

challenges, *The Quiet Crisis*.[33] Like Cole, he was also a friend and supporter of environmental advocate, Rachel Carson.[34] Despite the importance he placed on an ecological understanding of nature, Udall was nonetheless careful not to push what he considered excessively burdensome regulation.[35] While strong on environment, Udall's environmental sentiments occasionally gave way to outright frustration with environmental groups that appeared to place environment concerns over the needs of blue-collar workers.[36] Indeed, Udall's request for his subordinates to look into the matter for him was motivated by his uncertainty about the veracity of Cole's claims and about whether this article provided an opportunity for the DOI to evolve from a "Department of the West" merely concerned with the extraction of natural resources into a more robust presence on contemporary matters like environmental management.[37]

Before submitting his response to Udall, Buckley sought the input of Singer. After reviewing Cole's claims, Singer agreed with his contention that industrialized nations were releasing vast quantities of pollutants into the environment. However, he urged further investigation into how those pollutants interact with natural ecosystems. Like Udall, he envisioned Cole's article as an opportunity to: (1) increase the visibility of the DOI in issues of global environmental pollution; (2) utilize the DOI's authority over the Federal Water Pollution Control Administration (FWPCA) as a legal framework; (3) explore an opportunity -- per Udall's suggestion -- for an international cooperative program to monitor the global interrelationships

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[33] Udall, Stewart. *The Quiet Crisis* (New York: Gibbs Smith, 1963)
[34] For his opinion of Rachel Carson upon her death, see, Udall, Stewart, "The Legacy of Rachel Carson," *Saturday Review* (May 16, 1964): 33.
[35] For some accounts of Udall's conservation activities during the 1960s, see Smith, Thomas, "John Kennedy, Stewart Udall, and New Frontier Conservation," *Pacific Historical Review* 64, 3 (August 1995): pp. 329-362; Dean, Robert, "'Dam Building Still Had Some Magic Then': Stewart Udall, the Central Arizona Project, and the Evolution of the Pacific Southwest Water Plan, 1963-1968," *Pacific Historical Review* 66, 1 (February 1997): pp. 81-98.
[36] Gordon, Robert, "'Shell no!': OCAW and the Labor-Environmental Alliance," *Environmental History* 3, 4 (October 1998): pp. 460-487
[37] For a contemporary account of the Department of Interior's environmental management activities, see Stanley Cain, "Environmental Management and the Department of the Interior," *Public Administration Review* 28, 4 (July-August 1968): pp. 320-326

between the atmosphere, oceans, and land. Nonetheless, in spite of what appeared to be substantive issues addressed by Cole, Singer cautioned that Cole's urgent tone was "probably overly alarmist."[38]

Singer's response appears to have been motivated by two principal considerations. First, his denunciation of Cole's public claims as "probably alarmist" may have been a sign of his respect and admiration for geophysicists like Lloyd Berkner, who exemplified in his mind the highest caliber of dispassionate scientist.[39] Two weeks after Berkner's death on June 4, 1967, Singer wrote a heartfelt letter to his widow. "You know, of course, how deeply and strongly Lloyd's example has affected my own work and you know of my admiration for him," he began. Singer recounted how Berkner's example inspired him to think holistically about major scientific problems in the earth and planetary sciences, and the importance of wedding many disciplines together to solve them. "I have never forgotten this advice, and I am following it belatedly . . . Lloyd has influence my thinking and my work for many years and this influence will be felt for the rest of my professional career," he gently assured.[40]

Second, Singer's frustration with Cole may have also reflected a deeper suspicion toward what appeared to be overly vocal ecologists. While he was certainly sympathetic to their role in environmental discussions, he nonetheless believed that the human-nature relationship was "much too important a subject to be left <u>just</u> to the ecologists."[41] By this, he meant that ecological research provided ample room for scientists from a variety of disciplines, and especially those within the geophysical sciences. Instead of devaluing those whom he saw as ecologists who genuinely sought to advance knowledge, he explained his distrust of ecologists

[38] Singer to Buckley, n.d. Box 22, Singer Papers
[39] For a glimpse into Berner's opinions regarding the role of science and technology in American society, see Berkner, Lloyd. *The Scientific Age: The Impact of Science and Society* (New Haven, CT: Yale University Press, 1964)
[40] Singer to Lillian Berkner, 19 June 1967, Box 19, Singer Papers
[41] Singer to Max Edwards, 9 May 1968, Box 23, S. Fred Singer Papers

like Cole who appeared to him more willing to avoid basic research while pursuing public environmental advocacy.[42] Nonetheless, in spite of his reservations about the manner in which Cole engaged the general public, that did not mean that his claims did not deserve a hearing.

Singer's views must have struck Buckley as appropriate and measured because he virtually transcribed his memo when responding to Udall approximately a week after his initial request. For both Buckley and Singer, "probably alarmist" did not mean an outright dismissal of Cole's concerns; rather their concern was that Cole appeared to overlook significant uncertainties in his public claims about the ultimate effects of human activities on the natural environment. Like Singer, he also believed that Udall's suggestion for an international cooperative program should be looked into and he expressed confidence that large-scale effects of industrialization had not been studied, discussed, and regulated by existing international bodies. For Buckley, the United States was in a position to take advantage of such an opportunity: "We can afford it, our actions substantially affect it, and we would be demonstrating leadership in a peaceful way," he concluded.[43]

Two weeks after Buckley had submitted his own virtual replica of Singer's memo to Udall, Shaefer submitted his own review of Cole's article. "I quite agree with Dr. Buckley that the problems raised by Dr. Cole are important and bear serious investigation, but that his treatment is overly alarmist and, as I will point out below, he is a little careless with the facts."[44] Schaefer, like Singer and Buckley, noted how Cole was "certainly correct" in his central suspicion that industrial activity had affected the environment. Where they diverged, however,

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[42] The distinction between environmentalism and ecology is discussed in Bowler, Peter. *The Earth Encompassed: A History of the Environmental Sciences* (New York: W.W. Norton & Company, 2000). For more recent reflections on the boundary between environmental advocacy and ecological science, see Strong, Donald, "Ecologists and Environmentalism," *Frontiers in Ecology and the Environment* 6, 7 (September 2008): p. 347; Kinchy, Abby and Daniel Kleinman, "Organizing Credibility: Discursive and Organizational Orthodoxy on the Border of Ecology and Politics," *Social Studies of Science* 33, 6 (December 2003): pp. 869-896
[43] Buckley to Udall, 9 April 1968, Box 22, Singer Papers
[44] Schaefer to Udall, 23 April 1968, Box 22, Singer Papers

was in their different assessments of the risks. Schaefer specifically took issue with Cole's feeling of urgency regarding the disruption of natural balances -- if they existed all. While humankind may have influenced these supposed cycles and balances, he reasoned, that did not mean that "the world will necessarily become uninhabitable by man in the future." Given what appears to be his confidence that humankind will overcome such large-scale environmental problems, he again suggested that Cole "tends to some degree of unjustified alarmism and is a bit careless with the facts."[45]

The importance of the DOI's response to Cole's article was not in their mutual agreement that global environmental problems were worthy of investigation, but rather their objections to the way Cole communicated with the general public. By relying on what appeared to be overly "alarmist" language to convey urgency, and doing so in a way that appeared to overlook important uncertainties, Cole appeared to increase public anxieties without sufficient cause. While it would be presumptuous to assert that the language of "alarmism" was uniform within the DOI, their response to Udall's inquiry suggests that the term had agency and meaning within the context of environmental issues.

The great irony of "alarmism" within contemporary debates over environmental threats was in its capacity to prompt action by federal officials in spite of what some experts saw as exaggerated rhetoric. On one level, Singer expressed consternation that Cole's "alarmist" article could have such an influence on the activities of the federal government, and at how he was able to sway high-level but ultimately "ignorant" bureaucrats -- Singer once noted how Udall "was as ignorant as a babe about environmental facts."[46] Nonetheless, he could not escape the apparent

[45] Schaefer to Udall, 23 April 1968, Box 22, Singer Papers
[46] "SST and the Environment," Speech given on 5 April 1971 by S. Fred Singer, Box 36, Singer Papers

fact that Udall's feeling of unease may have been the very thing that prompted the DOI's interest in global environment issues. As he said in a speech at Rice University in 1971,

> When he <Udall> read in the *New York Times* that the world was going to run out of oxygen because Lamont Cole said so, and since Lamont Cole is a 'scientist', he <Udall> grew concerned, and he has been concerned about the environment ever since. And our activities in the department really only started because of this. So I don't know whether to thank Dr. Lamont Cole or not, but he certainly turned on Secretary Udall.[47]

While he was perhaps influenced by Berkner's steadfast reticence or distrust of overly vocal ecologists, Singer's experiences within the DOI were symptomatic of what appeared to be a loss of control of experts over the future direction of science. According to Don Price, who served as Dean of the Graduate School of Public Communication at Harvard University, the American "scientific establishment" after World War II acquired great authority to direct the future of science. No scientist, Price argued, enjoys the feeling that "his basic values and objectives have been set by others so rigidly that he cannot follow where his research leads him."[48] For Singer, the influence of what he saw as exaggerated environmental rhetoric by an ecologist like Cole on those who did not have the requisite knowledge to understand truth from reasoned speculation (i.e. Udall) was shocking and confusing. While Cole justifiably brought attention to important issues, his apparent advocacy contrasted starkly with Singer's own avowal of what he saw as traditional values of reticence, moderation, and patience.

Cole's article, at least for Singer, exemplified the dilemmas faced by scientists across the nation. On the one hand, what he saw as a series of "alarmist" claims were inappropriate because of the range of uncertainties that were either overlooked or dismissed for the sake of warning the general public. For all the virtues of ecology as a scientific discipline, he believed that scientists

[47] "SST and the Environment," Speech given on 5 April 1971 by S. Fred Singer, Box 36, Singer Papers
[48] Price, Don, "The Scientific Establishment," *Proceedings of the American Philosophical Society* 106, 3 (June 29, 1962): pp. 235-245, quote on p. 239

had a responsibility to conduct research apart from environmental politics. When Singer participated in the deliberations within the DOI regarding Cole's article, he witnessed first-hand how vocal ecologists could influence the course of affairs. As it turns out, geophysicists like himself and Helmut Landsberg began to wonder whether there was something that could be done to lessen the influence of ecologists like Cole.

FASHIONING A NEW IDENTITY FOR THE AGU

In early 1969, to commemorate its fiftieth anniversary, the AGU Executive Council decided to change the appearance and format of its oldest publication, *Transactions of the American Geophysical Union*.[49] Newly named after the Greek goddess of the dawn, EOS, the AGU decided to shift its focus to scientific problems more pertinent to the general public. Reflective of this evolution, *EOS* Editor, A.F. Spilhaus, believed that the AGU should solicit articles that "reflect responsible concern for the political, economic, and legal implications of scientific research."[50] Without such changes, AGU President Helmut Landsberg believed that the AGU was at risk of becoming stagnant: "We launch this new venture at the start of our fiftieth year of existence. It should signify that we are not becoming staid and hidebound with age."[51] For Landsberg and Spilhaus, one thing was clear: the times were changing, and geophysics needed to change with them.

The maturing environmental movement provided a window of opportunity for the AGU to redefine its priorities and re-evaluate the role of geophysicists in American society. As president, Landsberg felt that it was important for geophysicists to become more socially

[49] A comprehensive institutional history of the AGU has yet to be written. However, a very brief history of the AGU may be found on the AGU webpage: http://about.agu.org/our-history/.
[50] Spilhaus, A.F., "Forum," *EOS: Transactions of the American Geophysical Union*, Vol. 50, 1 (January 1969): p. 2. Instead of a quarterly publication, EOS would be distributed monthly in order to provide a better link between Union officers, staff, and members. The Executive Council also believe that a more frequent distribution would facilitate more responsive and open dialogue and debate among its members, and provide more 'lively discussion.'
[51] Landsberg, Helmut, "The New Transactions," *EOS,* Vol. 50, 1 (January 1969): p. 3

engaged. Like the ecological profession during the early 1960s, this concern for social relevancy appeared appropriate given changing societal needs. In her history of the ecological profession between 1890 and 2000, for instance, Sharon Kingsland notes that the role and shape of the ecological discipline -- not unlike all organisms that try to find their "ecological niche" -- "evolves over time in competition with other disciplines and in relation to an environmental and social context."[52] For the AGU, the time had come to pave a new road for geophysics -- broadly defined by Landsberg as the disciplines concerned with all scientific problems connected "with the atmosphere of the earth, the solid and liquid surface of the earth, and the interior portions of the earth."[53] Given a whole new cast of global environmental problems, he felt that the AGU could bring a unique perspective about the complexities of environmental threats.

Aware that the ability to influence the public's and scientists' perceptions was in part rooted in the size of membership, Landsberg believed that members of the physics community provided the most important reservoir of potential talent to bolster the influence of the geophysics community. Given what he envisioned as a "gap between physics and geophysics" that developed when quantum mechanics, nuclear physics, and high-energy physics became dominant in the training of physicists after World War II, he reasoned that classical physicists -- those who were most beneficial to the geosciences -- were left behind. This emphasis on "big science," he argued, "swung far away from classical physics which remains paramount in geophysical problems." For Landsberg, the divergent historical trajectories of the physics and geophysics -- and high energy physics from classical physics -- provided an opportunity. For those physicists who were underemployed or cast aside given what he characterized as a

[52] Kingsland, Sharon. *The Evolution of American Ecology, 1890-2000* (Baltimore: Johns Hopkins University Pres, 2005), p. 1
[53] Landsberg, Helmut. *Geophysics and Warfare* (1954): p. 2. For a broader history of geophysics as a discipline, see Good, Gregory, "The Assembly of Geophysics: Scientific Disciplines as Frameworks of Consensus," *Studies in the History and Philosophy of Modern Physics* 31, 3 (2000): pp. 259-292. For the relationship between geophysics and warfare and military patronage, see a series of articles in a special edition of the *Historical Studies in the Physical and Biological Sciences* 30, 2 (2000)

"tightening job market for physicists" after World War II, the environment provided a saving grace.[54] "A bit of missionary work by geophysicists seems to be well warranted," Landsberg concluded.[55] After all, geophysics was the "offspring" of physics and therefore should be reunited.[56]

Within a short time, President Landsberg wrote a piece in *EOS* that exemplified his own commitment to the new agenda. Simply titled "Environment," Landsberg argued that "incipient overpopulation" and "profligate use of scarce resources" led to a noticeable imbalance between human needs and the environment.[57] Critical of what he believed to be the *ad hoc* legal measures enacted to protect against environmental threats, Landsberg argued that the complexity of environmental problems warranted a weaving together of many sciences; no one science was sufficient to understand the scope of the problems. The issues were comprehensive and holistic, and geophysicists seemed especially suited.

As President, however, his environmental interests were inextricably wedded to his interests in strengthening the reputation and importance of the geophysics profession. Given the surging relevance of ecology in federal funding circles, as well as more stable disciplines in the natural sciences, the geosciences were primed for change. "Happily," Landsberg reassuringly stated as he reflected on the evolution of the geosciences since the establishment of the Coast and Geodetic Survey in 1807, "geophysics has always been in the position to do much for the government and the people. As a science, it has lived in symbiosis with government -- not always without strain -- almost since the inception of the Republic." Although geophysics had

[54] The frustration with high energy particle physics may have reflected a broader frustration with how the geophysical scientists had been treated within the scientific establishment. See, Friedman, Robert. *The Politics of Excellence: Behind the Nobel Prize in Science* (Henry Holt and Company, 2001)
[55] Landsberg, Helmut, "Conversion of Physicists," *EOS* 51, 5 (May 1970): p. 626.
[56] For a historical account of the position of high energy physics, see Kevles, Daniel. *The Physicists: The History of a Scientific Community in Modern America* (New York: Vintage Books, 1977): pp. 410-426.
[57] Landsberg, Helmut, "The President's Page: The Environment," *EOS* 51, 4 (April 1970): p. 243

gained important ground since the International Geophysical Year (1957-58), in large measure due to the efforts of geophysicist Lloyd Berkner during the early 1950s, Landsberg believed that more visibility and more financial support were appropriate given what he termed the "ferment of the times."[58] As he continued, there has "never been a question of relevance of geophysical work for the people. All of the operational work and much of the research are concerned directly with the safety, the resources, and the recreation of people." By situating geophysics within a broader historical narrative of the American experience, Landsberg believed that the AGU embodied patriotic ideals. This, he believed, justified more recognition commensurate with the emergent role within society, particularly when it came to discussions of the environment.[59]

With a firm belief that the AGU and geophysics, generally, had an active role to play in society, Landsberg created a committee to survey and evaluate the geophysical aspects of environmental problems in 1969. He called it the Committee on Environmental Quality (CEQ), and asked his colleague, S. Fred Singer, to direct the effort.[60] Perhaps driven in part by his desire to integrate classical physicists with the geophysics profession, the decision to nominate Singer met with approval from other elite members of the scientific community. For instance, Philip Handler, President of the National Academy of Sciences, expressed his "delight" that Landsberg chose "someone as capable as Dr. Singer to lead this enterprise."[61] Others chimed in with their support of the effort: "If I did not feel strongly about the urgency of this problem, I would be less enthusiastic, but at this point in time there is no choice," noted Robert Ragotzkie, Director of the Marine Studies Center at the University of Wisconsin.[62] With an approved budget of $2000, the

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[58] For Landsberg's reference to 'ferment of the times,' refer to, Landsberg, Helmut, "Scientists and Politics," *EOS* 50, 7 (July 1969): p. 471

[59] Landsberg, "Geophysics and Government," *EOS*, Vol. 50, 3 (March 1969): p. 75

[60] The AGU Committee on Environmental Quality should not be confused with the Council on Environmental Quality within the Executive Office of the President.

[61] Handler to Landsberg, 6 February 1970, Box 16: Committee on Environmental Quality, 1970, AGUC

[62] Ragotzkie to Singer, 3 February 1970, Box 16: Committee on Environmental Quality, 1970, AGUC

committee had made a "splendid start" and had clearly the "full backing of the officers and council."[63]

Singer's wide-ranging interests in global environmental problems combined with his professional background made him an apt choice to chair Landsberg's new committee.[64] One of the first and foremost reasons was because he had an acute understanding of global ecological issues. In December 1968, for instance, he was invited to speak before the Symposium on Bioengineering and Cabin Ecology at the AAAS, titled "Spaceship Earth -- A Global View of Ecology." Within his talk, Singer referred to the "close analogy" between the ecology of a spaceship cabin and the environmental processes on Earth.[65] He noted how the "natural balance" on Earth was altered by modern man's "gregariousness" in the creation of cities and subsequent discharges of waste in "large local concentrations" into rivers and into the atmosphere. With Cole in mind, he believed that issues like oxygen deprivation and carbon dioxide emissions were worthy of further investigation, and it was up to scientists to examine whether such changes could have far-reaching geophysical effects or far-ranging effects in the biosphere. "It behooves us, therefore, to examine very carefully and even conservatively all pollution effects from human activities. It is important also that this examination involve scientists from different specialties but with broad interests," he concluded.[66]

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[63] Landsberg to Karl Droessler, 8 May 1970, AGU Collection Box 16: CEQ, 1970

[64] Singer was involved with many of the most important developments in post-WWII geophysics, including early discussions that led to the International Geophysical Year from 1957-1958. For a brief account of Singer's presence at the dinner that initiated ideas of a third "international polar year," see Ludwig, George. *Opening Space Research: Dreams, Technology, and Scientific Discovery* (Washington, D.C.: American Geophysical Union, 2011), p. 68. For a comprehensive background to the IGY, see Susan Barr and Corneila Ludecke, eds. *The History of the International Polar Years* (Heidelberg, Germany: Springer, 2010). Also, in December 1968, the American Association for the Advancement of Science (AAAS) under Singer's leadership organized a symposium in Dallas entitled, "Global Effects of Environmental Pollution." To "kick off" the CEQ, Singer decided to publish the proceedings of this symposium in 1970. Singer to A.F. Spilhaus, Jr., 4 February 1970, Box 16: Committee on Environmental Quality, 1970, AGUC. Also, refer to: Singer, "Global Effects of Environmental Pollution," *EOS* (April 1970): pp. 476-479

[65] There is no shortage of literature on the importance of seeing the earth's isolation within space to the development of the global environmental movement. See, McCormick, John. *Reclaiming Paradise: The Global Environmental Movement* (1989); McQuaid, Kim, "Selling the Space Age: NASA and Earth's Environment, 1958-1990," *Environment and History* 12, 2 (May 2006): pp. 127-163

[66] Singer, S. Fred, "Spaceship Earth -- A Global View of Ecology," 30 December 1968, Box 42, S. Fred Singer Papers

Of particular interest to Singer was the economics of pollution. For him, analyzing the benefits vs. costs of environmental pollution was fundamental for realizing higher levels of environmental quality while meeting surging market needs. Given an emerging interest in the global effects of environmental pollution, and his own role within the Department of Interior, he was committed to what he saw as a responsible balance between population growth, resource use, and the limits of the natural environment. His awareness of federal bureaucracy was also informed by his position as a member of the Federal Council on Science and Technology (FCST).[67] Established in 1959 by President Eisenhower's Executive Order 10807, the FCST was designed to evaluate and coordinate federal policies and programs for scientific and technology research and education. These two positions -- member of the FCST and Department of Interior administrator -- provided a fruitful opportunity to understand and appreciate the burgeoning interest of federal agencies in matters of the environment and conservation.

After being asked to head up the CEQ, Singer submitted a proposed list of goals for the AGU Executive Council. Having discussed his ideas with Landsberg, Singer envisioned the CEQ as having many different but complimentary agendas that would serve to provide what he considered a "middle course" within environmental politics. First, it would act as a "clearing house for information" for geophysicists of various specialties. Second, the CEQ would be responsible for educating students and the "general scientific community" on environmental problems in order to stimulate interest in the burgeoning field of environmental quality.[68] Third, per the wishes of Landsberg in his effort to acquire manpower for the geophysics professions, the

[67] For a short account of the origins of the FCST, see Wang, Zuoyue. *In Sputnik's Shadow: The President's Science Advisory Committee and Cold War America* (New Brunswick, NJ: Rutgers University Press, 2009): pp. 161-165. The FCST was replaced by the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET) in 1976.
[68] The CEQ created an undergraduate scholarship program for group-study of environmental problems in 1971. This program encouraged students to participate actively in environmental regional issues and ultimately seek ways to improve the quality of the environment. Little evidence, suggests that the environment education programs were very successful, primarily due to budget considerations.

committee would be responsible for "retreading" physicists to "enter into the environmental sciences." To do this, the committee would cooperate with the Atomic Energy Commission and other organizations that ran "large national laboratories" which housed good physicists and ecologists. Lastly, the CEQ would seek to counter misinformation in the public sphere about the nature and consequences of environmental threats.[69]

Prospective members were "drawn widely from throughout the country" -- including the atmospheric sciences, water management, geophysics, chemistry, and ecology.[70] Besides working directly with Landsberg and Abelson, Singer envisioned the CEQ as a partner with the "business-industrial-mining community" and national research laboratories, such as those funded by the Atomic Energy Commission (AEC). While this was interpreted as heretical for most environmentalists because it would appear as if he was working with those who advocated for complacency, Singer believed that such relationships could serve to strengthen the ties among what he considered moderate voices within environmental discussions. In their minds, Singer and Landsberg were not creating another environmental activist organization; their vision was something different in that they sought to provide a forum in which interests of various stripes could come together to solve national problems. This effort, he realized, would require a great deal of promotion given what they considered to be the level of hyperbolic rhetoric between what he saw as more extreme elements within environmental politics.

On April 20-24, 1970, the AGU held its annual meeting in Washington, D.C.   This was an auspicious occasion because Singer took the opportunity to declare the interest of the AGU to

[69] Agenda Notes, 19 April 1970, Box 16; Committee on Environmental Quality, 1970, American Geophysical Union Collection, American Institute of Physics, College Park, MD (hereafter referred to as AGUC)

[70] A July 1970 list of individuals who agreed to contribute to the CEQ included (1) William Ackerman (Chief, Illinois State Water Survey); (2) Orson Anderson (Lamont-Doherty Geological Observatory); (3) Earl Droessler (Vice President for Research, State University of New York); (4) Edward Goldberg (Scripps Institute of Oceanography; (5) William Kellogg (National Center for Atmospheric Physics; (6) Robert Ragotzkie (University of Wisconsin); (7) S. Fred Singer (Department of Interior; (8) George Woodwell (Brookhaven National Laboratory). Ex officio advisory members included: (1) Helmut Landsberg; (2) Philip Abelson; (3) Francois Frenkiel; (4) Joseph Kaplan; (5) Thomas Malone; (6) Athelstan Spilhaus; (7) Abel Wolman. There is also evidence that others expressed interest in serving on the committee, including Robert Cohen (ESSA) and W.F. Libby (UCLA).

engage in environmental issues. While a transcript of his talk has yet to be discovered within existing archives, secondary accounts reveals how Singer framed the CEQ's mission. As Stuart Auerbach, a staff writer with the *Washington Post*, noted while covering the spring meeting, Singer seemed adamant that the CEQ's mission would extend beyond reaching out to physicists and educating the youth. According to his recounting, Singer stated that the goal was to create a "'truth squad' to combat 'exaggerated statements' about environmental problems."[71]

This was a shrewd tactical move because his statement coincided with the first Earth Day took place on April 22, 1970.[72] As revealed in Auerbach's article, the CEQ was not another activist organization for a better environment; their goal was instead how environmental issues were *communicated* and *framed* by scientists. Their efforts would be about grounding the discussion in science, not advocating for ways to better the environment directly as if another lobbying effort by concerned scientists. As prominent physicist Joseph Kaplan noted in his acceptance letter to join the CEQ,

> No one can argue that the AGU action group on the environment will be just another of the rapidly increasing number of Committees, Councils, etc., in the field. In fact, the apparent ignorance, on the part of many who should know better, of the structure and role of the AGU in this field is startling and at times, discouraging.

For Kaplan, the opportunity for the AGU was clear, and he looked forward to "work with my distinguished friends" such as Singer and Landsberg.[73] For Singer, the gauntlet had been set: geophysicists were asked to engage in the rough-and-tumble world of popular environmental discussions, and it would not be the typical environmental activism seen in the media.

By engaging with environmental politics, however, the CEQ had to tread lightly but effectively. On the one hand, the CEQ was interpreted as an excellent contribution to

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71 Auerbach, Stuart, "Scientists Told to Fight Exaggerations," Washington Post, 23 April 1970, p. A9
72 For a detailed historical account of efforts to organize the first Earth Day on April 22, 1970, see Rome, Adam, "The Genius of Earth Day," *Environmental History* 15 (April 2010): 194-205
73 Kaplan to Helmut Landsberg, 3 February 1970, Box 16, Folder 10, AGUC

environmental discussions because it brought geophysicists into the fold; more interdisciplinary work on environmental problems could only benefit understanding. On the other hand, some resented what appeared to be the audacity of Singer, an administrator from the Department of Interior, to claim the mantle of truth about environmental threats. Julian Holmes, a board member of the Prince Georges County Civic Federation in Maryland and President of the Citizens Association of Friendly, believed that Singer's "truth squad" was nothing more than "propaganda" for the Interior Department; by using the language of environmentalists, he implied, Singer acted as a kind of Trojan horse that could ultimately undermine what he considered genuine efforts by citizens and local groups to curb environmental degradation.

Holmes justified his characterization of Singer by rebuking what appeared to be the lackluster performance of the department's pollution control efforts -- which were "little short of dismal." He used the amount of sludge and waste being deposited in Piscataway Bay just off the lower Potomac River as a prime example.[74] "The Piscataway Bay scandal is the most embarrassing testimonial to their failure to enforce Federal anti-pollution law," he wrote. Holmes believed that Singer's role in the AGU was an affront to environmental interests by virtue of his direct association with the department; he would have rather seen someone "free from political obligation to play down the importance of our rapidly deteriorating environment."[75]

In spite of his clear antipathies toward some sectors of the environmental movement, Singer nonetheless saw himself as a sympathetic to their concerns that something should be done once uncertainties had been identified and resolved. As shown in his "spaceship earth" speech,

[74] Levey, Robert, "Tydings Asks Planning to Hit River Pollution," *The Washington Post*, 9 April 1970, p. B6. Also, see Holmes testimony in May 1970 about his concerns as a citizen, "To Consider the Matter of a Proposed Sewer Conduit Crossing of Piscataway Park Lands," Hearing before the Senate Subcommittee on National Parks and Recreation, 21 May 1970
[75] Holmes to Newell, 11 September 1970, Box 16, Folder 10, AGUC

he use the same language used by concerned environmentalists and appeared entirely willing to investigate what some perceived to be legitimate threats to the welfare of humanity. Additionally, his concerns for the influence of who he saw as more passionate individuals like Cole was not entirely divorced from more moderate perspectives within the environmental movement. In February 1969, for instance, Thomas Kimball, the Executive Director of the National Wildlife Federation (NWF), spoke to the American Society of Civil Engineers regarding what he perceived to be an insidious influence of the media on national environmental discussions. "Our concern," he began, "is born not of a union between prophets of doom and newspaper headlines, but rises from the genuine alarm expressed by diverse and respected segments of the scientific community."[76]

This was an important point because it distinguished Kimball's efforts from what he saw as more outspoken elements embedded within environmental politics. For him, it was crucial that the NWF not be grouped together with those who often practiced heated rhetoric while ignoring the realities of energy production in a quickly growing society like the United States. Rather than suspecting ulterior motives by credentialed scientists like Singer, as Holmes had done in 1970, Kimball believed that a pragmatic appreciation of the scientific establishment was warranted:

> As I have often said, the National Wildlife Federation diligently tries to practice the 'art of the possible.' And difficult as it is sometimes, the Federation avoids the unrealistic, extreme points of view which so often dominate the conservation headlines. Not that extremists don't sometimes have a good case; but in the final analysis, the extremist seldom makes a constructive contribution to the resolution of a critical issue.[77]

In spite of those statements that appeared to mirror the concerns of environmentalists, Singer's core motivations were never immune from suspicion by those who deemed the

[76] Kimball, Thomas, "Thermal Pollution is a Problem," Box 24, Singer Papers
[77] Kimball, Thomas, "Thermal Pollution is a Problem," Box 24, Singer Papers

environmental protection as the noblest of ambitions. According to individuals like Holmes, Singer's claim that he represented an environmental "truth squad" appeared disingenuous and propagandistic. For those like Holmes, Singer represented what appeared to be nothing more than collusion between industrial interests, the government, and elite scientists; without a concerted effort to stimulate fundamental change, those who considered themselves seriously committed to the environmental movement were doubtful that 'the system' would accommodate their interests. Nonetheless, Singer's statements appeared to resemble those of passionate but possibly less frustrated elements within the environmental movement. For individuals like Kimball, Singer's concerns would have sounded reasonable and tempered given what they saw as manageable problems.

Importantly, the existence of scientific uncertainties regarding the long-term effects of environmental pollution provided room for individuals to reasonably differ in their assessment of environmental risks. Those who expressed concern for the environment during the 1960s and 70s could differ in their appreciation of the risks involved, and individuals could pose solutions that were entirely proportionate to their assessments of the risks involved. The dividing line appeared when individuals -- some of whom were scientifically trained -- disagreed over the proper manner to engage the general public. The establishment of the CEQ was potentially problematic because it appeared to others as a politicized effort to disrupt and confuse the general public about serious threats. This contrasted with the statements of Singer, who suggested that it was an attempt recognize and communicate how scientifically uncertain such threats were. Indeed, the CEQ prompted Landsberg to reflect on how to maintain his professional ideals while contributing to discussions about the environment.

RISKS OF ADVOCACY

After creating the CEQ, Landsberg wondered whether scientists and the organizations they create to provide what he thought was sound knowledge could slip into advocacy. For him, the traditional goals of scientists -- publishing in journals, holding meetings, fostering scientific exchange -- seemed insufficient in modern environmental politics. He reflected on how scientific organizations and meetings could, if not properly managed, become the "tumbling places of political issues and the societies would soon be split asunder." Would it not be absurd, he imagined, to have "a Republican Physical Society or a Democratic Chemical Society or a Socialist Geophysical Union?" For him, the CEQ must avoid the perception that they were indulging in undue advocacy of political interests, a prospect that was all too real given what appeared to be the increased difficulty of maintaining what they considered to be a middle ground within environmental debates. "Let the scientific societies stick to their own knitting" and avoid "lobbying for one or another solution of a problem," he cautioned.[78]

Nonetheless, Landsberg believed that the CEQ skirted around this potential liability by allowing space to publicly advise and/or comment on what he called "social concerns." Indeed, Singer had already been mulling over the most effective way to engage with environmental issues without falling into the kind of advocacy and alarm that he perceived in Cole's activities. Unlike the generous Ford Foundation which provided funds for specific research agendas, a research-oriented RAND Corporation which essentially had little public engagement, and the federal government which appeared more driven by opportunism and political gamesmanship, Singer envisioned a "foundation" that "does some of its own thinking but also supports correlated work." For Singer, national and international discussions of environmental issues had become

[78] Landsberg, *EOS*, Vol. 50 (July 1969)

dominated by "people who are constrained by political considerations in the government."[79] The "foundation" would supplement federal efforts, but apparently provide a voice separate from the constraints of political considerations.

Months after the AGU established the CEQ, Singer was invited to testify in front of the House Subcommittee on Science, Research and Development on August 4, 1970. Reflecting his remarks when he introduced the CEQ in early April, he sought to provide an appraisal of the state of science in American society. His speech contained three principal segments: (1) the necessary role of the federal government in resolving environmental challenges given existing uncertainties; (2) his frustrations with popular anti-scientific and technology sentiment within popular culture; (3) and the need to direct society in a way that was devoid of what he saw as irrational and emotional claims about the future.

The first issue was relatively straightforward. All federal agencies, Singer argued, had the obligation to fund research specific to their missions. Science was fundamental; only through scientific research could federal agencies understand and effectively ameliorate environmental hazards. This naturally led to his second point about what he saw as accusations by environmentally-conscious advocates that the federal government was complacent. To the contrary, he argued, emotion-laden pleas were dangerous because they undermined the very goals of both institutions and government to protect the welfare of humanity *and* the environment. Singer's statements suggested that he was a pragmatic reformer, and sought policies that he believed could realistically deal with environmental problems without sabotaging the system.[80]

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[79] Agenda Notes, 19 April 1970, Box 16; Committee on Environmental Quality, 1970, American Geophysical Union Collection, American Institute of Physics, College Park, MD (hereafter referred to as AGUC)

[80] Michael McCloskey distinguished between multiple environmental groups during the 1970s, one of which was what he called "pragmatic reformers." This group did not believe that entire political and economic systems needed to be changed and that

His last point pertained to the role of 'proper' public communication in avoiding any confusion about the nature and urgency of environmental threats. Reverting to his ideas as set forth in his proposed goals of the CEQ months earlier, he reasoned that the alleged dramatization of environmental threats prevented the public from understanding the importance of science in their everyday lives. For Singer, the promise of science could only be realized if scientific communication was perceived as accurate and dispassionate. What he saw as emotional, irrational cries of environmental destruction appeared to distort the problems and made scientists appear more authoritative than the scientific evidence justified, while ultimately undermining the very weapon necessary to overcome environmental challenges -- expertise. "Unfortunately," Singer argued, "much information is presented to the public which is either incorrect, or based on fanciful extrapolations, or -- at the very best -- unbalanced or incomplete."[81]

His faith in the power of science and technology to solve the nation's environmental problems was born in part from his belief in the virtue of a compromise between pro-growth ideology -- what he defined as "the economic forces which are creating continually expanding and extravagantly wasteful consumer demands" -- and what he termed "radical" groups intent on "tearing down the capitalistic system."[82] In a 1970 letter to Russell Train, Chair of the President's Executive Council of Environmental Quality, Singer outlined what he believed to be the importance of attaining a quality of life without simultaneously sabotaging what had already been gained over centuries. "I don't think we should be defensive about growth; we want growth, but in the right direction," he argued. As he continued,

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existing institutions were sufficient to protect the environment. See McCloskey, "Twenty Years of Change in the Environmental Movement," in Dunlap and Mertig, eds. *American Environnmentalism* (1992), p. 78

[81] Singer, "Statement before the Subcommittee on Science, Research and Development," 4 August 1970, Box 42, S. Fred Singer Papers

[82] Singer, "Statement before the Subcommittee on Science, Research and Development," 4 August 1970, Box 42, S. Fred Singer Papers

> We want the type of growth which -- overall will improve what you have defined as 'quality of life' for the population as a whole. . . It is of fundamental importance, therefore, that we assess, as completely as possible and, of course, as accurately as we can do it, the environmental costs of all our economic activities.[83]

Singer's statements suggest that he was not overtly unsympathetic to individuals like Cole and Holmes. While his statements suggest a strong suspicion of some environmentalists who engaged the public in a manner that he saw as premature, he also appeared to question the virtue of unmitigated growth that undermined humanity's relationship with the environment. Ultimately, he used the opportunity to restore public confidence in the value of science in American society, and maintain what he considered to be a middle ground between industrial and corporate interests and environmentalists. Singer's statements reflected his frustration with what appeared to be the deteriorating relationship between the American public and scientific elites, a prospect experienced first-hand by his colleagues.

LANDSBERG IN AN 'UNENLIGHTENED AGE'

During the late 1960s, Landsberg experienced first-hand what appeared to him to be some sectors of American society to undermine the entire scientific apparatus from the ground up. How, he frequently wondered, could the general public suspect ulterior motives of legitimate scientists who sought ultimately to conduct research for their benefit? The apparent paradox became all the more visible to him while conducting climatic research in a small up-and-coming town just thirty minutes drive from the University of Maryland -- Columbia, MD.[84]

Having just arrived at the University of Maryland's Institute for Fluid Dynamics and Applied Mathematics after directing ESSA's Environmental Data Services, Landsberg became interested in the effects of urban growth on the local climate. Familiar with the Washington,

[83] Singer to Train, 29 April 1970, Box 22, S. Fred Singer Papers
[84] Stone, Brian Jr. *The City and the Coming Climate: Climate Change in the Places We Live* (Cambridge: Cambridge University Press, 2012): pp. 76-77

D.C. area, he was particularly attracted to the prospect of measuring and understanding the temperature disparity between the surrounding countryside and urban space itself, as well as the influence of development on pollution levels.[85] With funds provided by the National Science Foundation, Columbia -- as a fledgling community -- provided the perfect opportunity to see first-hand how urban growth influences over time the surrounding atmosphere. One of the first scientific attempts to monitor and understand the effects of urban growth after World War II on the climate, Landsberg's research pushed the frontiers of climatological investigations.[86]

After years of research, he noted what appeared to be a dramatic rise in urban temperature relative to the surrounding countryside -- what he called an "unmistakable" heat island effect.[87] This finding reaffirmed his long suspicion that human activity could have an appreciable effect on local climate systems, and -- for the first time -- he was allowed the opportunity to conduct a longitudinal study to determine the exact nature of those effects from the very birth of an urban community. This seemed to be an ideal state of affairs for an urban climatologist like Landsberg and for science generally. Nonetheless, while it seemed that his efforts had the broad support of the community, he soon discovered in the course of his research evidence for something else entirely: the effects of what appeared to be a popular suspicion and hostility to scientific investigation.[88] As he noted in 1979, "many of the townspeople were most sympathetic to our endeavors and a few volunteers cooperated with us for several years, there was also an anti-science element in a local college that sabotaged some of our work."[89]

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[85] For an account of his efforts in Columbia, MD, see "Air Pollution's The Nature of the Beast, Tests in Scratchbuilt City Indicate," *Daytona Beach Morning Journal*, 4 January 1971, p. E3

[86] For a brief account of Landsberg's role in urban climatology, see Gaffen, D. and R.D. Bornstein, "Case of Urban interactions with a Synoptic Scale Cold Front," *Meteorology and Atmospheric Physics* 38 (1988): pp. 185-194

[87] Landsberg, Helmut, "Atmospheric Changes in a Growing Community (The Columbia, Maryland Experience)," *Urban Ecology* 4 (1979): pp. 53-81

[88] For an overview of skepticism of citizens toward science during the 1970s, see Fischer, Frank. *Citizens, Experts, and the Environment: The Politics of Local Knowledge* (Durham, NC: Duke University Press, 2000). Also, Foley, Michael. *Front Porch Politics: The Forgotten Heyday of American Activism in the 1970s and 1980s* (New York: Hill and Wang, 2013), p. 103

[89] Landsberg (1979), p. 57.

Landsberg's reference to an "anti-science element" among America's youth was unique to his published results. His concerns were also conveyed in 1971 to Frank Eden, Program Director of Meteorology at the National Science Foundation and sponsor of Landsberg's meteorological study. As he described after noticing that his equipment had been damaged,

> In the past two years, we suffered again from acts of vandalism, directed against our installation in Columbia . . . It would perhaps be tolerable if the losses had to be attributed only to thoughtless mischief but we have evidence that some of it is due to an anti-science attitude among certain groups of people. Locks and property signs are of no avail against the slip backward into an unenlightened age.[90]

Landsberg's experiences in Columbia served to reinforce a growing suspicion toward activities and behaviors of some sectors of the general public that he believed sought to undermine the integrity of American science. Those whom he perceived as prophets of doom and overly aggressive environmentalists -- what historian of science Sharon Kingsland calls an "increasingly militant environmental movement" that was frequently accompanied by "feisty" rhetoric -- merely served to highlight what he saw as an underlying "anti-science attitude" that appeared to pervade American culture.[91] Where the destruction of monitoring equipment by vandals were motivated by a broader skepticism toward the motives of scientists, he reasoned, whom he saw as promulgators of future catastrophe were equally guilty for risking the credibility of the scientific establishment -- the destruction of equipment was merely a more tangible version of the destructiveness of what he saw as overly heated rhetoric. Ultimately, conveying the importance of the CEQ and the middle ground was not going to be easy given what appeared

[90] Landsbeg to Eden, 28 June 1971, Series 2.1, Box 2: Daily File January 1971-June 1982, PHL. For an expanded discussion of enlightenment ideals and intellectualism, see Michael, John. *Anxious Intellects: Academic Professionals, Public Intellectuals, and Enlightenment Values* (Duke University Press, 2000)

[91] The increasingly active public role of ecologists in the 1960s and 70s stands in contrast to their general avoidance of public controversies in the 1950s. See Kingsland, Sharon. *The Evolution of American Ecology, 1890-2000* (Baltimore: Johns Hopkins University), p. 220. For ecology as a "subversive science," see Sears, Paul, "Ecology -- A Subversive Subject," *BioScience* 14, 7 (1964): pp. 11-13

to be a tendency for individuals to retreat into what he and others saw as distinctive and adversarial camps within environmental politics.

COMMUNICATING THE MESSAGE

To combat what appeared to be a wildfire of hasty speculation regarding environmental threats, and prevent what appeared to be signs that America was becoming suspicious of the scientific establishment, Singer himself sought what he cast as a "measure of calm in the reporting of ecological disasters."[92] As he recounted during a talk at Rice University in 1971 regarding the motivation to establish the CEQ,

> The situation became worse and worse and new horror stories were being bandied about telling of things that were going to happen if we didn't do this or didn't do that, and of the world coming to an end, and I finally prevailed upon my major professional society, the American Geophysical Union, to set up a committee for environmental quality. We did, and of course they made me chairman. Then we had to do something, and we decided we would try to rectify the situation in a small way by spreading the true word about environmental facts.[93]

Claims about future disasters were not only a distraction, he argued, but a serious problem worthy of a concerted effort by elite scientists to understand and ultimately counter. For figures like Singer and Landsberg, contributing to discussions of environmental problem meant expanding one's responsibilities beyond the laboratory; it meant placing a greater prioritization on the communicative aspects of the scientific life. If sectors of the general public were already skeptical of the scientific establishment and they were susceptible to anxieties and fear toward the future, and the media appeared willing to spread and reinforce these messages of pessimism and fear, then retreating into one's comfort zone would only contribute to what appeared to be an imbalanced treatment of environmental issues.

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[92] Report of the Committee on Environmental Quality, 31 December 1970, Box 16: Committee on Environmental Quality, 1970-71, AGU Papers

[93] Singer, S. Fred. "SST and the Environment." Rice University. Houston, Texas. 5 April 1971.

The CEQ was, in effect, a way to "preempt" the spread of what Landsberg and Singer saw as the spread of unnecessary anxiety and confusion. "With respect to educating the general public," Singer described in 1970,

> We would probably provide a great service by presenting rational, non-emotional studies of a popular nature concerning environmental hazards. I find the field preempted by doomsday prophets who are usually not capable of analyzing the geophysical data, and I feel that some balance ought to be restored to the national discussion.[94]

As Landsberg also noted, environmental problems have been treated "subjectively and with great superficiality by alarmists and traditionalists alike." He went onto note that,

> we find everything from cries of Cassandra to business as usual. Knowledge in depth in all relevant areas, and interrelation of this knowledge in breadth, is needed to set the new course where man and other creatures can live harmoniously for a long-term future.[95]

Singer, as the face of the CEQ, took every opportunity to appeal directly to environmental scientists. One of many opportunities presented itself in November 1970, when he was invited to give a talk in Paducah, Kentucky. The site of an AEC-managed uranium enrichment plant built in the 1950s provided a symbolic backdrop to discuss his concerns within about popular fears over science and technology. According to the Paducah *Sun-Democrat*, the local newspaper that covered his talk, Singer talked about the importance of what he saw as sober and cautious statements about issues that were ultimately technical and scientific: "We need to provide a voice of reason, not just alarm. As scientists, we have the responsibility to speak up, but we must also know when to stop talking." The editorial staff conveyed their own beliefs on the matter in light of its coverage of Singer's talk: "We believe the advice Mr. Singer

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[94] "Report to the Council of the American Geophysical Union from the Committee on Environmental Quality," 19 April 1970, Box 16: Committee on Environmental Quality, 1970 (AGUC)
[95] Landsberg, Helmut, "The President's Page: Environment," *EOS: Transactions of the American Geophysical Union* 51, 4 (April 1970): p. 243

gave his colleagues is applicable to everyone. The problems of environmental pollution and man-made threats to the balance of nature should be viewed with reason as well as alarm."[96]

For Singer and the editorial staff at the *Sun-Democrat*, the balance of reason with alarm was long-hand for being alert but not what they considered hysterical. Alertness was not only deemed to be a path for responsible science; it also served as a warning to the environmental science community that prudent reticence with respect to public communication was equally important to maintain the credibility of scientific claims in societal affairs. All appeared to agree that there were very real environmental threats out there that could harm society and all appeared to agree that further investigation of the scope and nature of these threats was warranted. His message was not one of disbelief, but rather what appeared to be legitimate disagreements in how to understand and publicly discuss such threats.

Inspired by his experiences in the DOI, Singer published an article in *Science* in late 1970 entitled, "Will the World Come to a Horrible End?"[97] For those who already knew something about the motivations behind the CEQ, this article was unsurprising. Abelson, for instance, praised Singer for reminding "readers that the presently known facts do not justify the gloomy predictions of disaster that some members of the public seem to find satisfaction in making."[98] In the hope that his article could be useful in steering popular debates away from so-called "alarmist" claims, Singer sent it to the editor of the *Washington Post*, Howard Simons, just prior to its publication. Given the incentives of newspapers to highlight catastrophe, Singer believed that such a piece could "dampen down some of the extravagant claims that have been made concerning impending ecological disasters."[99]

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[96] "View with Reason as Well as Alarm," 18 November 1970, *Paducah Sun-Democrat*, American Geophysical Union Collection Box 16: Committee on Environmental Quality, 1970, American Institute of Physics, College Park, MD
[97] Singer, S. Fred, "Will the World Come to a Horrible End?" *Science* 170, 3954 (9 October 1970): p. 125
[98] Abelson (April 1971)
[99] Singer to Simons, 20 August 1970, Singer Papers, Box 3

One of the dangers, Singer believed, was that scientists would quickly lose their credibility and become the very prophets of doom that the CEQ was established to counter. As he noted,

> We should be careful not to cry 'wolf' needlessly or too often. The public and the media give special weight to statements from anyone who is a scientist, provided they make news. Scientific credibility can easily be lost by exaggerated claims and extravagant statements.

Like his talk in Paducah in August 1970, Singer believed that scientists -- and now the gatekeepers of the media -- needed to "provide a voice of reason, not just of alarm." But most importantly, he argued, "we also must know when to stop talking."[100] While it is unclear if Simons was receptive to his plea, it was clear that Singer was serious about counter-balancing the rhetoric within the news media.

For some industrial scientists, Singer appeared to represent their perspectives well. S. Baron, Vice President of Engineering for the engineering firm, Burns and Roe, Inc., wrote to Singer about what he saw as the ambivalent and occasionally contradictory messages communicated to the general public. He noted the tendency of "gloom-and-doom debaters" to quote and misquote the affects of water and air pollution on life and health of animals, while the "light and bright debaters" -- a term to denote those who were overly optimistic about the future -- fixate on the benefits of pollution and insignificant effects on the environment. The effects it had on the economy were apparently evident. The building of new power plants, he argued, was delayed by these objections by the general public and state agencies, all apparently under the banner that environmental protection was more important than human standards of living and contemporary needs. The answer to environmental problems, he argued, was to create politics

[100] Singer (9 October 1970): p. 125

that maximized environmental protection while making the costs of reducing pollution levels of power plants "not excessive."[101]

In light of general anxieties about what appeared to many environmentalists as collusion between the federal government, some interested members of the scientific community, and industrialists, one could appreciate what appeared to be Singer's growing reputation among some sectors of the industrial community. "You have performed a valuable service for the total good of the American public," said Frank Bodurtha, Senior Consultant in Air Pollution Abatement at the Dupont Company in Delaware.

> I am sure we both recognize the need to curb pollution based on specific problems that may be occurring. It is unfortunate, however, that many scientists, often out of their field and nonscientists have exaggerated the pollution situation and, thereby, have created undue alarm. Your remarks are timely and will be helpful in putting the pollution situation in proper perspective.[102]

There are many plausible reasons why Bodurtha saw common cause with Singer. First, Bodurtha's professional training in the geophysical sciences, as well as his standing within the professional engineering community, appeared aligned with Singer's own background in the geophysical sciences. During the late 1940s, he was a research assistant and instructor of meteorology, and later received his doctorate in meteorology at MIT. Alongside his position at DuPont, Bodurtha was also the Chairman of the Air Pollution Standards Committee of the American Society of Mechanical Engineers (ASME), a professional member of the American Meteorological Society, and Associate Editor of *Atmospheric Environment*, and international science journal.

Second, Singer perhaps appeared measured with regard to efforts to regulate pollution within the United States. In 1966, Bodurtha's committee within ASME proposed a set of

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[101] Baron, S., "What Price Power for Eliminating Pollution," c. 1970, Papers of S. Fred Singer, Box 24
[102] Bodurtha to Singer, 16 October 1970, Box 19, Singer Papers

technical norms to control national dust emissions entitled "Recommended Guide for the Control of Dust Emission." Arthur Stern, a representative of the Public Health Service, refused to approve it. The reason appeared to be Bodurtha's influence; as chairman of the ASME committee and high-level Du Pont engineer, he approved of a guide that would preclude any need on the part of Du Pont to invest further money into tougher pollution abatement measures. From the perspective of Stern, Bodurtha helped to forestall tougher regulatory measures that appeared to have not been in the best interests of Du Pont.[103] While Singer's statements as a whole and commitment to the middle ground suggest that he was not overtly driven to protect industrial and corporate interests, his efforts nonetheless appeared to indicate his greater sympathy for them rather than environmentalists about whom he frequently complained.

Singer's message was not, however, appealing only to industrial and corporate interests. On October 28th, Charles Houston, Professor and Chairman of the Department of Community Medicine at the University of Vermont, also praised Singer's attempts to steer the environmental discussion in what he considered to be the proper direction. While Houston argued that scientists must learn to share their knowledge with the general public, he nonetheless mirrored Singer's concern that scientists must not "lose our credibility by abusing the public ear." In Houston's mind, this was not a denunciation of those who had concerns; he himself shared concern that humanity was indeed facing a "major crisis." But there came a point where a line had been crossed, and scientific investigation somehow became secondary to making claims purely for the sake of inducing what he saw as public alarm.[104]

Singer also praised articles that he believed agreed with his own assessments of the communication problem. Early in his chairmanship of the CEQ, he lauded a recent editorial

[103] For an account of Bodurtha's role, see Uekotter, Frank. *The Age of Smoke: Environmental Policy in Germany and the United States, 1880-1970* (Pittsburgh: University of Pittsburgh Press, 2012): pp. 161-162

[104] Houston to Singer, 28 October 1970, Box 20, Singer Papers

written by the editor of *Environmental Science and Technology*, D. H. Michael Bowen. In his article entitled "Crying Wolf Once Too Often," Bowen argued that the media irresponsibly fueled an "apparently masochistic public with all manner of ill tidings from the environmental front." "You cannot," he further argued,

> tell people every day that they are being choked to death and poisoned without first scaring them out of their wits and then, as they realize they are still alive and in reasonably good health, making them disregard everything else you warn them about.[105]

That Singer contacted Bowen was entirely in keeping with the stated goals of the CEQ: members would contact newspaper, journal, and magazine editors and attempt to dissuade them from publishing articles that may induce or reinforce public anxieties about the future. Quite simply, was incumbent upon members -- Singer, especially, as the chairman -- to monitor how the scope and severity of environmental threats were communicated to the general public, and attempt to steer discussions away from what they considered to be unproductive exaggerations that were largely disconnected from what they considered the "true word."

When the President of the National Academy of Sciences, Philip Handler, supported Singer's appointment to chair the CEQ, his motivations in doing so extended beyond Singer's credentials. Like Abelson and others, he believed that so-called prophecies of doom required the active engagement of the most prominent members of the scientific establishment. His sentiments are vividly revealed when he was asked to testify in front of the U.S. House Subcommittee on Science, Research, and Development on July 21, 1970.[106] While the occasion seemed altogether normal -- the head of American science testifying to Congress about the

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[105] Bowen, D.H. Michael, "Crying Wolf Once Too Often," *Environmental Science and Technology* 4, 9 (September 1970): editorial.

[106] Testimony of Philip Handler, 20 July 1970, Hearings before the Subcommittee on Science, Research, and Development (Washington, D.C.: U.S. Government Printing Office, 1970): pp. 84-115; An abridged version of Handler's July 1970 testimony was later published. See, Handler, Philip, "Toward a National Science Policy," *Bioscience* 20, 17 (1 September 1970), p. 971

importance of science -- the occasion provided an opportunity for Handler to address what he believed to be a serious problem with environmental politics.

Like Singer, Handler warned that the national science apparatus was falling apart. He noted that undergraduate enrollments were down, that a pervasive lack of interest and skepticism toward science was undermining the role and reputation of science in society, and that these cultural forces were slowly eroding American scientific standing when compared to the post-Sputnik period of the late 1950s. Science, he argued, was acquiring a public reputation of being "immoral," "guilty of despoliation of the environment," and, maybe worse of all, "irrelevant" to fashioning a better life for Americans. Against the backdrop of this rather somber assessment of American science during the 1970s, the deflated president spoke about the role that scientists themselves played in the current state of affairs.

> I deeply regret that these trends have been fashioned in large part by some of my fellow scientists, prophets of doom who exaggerate the quite sufficiently serious environmental problems before us. I trust that this Nation will not pay too high a price for their self-indulgence in hyperbole.[107]

Importantly, Handler was not a member of the CEQ but he did sympathize with its broader objectives and acted when necessary. According to an account published in *Science*, Handler was given an opportunity in 1971 to induct the ecologist Lamont Cole into the NAS. In spite of what appeared to be a lengthy record of contributions to the science of ecology during the 1940s and 50s, Cole's subsequence activities as a public advocate for environmental causes seemed to breach what Handler believed to be traditional norms of reticence. The result: Cole was not accepted. Asked later about his decision, Handler suggested that it "behooves a scientist to be even more sure of his facts when speaking before the public than before a scientific

[107] Handler (20 July 1970), p. 85. Also see Philip Handler's follow-up interview, *U.S. News and World Report* 70, 1 (18 January 1971): pp. 30-34

body."[108] Unlike Singer, Abelson, Landsberg, or any others who expressed sympathy with the goals of the CEQ, Handler was unique in that he acted as a kind of official gatekeeper for the scientific establishment.

On the same day that Handler testified, there were others who reflected on the threat of so-called prophets of doom. Robert Sproull, noted physicist and president of the University of Rochester, submitted into the record his own theory regarding "prophets of doom." While mirroring the frustration of Handler regarding the prevalence of dire predictions within the media -- derided as sensational "slogan-mongering" -- Sproull presented a uniquely positive spin. Choosing to look at the greater scheme of things, those who were complicit in the media's tendency to sensationalize and exaggerate were not necessarily a problem. Instead, the "sensitivity" and "finely honed and useful sense of outrage" among the nation's youth was actually the first step to a healthier America:

> I look upon this sharply increased sensitivity as the major cause of optimism. The first step toward getting well is surely the recognition that one is sick. . . Ours is a vital and increasingly sensitive society; our growing sensitivity to pollution, to destruction of aesthetic values, to unfairness in human conditions, or to any behavior less than the best of which we are capable is our most valuable asset in making technology work for mankind.[109]

Like a sick patient, the declarations of "prophets of doom" were nothing more than the first sign on the path to a healthier society. While Sproull's comments were a rather optimistic interpretation of what Handler saw as a serious problem, short-term or otherwise, this divergent perspective highlights the important changes that were re-defining the role of scientists in American society.

[108] Walsh, John, "National Academy of Sciences: Awkward Moments at the Meeting," *Science* 172, 3983 (7 May 1971): pp. 539-542.
[109] Sproull, Robert, "Can We Make Science and Technology Work for Mankind?" Hearings before the Subcommittee on Science, Research, and Development (Washington, D.C.: U.S. Government Printing Office, 1970): pp.878-890. Sproull now serves on the Board of Directors for the conservative think-tank, the George C. Marshall Institute, located in Washington, D.C.

Sproull's discussion of youth's sensitivity and resentment toward science and technology was exactly the issue Singer had in mind when he aspired to educate the youth about environmental problems as chair of the AGU CEQ. For Singer, the youth had been co-opted by what he saw as ignorant emotionalism that resulted in the kinds of frustrations described by Landsberg and relation to his experiments in Columbia, MD. "One needs to capture them away from their senseless and energy-consuming efforts and demonstrations into the really vital social problems of our society," Singer noted in 1969.[110] Like Sproull, Singer was an optimist; he believed that the increased interest of the federal government and private industry in environmental problems of the 1960s would provide the institutional apparatus necessary to re-direct wasteful passions and energies. If they could be carefully guided to appreciate science and not steered to the whims of radicalism, the youth could be harnessed for the betterment of society.[111]

During the winter of 1970-1971, geophysicist Philip Abelson also began to sound the alarm about his concerns regarding the state of environmental politics. His first opportunity occurred in December 1970 when he gave a talk on what he termed the "The Environmental Crisis." According to the abstract of his talk,

> The mass media have saturated the public with dire predictions of imminent catastrophe arising from various forms of pollution. Some of the dangers that have been identified are real. Others are fictitious. This talk will provide an evaluation of the problems, progress, and prognosis. It will identify some of the ways in which geophysicists can participate in meeting the challenges.[112]

He followed up by publishing a short piece in the April 1971 volume of AGU's *EOS*. According to Abelson, the time had arrived for geophysicists to "obtain a piece of the

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[110] Singer to Patricia Marx, 1969 December 15, Box 21: S. Fred Singer Papers
[111] Singer, S. Fred," Man and His Environment: A Challenge to Education," *American Astronautical Society*, AAS 67-140; Singer, "Education for Today's Ecological Crisis," *Science Education*, Vol. 54, 4 (October-December, 1970): pp. 349-58. The latter publication was based on a talk Singer gave at the Annual Meeting of the AAAS on December 27, 1969.
[112] Anonymous, "Abstracts [National Fall Meeting, 1970]," *EOS* 51, 11 (November 1970): p. 734

environmental action" and suggested that they had a "special role" to play in putting "environmental problems in perspective." Like his colleagues in the CEQ, Abelson believed that the mass media had bombarded the "reasonably intelligent but scientifically uniformed" public with what he saw as disaster-laden stories of impending doom, which in turn built up into a kind of "hysteria" and "panic."[113]

Abelson was especially critical of what he saw as unthinking environmental advocates. In great need of moderation in the environmental rhetoric, he warned that the "mixture of truth, fiction, and exaggeration has created apprehension that will not soon be alleviated." Whether mercury in streams, DDT, oxygen starvation, climate change, or the eventual extinction of humankind, Abelson went so far as to note what he believed to be a latent hypocrisy of environmentalist "zealots" to ignore the Earth's culpability in creating the very problems frequently discussed in the media and by so-called "zealots". As he wrote, "It is fashionable these days to castigate man, science, and technology as great despoilers of nature. To some extent this is true, but many zealots lose sight of the fact that nature herself is a greater polluter."[114] By this, he did not seek to dismiss the very real problems humankind was creating for himself. To the contrary, he believed that acknowledging human-caused problems was appropriate; he only sought to remind readers of the importance of identifying the scope of Nature's contributions to environmental "pollution" relative to the contributions of humankind.

Given his dual-role as editor of Science and Vice President of the AGU, Abelson also took the opportunity to note his specific concerns about what he saw as "excessive emotion" within discussions over the environmental effects of detergents on the natural environment. According to his perspective, there was a great deal of uncertainty regarding the "plausible but

[113] Abelson, Philip, "The Environmental Crisis," *EOS: Transactions of the American Geophysical Union* 52, 4 (April 1971): pp. 124-128.
[114] Abelson (April 1971), p. 125

unproved hypothesis" that phosphates -- an important ingredient in detergents -- was the "crucial nutrient" that spawned damaging algal blooms. Given what appeared to be the unproved relationship, Abelson objected to what he called the premature and "heedless pressure" by environmentalists for the detergent industry to replace phosphates with what he perceived to be potentially more hazardous chemicals. This was not about dismissing the phosphate problem as unimportant; this was instead an opportunity to recognize the importance of acquiring what he saw as a robust understanding of environmental problems before forcing industries to change in a way that may, in the long term, prove more hazardous than the original problem.[115]

Taken together, Handler, Landsberg, Abelson, and Singer were not assaulting those who believed in the importance of the environment and the need for rational measures to alleviate the effects of industry on ecosystems across the country. This was not an attempt by the scientific establishment to marginalize environmental concern and dismiss it as "leftist" propaganda. Instead, they knew that the process of reforming environmental debates in their image would be an uphill battle, and would require a joint effort by scientists such as themselves. As Landsberg himself once attested, "Figuratively speaking, we may yet turn the ivory tower back into tusks again."[116]

Indeed, their motivation for pushing geophysicists to contribute what they saw as a more moderate tone into environmental discussions was their belief that the reputation and credibility of American science was at stake. While environmentalists would and did perceive their activities as supportive of a status quo system in favor of industrial needs, a perspective that was not altogether untrue, their statements and perspectives suggest a broader set of motives. First,

[115] Abelson, Philip, "Excessive Emotion about Detergents," *Science* 169, 3950 (11 September 1970): p. 1033. This was not the first time Abelson spoke on such matters. see, Abelson, "Pieces of the Action," *Science* 170, 3955 (16 October 1970): p. 265; Abelson, "Training Scientists for New Jobs," *Science* 1974, 4010 (12 November 1971): p. 651
[116] Landsberg, Helmut, "The President's Page: Environment," *EOS* 51, 4 (April 1970): p. 243

they believed that the recognition of scientific uncertainties and the pursuance of further research was a way to both restore the public trust and confidence in the scientific establishment over the long-term. While they clearly voiced what appeared to be a genuine frustration with outspoken environmentalists, the state of science appeared to support their interpretations and cautious approaches to environmental threats.

Second, they believed that conducting research away from what they saw as heated environmental politics was a way to protect their own professional ideals of organized skepticism and public reticence. This was a tricky situation. Both Singer and Landsberg occasionally reflected on whether their activities to combat what they saw as alarmism were indeed not in itself forms of political advocacy. Singer, as his experiences within the Department of the Interior, noted what appeared to him as the apparent irony that alarmism -- for all its drawbacks -- could benefit the state of science by pushing those in power to investigate environmental problems. What may be seen, in other words, as exaggerated rhetoric may actually be the very mechanism that prompts individuals like Secretary Udall to hire scientists like Singer, and motivate policy makers to invest additional funds into scientific research. In a very real way, the creation of the CEQ was an attempt by elite members of the American science community to engage in environmental politics while appearing to uphold their professional ideals.

CONCLUSION

By establishing the CEQ, the activities of Abelson, Landsberg, and Singer allow an opportunity for historians to combine two interrelated historiographies: public intellectual history and social movement theory. First, Singer, Abelson and Landsberg envisioned a role for themselves that, in some respects, mirrors the activities of public intellectuals. According to sociologist Charles Kadushin, public intellectuals are those who historically stood "at the door between intellectuals and their publics. It is an interstitial role."[117] As scientists, their ambition was to inform the general public in a way that would conform to their own understanding of the world (i.e. a worldview premised on a sound scientific understanding of environmental problems held by experts). They believed they had the knowledge and expertise to guide environmental discussions away from what they saw as the media's sensationalist portrayals and claims of so-called alarmists.

However, their efforts and their roles in society may not be in strict keeping with slightly Kadushin's definition; instead of popularizing science by directly engaging the public (they were not showmen), they instead sought to work with those who controlled the media that the general public read.[118] This, they believed, allowed them to engage with environmental politics in a way that both allowed them to protect their professional ideals while avoiding what they saw as the tendency by some sectors of the general public to dismiss the virtues of science and technology. They believed that science was strengthened when specialists like themselves discuss scientific questions amongst themselves, and keeping the production of knowledge separate from what appeared to be the crass and simplistic environmental politics as presented in the media.

However noble their intentions this pseudo-public engagement entailed its own set of risks. Their commitment to public reticence and avoidance of exaggeration made it very difficult to alter how the public actually understood and conceptualized the scope and urgency of environmental problems. They were not as 'visible' as other scientists. When they did speak in

[117] Kadushin, Charles. *The American Intellectual Elite* (New Brunswick, New Jersey: Transaction Publishers, 2009): p. xv. Also, Zinman, Richard, Jerry Veinberger, and Arthur Melzer, eds. *The Public Intellectual: Between Philosophy and Politics* (Lanham, MD: Rowman & Littlefield Publishers, Inc., 2003); Small, Helen, ed. *The Public Intellectual* (Malden, MA: Blackwell Publishers, 2002).

[118] A range of historical literature deals with issues of science popularization, particularly in the British context. See, Lightman, *Bernard. Victorian Popularizers of Science: Designing Nature for New Audiences* (Chicago: University of Chicago Press, 2007); Lafollette Marcell. *Science on the Air: Popularizers and Personalities on Radio and Early Television* (Chicago: University of Chicago Press, 2008); Bowler, Peter. *Science for All: The Popularization of Science in Early Twentieth-Century Britain* (Chicago: University of Chicago Press, 2009)

public, or when they did get their ideas reported in the newspapers, their communication of uncertainties may have had little potential to influence what they saw as highly-charged environmental debates. Historian Ronald Numbers describes the dilemma in the following way:

> Intense specialization, dependence on federal and corporate funding, and commitment to the ethos of disinterestedness, led many to avoid speaking out on public issues or, when they did, to speak with so much caution and nuance that their words carried little impact. Such reticence left the field open for popularizers and scientists with marginal credentials, who were anything but disinterested.[119]

Those whom Abelson, Handler, Singer, and Landsberg characterized as advocates and prophets of doom -- for all their drawbacks -- knew how to get the public's in a way that fueled public engagement. Indeed, that was their problem.

Second, the efforts of Abelson, Singer, and Landsberg may be characterized as a nascent social movement. According to social theorist John McAdam, social movements consist of people who "feel both aggrieved about some aspect of their lives and optimistic, that, acting collectively, they can redress the problem."[120] By establishing the CEQ, they in effect were attempting to resolve from the ground level what they saw as the problem of communication and public rhetoric about environmental dangers. For them, the AGU provided a familiar environment in which to organize and address their concerns, and therefore acted to both protect their interests while providing their goals with a certain level of credibility and coherency than if they had merely discussed the issues amongst themselves through private correspondence. That institutional platform was vital, and makes perfect sense given some scholarship in social movement theory.

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[119] Walters, Ronald, ed. *Scientific Authority and Twentieth Century America* (Baltimore: The Johns Hopkins University Press, 1997): pp. 8-9
[120] McAdam, Doug, et al. *Comparative Perspectives on Social Movements: Political Opportunities, Mobilizing Structures, and Cultural Framings* (Cambridge: Cambridge University Press, 1996): p. 5

According to Aldon Morris, a sociologist at Northwestern University, there is essentially two ways that scholars have envisioned social movements. The first envisions social movements as spontaneous and unorganized responses to significant social and cultural breakdowns. For him, scholars tended to present social movements as vacuous in terms of structure, guidance, and organization, and as a whole the patterns of activity tended to be irrational and emotional in response to highly-controversial contexts. Excitement and innuendo fueled members of these social movements, and they were largely reactive to outside circumstances. Consequently, this understanding of social movements provided little room for human agency, strategy, reason, and organization.

In contrast, Morris envisions the Civil Rights Movement in the United States as an opportunity to reframe how social movements acquire authority.[121] For him, social movements entailed the organization and planning of coherent responses to what were perceived as significant threats to a group's interest. Leaders mobilized resources and manpower, developed strategies, and ultimately guided social movements through the plethora of competing forces within society. This "political process model" of social activity provides room for a more robust analysis of those who seek to lead social movements.[122] Where sporadic and irrational popular movements tend to wither in the short-term, Aldon's research highlights the activities that drive long-term social movements.

In this light, establishing the CEQ was a strategy to counter what Landsberg, Abelson, and Singer felt were serious threats to the credibility and role of expertise in environmental

[121] See, Morris, Aldon, "Reflections on Social Movement Theory: Criticisms and Proposals," *Contemporary Sociology* 29, 3 (May 2000): pp. 445-454; Morris, "Black Southern Sit-In Movement: An Analysis of Internal Organization," *American Sociological Review* 46, 6 (December 1981): pp. 744-767; Morris, "A Retrospective on the Civil Rights Movement: Political and Intellectual Landmarks," *Annual Review of Sociology* 25 (1999): pp. 517-539

[122] For scholarship on the "political process model," see McAdam, Doug. *Political Process and the Development of Black Insurgency, 1930-1970* (Chicago: University of Chicago Press, 1982); McAdam, John McCarthy, and Mayer Zald, eds. *Comparative Perspectives on Social Movements* (Cambridge: Cambridge University Press, 1996); Sidney, Tarrow. *Power in Movement: Social Movements, Collective Action and Politics* (Cambridge: Cambridge University Press, 1994)

discussions. They sought to mobilize the scientific community, seek resources, and organize in a way that would allow them to implement what they deemed to be a long-term strategy to protect the credibility of the American scientific establishment. Toward that end, they each had to maximize the influence of their respective roles. First, as vice president of the AGU and editor of *Science* magazine, Abelson provided an expedient way for other like-minded individuals to address the scientific community. Second, as AGU President, Landsberg sought to acquire the manpower and institutional resources necessary to implement a broader conceptual strategy. For him, these actions were designed to strengthen the geophysics community, and construct an institutional platform on which to discuss and investigate more socially-relevant environmental issues. He was an agenda-setter. Third, Singer was the face of the CEQ, which meant that he ultimately took a more active approach to meeting its objectives. He would contact journal and newspaper editors, publish articles designed to mitigate popular fears of environmental disasters, provide congressional testimony, and coordinate with others within the scientific establishment to maximize the CEQ's influence.

Together, their hope was to stimulate a kind of social movement of scientists within American society without falling into forms of value-laden advocacy that could detract from their status as elite and dispassionate scientists. This meant discussing highly controversial and public environmental problems, and doing so in a way that would highlight the virtue of their strategy to monitor and regulate how environmental claims were made in the public sphere.

CHAPTER 2

LANDSBERG'S CRUSADE -- THE PRACTICE OF BOUNDARY WORK

In 1972, two years after leaving the presidency of the American Geophysical Union, Landsberg -- still a member of the AGU Executive Council -- reflected on the state of climate science. While most scientists would be thrilled at the opportunity to bring attention to their own field of specialization, Landsberg seemed rather restrained in his enthusiasm. As he wrote,

> In recent years much has been said and written about man's interference with climate. Alarming tales have been spread, many of them by persons whose standing as climatologists may well be questioned. And just as the competence of a cardiologist in neurosurgery may be doubted so may the judgment of atmospheric physicists or dynamicists in climatology. But the claims of omniscience in this field by some in other professions, even farther removed, such as biologists and even politicians are indeed astonishing and very misleading.[1]

Implied was a belief that the public communication of information is directly relevant to establishing one's credibility, and that one occasionally had to construct boundaries to insulate the discipline from those who he believed were untrained. This chapter is about Landsberg's attempt to protect his own discipline from what appeared to him to be the unqualified opinions of atmospheric physicists, science writers, and physicists.

As discussed in the previous chapter, Landsberg established the AGU Council of Environmental Quality for multiple reasons: (1) to "retread" classical physicists and increase the manpower of the geophysics community; (2) act as a "clearing house" for geophysical knowledge; (3) inject a geophysical understanding of the environment into national discussions; (4) act as a "truth squad" to combat exaggeration and sensationalism within popular culture; (5) construct what Singer called a middle ground perspective to restore the credibility of the scientific profession and exemplify the professional ideals that distinguished experts such as

[1] Landsberg, Helmut, "Human Influence on Climate," *Sartryck ur Lakartidningen* 69 (1972): pp. 2772-2778

themselves. But once his tenure had elapsed in 1970, Landsberg realized that climate change was becoming one of the most important environmental hazards to date -- and fodder for who he believed to be untrained scientists who felt that they had the knowledge to credibly and publicly speak about the climate.

Landsberg was not the only one who believed that popular discussions of climatology were potentially threatening to the credibility of professional and trained climatologists. Eight years after he spoke on the matter within his private correspondence, Arnold Court, a professor of climatology at California State University-Northridge and close colleague of Landsberg, believed that the situation had hardly been remedied. Speaking in front of his peers while giving a Presidential Address at the annual banquet of the Association of Pacific Coast Geographers in 1980, Court noted what appeared to him to be a tendency for scientists to emphasize single-factor explanations for climate change. "Climate is an elephant," he mused,

> being examined piecemeal by a horde of blind men. . . All wish to use the implications of their studies to estimate the conditions we and our immediate descendants will experience in the next decades and century. In their blindness, each of these elephant examiners reports and discusses his finding with little regard to the other parts of the same elephant, or the other elephants of different size and age. Most of these blind men are geologists, oceanographers, physicists, astronomers, biologists, paleontologists. Some are meteorologists, but very few are climatologists who have spent years studying the nature and variability and its measurement.[2]

Court's claims struck a chord. Writing to Landsberg a year later about what he had heard, David Miller of the University of Wisconsin-Madison characterized Court's speech as a "balanced statement of where we stand." He especially agreed with Court's concern with those who appeared to supplant the authority of trained climatologists with no justification. Even though they were untrained, he noted, they were nevertheless "willing to make pronouncements

[2] Court, Arnold, "Presidential Address: Changing Climate," *Yearbook-Association of Pacific Coast Geographers* 42 (1 January 1980)

about climatic matters and to robe themselves in what has become the Elisha's mantle of the 1980s 'climatology.'"[3] Landsberg, upon receiving Miller's letter, agreed with Court's assessment. "It is probably good for us climatologists to stand up on our hind legs . . . Your inspection of the climate-elephant is amusing and all too true," he wrote in the summer of 1981.[4]

Given the growth of climatology from what Landsberg called a "meteorological attic" of the 1940s and 1950s -- when "discarded meteorological observation were stored to be bundled occasionally into some bulk numbers" -- to a recognized discipline unto itself in the 1960s and 70s, he frequently voiced his concerns that climate was becoming yet another source of inspiration for so-called prophets of doom.[5] Landsberg believed that he could fulfill his AGU promise by protecting the disciplinary integrity of climatology from becoming just another environmental hazard discussed and dramatized within the public sphere. He wrote to journalists, appealed to scientists themselves, conducted behind-the-scenes correspondence campaigns, and monitored the cultural milieu for any infractions that he believed threatened the reputation and disciplinary integrity of climatology.

For such a young discipline, he believed that its scientific integrity and reputation had yet to be established and therefore needed protecting from who he considered to be the untrained. This chapter explores Landsberg's frustration with the peculiar tendency -- unwitting or not -- of some to overlook uncertainties, how these oversights contributed to popular and exaggerated speculations of future doom, and how scientists appeared to engage with the media in a way that he believed to be irresponsible. Nuclear physicist Howard Wilcox, science writer Nigel Calder,

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[3] Miller to Landsberg, 26 May 1981, PHL. The reference to Elisha's mantle is rooted in biblical scripture wherein the prophet Elijah passes his mantle of prophetic authority to Elisha in 2 Kings 2:11-14. The primary difference: some climatologists were not passing the authority onto others.
[4] Landsberg to Court, 23 July 1981, PHL
[5] Landsberg's reference to climatology as a "meteorological attic" is contained within a letter to the Secretary of the World Meteorological Organization (WMO) Committee on Climatology. Landsberg to Slavka Jovicic, 14 May 1970, Series 5, Box 18: WMO Commission for Climatology Correspondence (January-June 1970), HLP

as well as astrophysicists John Gribbin and John Eddy, frequently stirred Landsberg's passions. Without patient reticence, those who he believed were untrained would reign over the reputation of climate science, supplant experts like him, and claim the mantle of cultural authority for themselves. Given what he saw as the broader implications of America's potential slip backward into an unenlightened age and his own first-hand experiences with what he imagined as youthful vandalism in Columbia, MD, he sought to make sure that climate was not merely added onto a growing list of environmental catastrophes discussed in the media. These were concerns that began early on his career, beginning with his introduction to American science reporting of earthquake research in the 1930s.

EARLY VIEWS OF PROPER COMMUNICATION -- EARTHQUAKE PREDICTION

In 1934, Helmut Landsberg -- like many Jewish scientists in the 1930s -- migrated to the United States for a faculty position within Pennsylvania State College's Department of Mining Engineering. Knowing that his advisor and colleague from Germany, Beno Gutenberg, had already received a position a few years earlier at the California Institute of Technology, he saw great potential in migrating to the United States. For the first few years in his new position, he frequently published technical papers for the Pennsylvania State College Mineral Industries Experimental Station, and taught and developed courses in meteorology. Within a year, he built an instructional program in meteorology and became the program's first chair in 1935. With the development of courses and programs at other select universities after World War I, and the founding of the American Meteorological Society in 1919, meteorology during the 1930s was coming into its own as a professional discipline.[6]

[6] For a historical account of the professional development of meteorology during the interwar period, see Harper, Kristine. *Weather by the Numbers: The Genesis of Modern Meteorology* (Cambridge: MIT Press, 2008), Chapter 2.

It was during the first few years of his residence in the United States that historians are given a glimpse of Landsberg's personality and views about proper scientific communication. In 1935, twenty-nine year old Helmut Landsberg gave a talk about his latest seismological research at the 16th Annual Meeting of the AGU in Washington, D.C. This was an auspicious occasion for a young Landsberg, having just arrived from Germany the year before.[7] He discussed what he claimed to be a "rather remarkable positive correlation" between the occurrences of deep- and shallow-focus earthquakes.[8] Later published in the *Transactions of the American Geophysical Union*, the name of journal prior to becoming *EOS*, this was an important correlation because of its apparent promise to contribute to the science of earthquake prediction.[9] If one could monitor and record structural changes deep within the terrestrial crust and correlate those with earthquakes closer to the surface, Landsberg speculated, then one could potentially understand something fundamental about the timing and strength of future earthquakes. Deep-focus earthquakes could be a kind of geological prophet for the type of earthquakes that directly affected human societies. In spite of its long-term potential, however, Landsberg was careful to avoid the impression that this "positive correlation" represented a causal and theoretically robust relationship. "We present these things as an interesting item without emphasizing any theory too ardently," he concluded.[10] Prediction, forecasting, future-telling -- this was not his agenda, though it was apparently enough of a finding to warrant what he considered to be speculation by the media covering his talk.

[7] Unfortunately, it is unknown how Landsberg learned English.
[8] For a brief bibliography of Landsberg's earthquake research during the 1930s, see United States Department of the Interior, "Bibliography of North American Geology, 1929-1939," *Geological Survey Bulletin* 937 (Washington, D.C.: Government Printing Office, 1944): p. 549
[9] Landsberg, Helmut, "Some Correlations between the Occurrence of Deep- and Shallow-Focus Earthquakes," *Transactions of the American Geophysical Union 16* (August 1935): pp. 91-93
[10] Landsberg (1935)

Within days, *Science-News*, the news reporting publication of the American Association for the Advancement of Science, reported Landsberg's talk in a way that appeared to diverge from his original meaning. On the one hand, the article correctly noted that Landsberg "did not himself venture an earthquake forecast" and that he did show a "close hookup between deep-focus earthquakes and shallow-focus quakes." On the other hand, in spite of an explicit acknowledgment that Landsberg had not provided an earthquake prediction, they "inferred" from his research that a "severe earthquake" would occur somewhere in the world in July 1935 -- two months after his talk. While he -- naively perhaps -- aspired to a discussion of earthquake science isolated from external affairs, *Science-News* appeared to take journalistic license to situate Landsberg's talk within broader international concerns. Given a "deep-focus disturbance" that affected the small Japanese island of Formosa only days prior to Landsberg's talk, *Science-News* framed his research as a step forward in a long march to predicting future seismological dangers.[11]

Besides what he saw as the clear imprecision of such statements -- "somewhere in the world"; "close hookup" -- Landsberg wrote to *Science* to clarify his position. He disagreed with *Science-News*'s interpretation regarding what it incorrectly termed a "hint to earthquake forecasts." Given what he saw as the "obsolete" field of earthquake geography and geology, lack of trained specialists, and insufficient observational statistics, Landsberg believed that the science of earthquakes was severely underdeveloped. Prediction -- let alone accurate prediction -- was improper at best and dangerous at worst. Even so, Landsberg did not convey an aversion to the hope that, one day, accurate earthquake prediction would be a reality, and that new

[11] "Severe Earthquake in July if Apparent Rule Operates," *Science News-Letter* 27, 738 (June 1, 1935): p. 356; "Items," Science 81, 2105 (May 3, 1935): p. 11. For scientific accounts of the Formosa earthquake of 1935, see "Papers and Reports of the Formosa Earthquake of 1935" within the *Bulletin of the Earthquake Research Institute* of Tokyo Imperial University, vol. 3 (1936)

pathways of scientific exploration could be paved. "Summarizing, it has to be said that we have to admit 'ignoramus,' but there is no reason to believe in 'ignorabimus,' and the only conclusion which we have to draw is that more research is needed to attack the problem of earthquake prediction successfully."[12] By distinguishing "ignoramus" and "ignorabimus," Landsberg claimed that earthquake prediction was not necessarily impossible, but as of yet the knowledge was insufficient to make claims -- especially public claims -- about predicting future earthquakes.

Landsberg's reservations were an important indication of the state of geophysics -- and particularly seismology -- during the 1930s. During the early 20th century, seismology was primarily focused on local seismology with a smattering of interest in understanding the deep structure of the earth. As noted by historian Kai-Henrik Barth, the 1920s and 1930s ushered in the first generation of "program builders" -- figures like Beno Gutenberg, Charles Richter, and Hugo Benioff at California Institute of Technology; Maurice Ewing at Columbia University; Perry Byerly at the University of California, Berkeley; James Macelwane at St. Louis University.[13] Indeed, Landsberg's reaction to the *Science News* piece compelled his former doctoral adviser Beno Gutenberg to comment.[14] "Every once in awhile," Gutenberg began,

> the claim that earthquakes can be predicted is brought to public attention in the press or in other ways. Some of the claimants are obviously not competent; others proceed along rational lines but without due regard to the difficulties involved. Some may be publicity seekers; others are undoubtedly sincere.[15]

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[12] Landsberg, Helmut," The Problem of Earthquake Prediction," *Science* 82, 2115 (July 12, 1935): p. 37.
[13] Barth, Kai-Henrik, "The Politics of Seismology: Nuclear Testing, Arms Control, and the Transformation of a Discipline," *Social Studies of Science* 33, 5 (October 2003): pp. 743-781. For additional historical understanding of early 20th century seismology, see Geschwind, Carl-Henry, "Embracing Science and Research: Early Twentieth-Century Jesuits and Seismology in the United States," *Isis* 89, 1 (March 1998): pp. 27-49; Barth, Kai-Henrik. Detecting the Cold War: Seismology and Nuclear Weapons Testing, 1945-1970 (Dissertation: University of Minnesota, 2000).
[14] For a history of the development of seismology in California in the early 20th century, see Goodstein, Judith, "Waves in the Earth: Seismology Comes to Southern California," *Historical Studies in the Physical Sciences* 14, 2 (1984): pp. 201-230
[15] Wood, Harry and Beno Gutenberg, "Earthquake Prediction," *Science* 82, 2123 (September 6, 1935): pp. 219-220

Contrary to those who were incompetent, ignorant of the difficulties, or publicity seekers, Landsberg "quite correctly" summarized that earthquake prediction was impossible given the present state of knowledge. The notion that Landsberg "hinted" at a future earthquake, or that one could justifiably "infer" one, was just plain incorrect.

Like Landsberg, Gutenberg believed that such predictions -- however imprecise -- were unwarranted because of the risks involved. Due to the obvious importance of seismological investigations in southern California, Gutenberg was justified in thinking that a public announcement of a fallible earthquake prediction "is likely to be harmful and mischievous, causing unwarranted worry and apprehension among large numbers of the population." While protective infrastructural measures can be taken in areas known for frequent earthquakes, such as building more resident structures, geoscientists like Gutenberg and Landsberg believed that public claims of future earthquakes -- even if presented on a rational or empirical basis -- was best restricted to the scientific community given the distinct possibility of false alarm. As Gutenberg continued,

> Such prediction or forecasting should not be made public in the press, however, but simply notified to proper scientific groups who would subject it to test as to its realization and rational method, to determine its value. Most of the earthquake prophets who are sincere do not realize the obstacles which confront successful prediction -- the limitations as to place of occurrence and the high frequency of occurrence of shocks. . . Any moderately successful method of prediction for scientific testing will be welcomed by all seismologists, but public prediction in the present state of knowledge is nothing short of a menace.[16]

Landsberg's response to *Science-News* was indicative of a fundamental problem that guided his later objections within climate discussions during the 1970s: the principle of reticence. Gutenberg and Landsberg believed in the virtue of talking to one another about their

[16] Wood, Harry and Beno Gutenberg, "Earthquake Prediction," *Science* 82, 2123 (September 6, 1935): pp. 219-220

ideas within the confines of the discipline, particularly when it came to a topic as important as the general welfare of human society. This did not make them callous to the potential threats, but they were conditioned to believe that their value to society was rooted in an attempt to soberly assess future risks in light of what they believed to be reliable scientific evidence. While sincere concern was justified, they believed that science required a higher form of restraint and commitment. Scientists, according to Landsberg and Gutenberg, should not contribute to a public's fascination with prophecies that could just as likely be right or wrong. Reticence was fundamental to the practice of science itself.

EARLY VIEWS OF PROPER COMMUNICATION -- METEOROLOGICAL FORECASTS

Landsberg's philosophy of cautious reticence was not restricted to earthquake predictions; he was also deeply aware of the need to carefully develop and vet meteorological predictions early in his career. In the midst of the Dust Bowl that wiped out great swaths of crops in the American west during the 1930s, he saw popular demand increase for meteorologists -- like Chief of the U.S. Climatology Office, Joseph Kincer -- to understand and ideally resolve the challenges. The public looked to the experts to provide answers about whether the climate, as popularly imagined, was warming up. Meteorological predictions, like earthquake predictions, were just as valuable to society if only the uncertainties could be minimized and the predictions made more accurate. Unfortunately, meteorology during this period was beset by uncertainties and gaps in knowledge, and climatology seemed to serve little value (i.e. its focus was on past changes, and the past seemed to have little to say about the future).

Nonetheless, upon America's entry into World War II in 1941, Landsberg was given an opportunity to work with Carl-Gustaf Rossby at the University of Chicago. During this period, Landsberg was responsible for training new recruits for real-time meteorological analysis to help with the war effort. Within a short time, the United States Air Force asked Landsberg to compile and provide relevant climate and weather statistics for military strategists. Predictions had become a valuable commodity; if one could predict future storms, lives could potentially be saved. This was certainly advantageous, but his war-time experiences also showed him the risks of improperly communicating meteorological forecasts to authorities. Communicating the future was an uncertain endeavor, and the rushed atmosphere of wartime provided what appeared to him as a breeding ground for less-than-sound assessments of the uncertainties involved.

This is best illustrated in a formerly classified pamphlet that he authored entitled, *Geophysics and Warfare*.[17] Requested by his superiors within the Office of the Secretary of Defense, Landsberg sought to provide answers on why the military should sponsor research in the geophysical sciences. Certainly a strong believer in the inextricable link between science and the prosecution of wars, Landsberg argued for better communication between the military and scientist. For him, the technical language of scientists -- what he characterized as "shop talk, jargon, gobbledygook" -- presented difficulties for the "professional soldier" whose only goal was to understand and put into practice what scientists knew.[18] This interest in communication was an important one in modern warfare: without a proper and meaningful forecast of future weather conditions, the military was put at great risk.

While important, Landsberg was also exposed to the limitations of communicating of forecasts in wartime. The pressures and realities of warfare mandated that analysts provide forecasts that may not have been very reliable from a scientific standpoint. This rushed atmosphere provided the conditions for those who Landsberg called injudicious "charlatans,"

[17] Landsberg, Helmut. *Geophysics and Warfare* (Washington, D.C.: Office of the Assistant Secretary of Defense for Research and Development, 1954).
[18] Landsberg (1954): p. 1

"medicine men," or "court astrologers" -- all "too impatient to wait for the reliable results to come from the research laboratories." As he further characterized, "some long-range forecast schemes were being peddled to the military, but the salesmanship involved was far superior to their meteorological success."[19] If anything, Landsberg had developed a more astute sensitivity to the distinction between good science and good salesmanship during the war, which extended back to his seismological work of the 1930s.

Preliminary conclusions can be drawn from these two periods in Landsberg's life. First, his philosophy of cautious reticence carried over from his seismological work of the 1930s to his meteorological responsibilities during the 1940s. What he saw as the virtues of patient inquiry, internal scientific communication, and reticence reflected what he considered to be the appropriate professional values of expertise. For him, the hazards of inaccurate predictions (i.e. loss of credibility, betraying a commitment to organized skepticism) could not be overlooked. Prediction work was certainly an admirable goal for the geophysical sciences, and he applauded the use of the geophysical sciences in societal decision making, but Landsberg -- always aware of the hazards of false alarms -- was conditioned to be skeptical of the abilities of young, underdeveloped sciences like seismology and meteorology. As we will see in the remainder of this chapter, climatology -- a very young science in the 1970s -- became a contested territory whereby claims of future climate-induced catastrophe were being promulgated in the public, and public reticence appeared to become more crucial to uphold according to Landsberg.

CLIMATOLOGY -- A CONTESTED TERRITORY

When he was thirty years old, in 1976, climate modeler Stephen Schneider wrote a book entitled, *The Genesis Strategy*, wherein he used the biblical story of Joseph's advocacy of a long-term food storage strategy to advocate for better federal policies regarding the global food

[19] Landsberg (1954): p. 6

supply. For many of the older members of the atmospheric science community, the book appeared ill-placed and unrestrained. By examining Landsberg's reaction to Schneider's book, and particularly within the context of others' objections, one can clearly see the importance he placed on communication of environmental threats to society.

As recounted in the preface of *The Genesis Strategy*, Schneider was once asked by a TV reporter whether scientists cried wolf too often regarding future disasters. Schneider's response provides insight into *his* understanding of the hazards of climate change.

> The journalist was waiting for a definite answer, and all I could offer was a statement of probability -- the odds for human catastrophes related to the world among seemingly irreconcilable 'experts' about the seriousness, even the timing, of a host of prospective crises was a reflection of the uncertain state of scientific knowledge.[20]

Schneider's response was not an attempt to bypass the question, nor was it a direct response to the frequency of "cry-wolf" claims by scientists. Rather, he took it as an opportunity to give an honest assessment of the difficulties of science to deal with the inherent risks of living in a highly complex society. Schneider would have certainly preferred to provide more "definite" answers, but he felt it was his responsibility to note how uncertainty was part of modern policy making, and that measures taken in the past to ameliorate problems were no longer sufficient.

While this was hardly news to anyone, there was another more pertinent issue guiding his response. While he firmly believed that uncertainty was integral to modern living, his 1976 book reflected his fundamental belief that uncertainty was hardly a justification to stand in the way of ameliorative action. While uncertainty defined what it meant to live in what Ulrich Beck calls a

[20] Schneider, Stephen and Lynne Mesirow. *The Genesis Strategy: Climate and Global Survival* (Plenum Publication Corp., 1976), xi.

"risk society," he believed that the risks of not crying wolf were far greater than the risks of not crying out at all.[21] As he explained,

> Uncertainty does not simply that there are no problems; nor does it deserve a 'wait and see' attitude. . . Nor does political action require knowledge of the exact location of each tree behind which a wolf may be hiding. Rather, knowledge of the probability that wolves do lurk in the forest should be sufficient information for deciding whether to take preventive action. . . My chief concern for the future is political rather than scientific; it is that some wolves will attack long before we are certain enough of their existence to feel compelled to effect difficult political actions.[22]

Clearly, Schneider knew how to wear multiple hats -- one as a scientist, and one as a concerned citizen. By distinguishing his political concerns from his scientific ones, he emphasized the importance of intuition when dealing with uncertain threats. The incorporation of intuition and judgment into policy making, he believed, was a necessary and warranted precautionary response. As gatekeepers of important knowledge, scientists had an obligation to speak up to policy makers and the general public about the wolves that might be lurking in the forest.

Many prominent members of the atmospheric science community panned the book after its release. Labeling Schneider's book as a "simplistic basic thesis," Landsberg criticized what appeared to be Schneider's lack of discipline when dealing with complex, interdisciplinary topics. He charged that Schneider lacked "faith in the system" to cope with the hazards of climate change. Schneider's additional reliance on newspaper and magazine articles appeared to Landsberg as a "very haphazard way to achieve a balanced view of an extremely complex problem," and likely contributed to what Landsberg saw as Schneider's tendency to elevate conjectures to facts. "Inadequate information," Landsberg wrote, "is clothed in the mantle of scientific authority. In some instances, out rightly flimsy ideas are advanced paradigmatically to

[21] Beck, Ulrich. *Risk Society: Towards a New Modernity* (New York: Sage Publications, 1992)
[22] Schneider, Stephen and Lynne Mesirow. *The Genesis Strategy: Climate and Global Survival* (Plenum Publication Corp., 1976), xi.

make a point." As evidence for his claim, Landsberg noted Schneider's treatment of the primary reason that the U.S. became involved with climate change: droughts in Africa during the early 1970s. As Landsberg charged:

> One misses in the technical part of the treatise the dispassionate and critical attitude which has been such a distinguishing attribute of scientists. In particular, I disagree with the opinion that uncertain predictions derived from clearly inadequate mathematical-numerical models should be used for public (political) decisions. Nothing could erode the credibility of scientists faster than that. This does not mean one has to have absolute certainty, but one needs to give the decision makers a precise estimate of the uncertainty. Certainly, back-of-the-envelope calculations may lead to dangerously false conclusions.[23]

Landsberg also objected to Schneider's willingness to suggest policies. One of the central ideas discussed in *The Genesis Strategy* was the need for a fourth branch of government to deal with the complexity of science and technology-related issues. Not only was this "a bit vague and unbelievably naive," Landsberg argued that it was indicative of a deep ignorance of the international and domestic institutions that already existed to meet the needs of American society. Like those within the environmental movement who objected to what appeared to be a symbiotic relationship between government and big business, Schneider basically lacked "faith in the system." Landsberg mockingly suggested that if Schneider was so interested in changing society he should either 1) run for public office or 2) spend more time with the science and "spend less time going to the large number of meetings and workshops that he seems to frequent and also that he change his reading habits from newsprint -- all through the simple expedient of reading scientific journals and otherwise being a regular devotee of a first class scientific library."[24]

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[23] Landsberg, Helmut, "Review of The Genesis Strategy," *EOS: Transactions of the American Geophysical Union* 57 (September 1976): pp. 634-35
[24] Landsberg (September 1976): pp. 634-35

Landsberg's scathing appraisal of Schneider's book was not unique, and perhaps more tempered than those of other esteemed members of the atmospheric science community. Three months after receiving a copy of *Genesis Strategy*, Jule Charney -- one of the principal pioneers of numerical weather prediction and computer-based modeling techniques -- wrote to Samuel Day, Jr., the editor of *the Bulletin of the Atomic Scientists*. Agreeing with Landsberg's assessment that Schneider's book was a "lightweight affair," Charney unleashed what Day modestly referred to as an "informal appraisal":

> The author <Stephen Schneider> is apparently both incapable and unwilling to distinguish between good science and bad speculation based on bad statistics. He is like the sun-spot-weather people who adduce no causal connections and are therefore forced to rely on statistics, bad statistics. To involve oneself in criticizing them is a career in itself and a thankless one.[25]

While it is beyond the scope of this chapter to discuss in depth what he meant by "sun-spot-weather people," Charney essentially referred to a long-standing tradition within the scientific community since the 19th century to prove the existence of a physical relationship between sunspot activity and terrestrial changes in weather and climate. There were a few who persisted in their belief that a relationship existed, but frequently proven too elusive. A few weeks later, Charney remarked that none of the "speculative ideas of people like Bryson and Schneider on future climate change are worth the paper (usually newspaper) they are written on. They mislead the public and they do the field harm."[26]

Shortly after the release of *The Genesis Strategy*, Landsberg stood in front of his scientific peers at the 1976 Annual Meeting of the American Association for the Advancement of

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[25] July Charney to Samuel Day, Jr., 7 December 1976, Box 3 -- B, General Correspondence, 1949-80, Papers of Jule Charney, MIT Institute Archives, Cambridge, MA

[26] Reid Bryson was a climatologist with the University of Wisconsin Center for Climate Research, who promoted the idea that atmospheric dust would induce a global cooling. Jule Charney to Warren Kornberg, 12 October 1976, Box 13 -- NSF, 1955-81, Papers of Jule Charney, MIT Institute Archives, Cambridge, MA

Science in Boston to discuss the importance of discretion when making claims that could be interpreted wrongly. "Clearly," he began,

> in the absence of a valid theory of climatic change -- there are about as many hypotheses as investigators in the field -- there is every reason to be reticent about the future. . . Obviously, there is a need until reliable forecast techniques are developed to keep the climatic watch that has so admirably functioned for over a century. However, watchful vigilance is not a license to 'cry wolf' continuously.[27]

For Schneider, speaking up in public about future threats was an important function of scientists. How else could the general public and policy makers know that threats exist in the first place? Scientists served an invaluable function within modern society, and it would be remiss for them to reluctantly engage the public out of fear of rebuttals by their peers. Landsberg agreed that having an ethical perspective on matters that pertain to the public welfare was noble. However, he also believed that scientists were beholden to a certain set of values that precluded such public proclamations based on unreliable forecasts of the future. This was not just an epistemological disagreement about the expression of scientific uncertainty; this was also a philosophical disagreement over the role of values in science-based decision making. Landsberg and Schneider disagreed over the most effective way to benefit society; the former argued that reticence was fundamental until all the facts were properly vetted, at which point it would be appropriate to engage the public, while the latter felt that precautionary measures were more beneficial because it protected the public from possible future harm. Schneider appeared more like a doomsaying prophet than a tempered and informed scientist -- Schneider had, according to Landsberg, brazenly injected environmental politics into climate science.

[27] Landsberg, Helmut, "How is Our Climate Fluctuating," Paper given at the Climate and Plant Productivity Symposium at the American Association for the 1976 Annual Meeting of the American Association for the Advancement of Science (AAAS), Series 5, Box 1: AAAS Papers, Papers of Helmut Landsberg

Since the 1930s and 1940s, Landsberg had evolved from a research analyst during World War II to a globally renowned atmospheric scientist that witnessed and oversaw many of the most important changes in American climatology. After serving in World War II, he became the acting director -- later the director -- of the U.S. Air Forces Joint Research and Development Board's Committee on Geophysical Sciences. In 1951, he became the Director of the Geophysics Research Directorate of the U.S. Air Force Research Center in Cambridge, Massachusetts, followed by an appointment as Director of the U.S. Weather Bureau's Office of Climatology from 1954 to 1965. Subsequently, he was asked to direct the newly built Environmental Data Service within the Environmental Science Service Administration (ESSA) between 1965 and 1966, after which he finally finished off his career as professor at the University of Maryland.[28] Just as crucially, he witnessed the dramatic ways that scientists had become public figures, and how discussions of the future effects of climate risked the credibility of a young atmospheric science like climatology.

Due to what Landsberg saw as the responsibilities of scientists to produce the most useful and state-of-the-art knowledge for the benefit of society, he became more entrenched in his own understanding of what climatologists did with their time. Until well into the 1960s, climatology was "exclusively a data collection and tabulation business."[29] Traditionally, a climatologist was one who prided himself on the careful but laborious analysis of weather statistics to discover patterns in atmospheric phenomena. When climate scientists stepped out into the public with what might be perceived as speculation about the nature and effects of climate, they risked being perceived as either forgetting or overlooking the importance of conducting research insulated

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[28] The Environmental Data Service (EDS) was responsible for archiving and disseminating geophysical observations to the general public, federal, state, and local agencies, the scientific and engineering communities, and private corporations. For a very brief overview of the EDS, see Hughes (1970): pp. 157-58

[29] United States Department of Commerce, Advisory Committee on Weather Services. *Weather is the Nation's Business* (Washington, D.C.: US Government Printing Office, 1953), p. 24

from social affairs. Historian of science Spencer Weart has noticed this tension within the field of climatology during the 1960s and 1970s: "it was more common for a climatologist to avoid such speculation and carry out grinding numerical studies in hopes of pinning down recurrences and, perhaps, predicting them."[30] The grind of numerical analysis blended with a cautious approach to climate forecasting was typical among many figures like Landsberg.

According to historian of science Naomi Oreskes, the emphasis on data collection and analysis was in keeping with what she saw as a broader American tradition within the geophysical sciences. "The documentation of copious quantities of geological detail," she argued, "reflected a belief that good science was built on hard work -- the diligent finding, recording, and documenting of the features of nature, upon which any reliable theory would necessarily be founded."[31] For Landsberg, the climatologist -- a member of the geophysics community -- should be vigilantly skeptical of those who deemed themselves authoritative given the immense amount of work that had yet to be completed. As he wrote in 1978,

> It is probably an understatement to say that there is confusion and disagreement about the reality and magnitude of the influences of pollutants on climate. One of the major reasons is the fact that a great deal of speculation is going on about these influences. Much of this speculation has led to arguments that can only be resolved by recourse to an adequate body of facts.[32]

The apparent existence of public confusion and disagreement was unsurprising; Landsberg believed that the 'general public' equated analysis of meteorological variables like temperature and precipitation with the effects of climate -- discussed in terms of deaths, injuries, damages, and costs to taxpayers.

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[30] Weart, Spencer, "Rise of Interdisciplinary Research on Climate," *Proceedings of the National Academy of Sciences* 110 (26 February 2013): pp. 3657-3664, quote on p. 3658
[31] Oreskes, Naomi. *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York: Oxford University Press, 1999): p. 150
[32] Landsberg, Helmut, "Useful and Useless Data for Judging Air Pollution Effects on Climate," within A.L. Morris and Richard Barras, eds. *Air Quality Meteorology and Atmospheric Ozone: A Symposium* (American Society for Testing and Materials, 1978): pp. 224-234

For Landsberg, the lack of an established body of facts meant that scientists had an even greater responsibility to prevent unnecessary slippage from insulated discussions of climatic variables to what he saw as more speculative ventures of climate-induced effects. For those that were not professional climatologists, the rule appeared more applicable. The remainder of this chapter concerns Landsberg's disapproval of and frustration with some astrophysicists, physicists, and science writers, who made claims about the future climate's effects on society. Contrary to the philosophical perspectives of Landsberg, these individuals -- Howard Wilcox, John "Jack" Eddy, Nigel Calder, and John Gribbin -- either engaged in publicly forecasting future climate-induced catastrophe or incorporated climatology into their research agendas even though they were not professional climatologists themselves. By examining his responses to Wilcox, Eddy, Calder, and Gribbin, this chapter begins to understand not only boundaries between professional disciplines but also how some members of the climate science community saw the public activities of so-called outsiders.

RISE OF 'DILETTANTE' ASTRONOMERS

In 1974, two astrophysicists trained at the University of Cambridge, Stephen Plagemann and John Gribbin, published a book entitled *The Jupiter Effect*, wherein they predicted that a future alignment of the planets would "trigger" what they called the "next great Californian earthquake" in 1982. Based on research by astronomer R.A. Challinor, who suggested that changes in solar activity could influence the rate of rotation of the earth, Gribbin took the idea one step further: "One might even speculate that the San Andreas fault, now overdue for a major slippage according to some authors, might be triggered in this way in the late 1970s or early 1980's," he noted.[33]

[33] Gribbin, John, "Relation of Sunspot and Earthquake Activity," *Science* 173, 3996 (6 August 1971): p. 558

In spite of what he saw as broader reservations within the astrophysics community in Cambridge, commercial publishers in the United States believed his ideas had popular appeal. Gribbin contacted his colleague, Stephen Plagemann, about writing a manuscript that tied together their respective interests -- Gribbin for astrophysics, and Plagemann for the geosciences. After a first rejection, the manuscript was accepted by MacMillan Press in 1973, and published in 1974. When asked about the production of the book and its broader meaning, however, Gribbin repeatedly stressed that "it was just a writing task; as writing was my day job; in that sense it was nothing special. . . I had two objectives 1) be accurate and 2) make a living."[34] As he subsequently articulated, "I am a writer who knows a bit about science, not a scientist who does a bit of writing."[35] Even so, *The Jupiter Effect* was released during an important period in popular science publishing. With the rise of what Bruce Lewenstein calls a "popular science boom" during late 1970s, Gribbin admired popular science writings like Arthur C. Clark, Issac Asimov, and George Gamow.[36]

In writing *The Jupiter Effect*, Gribbin and Plagemann felt time had come to wed seismology to other disciplines like meteorology and solar astronomy. In their eyes, earthquake prediction had become a "respectable branch of science," a discipline they argued was once exclusive to soothsayers, astrologers, and prophets of doom who traditionally "caused alarm by spreading warnings about earthquakes that never in fact took place." While not intent on "proving a case in a court of law," implying that they could not meet the highest burdens of proof, they nonetheless relied "only on solid scientific evidence and reasoning" to justify the

[34] Interview with John Gribbin, 3 May 2013
[35] Interview with Gribbin, 3 May 2013
[36] Luey, Beth, "'Leading the Public Gently'": Popular Science Books in the 1950s," *Book History*, 2 (1999): pp. 218-253; Lewenstein, Bruce, "Was There Really a Popular Science 'Boom'?" Science, Technology, and Human Values 12, 2 (Spring 1987): pp. 29-41. For a historical treatment of one of the more prolific science writers, see Shermer, Michael, "The View of Science: Stephen Jay Gould as Historian of Science and Scientific Historian, Popular Scientist and Scientific Popularizer," *Social Studies of Science* 32, 4 (August 2002): pp. 489-524. For a biographical treatment of Asimov, see White, Michael. *Isaac Asimov: A Life of the Grand Master of Science Fiction* (New York: Carroll & Graf Publishers, 2005).

merits of their claims. Their goal, ultimately, was to establish an early warning system for earthquakes by combing through what they called "chains of logic," "chains of reasoning," and/or "chains of evidence" that tied natural phenomena together.

They knew that they were on the edge of respectability, but also believed that their ideas may prove sound if only established scientists divested themselves of their own biases and prejudices. As they wrote,

> Scientists are -- usually quite rightly -- unwilling to accept evidence like this until it can be related in some way to the broad framework of their understanding of what makes the Universe tick. . . In addition to this understandable caution it must be said that scientists, like other people, are all too often afflicted with human prejudices. They are sometimes capable of dismissing even the most reliable piece of evidence if it does not agree with their preconceived notions about what the world ought to be like.[37]

For them, their goal was simply to collect as many pieces of empirical evidence from a variety of different disciplines, weave them together into a coherent framework, and use that framework to predict an earthquake that would "herald the greatest disasters of modern times" -- an event they also referred to as the "day of reckoning" or the "apocalyptic date."[38] They considered themselves empiricists. "We are," they wrote, "wandering in the realms of the empiricist scientist, where important discoveries often take on their first misty shape before being captured and codified into physical laws."[39] By examining miniscule fluctuations in natural phenomena, weaving them together into a coherent framework of understanding, and doing so in a way that reflected state-of-the-art science was crucial for their personal and professional legitimacy. As they attested, "we will accept only the empirical evidence which stands up to searching examination. Mere coincidence is not enough." Indeed, they were not proposing a *statistical*

[37] Gribbin, John and Stephen Plagemann. *The Jupiter Effect* (London: Macmillan Press, 1974): pp. 90-91
[38] Jupiter Effect, p. 132 and p. 148
[39] Jupiter Effect, p. 106

relationship between natural phenomena; they wanted to unearth the *physical* relationships that bound the terrestrial world to the entire solar system.

By exploring the outer reaches of known science, Gribbin considered himself a "dilettante theoretician," one who enjoyed looking for almost imperceptible changes in natural phenomena -- what may be dismissed as instrumental "errors" or "externalities" -- and seeing if something more fundamental was occurring. Like an explorer, he sought to understand the relationship between geophysical and astrophysical phenomena: "Like Mount Everest, the problem is there and cannot be ignored. We hope that we will not be alone in attempting to surmount it."[40] Gribbin belonged to no one discipline, and he knew it.

In some respects, publishers had good reason to be interested in books like *The Jupiter Effect*. During the early 1970s, depictions of large-scale earthquakes were already a good source of revenue for Hollywood. *The Poseidon Adventure* -- a survival story about the capsizing of a passenger ship due to an underwater earthquake -- was released in 1972. Two years later, Universal Studios released their blockbuster disaster movie, *Earthquake*. The importance of these movies, however, is not their depiction of catastrophic earthquakes during the early 1970s. Rather, their importance rests on how they showcase the manner in which those in power choose to respond to warnings of future disaster. Both *Earthquake* and *The Poseidon Adventure* focus on maximizing suspense by dramatizing the relationship between decision makers and those who warn of future dangers. The structure is fairly uniform: someone warns of future dangers, the higher-ups dismiss the claims, disaster begins, authorities respond too late.[41] It was in this context of a popular science boom and movies about earthquake disasters that Gribbin and Plagemann's book struck a chord -- would their warnings be headed in time?

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[40] Gribbin, John, "A Solar Storm that Shook the World," *New Scientist*, 10 May 1973, pp. 339-340

[41] This is not an uncommon plotline, and was often used during the early 1970s. *Jaws* (1975) is a particularly good illustration, whereby a mayor's hesitancy to shut down a beach in spite of warnings yields devastating results to vacationers.

While it is unclear if Gribbin and Plagemann consciously sought to adapt their book to Hollywood, the evidence strongly suggests that they were playing to an audience receptive to astrology and the bible. "In 1982," they predicted, "'when the Moon is in the Seventh House, and Jupiter aligns with Mars and with the other seven planets in the Solar System, Los Angeles will be destroyed."[42] This kind of language suggests that the book was marketed in a way that would bridge millennial thought with established science, and do it in a way that would be favorably read by millions. This kind of language was also evident within the book's foreword. Written by popular science writer Isaac Asimov, Gribbin's book was an "odd (but rational) echo of astrological thinking." Instead of an attack on his ideas, Asimov's message was clear: defend the freedom to explore "scorned ideas" of the past and challenge established orthodoxy. Against the backdrop of a resurgent interest in popular works like Immanuel Velikovsky's 1950 work, *Worlds in Collision*, *The Jupiter Effect* traced the boundaries between "pseudo-science" and traditional forms of scientific investigation.[43]

Indeed, Gribbin and Plagemann also believed that established institutions had an incentive to squelch unorthodox ideas like astrology -- therefore allowing them to function as intermediaries between segments of popular culture and established science. He argued, for instance, that "NASA experts" quietly agreed with his assessment that astronomical alignments of planets could affect geophysical processes on Earth, who he termed "latter day-astrologers."[44] While it is difficult to establish who these individuals were, their identification was not his primary point: his ideas carried legitimacy by virtue of what he saw as tacit support within reputable institutions like NASA.

[42] Jupiter Effect, p. 148
[43] For a historical treatment of pseudoscience after World War II, see Gordin, Michael. *The Pseudoscience Wars: Immanuel Velikovsky and the Birth of the Modern Fringe* (Chicago: University of Chicago Press, 2012).
[44] Gribbin and Plagemann (1974)

Ultimately, Gribbin and Plagemann had many motives. They sought to appeal to astrologers and millennialists, researchers within multiple scientific disciplines ranging from seismology to atmospheric science to solar physics, and do so in a way that dramatized the importance of looking for what they saw as hidden relationships within the universe. From the available record, Gribbin was especially interested in tracing a line between his work and those whom he saw as contemporary soothsayers. Influenced by John Maddox, editor of the prestigious British scientific magazine, *Nature*, Gribbin believed that his prediction of impending crisis was not just another prophecy of doom. Like Maddox, he specifically believed that many claims of future resource depletion -- a pessimistic stream of thought embodied in the 1972 Club of Rome report entitled *Limits to Growth*.[45]

In spite of attempts to justify their claims using what they saw as the most robust science available, Gribbin and Plagemann's book met with broad disapproval by members of the geophysics community. Walter Orr Roberts, one of the founders of the National Center for Atmospheric Research in Boulder, CO, expressed his own reservations. "I do not have any substantial confidence in 'The Jupiter Effect' as described in the Gribben and Plagemann book. I think they have exaggerated and oversimplified. I say this, also, very privately."[46] His rebuke of the book is in some ways surprising and uncharacteristic. Given his own dealings with deep and rigid skepticism toward his own field of interest -- the influences of the sun on terrestrial weather -- he must have felt strong enough about the matter to risk appearing as overly critical of someone who was just as eager to pursue topics at the frontiers of knowledge. Additionally, Don Anderson, a geophysicist with the California Institute of Technology Seismological Laboratory, also wrote a fairly scathing review. The book, he argued, was a "commercial commodity of the

[45] Maddox, John. *The Doomsday Syndrome* (New York: McGraw-Hill Book Company, 1972)
[46] Roberts to Evelyn Horton, 14 October 1976, WOR 8: Correspondence H-H, 1975-76, Walter Orr Roberts Collection, University of Colorado Archive, Boulder, Colorado

crassest kind. The authors are clearly after the large cult and astrology market and the many Californians genuinely fearful of earthquakes. . . Clearly, we should look to the ground rather than the heavens for clues to earthquake prediction." Given what Anderson perceived as the responsibility of seismologists to "keep the public informed and alert but not unduly alarmed,"[47] *The Jupiter Effect* made the job that much more difficult. Generally, the media's treatment ranged from hostile to sensational -- but rarely supportive.[48]

For reasons that are not entirely understood, not until Gribbin and Plagemann's astrophysical prediction had failed in 1982 did Landsberg express his own views. On at least three separate occasions between 1982 and 1984, he expressed what he believed to be one of the finest examples of the risks of making public claims about future doom. First, Landsberg wrote to economist Julian Simon on October 26, 1982 regarding what he believed to be the "nonsense" promulgated by Gribbin and Plagemann back in 1974:

> The 'Jupiter Effect' was a book written by two naive British astrophysicists a couple of years ago. They predicted that the anticipated alignment of major planets on one side of the earth would lead early this year to earthquakes and other geophysical catastrophes. Nothing serious happened and they finally withdrew the nonsense.[49]

Second, Landsberg wrote a letter to Hugh Ellsaesser on June 14, 1983 regarding what he saw as sensational depictions of the future effects of global warming within the media. As a climate scientist, Landsberg was not hostile to the idea that carbon dioxide could influence the global atmosphere -- what he deemed to be the "only serious anthropogenic threat to global climate."[50] Nonetheless, he was frustrated with what he interpreted as the endless cycle of simplistic media-driven prophecies of doom that involved the climate. "I agree with you completely on the

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[47] Anderson, Don, "Review: The Jupiter Effect," *American Scientist* 62, 6 (November-December 1974): pp. 721-722
[48] Alexander, George, "Big California Earthquake in '82? Experts not Shaken by Theory," Los Angeles Times, 13 September 1974, p. C1; Rossiter, Al, "Forecast West Coast Doom," Chicago Defender, 19 September 1974, p. 16; "'Jupiter Effect': Mixed Reaction," Science News 106, 13 (28 September 1974): pp. 197-198
[49] Landsberg to Simon, 26 October 1982, Series 3, Box 5: Global 2000 Revisited, 1982-83, HLP
[50] Landsberg to James Norwine, 10 February 1976, Series 2.2, Box 8: James Norwine, 1976-82, HLP

uncertainties in the CO2 predictions," he wrote. "But think how much fun it is for some of our headline-hunting colleagues to predict doomsday. Remember the Jupiter Effect?"[51] Third, within a chapter for a collaborative book project with Julian Simon entitled, *The Resourceful Earth*, Landsberg wrote that "a bit of the 'Jupiter Effect' has crept" into a 1980 report by the Carter Administration.[52] In all three cases, Landsberg used *The Jupiter Effect* as an example -- a kind of cultural marker -- to illustrate the risks of making scientific-sounding but very speculative claims within the public sphere.

LANDSBERG, JOHN "JACK" EDDY, AND SOLAR-CLIMATE RELATIONS

While Landsberg waited until the prediction of Gribbin and Plagemann failed to speak on the matter, Landsberg initiated a more active campaign against astrophysicist, John "Jack" Eddy, during the late 1970s to early 1980s. On April 29, 1974, months before the release of *The Jupiter Effect*, Eddy addressed the newly created National Center for Atmospheric Research Climate Club about a topic he believed had traditionally been more aligned with reading tea leaves than a pursuit worthy of heavy investment of scientific talent – the relationship between sunspots and the climate and/or weather.[53] For dramatic effect regarding the risks of taking on unorthodox topics, he imagined himself to be a hypothetical physicist responding to question about the relationship. His response, a way of mocking the dismissive attitudes of more conservative scientists, indicated the risks of studying what had traditionally been dismissed or ignored. "Please don't ask me about Solar-Weather," he responded in regard to the hypothetical situation. "If I answer I will be guilty by association and labeled as one the lunatic fringe with freaks, wierdos, and other old men." Of course, Eddy perceived himself not as an old man,

[51] Landsberg to Ellsaesser, 14 June 1982, Series 2.2, Box 5: E, Miscellaneous Correspondence, 1978-84, HLP
[52] Simon, Julian and Hermann Kahn, eds. *The Resourceful Earth: A Response to Global 2000* (Oxford: Basil Blackwell, 1984), p. 302
[53] Scientific articles, as well as public pronouncements, frequently failed to rigorously define "climate" and "weather" throughout the 20th century. Consequently, weather and climate were often used interchangeably.

freak, or wierdo but rather as a young astronomer intent on finding that he found interesting in the field of solar physics.[54]

Indeed, Eddy was not deterred by the risks of being deemed illegitimate. His work on archaeo-astronomy (i.e. medicine wheels in the southwest) positioned him as an eccentric scientist willing to take-on unorthodox topics.[55] These interests led him to study what he deemed to be one of the most thrilling opportunities to advance understanding of 20th century astrophysics: the Maunder Minimum, a seventy-year period between 1645 and 1715 when the sun seemed to lose its sunspots. After two years of studying these historical phenomena, he finally published an article in *Science* simply titled "The Maunder Minimum." His central question was quite straight forward: what could account for the virtual absence of sunspots during the period between 1645 and 1715?

To Eddy, the sun during the Maunder Minimum was “wholly unlike the modern behavior of the sun which we have come to accept as normal.”[56] Indeed, this was of particular appeal to an astronomer interested in historical curiosities. Of particular interest to Eddy was to explain how the Maunder Minimum related to the Little Ice Age, generally defined as an unusually cold period from approximately 1300-1850 in Europe. To find the answer, he utilized what he considered to be two separate but useful methods of analysis: history and scientific.[57]

By historical data, Eddy believed that records produced by contemporary astronomers during the 17th century could be useful in documenting the *non*-existence of sunspots. After examining old records of astronomical observations going back to the time of Galileo's first

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[54] Eddy Papers, Talks, Box 2, 1974-9-30
[55] Eddy, John, "Astronomical Alignment of Big Horn Medicine Wheel," *Science* 184 (June 1974): pp. 1035-1043
[56] Eddy, John, “The Maunder Minimum,” *Science* 192, 4245 (18 June 1976): pp. 1189-1202, p. 1189
[57] For a technical account of historio-environmental analysis, see Moodie, D.W. and A.J.W. Catchpole, “Environmental Data from Historical Documents by Content Analysis,” *Manitoba Geographical Studies* 5 (1975), p. 3; Moodie and Catchpole, “Valid Climatological Data from Historical Sources by Content Analysis,” *Science* 193, 4247 (2 July 1976): pp. 51-53; Carney, Thomas. *Content Analysis: A Technique for Systematic Inference from Communications* (Winnepeg, 1972)

telescopic observations, he concluded that astronomers were aware of the lack of sunspots and that that their failure to document sunspots was not due to technical incompetence or lack of interest in observing them. According to his study of the historical record of past observations, he concluded that astronomers not only knew that sunspots *should* have existed they had the intellectual and technical ingenuity (i.e. the recently invented telescope) to conduct accurate investigations.

He was pursued other avenues of investigation to buttress his suspicions. According to his argument, an apparent lack of observations of aurorae by astronomers suggested something significant about the behavior of the sun. As he reasoned, aurorae were “especially valuable as historical indicators of solar activity since they are spectacular and easily seen, require no telescopic apparatus, and are visible for hours over wide geographic areas. They have been recorded far back in history as objects of awe and wonder.”[58] If there were no aurorae, then there were perhaps no sunspots. Ever the cautious astronomer, however, Eddy believed that he could not ignore the human capacity for observational error. “Proof seems blurred,” he acknowledged, “by the fact that scientists seldom describe what is missing or what is not thought to be important.”[59]

Given the risks of incorrectly interpreting historical records, Eddy sought another way of determining whether the sun really did exhibit signs of abnormal behavior. To buttress his historical records, he sought out Carbon-14 measurements -- or what he deemed to be "real, incontrovertible evidence." Eddy knew that solar activity was disproportionately correlated to ^{14}C amounts in tree rings, and therefore suspected that trees may record sunspot activity. His rationale was straight forward: intense solar activity induces less than normal carbon-14, while

[58] Eddy (1976), p. 1193
[59] Eddy (1976), p. 1197

less solar activity results in higher amounts in tree rings. After evaluating all of the available kinds of evidence -- historical and physical -- Eddy concluded that he could find “no facts” that contradicted the existence of a Maunder Minimum but much that supported it. Indeed, Eddy's research was essentially about trying to disprove the non-existence of a natural phenomenon -- the “the search for possible contradiction seems to me a promising path to truth."[60]

After accounting for what he deemed to be the most effective ways to determine the non-existence of sunspots, Eddy ventured into more speculative territory. According to what he believed to be an entirely sensible extrapolation of his results, he noted what appeared to be a possible “1:1 agreement in sense and time” between terrestrial temperature fluctuations on earth and sunspot frequency. This had remarkable implications for his research; if the absence of sunspots reduces terrestrial temperatures, then they may act as a kind of natural prophet for future climatic predictions. They only caveat, as far as he could tell, was a long lag-time between the occurrence of sunspots during the Maunder Minimum and resultant temperatures on earth.[61] This proposed integration of atmospheric science with astronomy was certainly bold, and caught the attention of one of the foremost authorities in the atmospheric science community.

The archival record suggests that Eddy and Helmut Landsberg first learned of one another in November 1976, two years after Eddy's talk at the NCAR Climate Club. The catalyst was a letter written on May 12th by Landsberg to NCAR climate modeler Stephen Schneider about his own interest in analyzing sunspot and aurorae counts made by prominent 19th century American meteorologist and astronomer, Elias Loomis. With little to offer, Schneider gave a copy of the letter to Eddy. Thankful for what he called the “very valuable” tabulations by Elias

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[60] Eddy (1976), p. 1199
[61] Eddy (1976), p. 1199

Loomis, Eddy nonetheless took the opportunity to write the recently-retired climatologist at Maryland to suggest that these new records in no way effected his conclusions within his 1976 *Science* paper. “I do not think,” Eddy began, “that it in any way contradicts the case made by Maunder or in my recent paper for a prolonged sunspot minimum.” At stake was whether the historical records used by Eddy in his 1976 *Science* paper were sufficient to argue for a lack of sunspots; by comparing the Loomis aurorae counts with those used in his paper, Eddy concluded that the new information was "meaningless" because the recorded number of sunspots were minor in comparison to the much more vast records he examined.[62]

Within two weeks Landsberg delicately responded by relaying his open-mindedness to Eddy’s claims about the Maunder Minimum, but cautioned that the jump from sunspots to climatic effects was a "large one."[63] Whether Landsberg was attracted to their mutual interest in historical records, or whether he saw in Eddy someone who appeared to exceed the evidence, Eddy must have felt comfortable enough to lament what he saw as the *un*critical reception of his ideas by the broader scientific community. As he wrote,

> It makes me wish very much that we could discuss these things together, for it is refreshing indeed to find someone who is both knowledgeable in these historical references and in their significance in modern science. It has concerned me that so few will doubt or criticize the claims made for historical data . . . I guess one always wishes his work in science would be more directly challenged, and this has been a little disappointing to me.[64]

Ironically, in one of those rare and dramatic moments in the history of science, Eddy unwittingly invited Landsberg to initiate what would ultimately become a two-pronged campaign against him.

[62] John Eddy to Helmut Landsberg, 22 November 1976, *PHL*
[63] Helmut Landsberg to John Eddy, 1 December 1976, *PHL*
[64] John Eddy to Helmut Landsberg, 8 December 1976, *PHL*

While it is unclear why it took him three years to finally criticize Eddy's work, Landsberg's criticism of Eddy appears most fervently in a brief exchange with Philip Handler, President of the National Academy of Sciences. Having just received a copy of the 1977/78 NAS Annual Report, Landsberg noticed what he called a misstatement in a "special report" entitled *Sun, Weather and Climate*.[65] The report was prepared by the Geophysical Research Board Study Committee on Sun, Earth, and Climate, and was chaired by astrophysicist John Eddy. While it is unlikely that Handler had any role in the panel's conclusions, Landsberg was concerned that Eddy's notions of a climate-sunspot connection were prematurely included in a formal scientific report. Landsberg suggested that Handler evaluate his recently-completed reconstruction of annual temperatures of the Northern Hemisphere from 1579 onward to put into perspective the claims of Eddy.[66]

This reconstruction was an important weapon for Landsberg. Contrary to Eddy's conclusion regarding a possible 1:1 correlation between sunspots and terrestrial temperatures, Landsberg's research showed that the Maunder Minimum was not particularly cold relative to the bordering decades. If, as Eddy claimed, there was a 1:1 correlation why wouldn't the coldest period of the Little Ice Age correspond with a period when sunspots were virtually non-existent? Landsberg concluded that it was "regrettable that a prominent academy report conveys myths instead of facts."[67] About three weeks later, Handler responded. In his defense, Handler reasonably asserted that he was not in a position to evaluate the merits of the dispute.[68]

[65] "Sun, Weather, and Climate," *National Academy of Sciences Annual Report, 1977-1978*, 96th Congress, 1st Session (20 December 1979): pp. 21-24.
[66] The graph originated from Groveman, Brian and Helmut Landsberg, "Simulated Northern Hemispheric Temperature Departures, 1579-1880," *Geophysical Research Letters* 6, 10 (1979): pp. 767-769; Landsberg, H.E., "Variable Solar Emissions, the 'Maunder Minimum' and Climatic Temperature Fluctuations," *Archiv fur Meteorologie, Geophysik und Bioklimatologie* 28 (1980): 181-191, graph on p. 189
[67] Landsberg to Philip Handler, 21 August 1979, *PHL*
[68] Philip Handler to Helmut Landsberg, 11 September 1979, Series 5, Box 13, *PHL*

One of the difficulties in deconstructing this dispute is evaluating whether Landsberg was more concerned with Eddy's claims about a Maunder Minimum, about his claims about the role of sunspots on terrestrial climate, or the idea that his claims reached an audience inappropriate for what he saw as pure speculation by Eddy. Regardless of the specific source of his frustrations, Landsberg set out to not only discredit what he envisioned as "myths" promulgated by Eddy regarding the existence of both the Little Ice Age and the Maunder Minimum, he sought to construct a rigid boundary between climatologists like himself and astronomers like Eddy. Preventing what he saw as misunderstandings regarding the state of climate science was integral to the reputation and credibility of climatology as a discipline.

Two months after the Handler exchange in the fall of 1979, Landsberg wrote to John Oliver at Indiana State University Department of Geography and Geology regarding his forthcoming book entitled *Climatology: Selected Applications.* Oliver had sent Landsberg the book draft for comment on its 1) factual claims, 2) the relative interest level, and 3) whether anything of importance was left out. Landsberg more than obliged by characterizing Oliver's use of the term 'Little Ice Age' (1430-1850 A.D.) as misleading since only the "latter portion" of that interval was "notably cool."[69]

In spring 1980, Landsberg sought to expand his efforts to counter Eddy's ideas. Having already contacted Handler, Rabb, and Oliver, Landsberg published an article within *The Journal of Interdisciplinary History*.[70] The issue -- the product of an earlier conference organized and funded by the Rockefeller Foundation in May 1979 -- reflected the editors' interest (of which Rabb was one) bringing specialists together to discuss and evaluate the influence of climate on

[69] Helmut Landsberg to John Oliver, 15 October 1979, Series 2.2 Box 8, *PHL*
[70] Landsberg, Helmut, "Past Climates from Unexploited Written Sources," *The Journal of Interdisciplinary History* 10, 4 (Spring 1980): pp. 631-642

human society.[71] Their goal was to "stimulate social and natural scientists to work collaboratively and individually on problems of climate and history, and history and climate," what they deemed to be "an exciting frontier for research."[72] The significance was not lost on Landsberg; this was an important opportunity to reveal their dispute to the general scientific community.

Having been familiar with Eddy's work for the last four years, Landsberg felt compelled to voice the entirety of his concerns about the scientific meaning of the term, Little Ice Age. One of the glaring problems he noted was in the interpretation of historical documents. Without a standardized set of observations prior to the instrumental record which began in the late 18th and early 19th centuries, gaps in historical record keeping may undermine a credible assessment of the scope and nature of past climates. The majority of documents, he pointed out, were "widely scattered" in libraries, archives, and collections of historical societies and centralized in Europe and North America, with the earliest dating to 14th century England.[73] Given the limitations of historical records when compared to the inauguration of government-sponsored weather services in the 19th and 20th centuries, Landsberg asked whether anyone can infer something about climatic fluctuations since the early modern period. His answer was emphatic: "With the lack of information from the southern hemisphere the answer is *no*."[74]

For Landsberg, the apparent bias of the historical climatic record to the northern hemisphere precluded any conclusions about the LIA. But he also had other concerns. Based on his own historical reconstruction of the climate record going back to the mid-16th century, he

[71] The Rockefeller Foundation provided $20,500 for the Journal of Interdisciplinary History to hold a conference on the "impact of climate upon history" in Cambridge, MA. For more information about the foundation's activities, see the *President's Review and Annual Report* (New York: Rockefeller Foundation, 1979).
[72] R.I.R. and Theodore Rabb, "History and Climate: Interdisciplinary Explorations," *The Journal of Interdisciplinary History* 10, 4 (Spring, 1980)
[73] Landsberg (Spring 1980): pp. 634-35
[74] *Ibid*, p. 640

questioned the very existence of the LIA -- a term suggestive of a prolonged cold trend. According to his analysis, he believed no trend existed at all: "There are clearly no hemispheric temperature trends . . . but rather irregular up and downs. There is little justification to label this whole interval the little ice age."[75] Given what appeared to the lack of sufficient southern observations, as well as what appeared to be the lack of a prolonged cold spell even within northern hemispheric records, Landsberg explicitly questioned Eddy's claims about a possible correlation between sunspots and the temperature record. As he wrote,

> There have been studies which advocated that the dearth of sunspots from 1645 to 1715, the so-called Maunder Minimum, resulted from a stretch of low temperatures. Our construction does not bear this out. The coldest period started six decades later, when solar activity seems to have been normal.

Importantly, Landsberg's response to Eddy was rooted in a broader frustration with what he saw as a lack of uniform definitions within the field of climatology. "In any science," he argued in 1975, "it is essential that there exists common ground for communication by agreed upon definitions and standards."[76] According to Landsberg, the period labeled as Little Ice Age (typically between 1300 and 1850) was not uniformly defined, especially in light of his statistical evaluation of temperature records and the seeming lack of glacial accumulation.[77]

Ultimately, the reason Landsberg disagreed with Eddy was due to different standards of evidence. In spite of Eddy's own admission that he was speculating about a "possible relationship" between sunspots and climate in his 1976 *Science* article, Landsberg's response yields insight into his sensitivity to scientific claims that could be suspected as erroneous. For

[75] *Ibid*, p. 641
[76] Landsberg, Helmut, "The Definition and Determination of Climatic Changes, Fluctuations, and Outlooks," presented at the Symposium on "Atmospheric Quality and Climatic Change" at the University of North Carolina – Chapel Hill, 21 March 1975
[77] While Landsberg was quite aware J. Roger Bray's work on Post-Pleistocene glaciation and the relationship to solar activity, Landsberg strangely ignored his many articles published between 1965 and 1971. This is particularly striking because Bray's work was an integral part of Eddy's argument regarding the Maunder Minimum-Little Ice Age link. See Bray, J., "Forest Growth and Glacier Chronology in North-West North America in Relation to Solar Activity," *Nature* 205, 4970 (30 January 1965): pp. 440-443; Bray, "Glaciation and Solar Activity since the Fifth Century BC and the Solar Cycle," *Nature* 220 (16 November 1968): pp. 672-674; "Solar-Climate Relationships in the Post-Pleistocene," *Science* 171, 3977 (26 March 1971): pp. 1242-1243

him, Eddy's claims -- however qualified -- appeared to overlook important uncertainties. To Landsberg, Eddy was quite simply engaging in a kind of scientific myth-building that deserved a harsh and quick response.

DISLODGING A 'BARNACLE'

By the time a second more targeted article was published in late 1980 in the *Archiv Fur Meteorologie, Geophysik und Bioklimatologie*, Landsberg's opinion toward Eddy's hypothesis had become more vitriolic.[78] To Landsberg, Eddy seemed to lack the necessary patience to examine all the records before making what he perceived as hasty generalizations: "his claim . . . may be brilliant speculation but it is not borne out by the dull analysis of available data."[79] This point cannot be overstated: Landsberg was part of a community of older climatologists who believed that one's professionalism was defined by a willingness to engage in the dullest aspects of scientific investigation. In 1974, for instance, Hubert Lamb, Director of the Climate Research Unit at the University of East Anglia and colleague of Landsberg, also expressed his belief that climatology was the "dullest branch of meteorology."[80] Lamb, who was about the same age as Landsberg, agreed that "dullness" -- contrary to the excitement of science at the frontier -- was the requisite attribute of a young science like climatology. To be clear, this was not a statement about the lack of emotional thrills in climatology. What they meant was that climatology required a willingness to patiently sift through endless streams of data to arrive at reliable conclusions over long periods of time. They greatly valued grunt-work, and believed that the mark of a good scientist was one who could do the dull analysis necessary to acquire credible

[78] Landsberg, Helmut, "Variable Solar Emissions, the 'Maunder Minimum' and Climatic Temperature Fluctuations," *Archiv fur Meteorologie Geophysik und Bioklimatologie* 28 (Winter, 1980): 181-191, p. 190

[79] *Ibid*, p. 183. Alan Robock wrote that the Groveman-Landsberg temperature reconstruction was the best available in spite of some imperfections. See, Robock, Alan, "The 'Little Ice Age': Northern Hemispheric Average Observations and Model Calculations," *Science* 206 4425 (21 December 1979): pp. 1402-1404. Robock's specific reference to the reconstruction appears on p. 1403

[80] Alexander T, "Ominous Changes in the World's Weather," *Fortune* 89 (February 1974): pp. 90-95, reference 2 on p. 90

knowledge. Hype, speculation, myth-making -- Eddy's research seemed unable to appreciate or subscribe to this vision of science.

The following year, in January 1981, Landsberg felt that his concerns over the claimed existence of the LIA justified skepticism toward the existence of the Maunder Minimum itself. Determining that the Maunder Minimum "may indeed be astronomical mythology," he wrote to Herbert Friedman, a rocket and solar scientist and member of the National Academy of Sciences.[81] Landsberg believed that he was on good ground for making claims about an astronomical phenomenon, one that existed outside of his own specialization in climate. After all, he believed that his conclusions were firmly reflective of other astronomers, including astronomers Xu Zhen-Tao of the Purple Mountain Observatory in Nanking, China, and prominent German astronomer Wolfgang Gleissberg, as well as long-time sunspot aficionado D. Justin Schove, Principal of St. David's College in England.[82] Writing to Friedman in regard to the release of a National Research Council report entitled "Solar Terrestrial Research for the 1980s," Landsberg and his staff at the University of Maryland were

> . . . Still dissatisfied about the 'Maunder Minimum' and are searching for old observational data. What we have now shows certainly that the solar rhythm was not interrupted, that the minimum was certainly over by 1704, and that in spite of the Vatikan's [sic] dictum that the sun had to be immuculate [sic] some people did have spots in their vision.[83]

This was a bold claim, and one that exposes one of the most fundamental aspects of the dispute between Eddy and Landsberg -- what 'minimum' meant. For Landsberg, a minimum meant no sunspots: X number of sunspots or aurorae is more than zero, and therefore Eddy must

[81] Helmut Landsberg to Herbert Friedman, 12 January 1981, Series 2.2 Box 5, *PHL*
[82] Zhen-tao, Xu and Jiang Yao-tiao, "The Solar Activity in the Seventeenth Century Re-Assessed in the Light of Sunspot Records in the Local Gazettes of China," *Chinese Astronomy and Astrophysics* 6, 1 (March 1982): pp. 84-90; Gleissberg, Wolfgang and T. Damboldt, "Reflections on the Maunder Minimum of Sunspots," *Journal of the British Astronomical Association* 89 (1979): pp. 440-449; Gleissberg, Wolfgang, "Betrachtungen zum Maunder Minimum der Sonnentatigkeit," *Sterne and Weltraum* 16 (1977): pp. 229-233; Schove, D.J., "Sunspot Turning-Points and Aurorae since A.D. 1510," *Solar Physics* 61 (1979)
[83] Helmut Landsberg to Herbert Friedman, 19 July 1982, Series 2.2 Box 5, *PHL*

have been wrong. Like his arguments against the existence of the LIA -- When was the northern hemispheric temperatures actually coldest? Have all the available records been examined? -- Landsberg expressed doubt. Eddy, on the other hand, meant "minimum" in *relative* terms. While he readily admitted that there were *some* sunspots and aurorae during the Maunder Minimum -- a fact he recognized in his earlier correspondence to Landsberg in 1976 -- this idle fact did not discount his belief that the number was *relatively* few when compared to bordering centuries. This was a critical defense: as long as new observations did not add up to a number that was significantly above those observations already used in Eddy's research, Eddy's argument could not be toppled.

Strongly believing that he was on the right side of the argument, the emboldened Landsberg sought to bring to an end Eddy's influence on the issue of sunspot-climate relationships. That same month, Robert Howard, astronomer with the Mount Wilson Observatory in California, published an article in *American Scientist* positing that the commonly-understood 22-year solar cycle of sunspots was caused by oscillations deep within the sun. To illustrate the importance of the sun to the Earths' climate, Howard, like many others, referred to the work of Eddy.[84] In spite of the fact that this single reference to Eddy played a minor if insignificant role in the broader point of Howard's article, Landsberg, within days of publication, felt that the error could not go unacknowledged. "I must," Landsberg began, "take exception to the paragraph on p. 32 in which you seem to lend credence to the hypothesis (or better speculation) of Eddy of the lack of activity in the second half of the seventeenth century and early eighteenth century."[85]

[84] Howard, Robert, "Global Velocity Fields of the Sun and the Activity Cycle: Measurements of Line-of-Sight Motions on the Solar Surface Suggest that the 22-Year Sunspot Cycle is Generated by a Fundamental Oscillation Deep within the Sun," *American Scientist* 69, 1 (January-February 1981): pp. 28-36
[85] Helmut Landsberg to Robert Howard, 27 January 1981, Series 2.2 Box 6, *PHL*

Indeed, Landsberg corresponded with astronomers from all over the world to validate his suspicions toward Eddy, including Wolfgang Gleissberg -- a German "sunspot expert" whose name inspired the well-known Gleissberg Cycle of solar activity. Believing that the Maunder Minimum was "invalid," Landsberg conveyed his frustrations that his 1976 *Science* article ever made it into print: "It's too bad that Eddy's work appeared in such a prestigious journal as 'Science.' That makes the myth he has started harder to eradicate."[86] This notion of eradication was especially pronounced in a letter to Cicely Botley, a Fellow of the Royal Astronomical Society, regarding the existence of the LIA:

> My main motivation in looking into this matter was Dr. Eddy's assertion that the interval was coincident with a cold period on Earth. That is pure climatological mythology. The sad part about it [is] that such myths cling like barnacles in the scientific literature and are hard to dislodge. Eddy's work is quoted all over the place . . ."[87]

By September 1981, Landsberg in no uncertain terms expressed that "references to Eddy should be expurgated from the climatological literature unless they are held up as horrible examples of poorly performed research (Paid for by the taxpayer through NCAR)."[88]

By 1982, it appears as if there was no resolving this dispute. Landsberg firmly believed that not only had Eddy engaged in what he saw as myth-building and hasty generalizations, he became increasingly hostile to the very idea that Eddy was circumventing professional boundaries. As Landsberg wrote to Thomas O' Toole, a staff writer for the *Washington Post*, the "mythology that the 'Maunder Minimum' interval was particularly cold also ought to be laid to rest." "Could we agree," Landsberg continued, "that astronomers should stick to astronomy?"[89]

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[86] Helmut Landsberg to Wolfgang Gleissberg, 23 March 1981, *PHL;* Helmut Landsberg to Wolfgang Gleissberg, 17 November 1980, Series 2.2 Box 5, *PHL*
[87] Cicely Botley to Helmut Landsberg, 8 January 1981, Series 2.2 Box 2, *PHL*; Landsberg to Botley, 16 January 1981, Series 2.2 Box 2, *PHL*
[88] Helmut Landsberg to Arnold Court, 1 September 1981, Series 2.2 Box 4, *PHL;* Helmut Landsberg to Arnold Court, 3 November 1981, Series 2.2 Box 4, *PHL*
[89] Helmut Landsberg to Thomas O' Toole, 6 May 1982, Series 2.2 Box 8, *PHL*

EDDY'S RESPONSE

While it is unclear whether Eddy knew of the extent of Landsberg's subversive activities since 1979, he clearly felt compelled to respond by 1983. The method: show how Landsberg himself had broken his own rules. Presumably aware of Landsberg's passionate zeal for upholding disciplinary boundaries, Eddy shrewdly yet tactfully showcased the apparent hypocrisy and showed how Landsberg had apparently misinterpreted his claims all along:

> Landsberg, a climatologist, questions as well the possible connection of what he calls 'the so-called Maunder Minimum' with the Little Ice Age, a longer contemporaneous climatic trend whose connection with solar behavior . . . is indeed equivocal. Landsberg implicitly equates the Maunder Minimum with a literal absence of sunspots or aurorae – in the sense that if one is found there must be more – and then endeavors to demonstrate how easy it is to find them.[90]

And indeed, this was exactly what Landsberg was doing all along. First, Eddy implicitly noted the irony that Landsberg, a climatologist who believed so sincerely in the value of disciplinary boundaries, could make claims about issues outside of his own discipline. Second, according to Eddy, Landsberg had clearly misinterpreted his work from the start. From Eddy's perspective, he never suggested that the Maunder Minimum-Little Ice Age relationship was certain; he merely noted what appeared to be a striking 1:1 parallel between sunspot frequency and temperature fluctuations and a "possible relationship."[91]

Third, and most crucially, Eddy crafted a detailed comparison of his and Landsberg's evidence used in each of their work. First, recall that one of Landsberg's primary criticisms of Eddy was that he rushed to judgment. To counter this criticism, Eddy showed that of the 52 "new" sunspots identified by Landsberg 41 of them were actually "duplications" of observations

[90] Eddy, John, "The Maunder Minimum: A Reappraisal," *Solar Physics* 89 (1983): pp. 195-207, quote on pp. 204-205

[91] Further research into the John Eddy Papers at the National Center for Atmospheric will allow me to tease out the subtle rhetorical strategies that Eddy used to differentiate correlation from causation. Preliminary evidence shows that while Eddy was quite reserved and often explicit about denouncing any suggestion of a causal relationship, it seems fairly clear that he often crossed the threshold from correlation to causation, a balance that was apparently dependent on different settings and the acquisition of new knowledge.

originally used in Eddy's original 1976 article in *Science*. In effect, Eddy implicitly mocked Landsberg's quick rush to judgment and pointed out how two distinct records regarding the same phenomena could only count as one observation. Additionally, the 11 sunspots that actually were unique to Landsberg's research were "surely insufficient support for Landsberg's claim of having found 'good evidence that the basic solar rhythms were maintained.'" Eddy concluded by calling out Landsberg and all the skeptics:

> A meaningful contradiction of alleged secular variations must surely consider the entire body of evidence on which the existence of episodes such as the Maunder Minimum is based . . . It is not impossible to find in 17th century records a neglected sunspot or a forgotten aurora, and perhaps we should renew our efforts to do that, to add more detail to what is known of these quiescent episodes in the current life of the Sun. But unless we unearth many hundreds of sunspots and scores of low-latitude aurorae in the 1645-1715 period we cannot have refuted the reality of the Maunder's minimum.[92]

When Landsberg returned from a trip to Europe in 1983, days after Eddy's defense was published, the message was loud and clear. "When I got back from Europe I found a preprint of an article by Eddy. It will probably appear in a short while. He gives me a bit of a black eye and I may write a letter to the editor."[93] While it is unclear whether Landsberg actually wrote the editor, Landsberg acknowledged his own hastiness toward Eddy in one fundamental respect: he himself had not obtained some relevant information that may have influenced his objections.

While the dispute over the Maunder Minimum and the Little Ice Age entailed many epistemic disagreements -- the meaning of 'minimum'; whether the state of climate science allowed any meaningful claims about sunspot-climate relationships -- Landsberg and Eddy had more fundamental disagreements over their approaches to scientific inquiry. Quite simply, they were very different kinds of scientists. Eddy saw value in uncovering what he imagined to be

[92] Eddy (1983), p. 206. Refer to Figure 1 for Maunder Minimum graphical illustration.
[93] Helmut Landsberg to Wilfried Schroder, 9 June 1983, Series 2.1 Box 2, *PHL*

unexplored physical relationships in the universe. He considered himself an explorer who took risks; he was a self-proclaimed dilettante who challenged what appeared to him as traditional disciplinary boundaries that served to hinder genuine discovery. As he described in a 1999 interview about his early career during the 1970s,

> My reasons for taking this less-traveled road were many. One is the inevitable thrill of discovery when you wander into new areas. . . Entering a new field with a degree in another is not unlike Lewis and Clark walking into the Camp of Mandans. You are not one of them. They distrust you. Your degree means nothing and your name is not recognized . . . I also think that many of the most significant discoveries in science will be found not in but between the rigid boundaries of the disciplines: the terra incognita where much remains to be learned. It's not a place that's hidebound by practice and ritual. I have always tried to keep moving between fields of study.[94]

For Eddy, not being 'hidebound' meant working between disciplines and taking risks. Pushing the frontiers of knowledge required a certain level of adventurousness to challenge what appeared to be accepted norms and customs.

Landsberg approached science very differently. He approached science as a restrained specialist who tended to shy away from making what he perceived to be broad generalizations about matters outside of his own discipline. In his mind, climatology was a very young discipline in the 1970s, he frequently expressed concern about the willingness of individuals from various disciplines to make claims about the climate without conducting what he perceived to be the 'dull' research and analysis required to produce reliable knowledge about the climate system. Eddy appeared too eager to make claims about the relationship between sunspots and the climate, which in Landsberg's view was perceived as myth-building rather than sound scientific investigation. Landsberg, in sum, was not a frontiersman; he perceived himself to be a standard bearer of the discipline. By venturing too far into what he considered to be more speculative pursuits would not only risk the credibility of climatologists like himself to speak on climate, it would destabilize what he felt was still a immature and fragile discipline.

[94] John A. Eddy Interview with Spencer Weart, 21 April 1999, American Institute of Physics Oral History Archives

SNOWBLITZ OR HEAT DEATH

Between 1974 and 1975, during the same period that Gribbin, Plagemann, and Eddy incorporated climate into their research agendas, Landsberg expressed his frustration with two publications released by physicist Howard Wilcox and science writer Nigel Calder.[95] Wilcox published a book in 1975 entitled, *Hothouse Earth*, in which he argued that humanity was at risk by what he called a "global heat disaster." Similarly toned, but opposite in conclusion, Calder discussed within his book, *The Weather Machine*, the possibility that humanity was threatened by an impending ice age. The principal source of his anxiety was what appeared to be a clear-cut case of the risks of untrained individuals speaking publicly about the future effects of climate change, namely the inducement of confusion and fear in the general public. As Landsberg publicly noted in a 1976 talk at an AAAS-sponsored symposium in 1976 entitled, "Climate and Plant Productivity,"

> Some, who are not particularly familiar with the peculiarly complex mechanisms of the atmosphere have come forth with prophecies of impending climatic catastrophes. Considering the sources, it is perhaps not too surprising that these projections are diametrically opposed."[96]

Given Landsberg's tendency to admonish who he saw as outsiders working on climate-related matters, the following section explores how these two authors -- Wilcox and Calder -- fueled his frustrations.

By publishing *Hothouse Earth* in 1975, Howard Wilcox was doing so from the vantage point of a trained nuclear physicist and an advocate for the environment. After working during World War II for the Manhattan Project, he became the Director of Research and Engineering for GM Research Laboratories in Santa Barbara, CA. By the late 1960s, he became chairman of the

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[95] Landsberg, Helmut, "Whence Global Climate: Hot or Cold? An Essay Review," *Bulletin of the American Meteorological Society* 57, 4 (April 1976): pp. 441-443
[96] Landsberg Talk at the AAAS Symposium "Climate and Plant Productivity," 1976, Series 5, Box 1: AAAS Papers, HLP

Santa Barbara Environmental Quality Advisory Board, and later the Manager of the Ocean Farm Project at the Naval Undersea Center in San Diego. He believed that a physicist had the requisite knowledge and experience to resolve American environmental challenges. Nonetheless, Wilcox's book suggests that he had broader ambitions than using physics for the betterment of humanity; he had a desire to warn the general public of future catastrophe. In his mind, the problems of industrial growth start as minor nuisances, quietly and with little notice. But over time, he argued, the consequences of never ending growth would become more apparent. He claimed that the earth would get hotter, ice caps would melt, oceans would overrun entire cities, national borders would collapse, large-scale inland migration would take place, and societies around the world would crumble.

There were multiple phrases he used to convey the seriousness of the problem, all meant to convey urgency if not outright fear -- thermal pollution catastrophe; hothouse crisis; global heat disaster; omens of impending disaster. The timetable was fairly short, he argued. If in 80-180 years human beings did not significantly alter their energy consuming ways they would inevitably enter a zone of no return. Contrary to religious prophets of the past, however, he appealed to what he called the "laws of nature" and a good dose of "good common sense."[97] Even if science could prevent a *precise* foretelling of future events, he argued, his training as a physicist allowed him to speak authoritatively on the *general* outlines of future calamity. Even if he could not see the "ultimate consequences" of this train of events – an apparent contradiction to his own visions of disaster -- no one, he argued, could deny the basic truth: "the omens of impending disaster" was *inevitable* if humanity did not get off what he termed the "growth road."[98]

[97] Wilcox, Howard. *Hothouse Earth* (New York: Praeger Publishers, 1975): pp. 104-105
[98] Wilcox (1975), p. 15

Instead of talking about the risks of a global heat death as Wilcox had done, British science writer Nigel Calder proposed that the greatest threat was a return to a global ice age. As he wrote within his 1975 book, *The Weather Machine*, the return of an ice age would likely not be gradual or smooth but rather an abrupt shift that could "easily kill two thousand million people by starvation and delete a dozen countries from the map."[99] His claims generally mirrored the release of a report by the National Academy of Sciences, "Understanding Climate Change: A Program for Action."[100]

Taken together, Landsberg believed that both books -- Wilcox's *Hothouse Earth* and Calder's *The Weather Machine* -- represented what he considered to be pseudo-scientific interpretations of existing science. While Wilcox may have believed he was legitimately using the "laws of nature" and "common sense" to warn of future disaster, Landsberg saw in Wilcox something quite dangerous. "Among the earth warmers, Dr. Howard Wilcox of the U.S. Navy Department is an extremist (not necessarily representing the views of that department). He is a physicist with a 'model,'" Landsberg articulated to Robert Ubell, Editor of the New York Academy of Sciences publication, *The Sciences*.[101] As Landsberg additionally noted in a review of Wilcox's book, Wilcox "is a physicist and has the attitude, which has prevailed in that profession for some time, that education in physics makes you an expert on almost anything."[102] Additionally, Landsberg objected to Calder's apparently slippery views: "In good journalistic form, Calder leaves avenues of retreat. He quotes his favorite scientists at length, and then covers himself by a sentence at the end that there are others with diverging opinions. . . To bring

[99] For those who reviewed Calder's book, this quote illustrated what appeared to be his dramatized depiction of the future effects of an ice age. See, Sullivan, Walter, "The Weather Machine," *American Scientist* 63, 6 (November-December 1975): p. 701
[100] "Understanding Climate Change: A Program for Action" (Washington, D.C.: National Academy of Sciences, 1975). Also, Douglas, John, "Climate Change: Chilling Possibilities," *Science News* 107, 9 (March 1, 1975): pp. 138-140; "NAS Warning on Climate Changes," *Science News* 107, 4 (January 25, 1975): pp. 52-53
[101] Landsberg to Robert Ubell, 14 March 1978, PHL.
[102] Landesberg, Helmut, "Whence Global Climate: Hot or Cold? An Essay Review," *Bulletin of the American Meteorological Society* 57 4 (April 1976): pp. 441-443, quote on p. 441

the reader back from Calder's slick ice to the solid ground of knowledge would require a sizeable monograph."[103] Ultimately, Landsberg believed that both Wilcox and Calder represented the very reason that he undertook to establish the AGU CEQ in 1970; by introducing what appear to be contradictory depictions of the future, untrained individuals can risk not only the credibility of science but unnecessarily alarm the general public into action that may, over the long term, prove more hazardous than the initial threat. Important to his intention to stay within the middle ground, Landsberg's intention was not to poke fun at exaggerations or to hastily dismiss what fundamentally were very real concerns. To the contrary, it was to point out the importance for scientists to maintain restraint when discussing the future of climate, and do so in a way that prevents genuine wisdom from being confused with soothsaying. As he concluded, "Perhaps we should here remind ourselves of the Baconian dictum that one of the stumbling blocks to knowledge is the ostentation of apparent wisdom."[104]

CONCLUSION

As seems clear from his responses to the work of Eddy, Gribbin and Plagemann, Wilcox, and Calder, Landsberg believed that restoring the credibility of the American scientific community -- and the atmospheric science community, more specifically -- meant that established scientists like himself had to protect the boundaries of disciplines. Only through specialization and 'dull' analysis of geophysical observations could scientists learn to understand the natural universe. Eddy's claims about the relationship between the Maunder Minimum and the Little Ice Age, as well as what appeared to be exaggerated claims about future doom by Wilcox, Calder, Gribbin and Plagemann, appeared to reinforce Landsberg's belief that the uncertainties of climate science were being overlooked for the sake of other ambitions.

[103] Landsberg (1976): p. 442
[104] Landsberg (1976): p. 443

Historian of science Spencer Weart has recently offered an explanation for these kinds of tensions. He argues that climate change was one of the few topics that pushed diverse fields into contact during the 1960s and 1970s. Since climate change was never the province of a single group of committed individuals, the topic ushered in a great deal of interest from a diversity of perspectives that occasionally clashed. Everyone seemed to have an opinion on the future of climate, and such discussions quickly evolved into what he considered fantastically worrisome depictions of future disaster. Whether due to what Weart called "physics envy," a frustration with climate becoming fodder for mainstream media's drive for sensational stories, or a fear that the boundaries of climate research was collapsing, this chapter has explored Helmut Landsberg's response to what he believed to be the consequences of diverse fields participating in claims about the effects of climate on human society.[105]

[105] Weart, Spencer, "Rise of Interdisciplinary Research on Climate," *Proceedings of the National Academy of* Sciences 110, 1 (February 26, 2013): pp. 3657-3664

Chapter 3

The CEQ Responds to the SST Controversy, 1971-1981

This chapter addresses one of the most controversial political issues of the early 1970s: the development of a supersonic transport (SST). While many historians have already examined the SST controversy in great depth, this chapter -- while it does not change the overall narrative of the SST history -- does shed light on an unexplored dimension of the debate.[1] By contextualizing Singer's role in SST politics in light of his chairmanship of the CEQ, one can gain an appreciation for why he believed the "middle course" perspective was so important. Second, while Helmut Landsberg was not as directly involved in the internal SST politics, he found common cause with Singer as they attempted to prevent what they saw as an undue influence of politics on science-based decision making.

For Singer and Landsberg, the activities of two individuals served to highlight why reticence was an important attribute of a professional science -- physicist James McDonald and epidemiologist Gio Gori. From Singer's perspective, McDonald appeared to overlook important scientific uncertainties regarding what he suspected to the be the likely negative effects of SST emissions on the ozone layer, and thus cause an increased incidence of cancer rates among the American population. Given the lack of upper atmospheric observations, Singer believed that McDonald overstepped in his claims about future health effects. Likewise, Landsberg criticized Gio Gori's apparent incompetence for allowing his results to become public prior to being vetted. While Gori, as will be shown, never intended his results to enter the public sphere, he nonetheless gave them to a policy maker -- William Proxmire -- who used them to bolster his case against further funding of the SST project. By examining their respective responses to

[1] Conway, Erik. *High-Speed Dreams: NASA and the Technopolitics of Supersonic Transportation* (Baltimore: Johns Hopkins University Press, 2005); Owen, Kenneth. *Concorde and the Americans: International Politics of the Supersonic Transport* (Washington, D.C.: Smithsonian Institution Press, 1997)

McDonald and Gori, their interest in insulating science from the public sphere and toning down what they considered rhetoric in light of existing uncertainties exemplified their broader concerns about the role of politics in scientific claims. The result: the "middle course" they sought to build appeared threatened.

MCDONALD'S TESTIMONY

On March 2, 1971, James McDonald, senior physicist employed with the Institute of Atmospheric Physics at the University of Arizona, testified at the behest of Representatives Sidney Yates (D-IL) and Henry Reuss (D-WI) about what he believed to be the environmental and biological risks of supersonic transport (SST) emissions on the stratosphere. While he had already gained some notoriety as a believer in unidentified flying objects (UFOs) since the mid-1960s, he provided what appeared to him as a credible scientific argument that even trace amounts of SST emissions (water vapor and nitrous oxides) could deplete the ozone layer and ultimately yield an increase in cancer rates. As a member of the National Academy of Sciences Panel on Weather and Climate Modification, McDonald's credentials served to lend weight to his authority as an expert.

McDonald's testimony focused on what he called three "broad generalizations."[2] For McDonald, each generalization was connected to the other, and therefore formed a chain of logic that appeared highly credible. First, his technical understanding of the available data led him to conclude that the residence time of emissions -- the amount of time the emissions resided in the stratosphere -- was significantly longer than in the troposphere (on the order of one hundred times). This was because chemicals released in the upper atmosphere could not be readily washed out by normal tropospheric processes like precipitation. Second, he argued that the

[2] McDonald, James. Statement to the Subcommittee of the Committee on Appropriations. Civil Supersonic Aircraft Development (SST), Hearing, March 3, 1971 (Serial 57-453). (Washington, D.C.: U.S. Government Printing Office, 1971), pp. 299-342, quote on p. 303

stratosphere was a region of high chemical reactivity in that small traces of chemicals can yield significant changes in its long-term composition. Third, he objected to the building of the SST based on his philosophical belief that its development would compel future technological developments in high-speed transportation that would yield even more serious long-term problems; if small changes can yield large consequences, then the problem could, he argued, quickly escalate into a serious and long-lasting problem should the technology be unrestrained. Believing that these generalizations -- two technical, one philosophical -- have been ignored by SST supporters, he contended that they "need to be weighed very carefully in any major national decision to undertake an SST technology."[3]

After delineating these three generalizations, he delved into a more speculative train of thought regarding the climatological and biological effects of SST emissions. These concerns were, he admitted, not strictly known but his calculations seemed robust enough to support his case. First, based on existing tracer studies of nuclear fallout, he believed that stratospheric SST emissions could possibly result in "climatically adverse effects."[4] Despite his own admission that the optical properties of particulates resulting from unknown stratospheric SST emissions were "poorly known," he nonetheless advanced a warning that the effects may be more severe than initially proposed (i.e. in the space of uncertainty, he tended toward a more pessimistic perception of the risks). Second, McDonald moved onto what he considered a more ominous possibility: that the depletion of the ozone layer would likely result in an increased incidence of cancer rates. "The evidence is now quite strong," he argued that emissions would increase the

[3] Testimony of James McDonald, 2 March 1971, p. 304
[4] Testimony of James McDonald, 2 March 1971, p. 304

transmission of ultraviolet radiation and causes "something of the order of 5-10,000" additional skin cancer cases per year in the United States alone.[5]

While climate was one thing, this particular possibility had potentially serious implications; what was once restricted to the upper atmosphere (distant from the concerns of humanity) had now become directly wedded to human welfare. To make his case appear stronger, McDonald relied on existing research regarding what appeared to be a strong correlation between ozone layer depletion and cancer rates, as well as a recently-released MIT report entitled, "Study of Critical Environmental Problems (SCEP): Man's Impact on the Global Environment: Assessment and Recommendations for Action."[6]

As alluded to in the previous chapter, the SCEP Report was released in August 1970, and was the end-product of a month-long workshop in Williamstown, MA. Directed and organized by Carroll Wilson, a professor with the Sloan School of Management at MIT, the meeting provided an opportunity for scientists from various specialties to examine the most pertinent environmental problems of the day, and derive some understanding of what to do about them. As noted in the report's preface, the study of such problems could "provide citizens, public policy makers, and scientists with an authoritative assessment of the degree and nature of man's impacts on the global environment."[7] Intended to provide valuable intellectual material for the 1972 United Nations Conference on the Human Environment, the report was also an opportunity to put into proper scientific perspective what Wilson and others believed to be the speculations and prophecies sensationalized within the media over the last decade. As reported in May 1970, two months before the meeting took place, the purpose was to "examine the conflicting 'doomsday'

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[5] McDonald Testimony, p. 305
[6] Wilson, Carroll, ed. *Report of the Study of Critical Environmental Problems* (Cambridge; MIT Press, 1970)
[7] SCEP (1970), Preface

predictions of man's destruction of the environment."[8] Some within the media speculated that the meeting was a response to Singer's appeal for an "environmental truth squad."[9] While the report was not isolated to one particular issue, the press focused most on the environmental implications of the supersonic transport, and for good reason.

On August 1, 1970, the Steering Committee released the report and held two days of briefings for reporters and a number of key officials from NASA and other federal agencies. This was not a typical fly-by-night affair. They discussed the issues of carbon dioxide and its effects on the global climate system, the effects of DDT on ecosystems, among other environmental issues. Of special relevance, however, was what the MIT study had to say about the SST debate; SCEP concluded that "no problems should arise from the introduction of carbon dioxide and that the reduction of ozone due to interaction with water vapor or other exhaust gases should be insignificant." However, due to suspicions over the reliability of the data, SCEP also concluded that uncertainties in data should be resolved before "large-scale operation" of SSTs begins. Ultimately, this was not an anti-SST statement as much as a pro-research one and it certainly was not an attempt to scare the general public into siding with SST opponents.

Tony Chandler, a professor of geography at University College London (UCL), agreed with the broader motives behind the study. He interpreted the report's publication (the papers presented at the July meeting in Williamstown were published months after the study was publicly released) as an attempt to "infuse more facts" into environmental discussions, and thereby temper what he saw as doomsday scenarios frequently sensationalized in the public sphere. As he wrote,

> In newspapers and on the radio and television, pollution fills many a gap in the available copy, and gloom and doom are poured fourth for public consumption. The interest is

[8] Ayres, James, "Panel will Study 'Doomsday' Theory," *Boston Globe* (31 May 1970): p. 96A
[9] McElheny, Victor, "Scientists Warn of SST Dangers," *Boston Globe* (2 August 1970): p. 1

> purposefully maintained by sensationalism so that it is almost impossible to separate truth from half-truth and both from pure flights of the imagination.[10]

For Chandler, the report was not designed merely to lay out technical arguments and facts isolated from society. Instead, the report was designed to combat prophecies of doom within the media while informing the general public.

Thus, when McDonald testified in front of Congress in March 1971 about the potential effects of SSTs on cancer rates, he strategically utilized existing research to buttress different tiers of his criticism. First, while he did not agree with the conclusions of the SCEP study that the ozone effects would be "insignificant" and that no climatic problems would occur, he did find it useful in that it revealed the concerns of highly credentialed scientists about the natural environment. He saw in its conclusions a precautionary strain of thought that wedded well to his antipathies to the SST program. Second, his claims that SST emissions could cause an increase in cancer rates were in large measure because of his analysis of existing research about what appeared to be a logical sequence of events. He believed that multiple things were known: (1) what he considered to be the robust evidence that ultraviolet (UV) radiation causes skin cancer; (2) the "critical role" of the ozone layer in filtering UV radiation; (3) a series of articles between 1944 and 1961 that revealed how one's geographical location on earth influences cancer rates; (4) a series of scientific articles pertaining to the influence of water vapor (a primary SST emission) on the ozone layer; (5) a range of variables that were used in calculations to derive what appeared to be a conservative result about the effects of large-scale use of SSTs on the stratosphere. While he acknowledged that there "may well be errors in my analysis from the

[10] Chandler, T.J., "Review: Man's Impact on the Global Environment: Assessment and Recommendations for Action: Report of the Study of Critical Environmental Problems," *Geography* 56, 3 (July 1971). For a brief account of his life, see "Obituary: T.J. Chandler," *Weather* 64, 2 (February 2009): pp. 53-54

various parts of the problem," he did suggest that the evidence was "quite strong" that modest changes in stratospheric composition could result in changes in cancer rates.[11]

Any hesitation or caution in McDonald's testimony regarding his estimates and conclusions was rooted the fact that he was "airing" his ideas to a non-scientific audience for the first time.[12] "Until reliable answers can be obtained through appropriate research," he concluded, "these present research estimates, albeit tentative, are much too disturbing to warrant further immediate moves to SST/HST technologies."[13] One of the challenges he faced was the integrity of his overall argument. While each step in his argument appeared supported by robust scientific evidence, his overall chain of reasoning was quite uncertain; the empirical evidence was insufficient to reduce the uncertainties that became increasingly apparent the longer his chain of reasoning became.

But, his claim was not just based on what he considered strong peer-reviewed evidence. And this was the point: he *felt* compelled to speak about a matter of urgency. "I don't think that 5,000 to 10,000 new cases of skin cancer per year are a burden which can be written off lightly and be regarded as something that we dare just wave off and regard as ridiculous" he testified.[14] To make his case even stronger, McDonald also relied on what he believed to be the "general principle" of unanticipated consequences of new technologies over the long-term, as well as his own step-by-step assessment of what he called the "SST skin cancer hazard." He explicitly laid out a philosophy of caution when determining whether funding the SST should be continued, and believed that his philosophy rested on solid scientific ground. The potential side-effects of new technologies, he reasoned, should be understood before they attain "so advanced a state of

[11] McDonald Testimony, p. 309
[12] McDonald Testimony, p. 323
[13] McDonald Testimony, p. 310
[14] McDonald Testimony, p. 326

development that too large an economic and social investment has been made to stop the new technology prior to its getting beyond a point-of-no-return."[15] Given the history of humankind, this appealed to common sense more than a systematic analysis of historical and technological change. Instead of diving into the pool, metaphorically speaking, he appealed to the cautionary idea that one should test the water with one's toe just to make sure there are no dangers lurking in the depths. One needs, he concluded, "to be reminded that adverse effects have repeatedly unfolded as a consequence of causal chains that connect seemingly very distantly related events.[16] This environmental hazard, he adamantly asserted, is "not 'kooky,' it is not nutty, it is not ecological extremism."[17] According to him, the combination of reasoned speculation, philosophical objections, and a reliance on existing peer-reviewed literature provided what appeared to him to be a credible argument against the continued funding of SST production.

SINGER'S TESTIMONY

The following day, on March 3, 1971, Singer was given an opportunity to lay out his own concerns in response to McDonald. He was introduced by William Magruder, Director of the Supersonic Transport Development at the Department of Transportation and one of the chief advocates for the SST project. This was an important moment because it would allow Magruder to characterize Singer as a credentialed expert, and thus frame him as above the political fray in spite of his own ambitions. He labeled Singer an "environmental expert" with "considerable professional interest in the field of environment both through Government and university service."[18] After being introduced by Magruder, Singer went onto note his role as chair of the CEQ, which he envisioned as a kind of personal trademark of his commitment to dispassionate

[15] McDonald Testimony, p. 307
[16] McDonald Testimony, p. 309.
[17] McDonald Testimony, p. 319
[18] The DOT SST Environmental Advisory Committee also consisted of: Myron Tribus, William Kellogg, Lester Machta, H.J. Mastenbrook, George Chatham, Harald Rossi, Paul Tompkin, Robert White, S.I. Gerainewohl, Arinur Wolff

inquiry. "One of our mandates," he noted, "is to actively insure that scientific information regarding environmental matters is neither unwittingly nor mischievously misused."[19] This was also important because he needed to clarify that he was not swayed by what he saw as Magruder's more politicized role in SST politics, a risk he sought to squash when initially asked to serve under Magruder within the DOT.

On January 12, 1971, Magruder invited Singer to succeed Assistant Secretary Myron Tribus as chairman of the DOT SST Advisory Committee, which was responsible for advising Magruder on the environmental impacts of the SST. Upon accepting the position, however, Singer wanted to make clear that his motivations were not aligned with Magruder. "In accepting the chairmanship of this Committee, I want to be sure that there are no misunderstandings between us," he began.

> I am not now a supporter of the SST program and have not taken a stand either for or against it previously. . . My field of expertise, of course, is environment and radiation, and here I will be happy to serve because I want to make sure that scientific data are not misused, either to support the SST program or to oppose it.[20]

As chairman of the CEQ, as well as his own explicit belief in the importance of dispassionate inquiry, his SST testimony was an important litmus test of how his ideals would meet the realities of strong partisan politics. This provided an opportunity to show what the "middle course" meant in practice.

"The main reason I am here," Singer began during his testimony on March 3, 1971, is to "tell you the views of our SST Environmental Advisory Committee on the principal environmental concerns which have been raised regarding large-scale operation of supersonic transports in the stratosphere . . ." Listing a range of reasons to withhold judgment on the

[19] Hearing, 3 March 1971, p. 528
[20] Singer to Magruder, 21 January 1971, Box 25, S. Fred Singer Papers

environmental effects until his advisory committee could do a more thorough analysis of the problem, he launched into a more personal perspective on how the SST had become a "symbol" for a coalition of individuals -- including scientists -- to subvert all technological progress and basic science. This was not, however, an attack on environmentalists concerns as a whole; it was instead an attack on those who he believed sensationalized dangers to influence national politics and public opinion.

> Now we have had many scares, some propagated even by scientifically-trained people, that we are going to run out of oxygen, because we are burning up the fossil fuels, or because we may be destroying the green plants in the ocean that produce oxygen. This scare has proven to be quite baseless. But that doesn't mean that other effects could not exist that are equally far-reaching.[21]

For Singer, there may very well be serious environmental effects involved should the United States develop a fleet of SSTs. He welcomed an opportunity to investigate in greater depth McDonald's range of charges, as well as any other possibilities that new technologies could result in environmental damage. There was a logic to his belief -- if something was potentially serious, investigate; if there was a measured effect, notice it but do not exaggerate until further research is conducted; if uncertainties exist, minimize them before engaging the public. There were legitimate economic and political concerns at play that could lead to the discontinuation of funding for the SST program, he reasoned, but he did not believe that environmental concerns as currently understood should be one them. "Let us put matters into perspective," he testified.

> There is no question whatsoever that many human activities are affecting the environment, are changing the atmosphere, and are putting out pollutants that are spread throughout the world. There is no question that the SST is going to release some pollutants into the atmosphere, but it is doubtful whether they will be of any significance. In the balance, I believe that the question of whether we should or should not have an

[21] Singer Testimony, p. 535

> SST must be decided on the basis of economics and national priorities, with the environmental effects having a very small weight indeed. If the SST is going to be turned down, let's be sure that it is turned down for the right reasons.[22]

What appeared to Singer as an appropriate level of caution could not have stood in more contrast to McDonald's claim that the effects of SST emissions on cancer incidence rates "must now be carefully weighed into the present decisions on the SST program."[23]

A few days after his own testimony, Singer visited McDonald's office in Arizona to discuss further his claims. After returning, he wrote to Magruder his concerns that McDonald's claims were short on direct instrumental measurements. For instance, McDonald did not account for five principal things when it came to UV exposure on the surface: 1) natural ozone variations according to what he called the "sun angle"; 2) the obvious influence of a lack of clothing for those who reside in the south relative to the north; 3) the influence of clouds on cancer rates; 4) the effects of other types of air pollution that may influence ozone; 5) human migration from the north to the south. However, insofar as this reasoning contributed to the SST being discontinued, McDonald's conclusions appeared "greatly exaggerated." Singer ultimately came to the conclusion that he presented "arguments to make this correlation plausible, but no hard, direct evidence whatsoever."[24]

Given what appeared to be the integration of environmental risk into what he saw as a range of other economic and political concerns, Singer became increasingly agitated by the whole process. He could not understand how those who he saw as untrained and environmentally-conscious individuals could influence technical discussions so heavily. For him, what he saw as reasoned and judicious restrained on the environmental risks of SSTs came

[22] Hearing, 3 March 1971, p. 538
[23] McDonald Testimony, p. 307
[24] Singer to Magruder, 15 March 1971, Box 36, Singer Papers

across to others as an outright dismissal of environmentalists concern for the environment. For him, environmentalists' concerns per say were not the issue; he shared them. However, he took issue when who he saw as untrained advocates appearing more scientifically learned than they actually were. When challenged by Representative Silvio Conte (R-MA) to better explain his opinion of environmentalists who chose to voice their concerns about the SST project, Singer made his point even more clear about the importance of distinguishing between what he believed to be a reasoned approach to investigating uncertainties and the tendency of environmentalists to assert truth without sufficient basis. This was ultimately about motivations as much as the concerns being vocalized by various parties.

> Mr. Conte. As a last question, is it your general position that the objections raised by the environmentalists that we have heard here in the last 2 days about this program are unfounded and of little concern?
>
> Dr. Singer. No. On the contrary, I think the objections that have been raised have to be looked at very carefully and I think every concern that has been brought forward needs to be examined on its merits . . . I think it is the fact that the concern has been brought forward that needs to be investigated.

<u>THE USE OF PROTOTYPES</u>

One of the fundamental disagreements between McDonald and Singer was whether the use of prototypes -- the testing of a small fraction of what may become a larger fleet -- would shed further light on the effects of SSTs on the upper atmosphere, and particularly the ozone layer. Given the lack of in situ measurements of the impact of emissions at such altitudes (~65,000 feet and even higher), a small number of prototypes appeared to be a reasonable approach. However, this apparently disarming approach was not as simple as some believed.

During his testimony, McDonald criticized the use of prototypes on two grounds. First, he was concerned that the argument for prototypes was merely a ploy to continue SST funding and ultimately build the kinds of fleets imagined by SST supporters. Rooted in his concerns over unmitigated technological progress, he concluded the following:

> I would think it would be rather dangerous to risk going beyond the no-return point in going to prototypes. . . The problem of assessing technological hazards early is a very crucial problem and we are right up against, I think, a question like that here, and it is a national and a public policy question of what do you do with SST technology. . .[25]

Policy makers, he argued, were in a difficult position. If they fund the building of prototypes to test existing speculations on the upper atmospheric effect of SST emissions, which could start a kind of chain reaction that would ultimately produce the kinds of effects that McDonald feared. For him, this prospect was entirely real and reasonably justified his opposition.

And, he further articulated, the use of prototypes to acquire in situ measurements would do little to understand the effect of hundreds on the global atmosphere. "Although a suggestion has a rather plausible ring to it," he argued, "careful examination . . . shows rather conclusively that availability of a few flying SST prototypes will do almost nothing to settle these scientific controversies." For him, the only way to clarify the effects and reduce the uncertainties involved what he referred to as "laboratory or computer work."[26] Accustomed to studying photochemical reactions on paper and computers, he believed that the building and use of prototypes would be useful for little more than "checking engineering-feasibility questions."[27]

Singer, however, offered another perspective. Whereas McDonald questioned the usefulness of SST prototypes based on his fear that it would unleash what he envisioned as unmitigated techno-enthusiasm, Singer took the opposite perspective. For Singer, this appeared immaterial to the issue of its as yet speculative effects on the climate and cancer rates, and was beyond the scope of a scientists' purview. "There is no environmental reason not to build them,"

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[25] McDonald Testimony, p. 330
[26] McDonald Testimony, p. 306
[27] McDonald Testimony, p. 307

he argued. "There is a minor environmental reason to build them because once they are built you can test certain things on them that will illuminate further the possible environmental effects of the fleet."[28] For him, the issue was the amount of certainty necessary to adjudicate whether SSTs would cause or not cause "significant" environmental damage. Why, he wondered, could a scientist not advocate direct empirical measurements? While he hesitatingly claimed in response to a question that he was 95 percent certain that the effects of a fleet would be insignificant, he sought to understand whether his certainty could be increased by virtue of in situ measurements provided by the SST prototype. While the prototype would most likely only reaffirm his suspicions of McDonald's testimony, he believed that using the prototypes had little risk and could result in significant gains in understanding.

Ultimately, Singer sought to uncouple the philosophical and methodological concerns of McDonald. The underlying problem, as he saw it, was that neither he nor McDonald could testify on the effects of SST's on cancer rates. Given the environmentalists concerns about the potential environmental effects of SSTs, Singer could not understand why the very mechanism that would provide the most direct kind of empirical evidence to adjudicate the issue was seen as problematic. As he articulated,

> We are talking about making certain measurements in the atmosphere which is there all the time for us to measure, measurements that have not been made up to now, measurements that will be specifically directed toward reducing certain concerns that we have about what will happen. Once these measurements are made and once we have the numbers, I think our uncertainties will be much reduced.[29]

The whole experience of taking what Singer believed to be a neutral position led to increased frustration with sectors of the environmental movement. The following month, on April 5, 1971, Singer --- who had also become an assistant administrator within the

[28] Singer Testimony, p. 595
[29] Singer Testimony, p. 585

Environmental Protection Agency (EPA) -- gave what he luridly labeled as his "confessions" about what he considered to be the influence of environmentalists on causing the SST project to be defunded at the end of March 1971. "While I am not sure that I would support at this time the building of an SST fleet," he began, "I am convinced that we have made a mistake by cutting off the development of the prototypes at this stage."[30] Give the enormous sums of federal dollars already invested in the project, he argued, it seemed reasonable to Singer to continue developing prototypes and acquire a more observation-based understanding of atmospheric effects. While there have always been individuals who were concerned with the environment -- those whom he referred to as "nuts, freaks, or odd in some way" or "little ladies in tennis shoes" -- it seemed that their influence had acquired a new kind of weight within environmental politics.

> With the rise of the environmentalists, there also came the people who jumped on the environmental bandwagon for reasons of their own -- basically anti-establishment types who saw it as a way of promoting their own ends, people who have argued with me and felt that the only way to get the environment clean was by destroying capitalism utterly and completely, people who ignored the fact that the Soviet Union is probably worse off from an environmental point of view at their stage of development than the United States, people who don't believe that a socialist society had still found no way to stop people from defecating. So we have with us what I call the environmentalist extremists as General Henry Jackson has referred to them, the eco-freaks . . . who argue not only against all forms of establishment but also against all forms of technology. It is frightening to me. . .[31]

His frustrations were also born out of what appeared to be his resentment for not being invited to participate in the Williamstown meeting in the summer of 1970 which produced the SCEP Report. As he recalled,

> I was rather annoyed by this committee -- I guess because they didn't ask me to come up or some reason like that -- and thought, well, what the hell do they know about it, I just

[30] "SST and the Environment," 5 April 1971, Box 36, Singer Papers
[31] "SST and the Environment," 5 April 1971, Box 36, Singer Papers

> organized a symposium on the subject, and here they are having another one and they didn't even have the decency to check with me.[32]

Singer believed he had some reason to be perplexed; he had indeed organized a symposium through the AAAS, entitled "Global Effects of Environmental Pollution" approximately a year prior to the SCEP meeting. Consistent with his belief that environmental science required collaboration among the geosciences, he brought together a wide range of specialists, including atmospheric chemists and physicists, oceanographers, biologists, pedologists (scientists who study the soil), as well as climatologists.[33] Implied was a belief that he could have acted as a stop-gap, and therefore provide what he considered to be a more honest assessment of the risks of SST. "And they really hadn't found anything to be concerned about, which was embarrassing, except one thing, and they blew that out of proportion because it was the only thing that they could talk about. It was the SST."[34] This was not about the media; this was about the motivations behind the organization of the SCEP press conference in August 1970. Even though many believed the study was the most authoritative and cautious statement about the effects of SST, Singer was -- perhaps unfairly -- perplexed by the steering committee's apparent myopic focus on what appeared to be the most sensational issue.

Beyond what may appear as petty frustrations, Singer also believed that the SST controversy yielded insight on the problematic role of politics and normative judgments in decision making and risk assessment. On April 26, 1971, he presented his reflections at the colloquium at the National Academy of Engineering. After describing the allegedly odd ways in

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[32] "SST and the Environment," 5 April 1971, Box 36, Singer Papers

[33] Those who contributed were: S. Fred Singer, Francis Johnson, Frederick Sisler, Syukuro Manabe, Rainer Berger, Willard Libby, Louis Jaffe, Elmer Robinson, Robert Robbins, Barry Commoner, D.R. Keeney, W.R. Gardner, T.C. Byerly, Arthur Hasler, Reid Bryson, Wayne Wendland, J. Murray Mitchell, Helmut Landsberg, Vincent Schaefer, Edward Goldberg, and George Woodwell. See, Singer, S. Fred, ed. *Global Effects of Environmental Pollution: A Symposium Organized by the Ameircan Association for the Advancement of Science, Dallas, Texas, 1968* (Netherlands: Springer, 1970). For a summary of the papers presented, see Singer, "Global Effects of Environmental Pollution," *EOS* 51, 5 (May 1970): 476-477

[34] Singer to Magruder, 15 March 1971, Box 36, Singer Papers

which humans make decisions, he launched into a discussion of what he called the "methodology of decision theory."[35] His idea boiled down to the following. In principle, one is frequently introduced to situations where they must assess the risks of potential benefits vs. hazards before making a decision. This seemed perfectly reasonable to him, except when it came to the SST controversy when the assessment of risks and benefits were "seldom independent."

> A great preponderance of individuals were either opponents or proponents of the SST. It would appear to me, admittedly from a small sample, that those who regarded the SST utility as low also estimated the risks as being very high, while those who regarded the utility as high felt the risks to be very low. . . Evidently, utility considerations distort risk estimates or risk estimates distort utility considerations.[36]

In short, Singer believed that one's belief on the value of the SST influenced their judgments on the risks of producing the SST.

This is crucial insight because it reveals how Singer conceived of his role as a paver of a "middle course" within environmental politics. For Singer, decision making regarding the SST reflected a general tendency of both proponents and opponents to allow their normative judgments about its potential utility to influence their technical judgments of the risks involved. Opponents, he believed, erroneously objected to the SST by conflating their belief in its low utility with what seemed like the high risks to human beings. For proponents, the opposite was in play -- they erroneously conflated their belief that the SST provided a high utility with an equal emphasis on the low environmental risks. People were conflating two separate issues -- utility and environmental risk -- because of what appeared to him as the influence of politics on scientific deliberation. For Singer, this kind of decision making was a poor way to address environmental concerns because the very practice of being for or against something distorted

[35] Singer, S. Fred, "The SST: A Case Study in Benefit-Risk Relationships for Decision-Making," Colloquium of the National Academy of Engineering April 26, 1971, Box 35, S. Fred Papers
[36] Singer, S. Fred, "The SST: A Case Study in Benefit-Risk Relationships for Decision-Making," Colloquium of the National Academy of Engineering April 26, 1971, Box 35, S. Fred Singer Papers

one's ability to make informed decisions. The middle ground, at least in his mind, was intimately wedded to his philosophy about the nature of decision making.

LANDSBERG'S OBJECTIONS

By the late 1960s Landsberg and Singer had become working colleagues. They were both associated with the American Geophysical Union, and Landsberg helped to organize symposia with Singer on at least two occasions in 1968.[37] Not only were they colleagues, they shared many of the same opinions about numerous science-related issues and particularly the science-based politics of the SST controversy.

For Landsberg, who had since left his post as the president of the CEQ by 1970, the whole ordeal of the SST controversy made him realize how difficult it would be to inject what he believed to be a rational assessment of the environmental effects. For instance, in October 1971, seven months after Singer testified, he wrote to John Firor, the Director of the National Center for Atmospheric Research, to discuss what kinds of projects NCAR could commit itself too, particularly given major gaps in knowledge regarding upper stratospheric photo-chemistry. He noted colorfully: "A little while ago, atmospheric scientists were asked to contribute to a practical question concerning events in that level, namely the SST affair. Our underwear was badly dragging. We had only enough ignorance to contradict each other and appear as fools."[38] For him, the problem was not that atmospheric scientists were ignored; it was that the politics of decision making outpaced the vetting process of scientific claims. Like his experiences with earthquake prediction in the 1930s, and meteorological forecasts in the 1940s, Landsberg

[37] Two conferences occurred in 1968. The first was entitled, "Global Effects of Atmospheric Pollution." The second was organized under the auspices of the National Academy of Sciences and the U.S. Department of Interior, entitled "Beneficial Modifications of the Marine Environment." The latter took place in March 1968, and included introductory remarks by Stewart Udall, concluding marks by Stanley Cain, and a luncheon speaker, Frank C. Di Luzio. Papers were presented by J.O. Fletcher, Robert Gerard, Donald Pritchard, and William Ackerman. Discussants included: William Donn, S. Fritz, Helmut Landsberg, Earl Droessler, Leonard Dworsky, J.P. Bruce, L. Eugene Cronin, and Joseph Caldwell.

[38] Landsberg to Firor, 18 October 1971, Series 2.1, Box 2: Daily File January 1971-June 1982, PHL

believed that scientists had little opportunity to truly produce reliable results, and therefore any judgments appeared hastily made.

After McDonald's testimony on March 2nd, Proxmire sent to forty specialists a request to discuss their thoughts on the biological effects of SST emissions. This was an opportunity to gauge the opinions of a variety of experts on the matter of environmental and biological effects of SSTs. Among those atmospheric scientists chosen to comment -- Lester Machta, Skuyuro Manabe, and Ried Bryson -- they failed to arrive at a consenus. Lester Machta, for instance, voiced what he believed to be the "many uncertainties in the calculations" regarding increased UV radiation from an increase in stratospheric water vapor. Additionally, he argued that the "purely climatic effects" of a decrease in ozone levels "might not be significant."[39] Likewise, climate modeler Skuyuro Manabe believed that the quantitative basis for McDonald's "chain of argument" was "not secure enough to be convincing."[40]

Of particular interest to Manabe was link between increased water vapor concentration in the stratosphere and the belief that more UV radiation would reach the ground. He noted the lack of a mathematical model to determine the effects of water vapor on ozone since it was unclear how altitude- and latitude-dependent water vapor would be once ejected into the stratosphere. Would it spread uniformly around the globe, and if so how would that affect the overall concentration of UV radiation on the surface? Manabe was also concerned about the apparent lack of "quantitative knowledge" regarding photochemical reactions at such altitudes. All scientists could reasonably do, he argued, was to accelerate research in the most crucial areas: mathematical modeling, photochemical theory, and acquiring more reliable measurements

[39] Machta to Proxmire, 4 March 1971, Box 36, S. Fred Singer Papers
[40] When asked to contribute to the discussion, Joseph Smagorinsky deferred to Manabe's expertise and judgment.

of how contaminants are distributed within the stratosphere.[41] Reid Bryson, a climatologist at the University of Wisconsin, appeared to be the only one who strongly endorsed McDonald's claims, pointing out that he was the "most meticulous, thorough scientist I know and is generally so regarded by his profession."[42]

One of the sources of Landberg's greatest frustration with the SST debate was with the activities of Gio Gori, Associate Scientific Director of Etiology at the National Cancer Institute. After the U.S. Senate voted to reject additional funding for the SST by a 52-41 margin in late March 1971, Senator William Proxmire (D-WI) put into the record what he called "new developments" that would only serve to reinforce his message that the SST was an environmental and biological threat. After listing a series of economic reasons why the SST should be discontinued, he launched into what he considered the "most difficult and delicate parts" of the issue -- cancer. Tepidly, he noted how whenever the issue was brought up advocates for the SST "say it is a scare tactic, that it is unfair and emotional."[43]

One of the individuals he wrote was Gio Gori. After doing his own campaign to find out as much as possible about the matter, Gori responded with his assessment. First, noting that humans have developed defense mechanisms to "just barely" withstand the fraction of UV radiation that reaches the surface, he argued that it was safe to conclude that an increase in natural radiation level due to a depletion of ozone would produce "significant biological effects of chronic nature, such as, for instance, on skin cancer incidence."[44] Second, while Gori acknowledged the lack of precise values of how much UV radiation actually reaches the surface, he predicted that a fleet of eight hundred SSTs would increase the water content of the

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[41] Manabe to Proxmire, 9 March 1971, Box 36, S. Fred Singer Papers.
[42] Reid Bryson to William Proxmire, 11 March 1971, Box 36, S. Fred Singer Papers
[43] *Congressional Record - Senate*, 23 March 1971, pp. 7464-7483
[44] Gio Gori to William Proxmire, 11 March 1971, Box 36, S. Fred Singer Papers

stratosphere and cause a depletion of ozone by between four and ten percent (with a commensurate rise of UV radiation by between twenty and ninety percent). Third, given the well-established link between skin cancer and UV exposure, he surmised that between 23,000 and 103,000 new cases of skin cancer would appear in the United States per year.[45]

Within a week, Proxmire held a press conference and released Gori's estimates -- as well as all of the other respondents' letters -- to the media. The result was unsurprising: headlines included such titles as "Proxmire using Scare Tactics" and "Skin Cancer Scare Halts SST Talks."[46] Among the bundle of respondents' claims that were released, Proxmire preferred to highlight Gori's. Indeed, given his objections to the SST program, Proxmire was likely undisturbed that the technical claims within Gori's response had been mildly sensationalized.[47] For reasons that are not documented, Landsberg learned of Gori's statements to Proxmire.

As President of the American Institute for Medical Climatology, and thus what he considered to be an authority on the medical effects of atmospheric changes, Landsberg's criticism of Gori appeared justified on three matters.[48] First, he questioned Gori's scientific claims. After reviewing all of the available literature on ozone and its behavior in the stratosphere -- a not unlikely assertion given his stature and decades of research -- he concluded that the problem boils down to the assumptions made regarding the photochemical reactions that take place between ozone and water vapor. Given what he believed to be the natural fluctuation of ozone levels, as well as his own discussions with the "world's greatest experts" in photochemistry, he noted how "unlikely" it was that ozone could be depleted by the amount

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[45] Gio Gori to William Proxmire, 11 March 1971, Box 36, S. Fred Singer Papers

[46] Doley, Edward, "Skin Cancer Scare Halts SST Talks," *Chicago Daily Defender* (18 March 1971): p. 8; "Proxmire Using Scare Tactics in SST Fight," *Chicago Tribune* (18 March 1971): a7

[47] The results disclosed to Proxmire on March 11th are actually different than the results disclosed in Proxmire's floor speech, the latter of which showed that the ozone would be reduced by 1 to 3 percent, the UV radiation would increase by 5 to 27 percent, and the estimated increase in cancer rates would increase by between 11,500 to 62,000 per year within the United States. *Congressional Record - Senate*, 23 March 1971, pp. 7464-7483

[48] Landsberg to Gori, 21 July 1971, Series 2.1, Box 2, PHL. Unfortunately, the correspondence to Landsberg from Gori was not found, so the broader context of the exchange was construed from Landsberg's perspective alone.

suggested by Gori. Even if the water vapor injections were as high as presumed by others, he continued, Landsberg was reassured by what he believed to be two facts. Since SSTs would operate over a very small percentage of the earth's surface, the lateral air motions would rapidly fill in any possible depletions of ozone that may locally occur (i.e. the atmosphere would naturally adjust). Additionally, since SSTs would likely be restricted to ocean areas Landsberg believed it a "very bold assumption" to assert that reductions in ozone would still persist over the continents.

Second, Landsberg attacked Gori's apparent sloppiness when it came to publicly revealing what he believed to be unreliable information. This is perhaps the most unfair criticism given the unlikelihood that Gori would have known Proxmire's intention to release each of the respondents' claims on the senate floor, and in turn to the media. While Gori was certainly justified in believing that he was providing the advanced knowledge of the issues involved, Landsberg reasoned that a "widening credibility gap" between the scientific community and the public required extra diligence with regard to "internal speculations" like Gori's.[49]

Third, Landsberg quite confidently cast himself as a better scientist given what he envisioned as his adherence to science than rather than ethical considerations. "Based on long experience," he began,

> It is likely that the vast atmosphere of the earth will quickly restore equilibria in case of relatively small disturbances by SSTs . . . While I applaud your ethical concerns it seems that sound decisions can only be obtained on the basis of facts, and the scientist's main function is to ascertain these facts rather than to engage in speculations that are apt to mislead the public.[50]

[49] Landsberg to Gori, 21 July 1971, Series 2.1, Box 2, PHL. Unfortunately, the correspondence to Landsberg from Gori was not found, so the broader context of the exchange was construed from Landsberg's perspective alone.
[50] Landsberg to Gori, 8 September 1971, Series 2.1, Box 2, PHL

For Landsberg, Gori's primary misstep was not just that he made claims about the effects of SST emissions on cancer rates. It was that Gori had apparently allowed his unreliable results to enter the public sphere.

CONCLUSION

Between 1980 and 1981, both Landsberg and Singer were given opportunities to reflect on the apparent lessons to be learned from the SST controversy. For Landsberg, the debate illustrated how easily scientific claims were prematurely released to the general public, and how easily politics would occasionally use claims to bolster their own specific causes. In April 1980, for instance, John Cumberland, an ecological economist and Director of the Bureau of Business and Economic Research at the University of Maryland, asked Landsberg to comment on a draft report detailing the possible climatic consequences of the CFC emissions, entitled *The Economics of Managing Chlorofluorocarbons*.[51] Since Landsberg was the leading authority on climate at Maryland, it only made sense to get his feedback before publishing. Pleasantly surprised that economics were beginning to incorporate climate into models of environmental impacts, he was all too glad to assist.

Concerned that portions of the draft were a bit too ambitious given the lack of sufficient and reliable evidence, Landsberg noted his opinion that any climate effects on the surface -- should CFCs harm the ozone layer -- would be lost in the natural variability of climate (i.e. noise). The implication is that even if there were human-made effects scientists would be unable to credibly identify and distinguish them given what Landsberg considered natural fluctuations in the global climate system. His suggestion: "tone down" any suggestion that CFC emissions would result in anything more than a "trivial" effect on the greenhouse effect in comparison with

[51] Landsberg to Cumberland, 17 September 1980, Series 2.1, Box 2, PHL. Also, Cumberland, John, James Hibbs, and Irving Hoch, eds. *The Economics of Managing Chlorofluorocarbons: Stratospheric Ozone and Climate Issues* (Washington, D.C.: Resources for the Future, 1982)

an "entirely controlled (and probably uncontrollable) effect of CO_2."[52] "It would also seem wise to state that our knowledge remains deficient and that very little might happen because of unknown factors," he argued. This was the first element of his rationale: to communicate uncertainty is to bolster one's credibility and wisdom about what he saw as the complexities of the climate system. For him, investing too much certainty into anything pertaining to the climate system given the relatively young science of climatology was to place oneself in a position that they may turn out to be wrong.

To be clear, Landsberg was not articulating that scientists be so reticent that science never progresses; he knew that to err is to advance knowledge in one way or another. His concern was more about accurately reflecting the state of science then available, even if it could not readily applicable to the state of politics on one or another pertinent issue. Relevant science is good science, but to make premature claims could risk the integrity of the scientific profession and thereby make it appear as if scientists do not have their act together. As he further noted to Cumberland regarding the SST affair years earlier, the recent volcanic eruption of Mount St. Helens on May 18th in Washington State served to buttress his caution when attributing changes in the global atmosphere to human activities. As he cautioned, "Remember that all the SST-NOx Cassandras now look very foolish. Also, we have just learned about the large Cl exhalations from Mt. St. Helens. Nature must have withstood such stuff for millions of years and the ozone layer is still there."[53] In effect, the science of climate was uncertain, and political issues that emerge should not detract from the responsibility of scientists to be neutral and dispassionate arbiters of what was known and unknown.

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[52] Landsberg to Cumberland, 17 September 1980, Series 2.1, Box 2, PHL
[53] Landsberg to Cumberland, 21 August 1980, Series 2.1, Box 2, PHL

This notion of proper pacing making claims about the human influence on the global atmosphere was deeply embedded in Landsberg's worldview. He felt that scientists should be patient and deliberate in making claims, and had a bias against what he believed to be premature ones. As he described a year earlier in June of 1979,

> my knowledge of climate convinced me that it is basically a very conservative system, in spite of its statistical fluctuations. The feed-back mechanisms of all kinds always try to restore the prior equilibrium. . . One can do this in engineering when the scientific principles are known but when the scientific 'bricks' are lacking the structure will be shaky.[54]

In short, making claims without a proper foundation of understanding make any subsequent chains of reasoning appear overly fragile and shaky.

Indeed, these experiences became a marker of what can go wrong when science and politics became too closely wedded. In January 1981, ten years after the United States federal government discontinued funding for the development and production of supersonic transports, S. Fred Singer, now Professor of Environmental Science at the University of Virginia, convened a symposium, entitled "The SST Controversy Ten Years Later: A Case study in the Use of Science for Decision Making."[55] While Singer's talk is not available, the abstract of his talk gives a fairly good impression of what he said. While most objections to the SST prior to 1970 were principally based on engine noise and the effects that a sonic boom would have on citizens, he believed that the discussion turned for the worst when James McDonald voiced what he considered his "erroneous" concerns about its environmental effects. For Singer, McDonald's combination of philosophical and technical judgments about the environmental effects appeared overly biased against the SST, and appeared too wedded to politics.[56]

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[54] Landsberg to Julius Chang, 22 June 1979, Series 2.2, Box 3, PHL
[55] The symposium was titled, "The SST Controversy Ten Years Later: A Case Study in the Use of Science for Decision Making." The participants were: Brian Martin, Harold Schiff, Hugh Ellsaesser, Harold Johnston, Robert Oliver, and S. Fred Singer.
[56] Singer, S. Fred, "The Situation Before 1971," Abstract, 8 January 1981, Box 36, S. Fred Singer Papers

For both Landsberg and Singer, the SST controversy revealed what appeared to be a slowly degrading apparatus of American science to speak on issues both relevant for society but technical enough to justify their geophysical perspective. According to Singer, McDonald injudiciously conveyed too much certainty about the environmental effects of SSTs on the upper atmosphere. This appeared to exceed what was then known, particularly given the lack of in situ instrumental measurements. Singer and McDonald had fundamentally different perspectives of the risks involved, and disagreed over the scientific value of prototypes. Landsberg had his own concerns. He believed that Gio Gori's analysis played off of existing concerns over cancer without sufficient evidence, and grew frustrated with what he saw as Gori's release of his results to the general public via William Proxmire. Gori, in Landsberg's view, failed to protect his data and thereby risked the credibility of the professional reputation of the scientific community -- in spite of Gori's own reassurances that he consulted professionals even within the atmospheric sciences. Landsberg may have been unfair in his criticism of Gori, but his frustrations in this instance only reaffirm his broader motivations.

In this respect, this chapter was not intended to provide a comprehensive history of the SST controversy. To the contrary, it provided a focused analysis of Singer and Landsberg's views to provide what I consider to be a point of historical ambiguity: whether Singer was a supporter of the SST program. On the one hand, Lydia Dotto and Harold Schiff's historical account of what they call the "ozone wars" refers to Singer's own belief that he was "not a supporter of the SST program."[57] Their argument rests on Singer's January letter to Magruder in which he accepted the chairmanship of the DOT SST Advisory Committee, and wherein he expressed his commitment to making sure that scientific data are not "misused." Additionally,

[57] Dotto, Lydia and Harold Schiff. *The Ozone War* (New York: Doubleday and Company, Inc., 1978), p. 57

they keenly argue that Singer did not reject environmental concerns "out of hand," and therefore he was not a dogmatic opponent of the environmental movement.

On the other hand, historian of science Joshua Howe argues that Singer was an "SST supporter" due to the apparent lack of convincing evidence that the SST would cause significant ozone depletion or climate change. According to Howe, Singer's belief that the SST project should not be cancelled on the basis of environmental claims suggests that he was complicit with proponents albeit cloaking his claims in the veil of his scientific credentials. His evidence base included two sources: Singer's congressional testimony in early March 1971, and a *New York Times* article that covered his congressional testimony. By voicing caution against discontinuing SST funding he became a conservative advocate for the SST.[58] Certainly, whether Singer was an advocate or opponent of the SST is an interesting historical question because both rely on a solid evidentiary base and both provide reasonable interpretations of the evidence. Even so, they arrive at very different conclusions.

By incorporating Singer's personal papers into the broader narrative of SST politics during the early 1970s, as well as his role as chairman of the CEQ, this chapter offers a way out of this state of affairs. Largely agreeing with the interpretation offered by Dotto and Schiff, this chapter does not see Singer as an outright SST supporter, as Howe had done. To the contrary, it appears that he had little problem discontinuing federal funding if grounded in what he believed for a series of reasons that existed outside of the environmental -- noise pollution and economic costs. When it came to its environmental effects, however, he believed that insufficient evidence existed to claim that it was actually a biological or climatic threat. While he never dismissed McDonald's claims outright, Singer questioned whether his speculative chain of reasoning was

[58] Howe, Joshua. *Behind the Curve: Science and the Politics of Global Warming* (Seattle: University of Washington Press, 2014), p. 53

sufficient to justify a discontinuation of the program. This is why he advocated for a research program based on prototypes as chair of the DOT SST Environmental Advisory Committee, namely to reduce or eliminate uncertainties and confirm whether SST emissions posed a hazard to the ozone layer and human health.

In this light, Singer's role in the SST debate should not be seen as a contentious debate between a politically conservative scientist and the environmental movement. While Singer was partisan in that he frequently revealed his own antipathies to some segments of the environmntal movement, a strict binary between proponents and opponents within the SST program easily overlooks Singer's own belief in the value of the "middle course," defined as an attempt to demarcate scientific discussions from what he considered to be overly dramatic claims about future threats to humankind. The most important lesson for him in the early 1980s was the dynamic nature of scientific knowledge, and the need for policy makers to be "very careful in accepting any scientific views on a complicated subject as final. Our experiences show that scientific views can turn 180 degrees in a short time," he cautioned.[59] Environmentalists, he believed, frequently sought to co-opt the language of science without proper acknowledgement of the complexities involved, and thereby confuse the issues in a way that made the possibility of 180 degree shifts that much more likely. While it would be difficult to quantify the respective influence of political, economic, and environmental considerations on the discontinuation of the SST program shortly after his testimony, Singer never assuaged his concern that environmental claims played a larger role than he would have hoped.[60]

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[59] Press Release for Singer Symposium, 8 January 1981, Box 36, S. Fred Singer Papers
[60] Singer, S. Fred., "Ozone Scare Generates Much Heat, Little Light," *Wall Street Journal* (16 April 1987): p. 30

CHAPTER 4

'Cautiousness of a Scientist and Administrative Official' -- Frank Press and the Development of the National Climate Program Act, 1975-1978

> "In reality, the grand solution is not broad but rather a kind of tunnel vision representing a stress on rationality, solvability, and the power of science combined with a disinterest in and lack of understanding of organizational processes and a distaste for politics."-- Walter Williams (1998, p. 126)

Since the 1930s, climatologist Helmut Landsberg sought to exemplify what he considered the ideal values of professional scientists -- caution when interpreting evidence given existing uncertainties, public reticence, and using one's cautionary stance to ultimately protect the integrity of the scientific community during a heightened period of anxiety and confusion over the future. This chapter examines Frank Press, a prominent but younger geophysicist and science advisor to President Jimmy Carter between 1977 and 1980, and how he exemplified these values during the development of the first national climate legislation in American history -- the National Climate Program Act of 1978 (NCPA).

The purpose of the NCPA -- other than funding additional research -- was to provide a legal mechanism that would allow the federal government to provide funds to develop regional and state climate centers to work with local farmers and interest groups. Climatologists, it was hoped, could tailor their climate research to the specific needs of the states and regions, and therefore allow farmers to more accurately forecast the future effects of climate on their specific crops. In effect, climate research would be driven by local and state needs and therefore provide a direct link between agricultural production and the climate science community. This was deemed a practical solution to the vagaries of climate by many members in Congress, and had ample bi-partisan support. The passing of such legislation would allow states and local regions to more quickly adapt to changing climatic circumstances, and allow a more flexible response to changing agricultural needs. However, Press along with many others within the Carter Administration advocated a research-only climate policy that separated short-term applications of science from the long-term growth of scientific knowledge. Not only did many prominent members of the executive cast suspicion on the need for urgency given existing scientific uncertainties, many -- including Press -- advocated a hands-off approach for the president.

This chapter explores what appears on the surface to be an odd state of affairs: a self-proclaimed liberal and environmentally-conscious administration that appeared more interested in federal sponsorship of basic research than in assisting farmers and local constituencies deal with the perceived effects of climate change.[1] While everyone believed that climate should be incorporated into long-term national planning, Press and others believed that the problem was not urgent enough to justify greenhouse-emission reduction policies or policies that could ultimately harm the credibility of both scientists and the federal government. Understandably, given what appears to be strong bi-partisan support for the NCPA, many members -- especially in the Senate -- grew very concerned by the administration's reluctance to support their interests. Climate, it appeared, illustrated the President's tendency to sabotage his own constituencies for the sake of what some saw as an over-idealistic commitment to eliminating pork-barrel spending and the power of lobbyists and interest groups.[2]

From Press and the administrations' standpoint, however, the quality of scientific research would be undermined if states could carry out their own research agendas tailored to the specific

[1] For his attempts to convey his populist sympathies, *see Domin, Gregory. Jimmy Carter, Public Opinion, and the Search for Values, 1977-81* (Macon, GA: Mercer University Press, 2003)
[2] Frisch, Scott and Sean Kelly. *Jimmy carter and the Water Wars: Presidential Influence and the Politics of Pork* (Amherst: Cambria Press, 2008)

needs of interest groups and special interests. Accordingly, it appeared to Press that basic scientific research was being supplanted by what he saw as overly hasty policies that could ultimately harm the integrity of the scientific establishment. Maintaining federal control over climate research and focusing on advancing what he believed to be genuine understanding of the global climate system was vital. For most members of the administration, a centralized attempt to understand the climate was more efficient than a more distributed network of climate research centers within individual states and regions.

By focusing on Press's role in these deliberations, historians are provided a unique perspective on how he sought to pave what he believed to be a kind of middle ground between the needs of politics and the needs of science. Contrary to the Senate acting as a "brake upon hasty legislation," I argue in this chapter that Press's support of a research-only climate policy illustrates the opposite conclusion -- the Senate, according to him, was intent on passing hasty legislation that focused too earnestly on citizens' needs.[3] For him, he greatly valued what he considered to be a cautious, research-only agenda because it represented to him a balance between panic and complacency. Research at the federal level appeared to represent both the administrations interest in a political solution to the problem of climate change, and seemed perfectly reasonable given what Press saw as serious scientific uncertainties about the future state of climate. Funding additional research appeared to be a cautious and reasonable policy objective that allowed him to both serve the president while adhering to the kinds of values he considered the mark of a professional scientist.

To be clear, Press's commitment to basic research did not mean that he considered climate a benign issue. Ample evidence suggests that he was greatly concerned with the climate,

[3] Chester, Giraud, "Contemporary Senate Debate: A Case Study," *Quarterly Journal of Speech* 31 (1945): pp. 407-411

occasionally advising President Carter that the climate could yield rapid and catastrophic results if not properly monitored and understood. Indeed, rather than seeing Press's commitment to research as a way to further inflame doubt and skepticism about the urgency of climate as a serious problem, one may perceive it as the mark of what he considered to be a good scientist. Scientific advisors, he believed, pushed for scientific programs and further investments in scientific research. He was cautiously optimistic about the role of the federal government in solving national problems, but he was not naive about the need for long-term planning that could reduce risks to future dangers. The national government for him was a tool to minimize but not entirely eliminate threat-levels, and scientists had an important role in using that tool for maximum gain. If, he believed, hasty policies were derived on what he saw as unsound and premature science then society would not benefit very much from individuals such as himself. Ultimately, this chapter is about the inner-workings of power-politics during a heightened period of anxiety about climate change, and how Frank Press -- as one of the most esteemed geophysicists of the post-World War II period -- sought to moderate how much politics influenced what he considered the research agenda of professional scientists. This reluctance by the administration was a hard-sell, especially for those senators who appeared most committed to establishing a national climate program.

THE SENATE RESPONDS

Within days of the swearing in of the 95th Congress in January 1977, a small cohort of Republican senators from southwestern states -- James Pearson of Kansas, Barry Goldwater of Arizona, and Harrison Schmitt of New Mexico -- introduced a bill responding to what they believed were the economic consequences of climatic change.[4] They titled it the National Climate Program Act (NCPA) -- S. 421. By doing so, they joined their House counterparts from the 94th Congress -- Philip Hayes (D-IN), Lawrence Winn (R-KS), Charles Mosher (D-OH), George Brown, Jr. (D-CA) -- who introduced three climate bills between October 1975 and January 1977.[5] Uniform through each of the House and Senate climate bills was an awareness that the effects of climatic variation could have far-reaching consequences to the national economy, security, and well-being. Suffice it to say, climate was both a national and bipartisan issue of concern by the mid to late 1970s.[6]

Given the particular susceptibility of southwestern states to drought, and eastern states to the severe energy shortages during the 1976/77 winter, Pearson, Goldwater, and Schmitt hoped that if climate could be predicted with sufficient reliability and accuracy, then these and other states could protect against future damages. Their hope rested on a belief that current research for providing seasonal and monthly forecasts was scientifically promising, but insufficient given the lack of a coordinated incorporation of climate forecasts into national planning. A national coordinated program, they argued, was a necessary step given the hazards of climate variability. "Climate legislation," Pearson argued from the senate floor on January 25, 1977, "may provide a much needed step to provide us with the knowledge necessary to minimize the potentially high

[4] The Senate would pass similar bills over the course of 1977: S. 1652 was introduced on June 9th, S. 1980 was introduced on August 2, and S. 2092 was introduced on September 14th. Only S. 421 and S. 1652 would receive hearings, while the latter two did not.

[5] In October 1975, Rep. Philip Hayes of Indiana first introduced bill designed to enact a National Climate Act. See, Extensions of Remarks - Friday, October 3, 1975," *Congressional Record* 121 (October 3, 1975): 31725. The House introduced follow-up climate bills -- H.R. 13736 and H.R. 783 -- on May 12, 1976 and January 4, 1977, respectively.

[6] While it may seem strange from the perspective of current debates over climate, Republican congressmen were very much interested in passing legislation to curb the negative effects of climate and believed that climate change was a reality. Barry Goldwater, especially, serves an important tracer to understand the relationship between conservative ideology and environmental politics. For an account of his environmental values and perspectives, see Brian Drake's chapter, "Green Goldwater: Barry Goldwater, Federal Environmentalism, and the Transformation of Modern Conservation," within Shermer, Elizabeth, ed. *Barry Goldwater and the Remaking of the American Political Landscape* (Tucson: University of Arizona Press, 2013): pp. 214-237

costs associated with climatic variables."[7] The goal was simple in concept: to take immediate action to implement a comprehensive national climate program that would allow greater understanding of climate variability, allow greater coordination between the states and the federal government, and help prepare the nation to respond more effectively to the perceived hazards of climate.

Some senators' interest in passing climate legislation revealed a strange blend of optimism and historical amnesia. Since the 1940s, many Americans optimistically believed that technology had finally evolved to a point that agricultural production could be effectively insulated from the harsh effects of climatic fluctuations. Even after the great nadir in agricultural production -- the Dust Bowl -- during the 1930s, American society continued to invest great hope in the belief that modern farming techniques could ultimately protect against the vagaries of weather and climate, and continue to increase agricultural yield.[8] What the 1970s showed, however, was that nature was hardly tamed -- and had never been. Producing enough water was for agricultural needs had been a particularly difficult problem, and the perceived changes in climate during the 1970s seemed to exacerbate the challenges of dealing with drought.[9] To be sure, their attention to climate appeared reasonable given what many believed to be signs of significant climate instability.

During the 1970s, many believed that agricultural and energy sectors within the global economic system provided the first signs of what appeared to be climate-induced stress on human welfare. In 1974, for instance, the American Central Intelligence Agency (CIA) drafted an internal report entitled "A Study of Climatological Research as it Pertains to Intelligence." A

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[7] Pearson, James B. S. 421 -- National Climate Program Act. Remarks in the Senate. *Congressional Record* [daily ed.] v. 123, Jan. 25, 1977: S1350-13514

[8] Worster, Donald. *Dust Bowl: The Southern Plains in the 1930s* (Oxford: Oxford University Press, 2004)

[9] Matthai, Howard. *Hydrologic and Human Aspects of the 1976-77 Drought* (Washington, D.C.: U.S. Government Printing Office, 1979)

general accounting of weather-related crises around the world since the late 1960s, its authors believed that climate had become a "critical factor" in understanding and predicting the future instabilities of nations. As evidence that the threat of climate had finally spread from the northern African countries -- those "backwashes of the world where death through starvation and disease were already common occurrence" -- the report noted a consensus among the world's leading climatologists that a "detrimental global climatic change" was responsible for "significant perturbations" within major developed nations around the world. Based largely on the research coming out of the University of Wisconsin Center for Climatic Research, and particularly the claims of climatologist Reid Bryson, climate change had apparently arrived on the doorstep of America, and that the US was becoming just as susceptible to conditions already observed in less developed countries in the Sahel region of Africa.[10]

Many supposed that the threat of a global cooling on food production around the world necessitated more concerted efforts to boost long-term food storage. Jerald Ciekot, Director of the World Hunger Project of the American Friends Service Committee in New York City, reflected on the CIA report and suggested that the "gloomy study" may have "stretched the realm of possibility" about the future consequences of climate change but believed that the report's tone provided justified precautionary measures. Against the backdrop of the internationally publicized 1974 World Food Conference in Rome, Ciekot suggested that time was of the essence for the United States to secure adequate global food reserves in light of the future risks of climate change. Given the unlikely continuation of what he interpreted to be the beneficial and stable weather of the period between the 1920s and 60s, Ciekot concluded:

> The world's food supply is in precarious balance with demand, and the odds are that it will not change for the better without deliberate steps to generate food security. We

[10] *Potential Implications of Trends in World Population, Food Production, and Climate* (Central Intelligence Agency Report, OPR-401, August 1974)

> would be remiss -- foolish, actually -- not to begin protecting ourselves when we have the capacity to do so.[11]

Ciekot's message was clear: while the CIA report's evaluation of future calamity may have pushed the boundaries of reason, that did not preclude the need for developed nations -- and particularly the United States -- to take necessary precautions and initiate programs designed to understand and respond to climatic fluctuations.

For many in agriculture, it seemed that technology -- for all its benefits in feeding the general populations of the world -- may have made America's sensitivity to climatic fluctuations more hazardous. Paul Weller, Vice President of Public Affairs for the National Council of Farmer Cooperatives, testified in 1976,

> The increased use of fertilizer, other chemicals, high potential genetic stocks, and mechanization has produced management systems requiring more sophisticated strategies for operating the farm. These management styles are infinitely more sensitive to weather events and climate change than the simpler schemes of earlier times or other areas of the world. . . The force of unfavorable weather conditions has not only neutralized the ongoing flow of technological progress, but has in fact, caused world food reserves to slip to undesirably low levels.[12]

The four climate bills introduced in Congress between October 1975 and January 1977 were a consequence of the complacency and inattentiveness to the realities of climatic fluctuations that pervaded American society -- both in terms of energy and agriculture. The system was quite simply not up to the task of dealing with what many perceived to be the hazards of weather and climate variation. Reassuringly, this interest in climate came during a time when climate research had effectively come of age. With the rise of satellites, global remote sensing networks, and computer-based modeling in the 1960s, there seemed to be no

[11] Ciekot, Jerald, "The Dark Shadow of World Food Shortages," *Los Angeles Times*, 23 June 1976, D7
[12] Testimony of Paul Weller, Hearing before Subcommittee on the Environment and the Atmosphere, 25 May 1976, p. 200

greater opportunity to start applying current climate knowledge and integrate climate into the management and planning efforts of the United States.[13]

THE SCIENTIFIC COMMUNITY RESPONDS

Shortly after S. 421 was introduced in the Senate, the National Research Council (NRC) sought to exploit growing congressional interest in climate. Noting a "rapidly growing concern" regarding the possible effects of climate change, the Climate Research Board (CRB) was envisioned as a "consistent and meaningful channel for advice to the government" and "effective interface" for climate-research activities within the National Academy of Sciences.[14] To garner approval by the White House, Philip Handler, the President of the National Academy of Sciences (NAS), sent Frank Press, science advisor to President Carter, a copy of the CRB proposal. Envisioned as an "important nexus" between government and the scientific community, Press reassuringly expressed his agreement with Handler that the effects of climate change was of "sufficient importance and uncertainty" to expand NAS activity in the area of climate, and maintain a "fruitful interchange" between federal agencies and the academy.[15] This was a critical moment. Bipartisan interest, executive approval, and scientific curiosity fueled a dramatic rise of interest in climate during the Carter Administration, and from then on Press was an important but heretofore unrecognized figure in the development of climate policy.[16]

Others also took interest in Pearson and Schmitt's interests in climate. On March 17th, stirred by S. 421, Steven Flajser, a staff member working on behalf of the Senate Committee on Commerce, Science, and Transportation, sent to chairman Adlai Stevenson III (D-IL) a proposal

[13] Edwards, Paul. *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge: MIT Press, 2010)

[14] Proposal for the Development of a National Climate Board, 5 March 1977, 1977 Climate Research Board – General, National Academy of Sciences Archive, Washington, D.C.

[15] Philip Handler to Frank Press, 5 May 1977, 1977 Climate Research Board – General, National Academy of Sciences Archive, Washington, D.C.; Frank Press to Philip Handler, 31 May 1977, 1977 Climate Research Board – General, National Academy of Sciences Archive, Washington, D.C.

[16] Where Press is considered in histories of climate policy and science, he is usually a behind-the-scenes instigator -- not an actual participant -- in the history of climate policy and science.

laying out what he believed to be a growing need for climate legislation. Noting the uncertainty of whether "we are moving into a period of global cooling, or global heating as a result of a green-house effect," he suggested to Stevenson the need to hold senate hearings to understand and assess a wide range of opinions regarding the influence of climate on society.[17] Those invited to testify included representatives of federal agencies, environmental groups, labor organizations, business and the financial community, and professional/technical societies. While the motivation existed to study and assess the climate's impact on society, there were significant roadblocks to passing legislation that could meet the needs of everyone involved -- including Press himself, as both a scientist and administrative official.

Handler was not only supportive of Press's efforts to understand the climate, he was also believed in the importance of preventing "prophets of doom" from influencing national science. On June 17, 1977, Estella Leopold, Director of the Quaternary Research Center with the University of Washington, suggested to both Handler and Robert White (recently appointed Chairman of the National Research Council Climate Research Board) that they include climate scientist Reid Bryson on the Climate Research Board. As the Director of the Institute for Environmental Studies at the University of Wisconsin-Madison, Leopold described Bryson's "uniqueness" as a person, his "real effort" to integrate climatic studies with other scientific fields, and finally his role in bringing popular attention to climate. As she reasoned,

> Even though there are scientists in the meteorology community who find it difficult to accept every single statement that Bryson makes, I think none of us could question the fact that he has been a key person in inspiring the type of work which this Climate Research Board intends to carry out.[18]

[17] Steven Flajser and John Stewart to Adlai Stevenson, 17 March 1977, RG 46 - Box 13, National Center for Legislative Archives, Washington, D.C.
[18] Estella Leopold to Robert White and Philip Handler, 17 June 1977, 1977 Climate Research Board – General, National Academy of Sciences Archive, Washington, D.C.

Given Leopold's prominence in botany – she was elected to the National Academy of Sciences only a few years earlier in 1974 – her primary motivation extended beyond the mere inclusion of Bryson's opinion regarding climate-related matters. Her concern for the conservation of wildlife and the environment, which won her the 1969 Conservationist-of-the-Year-Award by the Colorado Wildlife Federation, suggest a deeper belief that *outspoken* scientists should be represented within the heart of organized science. Bryson appeared to be someone who could advance an environmentalist cause via what many believed to be very public warnings of climatic catastrophe.

Even so, Leopold's embrace of Bryson's charisma fell flat given another value-system deeply represented within the National Academy of Sciences -- the value of organized skepticism and reticence. Approximately two weeks after receiving Leopold's letter, Handler explicitly expressed his reluctance. Reminiscent of his intolerance of Lamont Cole's public statements in the late 1970s, and his subsequent decision to prevent his inclusion within the NAS, Handler appeared resolved to keep "alarmists" out of the scientific establishment. As he responded,

> I could be happier with your suggestion if Dr. Bryson had confined his persuasive efforts to the scientific community rather than prematurely alarming the public, and if his projections of the future had paid some attention to the climatic consequences of the rapid increase in combustion of fossil fuels. Meanwhile, many thanks for your suggestions.[19]

Two points deserve emphasis. First, Handler cast Bryson as one who not only "prematurely" engaged the public, but he did it in an alarming way. Second, Handler attacked what he perceived to be Bryson's exclusion of carbon dioxide from his projections of the future climatic change. By excluding what many within the atmospheric science community to be

[19] Philip Handler to Estella Leopold, 3 July 1977, 1977 Climate Research Board – General, National Academy of Sciences Archive, Washington, D.C.

integral to the effects of fossil fuels on climate, Bryson appeared to be incomplete in his analysis and too myopic on the cooling effects of atmospheric dust.[20] This was an important indicator of Handler's role, and the practice of "othering" figures like Bryson who appeared not to abide by a code of proper conduct within the scientific community. However, Handler was not the only one to fight battles for the sake of maintaining what others also believed to be the importance of monitoring and regulating how science was communicated and understood by policy makers and the general public.

FRANK PRESS STEPS ON STAGE

Up to the time of his nomination as science advisor in the early spring of 1977, Press was considered an unlikely candidate. Initially labeled by *Science* magazine a "long-shot candidate" and "dark horse" Press was not a particularly visible person within Washington circles. Most considered him a so-called "scientist's scientist" better known for his research on solid earth geophysics rather than political accomplishments.[21] Indeed, up to the time of his nomination he had little contact with the Carter campaign. Nevertheless, his credentials and contacts were impressive. Press was the Chairman of the Earth and Planetary Sciences Department at the Massachusetts Institute of Technology, and well-respected within the scientific community for his work on earthquake prediction and role on nuclear-proliferation issues; he was heavily involved with international negotiations concerning nuclear testing during the 1960s and served as a science consultant for multiple federal agencies throughout his professional life. In addition, according to a *Science* magazine survey, many prominent players in Washington expressed their

[20] While Bryson's work was not exclusively regarding the cooling effects of atmospheric dust, he became quite visible within the general public regarding the potential onset of an ice age. Bryson, Reid, "A Perspective on Climatic Change," *Science* 184, 4138 (17 May 1974): pp. 753-760; Bryson and Thomas Murray. *Climates of Hunger: Mankind and the World's Changing Weather* (Madison: University of Wisconsin Press, 1979); Bryson, Reid, "The World is Turning Colder," *The Sun*, 16 January 1977, p. K3
[21] According to *Science* magazine, Jerome Wiesner, Wolfgang Panofsky, and Lewis Branscomb were considered likely candidates. See, Wade, Nicholas, "Search for a Science Advisor: The Names on the List," *Science* 195, 4273 (January 7, 1977): pp. 31-33. For an account based on this article, refer to Herken, Gregg. *Cardinal Choices* (Stanford, CA: Stanford University Press, 2000), pp. 184-186

admiration and support for Press including former presidential science advisor Jerome Wiesner.[22] He seemed a natural choice.

For Carter, Press offered what previous science advisers apparently had not: an environmental perspective and the fact that he was *not* a physicist.[23] After a private meeting with Press on February 9, 1977, Carter noted in his diary: "In the past most of them have been physicists – in fact, the first six recommendations that I got were for physics majors – but I wanted to get an earth science professor to help me in a more general way to assess some of the questions raised by the first report of the Club of Rome."[24] Carter's reference to the Club of Rome was not without significance. Widely recognized as a seminal publication of anti-growth literature of the 1970s, the Club of Rome's first report -- *The Limits to Growth* (1972) -- was translated into dozens of languages and spurred widespread discussion about the future consequences of industrial and population growth. Indeed, when Carter spoke about his interest in the Club of Rome's report, he was doing so from a vantage point of sincere concern and curiosity about Press's capacity to advise him on important global environmental issues.[25]

Press's greatest asset appeared to contribute to his apolitical reputation and impartial image. As he recounted later,

> Although there is something to be said for a prior political and personal relationship between a President and his science advisor, I have found it advantageous to be viewed primarily as a professional rather than political appointee . . . There are political

[22] "Frank Press, Long-Shot Candidate, May become Science Advisor," *Science* 195, 4280 (February 25, 1977): pp. 763 +765-766. Little secondary sources outside of formal interviews of his life and career exists. An excellent start would be to visit the following webpage: http://www.aip.org/history/acap/biographies/bio.jsp?pressf.
[23] Carter's claim that science advisors tended to be physicists appears inconsistent with history. Based on a brief tabulation (by no means systematic) of the eleven science advisors between 1939 and 1981, three were physicists, four were engineers, two were chemists, one was geophysics (Press), and one (James Killian) was considered more of an administrator than a scientist.
[24] Carter, Jimmy. *White House Diary: Jimmy Carter* (New York: Farrar, Straus and Giroux, 2010): p. 18.
[25] Meadows, Donella and Dennis Meadows. *The Limits to Growth: A Report for the Club of Rome's Project on the Predicament of Mankind* (Madison, WI: Universe Books, 1972); Meadows, Donella and Dennis Meadows. *Limits to Growth: The 30-Year Update* (White River Junction, VT: Chelsea Green Publishing Company, 2004). For a broad historical account, see Neurath, Paul. *From Malthus to the Club of Rome and Back: Problems of Limits to Growth, Population Control, and Migrations* (New York: M.E. Sharpe, 1994)

> differences even within the White House staff and the Cabinet, and the credibility of my advice was enhanced by the apolitical and impartial image of OSTP.[26]

According to the contemporary work of journalist Marcus Walter Williams, Press's self-evaluation may not have been far off the mark. Within the administration, he was believed to be a conscientious advisor and unwilling to be a political lapdog for Carter. Based on in-depth interviews with Carter's Domestic Policy Staff (DPS), he was well-respected for being balanced and restrained. Press had the reputation of never going beyond his expertise, and Carter's closest advisors appreciated that he was willing to express his opinion even if that opinion departed from the views of others.

Seldom did he believe that his access was limited, and in fact he was given more access than most. "I feel that I had access. I felt that I could write directly to him and it wasn't shunted aside by a staff person."[27] That access was fundamental to Press, both as Carter's advisor and to cabinet members.

> I think my relations with the political elements in the administration evolved over time. I think, from what they tell me now, they found me to be a very credible source of advice; we have all our biases, but that's life. . . I think they came to evaluate me as a fair person as straight, as open, letting people know exactly how I felt about these things, but also they saw the President's response to some of the things that I have done. . . I found a very positive response and I felt that I was used adequately and well in the best sense of the word.[28]

After many months of informally advising Carter during the early months of his transition into the presidency, he formally became his science adviser -- and Director of the Office of Science and Technology Policy -- in May 1977.

[26] Press, Frank, "Science and Technology in the White House, 1977 to 1980: Part I," *Science* 211 (January 9, 1981): pp. 139-145, quote on p. 139.
[27] Greenberg, Daniel, "Interview: Frank Press," *Omni* (June 1979): p. 70
[28] Greenberg (1979) Interview

Upon Carter's invitation to become his science and technology adviser, Press submitted his views on what the position would entail. First and foremost, Press argued, his role was considered a "personal assistant" to the President. This meant, of course, that he would work closely with other senior advisers -- such as the Director of the Office of Management and Budget, as well as those involved in economic, domestic, and national security affairs -- on matters that involve scientific and technological considerations. Press also outlined that he would operate with a small staff, and work with experts in universities, industry, and government agencies at his discretion.

One of the central considerations was Press's interest in establishing the proper scope of his role within the administration. While he would personally have a great deal of access to Carter, that did not speak to the size and scope of his staff. How large would his staff need to be in order to be effective? This was not a small question, and one that would reverberate through his tenure as science adviser. With no answer immediately in mind, Press offered two suggestions to Carter. First, he believed that a staff size of thirty two could provide the necessary amount of professionals given the science adviser's many other functions, including Chair of the Federal Coordinating Council for Science, Engineering, and Technology (FCCSET), Chair of the Intergovernmental Science, Engineering and Technology Advisory Panel (ISETA). There was another option, however. Should the President decide to transfer some of the responsibilities to the National Science Foundation and other federal scientific bodies, then the staff number could be significantly reduced. This had the advantage of appealing to Carter's desire to reorganize the federal government to be more efficient, but it may mean that "a fewer number of legislatively mandated functions" could be adequately performed.[29]

The staffing issue was also on the minds of Carter's cabinet. With Carter's interest in reorganizing the Executive Office, OMB Director Bert Lance advocated for a "scaled down" staff of "no more than 10 to 12 permanent professionals."[30] This had drawbacks, however. While a reduction of professional staff could "concentrate more completely on the advisory activities for the President," Al Stern, a member of Carter's Domestic Policy Staff, felt that that the Executive Office may not be responsive to congressional interests. In addition, it could mean a "loss of consumer/citizen perspective on science policy" and a priority on "producer/research oriented" concerns.[31] While he perhaps failed to see how this would affect the pending climate legislation, this focus on "research oriented" concerns and a loss of "consumer/citizen perspective" contributed to the administration's growing apprehension toward the NCPA.

Despite these potential shortcomings, Stu Eizenstat, Chief Domestic Policy Adviser, believed that the treatment of Nixon and Ford toward the scientific community "downgraded" the science advisory role within the Executive Office, and that the reintroduction of the position could "gain the influential support of the scientific community." This was a primary agenda point, and clearly Press's nomination represented a step forward in a rebuilding process. Science was to have a part in national decision making, and Carter knew that Press had ample expertise and credibility within the scientific community.[32]

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[29] Press to Jimmy Carter, 10 February 1977, JC-1056 -- Box 7 -- 2-14-77 (1), Jimmy Carter Library and Archive, Atlanta, GA (hereafter referred to JCL)
[30] Lance to President Carter, 21 February 1977, JC-1056 -- Box 9, JCL
[31] Al Stern to Eizenstat, 16 February 1977, JC-1056 -- Box 9 -- 2-24-77, JCL
[32] Eizenstat to President Carter, 21 February 1977, JC-1056 -- Box 9, JCL

Ultimately, two sources suggest that Lance's vision for a "scaled down" OSTP office was realized. First, Phil Smith, OSTP Associate Director under Press, confirms that Carter wanted to trim the number of committees and individuals within the Executive Office. This meant that Press -- in accordance with his objective to meet Carter's needs -- outsourced many of the tasks of the OSTP to outside agencies and organizations, such as the National Academy of Sciences.[33] Second, according to a 1980 Report by the Comptroller General of the United States, the OSTP consisted of between fifteen to eighteen professional staff members split between three major groups: (1) national security and international and space affairs (led by Benjamin Huberman); (2) human resources and social and economic services (led by Denis Prager); (3) natural resources and commercial services (led by Philip Smith).[34] As will be seen shortly, this reduction in staff -- and the fear that it could privilege the OSTP's advisory function to the president relative to congressional interests -- had a serious effect on Press's (and the administration's) response to the National Climate Program Act.

CLIMATE POLICY AND A PRAGMATIC ETHIC

To sort out the dimensions of the pending climate legislation, the Senate convened a series of hearings in the summer of 1977. To understand the institutional apparatus that would be responsible for enacting the climate legislation, Frank Press and Edward Epstein, the Associate Administrator of the National Oceanic Administration (NOAA), was asked to testify. Their testimony was important because they were both scientists, and they were both responsible for creating a national climate program that could effectively benefit the country. Part of their

[33] Proceedings of the White House Office of Science and Technology Policy, 25th Anniversary Symposium, Cambridge, MA, 1 May 2001. A PDF of the symposium is available online.
[34] Report by the Comptroller General of the United States, "The Office of Science and Technology Policy: Adaption to a President's Operating Style May Conflict with Congressionally Mandated Assignments." (Washington, D.C.: U.S. Government Printing Office, 3 September 1980)

job was to regulate expectations, and avoid undermining their scientific credentials by overpromising.

One of the first steps was to properly address the uncertainties of climate forecasting. This, it was believed, would showcase how little was truly known about the future climate, and establish outright the limits of science in designing a policy to help citizens dealing with the perceived effects of climate. Epstein, for instance, testified that the best knowledge available could only provide climate forecasts ten percent more reliable than pure chance. While Epstein expressed optimism that proper federal investment could lead to better knowledge of whether more accurate predictions were possible, he noted his reservations: "I think we have made some important strides," Epstein argued, "but I just cannot say with certainty that we are going to achieve some particular level of predictive capability at all because I just don't know what is really possible. I think we tend to overestimate our short-term progress as a rule and underestimate it in the long run."[35] Senator Harrison Schmitt seemed taken aback by Epstein's apparent caution. "I am a little bit surprised about your pessimism as to what may be possible," Schmitt began.

> I have dealt with the effects of climate as a geologist. But my impression over the last few years has been that there is some degree of optimism in the community of climatologists and meteorologists and other related scientists that this system of Earth, weather, and climate that we have can, in fact, be modeled and that we have a tremendous potential for long-term climate predictions. . . I guess I find myself a little bit disturbed that you as a meteorologist and as an official of NOAA come across as pessimistic in this area.[36]

For Schmitt, the lack of confidence expressed by Epstein was hardly reassuring, and perhaps overly cautious given his own expectations as a geologist. For Epstein, however, maintaining his credibility as a scientist and as a witness meant that he could not be overly

[35] "Testimony of Edward Epstein," 9 June 1977, pp. 106-121
[36] Testimony of Edward Epstein, 9 June 1977, p. 114

optimistic about applying climatic knowledge for the benefit of southern states and the nation, in general. Too much optimism may have made Schmitt less concerned, but Epstein felt that it would have potentially undermined the integrity of climate science.

Epstein was not alone in his reservations. Given Press's scientific background and prominent advisory role, Stevenson also requested his testimony on behalf of the administration. Press's testimony boiled down to three components -- the practical applicability of probabilistic forecasting, the need for further research, and the needs of the general public. While Epstein cautioned against over-optimistic assessments of the accuracy of climate predictions, Press went a step further by asking whether probabilistic forecasts could be applied for practical short-term benefits at all. This, after all, was the bottom line question. Based on information gathered during a prior informal convening of a dozen research scientists, each of whom were conducting research at the "forward edges" of the climate problem, Press conveyed what he learned to Schmitt. "No one was very optimistic about what we can do now for the short term. . . Currently, there is some slight skill in 90-day temperature forecasts and in 30-day precipitation forecasts, but seasonal and yearly forecasts are presently beyond the state of practical art." As he continued,

> There is a very real problem in using research findings, which are in this field apt to be in probabilistic in nature, in a practical way. If you were to ask whether probabilistic predictions of relatively low confidence can be used, the answer is yes and no. It depends on what it would be used for. Such predictions are useful in policy planning and analysis on a broad scale. They may not be at all useful for an individual farmer who has to decide whether to plant, for example, a drought-resistant crop of low yield, or a high-yield crop which needs more moisture. But in policy decisions and planning in which risk factors are normally incorporated, and which can use any information better than chance, such prediction could be helpful.[37]

[37] "Testimony of Frank Press," 9 June 1977, p. 96

This distinction between local farmers and national policymaking was integral to defining the "state of practical art." The incorporation of climate forecasts into policy making was considered practical; the incorporation of climate forecasts into local farming activities was considered impractical. In Press's framing, "the usefulness of predictions depends to a great extent on usefulness to whom. . . Where certainty or near certainty is needed, short-term climate prediction may never be helpful at all."[38]

Press's apparent restraint and caution was in part derived from a more philosophical perspective of the growth of science. In 1970, President Richard Nixon's National Goals Research Staff (established in 1969) released its first report about the need for "balanced" growth. As it articulated,

> To the extent that society insists that basic scientists do work that is more relevant to present social needs . . . scientists will be less able to work where nature appears willing to answer their questions. They may be required to work on relevant questions that perhaps cannot be answered at all at present, or can be answered only with uneconomic use of resources. Thus, excessive efforts to make science more productive in terms of immediate social goals may actually make it far less productive in the long run.[39]

Press and Epstein were not indifferent to the stresses apparently caused by climate change, nor to the plight of local citizens (i.e. farmers). To the contrary, they were very much concerned but were nonetheless committed to a philosophy very much distinct from what appeared to them as hasty legislative efforts within the senate.

Consistent with Carter's overarching goal to formulate policies to create a foundation for the future -- a strategy entitled the New Foundation -- Press believed that further research was

[38] Testimony of Frank Press, p. 94
[39] "Toward Balanced Growth: Quantity with Quality," Report of the National Goals Research Staff (Washington, D.C.: U.S. Government Printing Office, 1970). Also, see Philip Abelson's use of the quote in "Science and Immediate Social Goals," *Science* 169, 3947 (21 August 21, 1970): p. 721

the first step to realizing what all agreed was a noble goal.[40] Press urged a research-only strategy that could -- over the long-term -- potentially benefit both entire nations and local farmers. Press believed that the predictability of climate was under an appropriate level of discussion within the highest levels of government, and that most would have concluded that more research was necessary before any conclusions about its applicability were made. He was very much acting within what appeared to be mainstream, and it struck him as entirely inappropriate for scientists to essentially bypass genuine long-term understanding for short-term application of as-yet underdeveloped knowledge.

Regardless of what senators like Schmitt may have thought about climate prediction, or its practical benefit, scientists like Epstein and Press agreed that overpromising may have risked the integrity of climatologists over the long-term. Believing that the federal government quite simply did not know how to predict climate, let alone apply such knowledge for the welfare of farmers, Press argued that "we must proceed systematically without unduly raising expectations in the matter of such vast importance to millions of people which we may not be able to satisfy. This only partly is the cautiousness of a scientist. It is also the cautiousness of an administration official."[41] This distinction between scientific cautiousness and administrative cautiousness was revealing because it struck to the very core of presidential science advising. For Press, his position as science adviser was not about representing the scientific community as a kind of lobbyist; it was about informing the president about the scientific and technical considerations

[40] For a discussion of Carter's administrative agenda, see Strong, Robert, "Recapturing Leadership: The Carter Administration and the Crisis of Confidence," *Presidential Studies Quarterly* 16, 4 (Fall 1986): pp. 636-650
[41] "Testimony of Frank Press," 9 June 1977, p. 102

involved in national decision making. "First and foremost," Press wrote, "the Science and Technology adviser is a personal assistant to the President."[42]

Press was not alone in questioning the virtue of applying unreliable climate forecasts for the purpose of helping certain constituencies. A year earlier, John Wallace, a professor within the Department of Atmospheric Sciences at the University of Washington, spoke to the bill introduced in October 1975 by Philip Hayes regarding user needs.[43] Like Press, he believed that the expectations game was an important issue to consider in any public deliberations about a national climate program. "I think we want to be careful not to oversell the prospects of predicting year to year or season to season climate fluctuations. We do not yet know whether such predictions are theoretically possible, let alone whether they could be practically feasible."[44] This cautious tone on the part of atmospheric scientists was perfectly reasonable given significant uncertainties of climate prediction, and Press -- a geophysicist -- appeared perfectly in sync.

The cautiousness of Press was not because he was ignorant of the serious matter of climate change, nor callous to the needs of the nation's interests. Having served as the President of the American Geophysical Union from 1974 to 1976, Press was very much aware of the issue. As an esteemed scientist in his own right, he frequently sought to reflect what the scientific community told him about climate, and reinforce the message to Carter himself. As an administrative official, however, he believed that the public deserved what he considered to be

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[42] Press to Carter, 10 February 1977, JC-DPS - Box 273, Science (Advisers, Issues, Concerns), Jimmy Carter Library and Archive

[43] John Wallace received his Ph.D. in 1966, followed by a career as Professor of Atmospheric Science at the University of Washington. During the 1970s, he was affiliated with many of the most important bodies of atmospheric research, including the U.S. Global Atmospheric Research Program (GARP) Climate Dynamics Panel. For additional biographical information, see http://www.atmos.washington.edu/wallace_more.html, last accessed February 20, 2014.

[44] John Wallace to John Kutzbach, 2 March 1976, Papers of the Center for Climatic Research, Box 3-General Files, M-U (1 of 2), University of Wisconsin-Madison, Madison, Wisconsin.

policies that would actually benefit their lives -- not provide what he considered illusions and overblown expectations. As an administrator, his apparent restraint also reflected Carter's broader desire to avoid the complexities of provincial or special interests, and that a research-only strategy was one way to avoid such concerns.[45]

As an administrator, Press had to balance what he believed were two roles, each of which entailed its own threshold of what it meant to be "cautious." Politically, he believed that his primary responsibilities rested with the Administration. Due to a provision within President Carter's National Energy Plan (NEP) -- released in April -- Press aligned himself with the President's goal to primarily fund further research. Under the direction of Secretary of Energy James Schlesinger, the NEP Task Force allotted $3 million to study the long-term effects of carbon dioxide on climate. Furthering research was the agenda because of what were perceived as significant uncertainties within the science, and this certainly merited additional funding. This was not an attempt to minimize the importance of climate to national affairs, but a reasonable attempt to acquire more information before making fundamental changes to the federal government that could have serious long lasting effects. This was a science first-policy.[46]

Press's cautiousness was not unique to the NCPA. In April 1977, a few months before he testified in Congress about the NCPA, he was asked to testify on a congressional bill designed to create a national earthquake preparedness program. Senator Schmitt -- the same senator that

[45] According to journalist Walter Williams, who interviewed at length Carter's Domestic Policy Staff, Carter was "hostile to interest groups or any form of particularism that might block comprehensive solutions." See Williams, Walter. *Honest Numbers and Democracy: Social Policy Analysis in the White House, Congress, and the Federal Agencies* (Washington, D.C.: Georgetown University Press, 1998): p. 125. For an overall look into Carter's governing style, Hargrove, Erwin. *Jimmy Carter as President: Leadership and the Politics of the Public Good* (Baton Rouge, LA: Louisiana State University Press, 1988); Dumbrell, John. *The Carter Presidency: A Re-Evaluation* (Oxford: Manchester University Press, 1995).
[46] In February 1977, President Carter asked his advisor on energy, James Schlesinger, to organize an energy task force to develop what would become his "National Energy Plan." The study of carbon dioxide was one of 113 provisions included in that plan. See Jimmy Carter: "National Energy Program Fact Sheet on the President's Program.," April 20, 1977. Online by Gerhard Peters and John T. Woolley, *The American Presidency Project*. http://www.presidency.ucsb.edu/ws/?pid=7373

would question Epstein and Press in the summer of 1977 -- urged that places like San Francisco could realize immediate benefits from federal spending on earthquake hazard preparedness. "Coming from the world that I came from, that doesn't seem too unreasonable, considering what is possible at least to get started with this project," Schmitt emphasized.[47]

Press perceived this interest in the public welfare as sincere, but hasty given the state of seismological knowledge. During his testimony, he emphasized the desire of the Administration to avoid raising expectations of the American public, and sticking closely to what geoscientists could actually do and know. "I don't think we know enough right now to design and install an operating system of the kind that you suggest," he replied. Based on a range of technical problems that beset accurate predictions of earthquakes, Press was weary of Schmitt's optimism regarding the application of state-of-the-art systems.

> I think that our research system, under certain circumstances, can provide a partial capability to do the things that you want to do. But I would be loath to call it an operating system and get everybody's hopes up too high because I think there will be earthquakes the systems may miss and there may be some false predictions coming out of the system in the beginning, early stages of research.[48]

For Press, what he perceived to be the hasty application of what was currently unreliable knowledge about earthquake prediction may have threatened the credibility of the federal government. For both, the public welfare was a top priority. The difference was how one believed the government could ultimately provide for the public welfare. While Schmitt advocated for the application of knowledge in real-time in spite of apparent uncertainties, Press believed that protecting people from what he believed to be unrealistic and heightened expectations was crucial.

[47] Testimony of Frank Press, 19 April 1977, Hearing before the Senate Subcommittee on Science, Technology, and Space (Washington, D.C.: U.S. Government Printing Office, 1977), p. 28
[48] Testimony of Frank Press, 19 April 1977, pp. 28-29

This did not meant that Press was unconcerned with those who could profit from knowledge provided by the federal government. He placed great importance on ideally bringing together the research community and the user community, or those who had to make the day-to-day decisions with regard to earthquake hazards. Instead, Press's caution was ultimately derived from how he perceived the practice of decision-making in light of very real scientific uncertainties. Bad knowledge can lead to bad decision-making, while good knowledge would yield a greater likelihood of better outcomes. "The wedding of the research and user communities is not an easy task in any field . . . It is a complex problem that requires careful analysis," he noted.[49]

In many ways, Press embodied the very principles of the Carter Administration. According to journalist Walter Williams, Press saw himself as a "neutral scientist, an image quite appealing to Carter . . . Press packaged himself and OSTP as scientists obtaining the facts and rendering nonpartisan, science-based judgments." Those within the President's inner circle saw Press in similar terms. Part of his clean reputation was due to his ability to work within his confined role as science adviser. "Press represents," Williams argued, "the prototypical example of the technically oriented generalist willing to synthesize across policies, given that policies are well within his office's technical competence, but not move generally into the political arena."[50] Given Carter's background and interest in science, both believed in the virtue of science and rationality to solve national problems. The goal was to prevent hasty actions before adequate knowledge had been obtained, while still staying true to what scientists had to say about the issue of climate policy and forecasting.

[49] Testimony of Frank Press, 20 April 1977, Hearing before the House Subcommittee on Science, Research and Technology (Washington, D.C.: U.S. Government Printing Office, 1977): p. 32
[50] Williams, Walter, "The Carter Domestic Policy Staff," *Research in Public Policy Analysis and Management* 3 (1981): pp. 23-66, quotes on pp. 45-47

Press's testimony reflected what had become common knowledge during the 1970s about the value of applying uncertain climate science to policy making. Between 1974 and 1977, multiple federally-funded reports described what many believed to be significant uncertainties of climate forecasts and their minimal practical use in the short-term. One report summarized that: "Advanced knowledge of long-term future changes of climate, of undoubted value to modern society, is not yet available . . . At present, we do not even know enough about the problem of climate predictability to know whether long-term predictions are a realistic possibility."[51] A year prior to Press's testimony, the Congressional Research Service (CRS) published a report entitled, "A Primer on Climatic Variation and Change." Specifically requested to summarize the state of climate science for legislative purposes, the report stated in no uncertain terms: "So far, there is no single comprehensive theory, or even a combination of a small number of theories, that completely explains -- much less predicts -- climate fluctuation or change."[52] A year after his testimony, Patsy Mink, a Carter-appointed Assistant Secretary of State for Oceans and International Environmental and Scientific Affairs, addressed the National Research Council about the challenges of climate policy.

> Without adequate knowledge policy planners are unable to see how best to allocate and distribute available funds, whether for research and development, or for assistance . . . the general scientific opinion is that our understanding of climate and climate variability is far too meager to warrant serious pronouncement. . . Both the data and the physical models are uncertain.[53]

And that was the very point: how to distribute federal resources was difficult. In spite of Carter's public declaration to commit $3 million for climate research, some members of Congress wanted

[51] "Report of The Ad Hoc Panel on the Present Interglacial" (Washington, D.C.: Federal Council for Science and Technology, 1974), iii.
[52] "A Primer on Climatic Variation and Change," Prepared for the Subcommittee on the Environment and the Atmosphere of the Committee on Science and Technology, U.S. House of Representatives, 94th Congress, Congressional Research Service (Washington, D.C.: U.S. Government Printing Office, 1976)
[53] Patsy Mink Address to the Climate Research Board, 31 March 1978, 1978 Climate Research Board -- General, National Academy of Sciences Archive, Washington, D.C.

to go beyond basic research -- they wanted application and service to the people of the United States. Carter chose research; figures like Schmitt wanted assistance. Press's cautiousness as an administrative official and scientist favored the former approach.

One may very well ask: was Press's cautiousness a reflection of his skepticism that climate was even a threat? The answer is no. On July 7, 1977, he wrote a memo to President Carter entitled, “Release of Fossil CO2 and the Possibility of a Catastrophic Climate Change.” Noting what he believed to be the growing weight of scientific support within the "authoritative" National Academy of Sciences, Press advised that the potentially "rapid" and "catastrophic" effects of carbon dioxide had evolved from speculation to a "serious hypothesis worthy of a response that is neither complacent nor panicky."[54] What Press had in mind when he advocated a research-only policy was not just because he wanted to represent both the interests of both the scientific community and the President; he was also seeking a middle ground policy that was neither complacent nor panicky.

In spite of his own acceptance that carbon dioxide was a potential threat, he was not an advocate for taking drastic measures to limit carbon dioxide emissions: "The present stage of knowledge does not justify emergency action to limit the consumption of fossil fuels in the near term," he noted. The appropriate response was instead to cautiously "exploit nuclear energy more fully" while simultaneously promoting basic research in solar electric, biomass conversation and other renewable energy sources. The goal of this strategy was to take into account the "potential CO2 hazard" in developing a long-term energy strategy, and -- as his

[54] Frank Press to President Carter, "Release of Fossil CO2 and the Possibility of a Catastrophic Climate Change," 7 July 1977, JC-1006 -- Memos and Correspondence, 5/25/77-8/3/77, Jimmy Carter Library and Archive, Atlanta, GA. In his memo to Carter, Press references a soon-to-be released statement on the potential effects of carbon dioxide; however, it is not clear whether this was due to the 1977 Energy and Climate Report published by the NAS Geophysics Research Board.

testimony about the National Climate Program Act -- he did not favor hasty political actions by the Congress or the President.[55]

As an administrator, Press believed in the value of working well with Carter's closest advisers. One of them was OMB Director, Bert Lance. "Among all of Carter's White House policy advisers, the one man who seems to wield influence greater than that of all the others is Bert Lance, whose role might accurately be described as confidential adviser to the President," stated Carter scholar, Joel Havemann.[56] Lance's position carried weight on important policy matters given Carter's overall attempt to reduce the complexity of federal bureaucracy -- a task that would require a firm budgetary hand.[57] As such, he was uninterested in climate legislation that would expand the federal bureaucracy. Three days before Press's testimony, Lance shared his thoughts with George Brown, Jr., Chair of the House Subcommittee on Environment and Atmosphere and sponsor of four climate bills in the House.[58] Lance assured Brown that the Administration shared his belief in the need for a climate program that could "provide this nation with necessary, useful and credible climate information in the most cost effective manner." However, Lance cautioned against the need for "additional legislative authorities" given what many reports and scientists were saying.[59]

Many others also voiced their skepticism about the need for political involvement in climate policy, whether it be for the NCPA or for carbon mitigation. In lieu of Press's July 7th memo to Carter regarding the potentially catastrophic consequences of climate change, Secretary

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[55] Press to President Carter, 7 July 1977

[56] Williams, Walter. *Honest Numbers and Democracy: Social Policy Analysis in the White House, Congress, and the Federal Agencies* (Washington, D.C.: Georgetown University Press, 1998): p. 126. Also, Havemann, Joel [need citation here]

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[58] For reference purposes, all four House bills -- HR 783, HR 3399, HR 4468, and HR 5722 -- were introduced between January and March of 1977.

[59] Bert Lance to George Brown, Jr., 6 June 1977, RG 233, Box 4 -- HR 6669, National Center for Legislative Archives, Washington, D.C. Lance specifically cited two reports, *Understanding Climatic Change: A Program for Action* (National Academy of Sciences, 1975) and *A United States Climate Program Plan* (Washington: Federal Coordinating Council for Science, Engineering and Technology, July 1977)

of the Department of Energy, James Schlesinger, believed that restraint crucial. Given Carter's investment of three million dollars to study carbon dioxide within his National Energy Plan (only months old) was sufficient for the present time. Anything more than research funding struck Schlesinger as unnecessary, and ultimately beyond what he believed "the experts" were suggesting about the urgency of climate change. “My view is that the policy implications of this issue are still too uncertain to warrant Presidential involvement and policy initiatives,” he noted on July 8th.[60] On the morning of July 25th, the issue of climate change once again came up. Secretary of Agriculture, Robert Bergland, inquired about recent press accounts regarding the potential climatic effects of burning coal. Schlesinger responded by advocating for careful observation given what he deemed to be speculation about future climatic conditions. While many -- including Carter himself -- noted that the warming effects could be realized by the 21st century, Schlesinger referred to some evidence that suggested the opposite conclusion.[61] This ambiguity about the overall trajectory of climate provided a space in which Schlesinger could reasonably argue for a research-only policy in the short-term.

As a political appointee, Press believed that his position was premised on working closely with members of the cabinet, most of all Bert Lance.[62] Indeed, Press approached his position with the belief that his effectiveness as an administrator was a function of his alignment with the administration's positions regarding science- or technology-based issues. As Press reflected in 1981,

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[60] James Schlesinger to Rick Hutcheson, 8 July 1977, Office of the Staff Secretary Box 31, 7-18-77 (1), Jimmy Carter Library and Museum, Atlanta, GA (hereafter JCL).

[61] For Schlesinger’s reference to the greenhouse effect as “speculation,” see Vertical File -- Cabinet Meeting Minutes, Box 7, 6/6/77-9/26/77, JCL

[62] Press/Carter Appointment Correspondence, February 1977, JC-DPS -- Records of the Domestic Policy Staff, Box 273 -- Science (Advisors, Issues, Concerns), Carter Library and Museum, Atlanta, GA. According to Press, his 'excellent' relations with the OMB were in part due to his historical working relationship with the OMB. See, Pielke, Roger, Jr. and Roberta Klein, eds. *Presidential Science Advisors: Perspectives and Reflections on Science, Policy and Politics* (Dordrecht: Springer, 2010), p. 39

> It was our early assessment that, in order to be effective and to have a significant influence on major policy and program decisions, we had to prove ourselves to be a valuable source of advice and had to operate in a manner consistent with, and complementary to, the policies and operating styles of the President and other members of his immediate staff with whom we would work on a day-to-day basis.[63]

In this light, when Lance expressed his reservations to Brown three days before Press's testimony, and when Press testified about his own reluctance to support climate legislation, Press and Lance were working as partners against an issue that they believed was not in the president's best interest. As Press himself articulated to Schmitt in his 1977 testimony, "this administration does not in general see the need for legislation in this area."[64]

To be clear, these cautious statements regarding the National Climate Program were not dismissals of the tremendous advances in technology that were allowing scientists to build sophisticated models to understand the climate. Throughout the 1970s, satellites were providing useful observations, and computers were allowing scientists to model large-scale atmospheric processes for the first time. This was the heyday of technology for those interested in understanding global atmospheric patterns and changes, and there was a lot of optimism that technology would provide a lot of answers.[65] Instead, the issue was whether the available information was reliable enough to base national policy decisions on for the benefit of users, and whether the application of climate knowledge was warranted. As illustrated by some of Carter's closest advisers -- Schlesinger, Lance, Mink, Epstein, and Press -- the only thing agreed upon during the mid-to-late 1970s was that more research was necessary. Federal funding toward that

[63] *Science* (January 9, 1981), p. 139
[64] Testimony of Frank Press, 7 June 1977, p. 102
[65] Edwards, Paul. *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge, MA: The MIT Press, 2008)

end was justified; translating that knowledge into practical application appeared imprudent and unjustified.[66]

Of course, by early 1978, Congress was on its own mission to pass an act that paid attention to the interests of local users.[67] While many staffers considered the legislation uncontroversial and bipartisan and recommended that "everything should be greased" for passage, those involved were still concerned that the administration sought to prioritize federal priorities over state and local interests.[68] By 1978, after the NCPA passed both houses of Congress, other interests started to make their interests known that conflicted with the administration's perspective. One was the American Association of State Climatologists.

<u>THE AASC STEPS ON STAGE</u>

One of the most significant supporters for user-oriented legislation was the American Association for State Climatologists (AASC). Paul Waite, President of the AASC and state climatologist for Iowa, sent Senator Howard Cannon (D-NV), Chairman of the Committee on Commerce, Science and Transportation, a climate plan in March 1978 entitled, "A Service-Oriented State Climate Program for the Citizens of the United States."[69] Asserting the need for research programs dedicated to evaluating "the impact of climate on human social and economic systems at local, state, and interstate levels," Waite believed that the delivery of climate information and services in a timely manner was integral to economic growth. Those that could

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[66] Indeed, members of the Carter Administration believed that President Carter could implement a national research policy without legislative approval. See, Draft of Presidential Executive Order for a National Climate Program, 2 March 1978, RG 46 - Box 13, Center for Legislative Archives, Washington, D.C.

[67] H.R. 6669, the officially agreed-upon bill for the National Climate Program Act, was passed by the House on September 9, 1977 and passed by the Senate on April 24, 1978. The House and Senate entered Conference Committee on May 31, 1978. The appointed conferees included members from the House (Teague, Brown, Ambro, Wirth, Beilenson, Walker, and Winn) and Senate (Cannon, Magnuson, Stevenson, Pearson, Schmitt). See National Climate Program Act Conference Report, No. 95-1489, 14 August 1978

[68] Flajser and John Stewart to Aubrey Sarvis and Ed Hall, 3 March 1978, National Archives -- National Climate Program Act, RG 46 -- Box 13, National Center for Legislative Archives, Washington, D.C.

[69] Waite to Howard Cannon, 3 April 1978, RG 46 - Box 13; "A Service-Oriented State Climate Program for the Citizens of the United States," American Association for State Climatologists, March 1978, National Center for Legislative Archives, RG 46-Box 13, Washington, D.C.

take advantage of such information ranged from individual farmers to private and governmental agencies. Local was key; climate specialists should be available to the local community and help interpret scientific information that could provide useful to their needs. "The local user of climate information and services," he argued,

> needs the personalized assistance of individuals who understand the specific social, economic, and environmental conditions that act to modify climate-induced problems at the local and state level. . . major investments and commitments for the future made by farmers, ranchers, and businessmen would benefit from advice given by individuals who are members of the local community and who would have the interests of the local community at heart.[70]

Fundamentally, the AASC's interests in climate services and user needs was perfectly aligned with senators like Schmitt. For them, the immense regional variability of climate-induced agricultural and economic changes mandated a local user-oriented perspective.[71]

For some of its members, the national climate program also represented a rare opportunity to reinvigorate state-level funding for climate research after the state climatology program was cut in 1973. In 1973, NOAA administrator Robert White defunded the State Climatological Program due to budget cuts -- a program that had existed under the aegis of the National Weather Service since the 1950s.[72] L. Dean Bark, climatologist with the Kansas Agricultural Experiment Station, wrote to Stevenson about his concerns about the lack of sufficient state funds for climate research.[73] For him, and many of his colleagues throughout the nation, the climate legislation -- now titled H.R. 6669 -- represented a "tremendous loss of

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[70] Waite to Howard Cannon, 3 April 1978
[71] Waite to Howard Cannon, 3 April 1978
[72] The State Climatological Program began during the mid-1950s under the direction of Helmut Landsberg, Director of the U.S. Weather Bureau's Office of Climatology. For a biographical sketch of Landsberg immediately after his passing in December 1985, see Droessler, F.G., "Helmut Landsberg," *EOS* 67, 19 (13 May 1986): p. 271.
[73] Little is known about Bark other than what appeared in obituaries after his death in 2010. L. Dean Bark earned his Ph.D. in Physics in 1956 while working as a bioclimatologist in the U.S. Weather Bureau. He was a Professor of Climatology at Kansas State University from 1967 to 1999 and Kansas State Climatologist from 1973 to 1990. See "Obituary: L. Dean Bark" within *Topeka Capital Journal*, 6 May 2010.

service to the general public" if it did not include provisions for user needs.[74] Howard Critchfield, state climatologist for Washington, also reflected on the need to compensate for the "immediate decline in the availability of climatic data."[75] While it is unclear whether Critchfield and Bark's opinions represented all state climatologists, archival evidence suggests that the national climate bill was seen as a potential source of much needed revenue for states that were essentially holding on by a thin margin. Scientific research at the national level could financially improve the conditions of climate scientists across the nation, and help guide research in a way that could benefit local communities.

NCPA REACHES THE CONFERENCE COMMITTEE

By the time the NCPA entered conference in late spring 1978, it was clear that the administration's reluctance contrasted greatly with Congress, as well as the AASC. Indeed, it appeared that the administration was losing a foot-hold on the discussion, and stepped into action. Upon hearing that the bill was about to enter conference in late May 1978, Press wrote Cannon to convey what he believed to be the Administration's "serious concerns" over this local, user-oriented perspective. Contrary to the conference committee and the AASC, Press argued that tailoring the National Climate Program Act to state or local needs would undermine the importance of a *national* climate research policy. As evidenced in his earlier 1977 Senate testimony, Press's reluctance was rooted in a belief that the science of forecasting was not advanced enough to meet the needs of local interests; the focus by Congress on the application of knowledge was “premature." Once again relying on what he believed to be "great caution," Press argued that funding and coordinating further research at the national level would provide the "proper balance between doing too little to ensure that we apply our emerging scientific

[74] Bark to Stevenson, 7 February 1978, National Center for Legislative Archives, RG 46 -- Box 13
[75] Critchfield to Magnusen, 5 January 1978, National Center for Legislative Archives, RG 46 -- Box 13

understanding and promising too much." He believed that the legislation as designed injudiciously privileged user-level needs as equal or more important than the advancement of scientific research.[76]

This was a critical juncture because the Administration -- via Frank Press -- and Congress had fundamentally different views on the purpose of a national climate program. As Press saw it, Congress -- by placing user needs at the core of this climate legislation -- was effectively usurping what the science could reliably offer and risked the credibility of the federal government should it fail to deliver on those expectations. On the other hand, senators like Schmitt believed that meeting user needs was the only desirable course of action. Otherwise, how could states like New Mexico benefit? Indeed, this was a very real division regarding the proper scope of a national climate program; while users -- farmers, etc. -- were entirely justified in their efforts to have taxpayer dollars fund research that could benefit them, Press and the administration believed that any deviation from a national research agenda would undermine the purpose of the legislation. Which community was most important -- scientists or users?

On May 17th, barely more than a week after Press's "serious concerns" letter to Cannon, it was clear where Congress stood. Steve Flasjer informed Stevenson over what he interpreted as the “confusing and disturbing” messages being promulgated by the OSTP. Implying doubt over Frank Press’s capacity to speak for the Administration, Flajser expressed surprise over the Administration’s opposition. Dismayed, Flasjer objected vehemently to the Administration’s narrow and “insensitive” position toward users of climate information; he cast the OSTP’s position as “self serving” for the scientific community with little to no regard for the every needs of citizens. While Flajser could not precisely locate the origin of Press’s concerns, he rightly

[76] Frank Press to Howard Cannon, 8 May 1978, RG 46 -- Box 13, National Center for Legislative Archives, National Archives, Washington, D.C.

suspected -- with every justification given his relationship with Lance in 1977 -- the OMB's "heavy hand."[77]

The following day, Pearson and Schmitt wrote Press directly regarding their own serious concerns. Conveying their surprise at the audacity of the executive to ignore user needs while overlooking the significant cost savings of climate legislation, Schmitt and Pearson were clear: "For the Administration now to attempt to undermine this effort casts serious doubt on the intent and willingness of the Administration to make available the benefits of the National Climate Program to State and local level users throughout the Nation."[78] How could an Administration choose to ignore the state and local users of climate information? For them, the perspective of Press seemed nothing short of bizarre.

Within the Administration, Press was equally surprised by what he believed to be a serious misunderstanding:

> At no time have we indicated that this section of the bill was one that was endorsed by the Administration and, in fact, in staff discussions care has been taken to point out that this section of the proposed legislation was one that was troublesome to the Administration. Our reason for believing that climate analysis should not be set up as a separate state-by-state research or grant program centers is the belief that climate research may best be pursued on a national, interstate or regional level as well as at the State level. The proposed approach that we have suggested would make the climate related activities at the state level a provision of the overall research program and its transfer or use.[79]

Research priorities should be set at the national level, and all research activities within states be a part of the nation's overall research agenda. As long as state research centers contributed to the "overall research program," the climate legislation would have the full support

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[77] Steve Flajser to Adlai Stevenson, 17 May 1978, RG 46 -- Box 13, Center for Legislative Archives, National Archives, Washington, D.C. Since Lance had resigned long before the conference committee met, it is unclear how his successor, James McIntyre, viewed the issues. Thus, it is unclear why Flasjer felt that the OMB was involved at this late stage.
[78] Schmitt and Pearson to Press, 18 May 1978, RG 46 -- Box 11, Center for Legislative Archives, National Archives, Washington, D.C.
[79] Press to Adlai Stevenson, 19 June 1978 and Press to Pearson and Schmitt, 19 June 1978, RG 46 - Box 11, Center for Legislative Archives, National Archives, Washington, D.C.

of the administration. Press, however, objected to the installation of state research centers for the purpose of conducting experiments and research tailored to local user needs. How could anything be fundamentally understood about climate -- a large-scale phenomena, temporally and geographically -- if the focus was on local variations?   As if a reaction to Flajser's insinuation that Press did not represent the Administration's views, Press attested that "this approach is consistent with the President's thinking."[80] If states were allowed to develop their own non-research agendas for the benefit of users discordant with the research interests of federal agencies, then the program would lead to the organizational inflexibility and bureaucratic deadlock that the program was designed to avoid. Carter was not interested in provincial interests; his administration was about national needs, and establishing a foundation to meet those needs.

Word of Press's resistance must have spread fairly rapidly among state climatologists. Robert Muller, state climatologist for Louisiana and advocate for re-establishing the AASC, wrote to Press to impress upon him the importance of user needs. Because of the role that climate played on all facets of human society, Muller reasoned that a cooperative program "would be much more effective than a totally federal program scattered across the many environmental agencies of the federal government." For Muller, federal grants would go a long way to produce a much needed and unique service to American citizens.[81] For those who convened in conference committee, the objective was clear: user needs were most important and Press's "serious concerns" were hardly enough to distract from the real goals.

By the summer of 1978, Press perhaps felt that his "serious concerns" carried little weight in the scheme of things. Indeed, it was clear that the conference committee had no interest in his

[80] Press to Adlai Stevenson, 19 June 1978 and Press to Pearson and Schmitt, 19 June 1978
[81] Muller to Press, 14 June 1978, RG 46, Box 11, National Center for Legislative Archives, Washington, D.C.

ideas; in fact, they chose to surge forward by incorporating language into the bill that would provide *greater* latitude for user needs. To do this, the committee managers replaced the term "research" with the term "studies." While seemingly minor on first look, deeper inspection suggests an effort to secure the interests of Congress over the interests of the Administration. As the conference managers described in the final report detailing the changes made to H.R. 6669,

> The committee of conference views as a high priority component of the program the need to greatly expand and improve upon existing climate-related services to user groups throughout the Nation. Too often Federal programs are developed without attention to user group needs and mechanisms for dealing with them. A user group focus for the national climate program is essential since the Congress, in passing this act, wishes to emphasize that the program is not to be viewed as only, or even primarily, a climate *research* effort.[82]

Due to the difficulty of concisely defining the climate itself for the purpose of making climate forecasts practically useful to users – climate had "so many different operational definitions" – the managers took great care to reflect in their legislation the complexities of the terrestrial climate system and sought as much flexibility as possible to study that which may not *directly* relate to the "climate" (i.e. "climate" was influenced by non-atmospheric systems, like the biosphere and the oceans). Given the ever-changing environment, the committee also felt that flexibility to study the various ways that user needs was critical for establishing greater flexibility when responding to a shifting environment. This meant a greater need for *organizational* -- as well as *conceptual* -- flexibility. As they argued,

> The national climate program is a prototype of the class of programs which can only succeed if involved organizations are able to transcend the classic patterns of bureaucratic rivalry and inhibited information flow. The entire point of making climate the focus of a national program is to emphasize that it is a subject which cannot be confined to ordinary

[82] Italics is meant for emphasis. Unfortunately, a close examination of a staff working draft of H.R. 6669, in consultation with the Conference Committee Report and other archival material, does not yield definitive evidence that this change to the ICP was due to these broader tensions between the administration and members of the conference committee. See, "National Climate Program Act," Conference Committee Report No. 95-1489 (August 14, 1978), RG 23 - Box 4; H.R. 6669 Staff Working Draft (Report No. 95-740), 3 August 1978, RG 46 - Box 11, Center for Legislative Archives, Washington, D.C.

> organizational boundaries, and that it must be considered as a critical element of strategic planning in almost all areas of human endeavor."[83]

Designing a climate bill to meet user needs was critical given the dynamic relationship between climate and humankind (i.e. tailoring climate knowledge to specific circumstances would allow greater organizational flexibility). In short, the coupling of government flexibility to the ever-changing climate reflected an emphasis on an "informed, intelligent action in response to climate" instead of what they believed to be a more bureaucratically restrained and inflexible "passive adaptation to climate."[84] In spite of Press's and the administration's concerns and annoyance with what appeared to be the senate's commitment to user needs -- President Carter once referred to Schmitt as "one of the biggest jerks in the Senate" -- the Conference Committee pressed forward and agreed to pass H.R. 6669 with the provision that "studies" would replace all instances of "research" in the final bill.[85]

Once passed, it appeared that atmospheric scientists were supportive. Adlai Stevenson, as one of the prime drivers of the legislation, received letters of congratulations from both policy makers and scientists from around the country. John Imbrie of Brown University expressed his gratification: "I feel strongly that its provisions will give the nation what we have needed in this area for so long: a coherent program aimed at the fundamentals of an important scientific and social problem."[86] Suffice to say, the dream of three years earlier to establish the first nationally coordinated climate program -- P.L. 95-367 -- was signed by President Carter on October 31, 1978, in spite of the dogged attempt by the administration to limit its user-oriented scope.

<u>CONCLUSION</u>

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[83] "HR 6669 Climate Conference Report Working Draft," RG-46 -- Box 11, Center for Legislative Archives, National Archives, Washington, D.C.

[84] "Joint Explanatory Statement of the Committee of Conference," RG 233, Box 4 – HR 6669, Center for Legislative Archives, Washington, D.C.

[85] Carter, Jimmy. *Jimmy Carter: White House Diary* (New York: Farrar, Straus, and Giroux, 2010), p. 126

[86] Imbrie to Adlai Stevenson, 18 September 1978, Box 11: Correspondence HR 6669 Conference Report, National Archives

Scientists, Pielke argues, must choose between four idealized roles when influencing the policy making process. First, the "pure scientist" is one who is largely divorced from politics and policy, believing that the autonomy of science should be privileged above all else. Second, the "science arbiter" is one that answers scientific questions for decision makers but avoids normative advice that may influence policy decisions. Third, the "issue advocate" is one that uses scientific information to advance a political agenda. Fourth, the "honest broker" integrates scientific knowledge into an assessment of a range of possible policy alternatives. Rather than demarcating science from policy advice, the "honest broker" relies on science to inform his active engagement with policy making.[87] According to this scheme, I argue that Press's role as science adviser may fit best in between the "science arbiter" and "issue advocate" positions because of his insistence on working well with others within the administration, reflecting the President's wishes, while still adhering to what he believed to be cautious policies that maneuvered between complacency and panic.

Indeed, two years into his tenure as Carter's science advisor, he had an opportunity to reflect on the nature of his job. As he articulated, science advisors are positioned between what he called the "seemingly rational world of science and engineering" and the more irrational "confrontational world of politics."[88] His job was to judiciously maneuver between these two worlds in an attempt to reign in the inherent uncertainties of living in an uncertain world. "All life, and its evolution," he argued, "is to a great extent an experiment. We can try to make it a less precarious one, but we shall never determine a final outcome."[89] Given the exigencies of

[87] Pielke, Roger, Jr. *The Honest Broker: Making Sense of Science and Policy in Politics* (New York: Cambridge University Press, 2007)
[88] Press, Frank, "Science and Technology in a Conserving Society," *BioScience* 29, 12 (December 1979): pp. 726-730, quote on p. 726
[89] Ibid, p. 726

political life, government could not eliminate all risk, but it could provide a carefully-designed solid foundation on which future generations can plan and organize.

What makes policy making difficult was not only the inherent uncertainties of the world. It also had something to do with what he called the precarious "Problems of Hindsight." At one end of the spectrum, he argued, a reliance on hindsight may serve as a source of "destructive guilt" about past abuses of past science and technology. This guilt, according to Press, may introduce what he called a "paralyzing impotence" when attempting to solve societal problems to ensure human survival; the fear of doing something wrong (as in the past) becomes a justification for doing nothing at all. At the other end, hindsight may induce one to believe that past mistakes lead to more wisdom, thus engendering a false reassurance that policies are mistake-free.[90]

Press worked to balance these two extremes, and likened the challenges and uncertainties of policymaking to what he called the "age-old problems of 'Faustian bargains'" or the practice of "making decisions about activities that may not have a harmful impact for years, if not generations, or for which the outcome, based on today's knowledge, is uncertain."[91] His ethical restraint -- his cautiousness as a scientist and administrative official -- was rooted in recognizing the limitations of government to resolve the difficulties of life while not complacently shunning action. Furthering research, in Press's mind, was the most appropriate response for the federal government given both the uncertainties of life and the uncertainties of science.

[90] Press, Frank, "Science and Technology in a Conserving Society," *BioScience* 29, 12 (December 1979): pp. 726-730, quote on p. 726
[91] Ibid, p. 729

Chapter 5
The "Great American Forecast"
The Production and Reception of the Global 2000 Study, 1977-84

This chapter examines one of the most ambitious and controversial reports produced during the environmental movement within the United States -- President Carter's *Global 2000 Report to the President*. The report was contentious because it reinforced and deepened pre-existing tensions between those who advocated what they believed to be a more sober alarm-raising and those who believed that environmental risks were great enough to warrant dramatic and urgent language to induce a public and political response. For some critics of the report, Global 2000 represented a step in the wrong direction -- what they classified as doomsaying had become sanctioned by some sectors of the environmental movement, the media, and now the federal government. For supporters, the report perfectly represented the spirit of the environmental movement by showcasing what they had known all along; environmental risks were real, they were serious, and they justified a strong national and international response.

This chapter will be split into the following sections. First, I demonstrate that the Global 2000 study was conceived by President Carter and his Council of Environmental Quality (CEQ) as an opportunity to use federal resources to better understand how global environmental problems were interconnected and complex. Second, this chapter explores how the report's authors aspired to publish a report that was at once a state-of-the-art forecast about the future environmental effects of growth as well as an attempt to convince the American public that current ways of living was not sustainable. This was a fine line, however. Given the stakes involved, Gerald Barney, the Executive Director of Global 2000, believed that a public relations strategy was necessary to maximize its effect without being perceived by as a so-called doomsday report. Third, and contrary to the authors' expectations, the report was perceived by the media and its critics as exactly that -- a doomsday report of the soberest kind. This chapter argues that Global 2000 inflamed tensions between the environmental movement and the emerging New Right.

CARTER'S HOLISTIC ENVIRONMENTALISM

In his Environmental Message to Congress on May 23, 1977, newly-elected President Jimmy Carter laid out a lengthy and detailed plan to make government more responsive to a myriad of long-term environmental concerns. His message had been planned for months. More than three months earlier, Stuart Eizenstat, Carter's Chief Domestic Policy Adviser, advised Carter that a message of this kind would project "maximum leadership" regarding environmental concerns, and provide clear "marching orders" for members of his administration. Additionally, the goal was to "spur action" in Congress and federal agencies, differentiate himself from the Ford Administration, and ultimately frame environmental concerns within a "global survival context."[1] It was seen as an opportunity to reinforce to the public that humanity's survival was at risk and that federal involvement was integral to staving off future disaster.

Toward this end, Carter asked the Council on Environmental Quality (CEQ) to simplify environmental regulation and collaborate with the Department of State and about a dozen federal agencies to develop a one-year study of the probable changes in the world's population, natural resources, and environment through the end of the century. This study -- what was later called the Global 2000 Report -- was envisioned as his centerpiece contribution to the environmentalist agenda. But it was more than that; his concern for the environment was consistent with his overall agenda to make government more effective and efficient, and provide a long-term basis

[1] Stu Eizenstat to Jimmy Carter, 8 February 1977, JC-1056 -- Box 7 -- 2-14-77 (2), Jimmy Carter Library and Archive, Atlanta, GA

for sustainable growth. "This study," he promised, "will serve as the foundation of our longer-term planning."[2]

President Carter asked the Chairman of the CEQ, Charles Warren, to reinforce the significance of his message during a press conference later that day. Described as the "sharpest shift in national policy on environmental matters" in decades, Warren emphasized the President's intent to advance "beyond the conservation ethic" that frequently framed environmental problems in terms of "lifestyle amenities."[3] No longer, he argued, would environmental politics be characterized as a concern for the aesthetic appeal of environmental preservation, nor ad hoc responses to isolated environmental problems.[4] "The point which we attempt, which we are trying to make," Warren described,

> is that the environmental emphasis is no longer on such matters as have been the traditional focus of concern; that is, some of the critics refer to them as the posy pluckers, the Bugs Bunny and the bees people, the elitists who are concerned with the environment. . . we are recognizing that there is a higher elevation of concern. We are no longer dealing with lifestyle amenities. We are dealing with life-sustaining necessities.[5]

His language was certainly provocative and dramatic. This was to be a new phase in environmental concern, one that looked beyond the polluted rivers and air that were frequently localized to specific areas, and which governed the activities of federal agencies like the EPA. The new vision would be global and holistic, extending beyond the purview of any one agency. While not commenting directly on Carter's message, Michael McCloskey, Executive Director of

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[2] Jimmy Carter: "The Environment Message to the Congress," May 23, 1977. Online by Gerhard Peters and John T. Woolley, *The American Presidency Project*. http://www.presidency.ucsb.edu/ws/?pid=7561.

[3] Press Conference of Charles M. Warren, 23 May 1977, Records of the Domestic Policy Staff (JC-DPS) - Box 203, Jimmy Carter Presidential Library and Museum, Atlanta, GA.

[4] The Global 2000 Study may be envisioned as a product of what Professor Arne Naess of the University of Oslo called "deep, long-range ecology movement." See, Naess, Arne, "The Shallow and the Deep, Long-Range Ecology Movements: A Summary," *Inquiry* 1, 16 (1973). For a broader history of this distinction, Devall, Bill, "The Deep, Long-Range Ecology Movement, 1960-2000 -- A Review," *Ethics and the Environment* 6, 1 (Spring, 2001): pp. 18-41.

[5] Press Conference of Charles M. Warren, 23 May 1977, Records of the Domestic Policy Staff (JC-DPS) - Box 203, Jimmy Carter Presidential Library and Museum, Atlanta, GA.

the Sierra Club, observed what appeared to be a fundamental shift of emphasis after 1970: "If anything distinguished the post-1970 environmental movement from what preceded it," he argued, "it was its holism and the breadth of issues it embraced."[6]

Carter's message had most of the desired political effects imagined by Eizenstat three months earlier. Gladwin Hill with the *New York Times* differentiated Carter's robust attention to the environment with President Ford, who, he said, "sometimes seemed to equate environment simply with recreation."[7] Many saw Carter's inclusion of the CEQ as promising, and the environmentalists' worry that Carter was not treating the environment as a national priority during the campaign seemed to be assuaged by his message. Peter Harnik of Environmental Action characterized his message as a "breeze of fresh air," and Arlie Schardt, Executive Director of the Environmental Defense Fund, expressed his support. Those in the business community were not as supportive, but were open to a conversation. Chris Farand with the U.S. Chamber of Commerce believed that Carter's goals were noble and ambitious, but unanchored to the everyday realities faced by industrial and business interests.[8] All in all, most were comforted and curious about Carter's new kind of environmentalism.

A reading of the transcript of Warren's press conference reveals the uniqueness of the vision. Part of the problem was what prominent ecologist Eugene Odum called the "one-problem, one-solution" paradigm that dictated how individuals conceptualized environmental

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[6] Some scholars argue that "modern environmentalism" had become "formalized" by the first Earth Day on April 22, 1970. This brand of environmentalism "marked the replacement of conservation with the full panoply of environmental issues" and mobilized a broad-base of support atypical of previous waves of conservation. For McCloskey's quote, see "Twenty Years of Change in the Environmental Movement," in *American Environmentalism: The U.S. Environmental Movement, 1970-1990,* eds. Dunlap and Mertig (Washington, D.C.: Taylor and Francis, 1992): p. 80

[7] "President is Praised on Environmental Aim," *New York Times,* 24 May 1977, p. 20

[8] "Carter for the Environment," *The Sun,* 24 May 1977, p. A14; "Environmental Message Seen as an Indication of Carter's Commitment and a Push for Advisory Panel, " *New York Times,* 24 May 1977, p. 21

challenges.[9] Odum believed that environmental problems should be studied holistically, which meant taking into account the multitude of feedbacks and complexities that characterize the relationship between humans and their natural habitat. As an ecologist, he appreciated that the appearance of one environmental problem is actually a multitude of problems that require a more sophisticated appreciation of the interconnectedness of the natural world.[10]

This perspective was later reflected by Lee Talbot, assistant to Charles Warren within the CEQ, who articulated,

> Up to now, international resources and environmental policy developments within the U.S. has been largely on an ad hoc basis, in response to perceived immediate problems and needs . . Therefore, an effective base for planning and policy development requires an holistic approach which adequately deals with the synergistic nature of the relationships among these factors.[11]

Synergy, holistic, long-term -- this was a new breed of environmentalism, and Carter sought to galvanize federal resources to ameliorate the problems that human societies would face should current growth trends continue beyond the year 2000.

FINDING A STUDY DIRECTOR

After months of searching, Warren appointed a well-respected systems modeler to execute Carter's vision, Gerald Barney. After receiving his Ph.D. in high energy fusion physics from the University of Wisconsin-Madison in 1967, Barney received a post-doctoral position at MIT to study how humans can manage complex systems. Soon, he found himself working in the CEQ under Russell Train's leadership during the early 1970s, wherein he received his first glimpse of the power of government to deal with complex environmental problems. This was

[9] For a contemporary account of the Press's uptake of this new ecological vision, see Shoenfeld, A., et al., "Constructing a Social Problem: The Press and the Environment," *Social Problems* 27, 1 (October 1979): pp. 38-61. Also, Odum, Eugene. *Ecology* (New York: Holt, Rinehart and Winston, 1973)
[10] Craige, Betty. *Eugene Odum: Ecosystem Ecologist and Environmentalist* (Athens: University of Georgia Press, 2001)
[11] "Concept Paper: Year 2000 Study," 7 July 1977, Stories -- Study Plan -- 1977-07-07 Talbot's Concept Paper. GB Personal Collection.

followed by a four year position as head of the Population and Resources Program of the Rockefeller Brothers Fund, a philanthropic organization founded in 1940 in New York City. While his time in CEQ was perhaps an introduction, his time at Rockefeller would introduce him to the specific skills required to assess environmental problems, and hone his administrative acumen when it came to managing large projects.[12]

It was during Barney's four years in New York between 1973 and 1977 that he directed what was known as the Environmental Agenda Project. Envisioned as an opportunity to assess the goals of the environmental movement and survey the "most important" environmental challenges, Barney oversaw the completion in 1977 of a sweeping report entitled, *An Unfinished Agenda*.[13] At the time, the U.S. environmental movement consisted of thousands of organizations (mostly small and local citizen-groups) with interests and activities ranging from wildlife preservation, to pollution abatement, to the future of the human race. Those most identifiable were obviously the national organizations such as the Environmental Defense Fund, Friends of the Earth, National Wildlife Federation, National Resources Defense Council, the World Watch Institute, the Conservation Foundation, among many others. Most were located in either New York City or Washington, D.C., though a few were scattered in states like California and Virginia.

To filter out the most serious and long-term environmental challenges, he established the Environmental Agenda Task Force (EATF). Members included: Gerald Barney, John Adams (Natural Resources Defense Council), David Brower (Friends of the Earth), George Davis (The Wilderness Society), Robert Dennis (Zero Population Growth), Thomas Kimball (National

[12] Basic biographical information about Gerald Barney may be found on his personal website: http://www.geraldbarney.com/AboutPage.html, last accessed March 25, 2014
[13] Barney, Gerald, ed. *The Unfinished Agenda: The Citizen's Policy Guide to Environmental Issues* (New York: Thomas Crowell Company, 1977)

Wildlife Federation), Ian Nisbet (Massachusetts Audubon Society), G. Jon Roush (The Nature Conservancy), Arlie Schardt (Environmental Defense Fund), Matland Sharpe (Izaak Walton League of America), Anthony Smith (National Parks and Conservation Association), Elvis Stahr (National Audubon Society), and Paul Swatek (Sierra Club). By enlisting "the constructive thinking of the nation's most knowledgeable and professional environmental leaders," Barney believed there was a good chance to produce a "consensus document" that distinguished the severe from the less severe. The report was branded "simply as a consensus statement of what the participating environmental leaders regard as the Unfinished Agenda."[14]

Suffice it to say, he was already well-versed in environmental concerns when asked by Warren to direct a multi-agency study of global environmental challenges. He also had the networks, skills, and administrative acumen to undertake what was then the most complex forecasting endeavor by the federal government since the environmental movement began in the early 1960s.

With a director in place, the Global 2000 Executive Council repeatedly met to discuss what they believed to be the study's proper scope and function. First, one of the most important concerns was to precisely define the study as a projection, and not a prediction or forecast. While often boiling down to semantics in popular culture, the legitimacy of the study was premised on properly locating its conclusions in a broad spectrum of terminology within future studies. "Predictions and forecasts," Barney argued early on, "are more or less precise statements of what someone expects to happen in the future. Projections, on the other hand, are efforts to say something about what will happen in the future <u>if certain assumptions are met</u>."[15]

[14] Barney, Gerald, ed. *The Unfinished Agenda: The Citizen's Policy Guide to Environmental Issues* (New York: Thomas Crowell Company, 1977)
[15] Barney to Elinor Terhune, 20 October 1977, Drafts -- Entering the 21st Century

By perceiving Global 2000 as a series of projections, and not predictions, the authors wanted to avoid the unsavory perception that they were investing more confidence than technically permitted.

Second, the study was designed to examine the *inter*relationships of important trends by pulling together information already available within federal agencies. The study was not meant to produce original research, but rather an attempt to link existing agency models to produce a more coherent vision of the future. Ambitious, Barney was well aware that the project could take on a life of its own. As Barney noted early in the planning stage: "The expectations must be kept within reasonable bounds and that the study would not be a massive state-of-the-art computer analysis. The initial emphasis will be on identifying, inventorying, accumulating and collating relevant information."[16] Study expectations must be "kept in line" by focusing not on problem solving but rather on problem defining and laying a sufficient foundation for the federal government to integrate and expand upon forecasting work.[17] This was an internal reconnaissance mission that would expose structural deficiencies within the federal government in dealing with global environmental problems. As later noted during an interagency meeting, the project would focus "on the Federal Government's capability in forecasting; it is an exercise of bringing together, synthesizing, USG <US Government> projection capabilities on environment, population, and natural resources, and how these capabilities can be improved."[18]

Third, as described at length within a technical volume (Volume 2), the most difficult technical challenge was the integration of existing agency models. After identifying each

[16] "Interagency Meeting on the Year 2000 Projection," 1 September 1977, Briefings -- 1977-09-01 ExecGroupMeet -- 1977-09-01 Warren to Files Interagency Meeting. GB Personal Collection.
[17] "Year 2000 Project Meeting Minutes," 1 September 1977, Stories -- 1977-09-01 ExecGroupMeet -- SS-to-GOB Minutes of Ex Group Meet. GB Personal Collection.
[18] "Interagency Meeting on the Year 2000 Project," 20 September 1977. Stories -- 1977-09-01 ExecGroupMeet -- GOB Full Rpt Re Interagency Meeting

agency's abilities, they realized that eleven "elements" or variables used within existing agency models would be useful to construct a new integrated model: (1) population, (2) gross national product (GNP), (3) climate, (4) technology, (5) food, (6) fisheries, forestry and water, (7) energy, (8) energy residuals, (9) fuel minerals, (10) nonfuel minerals, and (11) environment. However, even if they had the raw materials (i.e. elements) required for a new model, they knew that integrating them was a serious technical challenge in three specific ways. First, each agency model was based on certain assumptions that guided the model's purpose, thereby making the assumptions built into each agency model "mutually inconsistent." What they wanted was a "mutually consistent" set of assumptions that could ultimately produce a coherent and internally consistent projection of the future -- a uniform statement that could be represented as the government's first global model. Second, each agency had independent data streams that fueled their respective models; since the Global 2000 was basically a bringing together of those different streams, it was also an inventorying of what data led to what conclusions and how they related. Third, each agency had different needs that dictated what models were created and how they would be used. Ultimately, the authors of Global 2000 knew that these technical challenges were immense.

Fourth, the process of developing the Global 2000 would require ample inter-agency collaboration. This was not technically difficult in theory, as with integrating existing agency models, but it did fly in the face of established tradition within Washington. With one or two exceptions, "none of the agency experts had met each other previously, and none knew anything about the assumptions, structures, requirements, and uses of the others' calculation procedures."[19] Specialization hindered communication between agency experts because there was no incentive

[19] Global 2000 Study, vol. 2, p. 457

for collaboration. As William Ascher, Professor of Public Policy Studies and Political Science at Johns Hopkins University, described:

> A specialist in one forecasting area (e.g. energy forecasting) must rely implicitly or explicitly on forecasts in areas that are beyond his own expertise (e.g. population forecasting). Since his knowledge of appropriate assumptions outside his specialty is limited, he will not produce definitive forecasts in these other areas.[20]

Global 2000 was not just about creating a new kind of environmentalism, but overcoming a fragmented bureaucracy. And, this is why Barney's experience and skills as an administrator were useful; he consistently encouraged agency heads to exchange views and ideas, which would allow a more fluid and coordinated *system* of forecasting to emerge. Without this kind of fundamental social transformation, Carter's commitment to creating a long-term foundation on which to guide future policies would be little more than a fantasy.

In general, Warren, Carter, Talbot, and Barney did not want to replicate the tendency of previous administrations to create a new federal agency to deal with a specific array of problems. Instead, their goal was to create a government that -- metaphorically speaking -- emulated the interconnectedness of the natural world. This is what Warren and Talbot meant when they wanted to expand beyond Odum's "one problem, one solution" paradigm; they wanted a government that was flexible enough to fluidly respond to an always-changing global environment. They wanted a federal bureaucracy to mimic the natural world's ability to adjust and adapt; instead of delegating energy to the Department of Energy, and environment to the Environmental Protection Agency, which only reinforced a fragmented and ad hoc response system, they wanted to find a way to establish a more symbiotic relationship between agencies to

[20] Ascher, William. *Forecasting: An Appraisal for Policy-Makers and Planners* (Baltimore: Johns Hopkins University Press, 1978), p. 202

more effectively manage the inherent complexities of environmental challenges. As stated in the final published study,

> the real-world phenomena interact -- especially in the longer term -- in ways that do not conform to the bureaucratic division of responsibility or to narrowly focused sectoral models. Hence, the government's global model in its present form can only imperfectly project the consequences of these interactions.[21]

In sum, the Global 2000 Team wanted to mobilize and organize available federal resources to create an integrated global model with "mutually consistent" assumptions built in, and structurally reform government -- if ever so slightly -- to not only become more efficient but more integrated and involved in resolving environmental challenges. Consistent with Carter's vision for a new kind of environmentalism, Global 2000 was as an opportunity to alter how government functions while providing a roadmap for nations to create a more symbiotic relationship with nature.

STRATEGIZING GLOBAL 2000'S RELEASE

Global 2000 was -- as already evidenced by Eizenstat -- a cultural and political statement as much as an environmental one, and the authors took a great deal of time to assess the best way to present the report to the world. They knew that the study would be dissected by a variety of audiences with entirely different ideas about the environment and the future. This was going to be a divisive document no matter what the contents were, but the question was whether it would have the kind of effect they wanted. No one could foretell what the final product would look like, but there were certain guiding principles and goals that its authors felt were important benchmarks. For them, the report's technical ambitions were certainly admirable, but it soon became clear that the central thrust of the document would be in its ability to convey the right tone and proper timing.

[21] Global 2000 Study, Vol. 2, p. 461

For everyone involved, the stakes were quite high and the world was watching. As Lester Brown, President of the Worldwatch Institute and colleague of Barney's, wrote in November 1979 (seven months prior to its release), "In my opinion the study will be seen as a major U.S. contribution to the international understanding of the difficult environmental and resource issues confronting humanity. . . it should be seen as the beginning of a sustained address of the complex of environmental, resource, and economic issues facing countries everywhere."[22]

To maximize the study's impact on environmental politics, public sentiment was monitored closely. One year after Carter's message during the summer of 1978, Pat Caddell, Carter's pollster, talked with Barney and Story Shem, a staffer within the Department of State Office of Environmental Affairs and member of the Global 2000 Study team, about his ideas. Shem was designated as the primary lead in drafting a release strategy that considered a range of variables: how involved would the president be in its release, when would the public release take place, how would it be received given the "public mood," what should be emphasized and highlighted, among a range of other factors that would dictate its reception. When talking with Barney and Shem that summer, Caddell believed that the "public mood was perfect" given positive public sentiment toward the environment. Given what appeared to be an opportunity for the government to step in -- the government was "not prepared to think about these problems in either a long-term or holistic manner" -- it seemed that Carter was primed to release a report that exploited such an opening.[23] But, as Caddell knew given his knowledge of public sentiment, things change quickly.

In October 1978, for instance, Shem expressed his reservations. Given what he saw as a "difficult political climate for policymakers" due to the public's tendency to be misinformed

[22] Brown to Pickering and Speth, 20 November 1979, Drafts-Vol1
[23] Barney to Warren and Pickering, 20 September 1978, Chronology, Personal Papers of Gerald Barney

about environmental challenges, he realized that marketing the piece would require an astute eye to public forces and that messaging would be crucial. This was an attempt to convince the public that Global 2000 was useful, and that the federal government was primed to act. "From a marketing point of view," he wrote,

> we have a viable product to sell; it's not necessarily going to be easy to promote, nor for the general public to consume, but it is credible. What is at issue is that 'we' believe in the importance of integrated long-term planning in the United States government, and are willing to do something about it."[24]

A significant part of the problem was the possibility that the public might dismiss the report as overly gloomy and disregard its conclusions. In spite of his belief that the study was "not a doomsday prediction," he felt that the administration should focus on only those elements that were positive about the report.[25] As he outlined in a release strategy draft in November 1978,

> We need to outline the 'pluses' of the study; point out the political benefits; cite public opinion polls -- the mood of the country; discuss the low-risk involvement, and generally frame this first attempt of examining the U.S. Governmental analytical capabilities in a positive posture. The global leadership tact is the best venue.[26]

It was not until late 1979 and early 1980 that the Global 2000 team felt that the time was finally right to begin planning for its actual release.

Between September 1979 and April 1980, members of the Global 2000 team drafted what was known as the report summary.[27] This would serve as the first of a three volume study, and everyone knew that it would be the first thing that policy makers, the general public, the media, and other countries would fixate on. The summary would be the face of the project, and it was here that the primary conclusions and tone of the report would be dissected and interpreted.

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[24] Shem to Barney, 27 October 1978, Release -- Strategy -- 1978-10-27_Release_Strategy_Story_Shem, Personal Papers of Gerald Barney

[25] Shem to Barney, Draft Release Strategy, 27 October 1978, Release-Strategy, Barney Papers

[26] Shem to Warren, Draft Release Strategy, 22 November 1978, Release-Strategy, Barney Papers

[27] *The Global 2000 Report to the President: Entering the Twenty-First Century, Volume One* (Washington, D.C.: U.S. Government Printing Office, 1980)

There was a lot riding on the summary striking the right balance between candor and practical strategy. Given the stakes, significant disagreements arose on a range of issues, principally whether it would be interpreted as a doomsday report, whether an objective assessment of uncertainties would undermine the report's credibility, and how the summary could portray the administration -- President Carter, specifically -- in the best light. At times, the discussions became quite heated.

Given how much was riding on properly defining the scope and tone of the report, Barney needed someone with professional expertise in public relations. For this, he requested the assistance of Calvin Kytle Associates, a well known public relations firm in Washington, D.C. When presented with an early draft, he became immediately concerned: "The composite picture the report sketches provides little comfort," he began. "This generally disheartening report comes on the heels of a series of gloomy assessments of the mood of the American people."[28] He cited a recent 1978 poll suggesting that the nation was not well off relative to five years earlier, and referenced Carter's repeated acknowledgments of a "lingering public disenchantment, or malaise" as evidence that perception was an important variable in the release strategy. Unless the report was released with encouraging ideas or promises of action, the Global 2000 study could be "received with indifference ('So what else is new?') or to contribute further to deterioration of public morale." Ultimately, the answer was clear: "We must stress that this is not a 'doomsday' report by some academic fringe group. It was prepared on the basis of the best data resources and most sophisticated analytical capabilities of the Executive Branch."[29]

[28] Kytle Release Strategy, 28 January 1980, Release-Strategy, Barney Papers.
[29] *Ibid*. For the "malaise speech," see Jimmy Carter: "Address to the Nation on Energy and National Goals: 'The Malaise Speech'," July 15, 1979. The American Presidency Project. http://www.presidency.ucsb.edu/ws/?pid=32596, last accessed 8 January 2014.

To draft the summary, the team turned to a Department of State staffer named Katharine "Kitty" Gillman. While not much is known about her professionally or personally, she submitted her first formal draft on November 22, 1979 for review. Organized into three sections -- "preface," "summary and findings," and "findings" -- she sought to balance what she saw as the positives as well as the negatives of the study, attempting to strike the right tone of candor and optimism.[30] On the positive side, she noted the report's uniqueness as the first government forecasting effort to produce a model of long term changes in population, resources, and the environment. Its virtue was in its ambition and unprecedented scope, that it was a first step on a march toward better forecasting.

However, Gillman was also careful to note what she saw as problematic areas. As Barney noted before, the report was not a forecast but rather a projection of existing trends. The study assumed no significant policy changes among nations that could alter their conclusions, but instead assumed that present trends in the variables used would continue for the next twenty or more years. To hedge against any potential criticism that these trends were overly pessimistic, she hedged a bit by noting that there was always an opportunity for nations to stave off future disaster. "Where the projections show alarming trends, they should be taken as warning signals, not necessarily as omens of disaster."[31] Ned Dearborn, a member of the Global 2000 team, agreed: "presentation of findings is balanced -- i.e., the report could never be mistaken by anti-environmentalists as just another CEQ jeremiad."[32]

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[30] Gillman Draft, 22 November 1979, Drafts-Vol1, Barney Papers
[31] Gillman Draft, 22 November 1979, Drafts-Vol1, Barney Papers
[32] Dearborn to Barney, 9 February 1979, Draft-Vol1, Barney Papers

Nonetheless, Gillman's apparent caution was not particularly in keeping with what some saw as the project's purpose; it struck some as too cautious. On January 10, 1980, Peter Freeman conveyed his impressions:

> The findings of the Global 2000 are alarming. We should be alarmed. We should be sufficiently alarmed to dedicate ourselves to appropriately vigorous actions. True, you run the risk of being branded neo-Malthusian but it is also truer than ever that those who would apply that label are irresponsible. One can't take such criticism seriously.[33]

According Freeman, Gillman's style of presentation is so "low key" that the drama of the projections and their impacts are lost. He thought that the apparent attempt to "assuage feelings of alarm" was "counter-productive," and that the "optimistic bias" within the report should be front and center to showcase that things could be far worse by the year 2000. He objected, for instance, to Gillman's cautious statement that the "alarming trends" were "warning signals" but not "necessarily omens of disaster."[34]

Additionally, Gillman candidly detailed many deficiencies in the report. According to her November draft, the effort to integrate the various elements within the study's projections were "only partially successful at best." Essentially, the projections did not take into the consideration the many feedbacks evident in nature, and therefore the projections were highly unrealistic. Thus, she concluded,

> the Global 2000 Report cannot be considered the definitive study of trends in population, resources, and environment over the next 20 years. Nonetheless, it provides the most internally consistent, interrelated set of projections yet produced by the government. . . This report is a beginning.

This reference to the report being "overly optimistic" is strange if it did not have so much sense for the authors who understood the report. For them, their projections -- given the structural

[33] Freeman to Barney, 10 January 1980, Draft-Vol1, Barney Papers
[34] Gillman Draft, 22 November 1979, Drafts-Vol1, Barney Papers

deficiencies within the report -- actually *understated* the severity of the consequences in store for human civilization should present trends continue into the near future. This is what Gillman meant by "overly optimistic"; she was saying that the report's projections may not accurately reflect the future in that they may not be pessimistic enough. As noted in the final report of 1980, "there is some justification for concluding that the Global 2000 Study's projections are by and large 'optimistically' biased and in need of a 'pessimistic' correction."[35] The result: things that appeared urgent may in fact be far worse than projected by the model.

Others argued that a discussion of structural deficiencies within the model projections was inappropriate for the summary volume. Bill Long with the Department of State believed that such "excessive apologies for, and explanations of, why the models are deficient" would cause people to "scrutinize the trees and not the forests."[36] Barney himself reasoned that "if the discussion of assumptions is done objectively it will destroy virtually all of the projections' credibility."[37] Barbara Blum, a staffer in the Environmental Protection Agency, also noted her objections that a technical analysis was too ambitious given time constraints.[38] According to hand-written notes by Barney regarding a call that he had with Gillman, Speth agreed with Blum that the list of "caveats and assumptions in the present draft of the preface come out and go later."[39] For figures like Freeman, Long, Speth, Blum, and Barney, the report served as a unique opportunity to really shake things up; too much candor and caution could stifle its meaning to the broader public and, most importantly of all, to policy makers.

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[35] Global 2000 Study, Vol. 2, p. 482
[36] Long to Barney and Gillman, 31 January 1980, Drafts-Vol1, Barney Papers
[37] Barney to Lindsey Grant, 12 September 1979, Drafts-Vol1, Barney Papers
[38] Barney Memo, 4 December 1979, Drafts-Vol1, Barney Papers; Blum to Pickering and Speth, 12 October 1979, Admin-People-Barbara Blum, Barney Papers
[39] Hand-written notes, 2 January 1980, Draft-Vol1, Barney Papers

Ultimately, it appeared that her candor and caution made the report less than interesting to those who really mattered. While most believed that it was well-written, opinion was fairly uniform that the summary was fairly superficial and politically not very useful. John Richardson, a global systems analyst and consultant for the project, believed for instance that Gillman had done an "impressive, skillful job of condensing and focusing a large amount of material" and believed that the tone of the summary "communicates urgency, but is not strident." By this, he meant that the report was balanced and would not "turn off" readers.[40] Allan Matthews, a materials consultant, agreed that it was well-written but also believed that policymakers, journalists, and others with a "generalist interest" will find it of limited significance. "Although well written, it is sketchy on substance, both in presentation and posing of issues."[41] Samuel Baum, a demographer within the Bureau of Consensus, mirrored Matthews's sentiments: "although the general public might be satisfied with this brief treatment of the situation, I have serious doubts whether the informed policy makers will find the presentation sufficiently convincing to pay much attention to the issues raised."[42]

Ultimately, Gillman's draft was the opening salvo that would determine how the report would sound to the general public. Its reception was crucial to its credibility, and they knew that any opportunity to present a dramatic portrayal of the future would be a positive way to engage the general public. Gillman and many others believed that qualifying their warnings with caveats as necessary to maintain the distinction between a projection and prediction, while others believed that it merely limited the potential for Global 2000. Urgency without stridence, alarm blended with hope, candor and credibility -- these issues went to the heart of the report's construction.

[40] Richardson to Bill Long, 9 January 1980, Draft-Vol1, Barney Papers
[41] Matthews to Barney, 16 December 1979, Draft-Vol1, Barney Papers
[42] Baum to Barney, 19 December 1979, Draft-Vol1, Barney Papers

Gillman's draft also sparked a concern that it did not adequately account for how the report would inform public perceptions of President Carter himself. This was identity politics as much as environment politics, and, on January 8, 1980, Ned Dearborn argued that the summary as drafted in November wasted an important opportunity to enhance the prestige of the President and the Administration. "Massive effort went into this study, and its finding are of corresponding significance. But where the draft report should roar, it whimpers," Dearborn objected.[43] To illustrate his resolve, he also sent his complaints to many members of Carter's staff: (1) Alonzo MacDonald, White House Staff Director; (2) Hamilton Jordan, White House Chief of Staff; (3) Stuart Eizenstat, Executive Director of White House Domestic Policy Staff; (4) Kit Dobelle, Chief of Staff to First Lady Rosalynn Carter. This was a significant move because he essentially went outside of the chain of command; while he let Barney know his concerns, he did not wait to hear his response.

Suspecting that some of those involved were ashamed of their own achievement, Dearborn suggested that could be avoided if the report was framed as a demonstration of the President's major strengths: intellectual ability, foresight, courage, and readiness to reshape federal bureaucracy to meet the nation's needs. For him, Carter's initiative to commission the study was nothing short of courageous in that it revealed fundamental deficiencies of the federal government to meet the needs of the nation. He believed that the deficiencies noted in Gillman's draft had a second virtue in that it could highlight the President's willingness to step into the frontier of knowledge and take a hard look at a host of complex problems that were undermining

[43] Dearborn to Barney, 8 January 1980, Barney Papers

the nation's long-term viability. For him, the report enhanced the environment's prestige, but it did little for the man who made it all happen.[44]

A week later, Dearborn again sent McDonald a stern warning that not enough was being done to represent the true meaning of the study. Not content with MacDonald's willingness to direct his concerns to the conventional political channels, he believed that a "timorous and queasy bureaucracy" was at the heart of a problem.

> I believe that a cover-up is proceeding, and that White House intervention is essential if the cover-up is to be avoided. . . If I do not perceive the White House to be taking effective action with regard to the issues I raise, I will go outside my chain of command to the various Presidential candidates, Jack Anderson, Ralph Nader, and whoever else will listen. I believe this is my duty as a citizen and as a moral human.[45]

Dearborn's insinuation of a cover-up forced Barney into a rather awkward position. According to Speth, his claim of a cover-up crossed a line. Concerned that one of the lead authors in the report was at risk for termination or censure, Barney assure Speth directly that he would not act rashly by taking the study to the press or other presidential candidates. Dearborn himself was aware of what he was doing and appealed directly to Barney in a hand-written note. "Jerry," he began, "Again, I have acted without your knowledge, much less approval, out of moral convictions which are becoming stronger each day."[46]

GLOBAL 2000 FRAMED AS COLD-WAR ANTIDOTE

To minimize what was clearly the contentious issue of whether it was appropriate to include structural deficiencies within the report summary, and whether it cast as a doomsday was beneficial, all seemed to agree that Dearborn's advice had a certain virtue. One of the guiding premises in the public release of Global 2000 was the belief that it could not only redefine environmental discussions; it could also reframe the Cold War. Kytle, within his release

[44] Dearborn to Barney, 8 January 1980, Barney Papers
[45] Dearborn to McDonald, 15 January 1980, Barney Papers
[46] Dearborn to McDonald, 15 January 1980, hand-written note connected to memo, Barney Collection

strategy, saw beyond a myopic focus on military preparedness, the charting of troop movements, the funding of new strategic weapons systems, and other short-term attempts at crisis-management. Instead of international competition, the report would reveal the President's commitment to international cooperation as a means for "building a bigger stick" to "deal realistically with the Soviet threat" while simultaneously projecting genuine global leadership on the environment. As Kytle described,

> The report would serve to illuminate the precarious state of the physical world that now makes the possibility of limited warfare between the two great powers an obsolete, romantic notion that renders Pyrrhic any victory from nuclear war. In the real world of Global 2000, a cynic might regard nuclear war as the answer to the population problem but even the ultimate cynic would have to concede that to do so would be only to exacerbate the resource and environmental problems to an intolerable degree.

Indeed, he felt that it was important to fine the right balance between striking fear into the general public -- which could only serve to inflame critics of alarmist claims -- and managing those fears productively to inaugurate a better future.

> Citing this scientific data, the President could argue that the generally bleak consequences the report foresees are not inevitable. Thus he could avoid on the one hand being portrayed as a Jeremiah and on the other being branded as naive. In the international forums in which demands for a new world economic order are constantly made, he could present this factual report without ideological interpretation.

The president could do three things to realize these goals. First, instruct each cabinet member to identify realistic and proactive policy options addressed to the relevant findings of the report. Second, issue an Executive Order to establish a long-range planning mechanism within the federal government. Third, invite national and international public discussion of the report. Fundamentally, it was critical that Carter and the CEQ balance the somber conclusions of the report in light of both broad public malaise and skepticism and the general tensions of the Cold War. The report was a potential game-changer, and only through well-developed release strategy could the report acquire a significance aligned with its conclusions.[47] As the months passed, it became more clear that Gillman and Barney's drafting of the report summary was smoothing out some of the rough edges, which was finally accepted in May.

FINAL RELEASE OF GLOBAL 2000 REPORT

On July 24, 1980, President Carter addressed the public about his signature achievement. "Never before had our government or any other government attempting to take such a comprehensive, long-range look at interrelated global issues . . . I believe America must provide special leadership in addressing global conditions," he urged. While he did not address the specific questions of the report, he did convey the enormous scope of the tasks that lie ahead. First, he appointed a special committee, the Presidential Task Force on Global Resources and Environment, to be chaired by Gus Speth. The committee would include the Secretary of State, the Assistant to the President for Domestic Affairs and Policy, the Director of the OSTP (Frank Press), and the Director of the OMB. Ultimately, he argued, it was time to look forward to the next century:

> It is my firm belief that we can build a future in which all people lead full, decent lives in harmony with a healthy and habitable planet. And I believe that the skills, experience, vision, and courage of the American people today make the United States a natural leader in charting and guiding humanity's course towards a better tomorrow.[48]

Subsequent to its release, the framing of the Global 2000 Report was absolutely critical to its reception within Congress. On September 4, 1980, Thomas Pickering and Gus Speth were invited to testify in front of the congressional Subcommittee on International Economics, co-

[47] "Response to Plans for Release of Global 2000 Report," 28 January 1980. Release -- CalvinKytleAssoc -- 1980--1-28 Kytle on Release Plans; "Responses to Four Key Questions Concerning Release of the Global 2000 Report," 5 February 1980. Release -- CalvinKytleAssoc -- 1980-2-5 Kytle Responses to Four Key Questions. Gerald Barney Collection.

[48] Jimmy Carter: "Global 2000 Study Statement on the Report to the President. ," July 24, 1980. Online by Gerhard Peters and John T. Woolley, *The American Presidency Project*. http://www.presidency.ucsb.edu/ws/?pid=44808

chaired by Rep. Henry Reuss (D-WI).[49] This was an important occasion because it provided a forum to address the broader ramifications of the report, and advance the conversation of international planning forward. Indeed, from the moment the hearing began, it was clear that the report was still bound to the pessimism-optimism spectrum of debate. "It makes for grim reading," began Reuss. "The study . . . documents a world a bare 20 years from now that is desolate and dying, the result of the past, present, and prospective follies of its people."[50] Nonetheless, after detailing the many problems addressed by the report, he reassuringly stated that all was not lost. "Despite these depressing forebodings, the Global 2000 Report ends on a potentially upbeat note. Disaster need not be our inevitable fate."[51] Reuss's introductory remarks illustrated back-and-forth tendency to frame the report as an optimistic message hope and the pessimism that pervaded the previous decade. The report's tone, as much as the substance, was the challenge of Speth and Pickering -- how to balance the two depictions. Indeed, Reuss was particularly interested in understanding what could actions could be taken to prevent future catastrophe, and encouraged both Pickering and Speth to speculate on what at first may appear to be "far out ideas."[52]

To advance the conversation beyond the tone of the report, Speth emphasized three things that could avoid the overly pessimistic tone fixated on by the media and critics.[53] First, the report was not a prediction of what would occur, but a projection of what could occur if current trends continued into the 21st century. This was an important point; action could not be taken if one was mired in the apathy of future inevitability. Second, he argued that economic development values and environmental values were not antagonistic; sound policies geared to

[49] "The Global 2000 Report," Hearing before the Subcommittee on International Economics of the Joint Economic Committee, 4 September 1980 (Washington, D.C.: U.S. Government Printing Office, 1980)
[50] Opening Statement of Representative Reuss, 4 September 1980, p. 1
[51] Opening Statement of Representative Reuss, 4 September 1980, p. 2
[52] Opening Statement of Representative Reuss, 4 September 1980, p. 2
[53] Statement of Hon. Gus Speth, 4 September 1980, pp. 2-10

economic development was necessary if crisis was to be averted. Debates over the environment had become culturally stagnant, and would hardly yield the solutions required to deal with both domestic and international problems laid out in the report. Third, both renewable and nonrenewable resources were in decline. This was a baseline admission, one that could orient the debate to what Speth believed were the realities of the situation nations now faced.

Ultimately, the purpose of Speth's testimony was not to rehash what was already stated in the report, nor to continue discussing the almost banal considerations of the report's tone. Instead, he highlighted the importance of the Task Force created by Carter to gather information and suggestions from relevant government agencies, inventory agency resources, and provide a strategy for future action. Time for action was now. By pointing to the steps currently being taken by the Administration, he in effect stressed that the report does not preordain future disaster. "In closing, Mr. Chairman, I would like to emphasize once again that the Global 2000 Report should be viewed not as a counsel of despair, not as a prediction of a gloomy future, but as a challenge and a unique opportunity for leadership."[54] The meaning of Speth's message was clear: the report was a stepping stone to growth, a challenge no different than many of the national challenges met of the past. The report was, in a single phrase, a call to arms, not a cry of resignation under the weight of future crisis.

Complementary to Speth's testimony, Pickering opportunistically showcased the rapid uptake of the report's conclusions in other developed countries.[55] Depicting the study as an international bestseller, Pickering noted how the overseas reporting tended to highlight the "major findings and conclusions" rather than the "study's methodological imperfections."[56] Like Speth, he sought to move the conversation forward; nit-picking on the report's methodology lost

[54] Statement of Hon. Gus Speth, 4 September 1980, p. 4
[55] Statement of Hon. Thomas Pickering, 4 September 1980, pp. 10-19
[56] Statement of Hon. Thomas Pickering, 4 September 1980, p. 15

sight of the broad-scale actions necessary by all nations. This was clearly an attempt to reorient how individuals perceived the report; the forest was more valuable for long-term planning than the individual trees. Problems existed, yes, but the overarching theme of the report was that action and leadership was necessary:

> At a time of extremely tight budgets here at home and fiscal austerity throughout the international community, our country has, in effect, in a role of leadership, called for a new attack on a complex and difficult series of global problems. . . We are sustained in our efforts here by the conviction that we have the right issues and that the world and our own country will ignore and neglect them at our peril.[57]

The time for research was effectively coming to an end, and action was necessary now. Certainly embedded in Cold War tensions between east and west, the report was positioned as an opportunity to embark on future of growth. This was Carter's signature contribution to humankind, an environmental statement that pushed beyond the tired considerations of the past. Global, holism, ecological -- the report combined a Cold War ethos with environmental goals.

On December 15, 1980, five weeks after the election and seven months after the report's release, Speth wrote an "information only" memo to Carter to convey his thoughts on how to ensure that Reagan took the report seriously. As Speth advised, "We would take steps to see that it is as favorably received as possible by the incoming administration. . . It would indicate that there is a logical break between your efforts to raise the issue and those of the new administration to formulate a response."[58] The idea is that a memo would be written to the president-elect, which would "set a tone" for the incoming administration. Striving to avoid the perception that that Carter's staff was trying to "predetermine" Reagan's future decisions, they

[57] Statement of Hon. Thomas Pickering, 4 September 1980, p. 15
[58] Memo from Gus Speth to Jimmy Carter, "Global Resources and Environment Task Force -- Completion of Work," 15 December 1980, JC-DPS: Records of the Domestic Policy Staff, Box 34, Global 2000 Report (1), Jimmy Carter Museum and Archive

did want to encourage a "positive response" from the new administration. The initial signs that this would happen were not encouraging.

In the basement of the Fairmont Hotel three months earlier on the morning of September 25, 1980, a reporter asked California Governor Reagan whether he would speak on the Global 2000 Study. While the media heavily covered the report even prior to its release in the summer of 1980, Reagan was caught off guard by the reporter's request because he was entirely unaware of the report's existence.

> **Reporter**: Governor Reagan, are you planning to speak to the issues of Global 2000?
> **Reagan**: A . . . a . . . What is that? . . .
> **Reporter**: It's a report that was issued by the Council on Environmental Quality and the State Department. It talks about the important issues facing this nation and the world -- overpopulation, diminishing resources, threats to the life support systems and nuclear war.
> **Reagan**: I don't know about it.
> **Reporter**: The American people want to hear your answers to these issues.
> **Reagan**: I will when I get a chance to familiarize myself with it. . .

Apparently, the exchange was taken seriously enough for Reagan to read the summary of the report, or have one of his staff look it up on his behalf. That would explain why, after receiving a copy the following day, he responded: "Well, I have only read the highlights from the report and so I am not prepared to respond at this time." To the question of what he thought about the highlights, he seemed to gravitate more to a critique of its tone than a substantive understanding of its conclusions or general purpose. "Well, you know there was this fella named Malthus who thought we were going to run out of food. But Malthus didn't know about fertilizers and pesticides."[59]

[59] 1980 Press Clips, Barney Papers

That the report was hardly discussed during an election year was hardly surprising. While Carter occasionally acknowledged the report's existence while campaigning, the report seemed to not have as much of a draw in national presidential politics. Even those democrats close to environmental issues were not surprised by the lack of interest. As John Dingell (D-MI), Chairman of the House Subcommittee on Energy and Power, conveyed in his congratulatory note to Gus Speth on completing the report, "efforts of this type are particularly difficult to understand in an election year, but I must say that those issues and those problems transcend political elections and require sustained efforts on the part of the government."[60] The issues were complex, the issues were global, the issues were beyond current environmental thinking.

JULIAN SIMON, THE MEDIA, AND *RESOURCEFUL EARTH*

Contrary to the desires of Gillman, Kytle, Barney, and others, Global 2000 was largely framed as a disaster report -- with a slight caveat. "For the first time, the U.S. Government has added its full voice to the chorus of environmental Cassandras deeply distressed about the future," *Time Magazine* began in a 1980 article entitled, "Toward a Troubled 21st Century."

> As compared with such doomsday forecasts as that of the Club of Rome's 1972 *The Limits to Growth*, which predicted mass starvation, political chaos, and general catastrophe by the middle of the next century, the study is cautiously restrained, even muted, giving its warnings more impact in a way.[61]

Like many articles that appeared in the wake of the public release of the Global 2000 Study, whether in journals, popular magazines, or newspapers, the study was immediately framed in terms of a pre-existing gloom-and-doom discourse.

Time's characterization of the study provides a curious spin on what had been a traditional depiction of "alarmist" claims about the future. Since the early 1970s, prophecies of

[60] John Dingell to Gus Speth, 23 September 1980, JC-DPS, Box 34 -- Global 2000 (1), Carter Library, Atlanta, GA
[61] "Toward a Troubled 21st Century: A Presidential Panel Finds the Global Outlook Extremely Bleak," *Time Magazine* (4 August 1980): p. 54

future catastrophes were frequently defined as an exaggeration of future threats disproportionate to the evidence, a privileging of emotion over reason, or a fixation on preserving natural habitats and animal species over the welfare and needs of human beings. In contrast, as illustrated in *Time Magazine*, concerns about the future was not a result of soothsaying and wolf-crying; instead, the warnings gained potency by avoiding these traditional rhetorical strategies. By acknowledging the uncertainties and methodological difficulties, the report's cautiousness acquired greater potency. This characterization of doom-and-gloom resembled the opinion of James Kilpatrick in a 1981 article, entitled "The Coming Catastrophe." Published in *Nation's Business*, a business advocacy magazine published by the US Chamber of Commerce, Kilpatrick noted:

> Yes, this is a gloom and doom report, made all the more disturbing by the constrained and dispassionate style in which the authors lay out the evidence. But it is not a report without hope. Their message, finally, is that the impending catastrophe can indeed be averted -- or at least minimized -- if sensible public policies are put in motion now.[62]

Indeed, that it was perceived as a gloom-and-doom report by the media only confirmed what others had long though.

In 1981, Julian Simon, an outspoken economist at the University of Illinois Urbana-Champaign published a piece in the journal, *Public Interest*. Having read many of the accounts published in the general media, as well as reviewing the contents of the Global 2000 report, he began a campaign to counter the report's pessimistic claims and, more broadly, what he saw as an attempt to inaugurate a neo-Malthusian agenda by the federal government.[63] Cautioning

[62] Kilpatrick, James, "The Coming Catastrophe," *Nation's Business* (February 1981): p. 17
[63] Aligica, Paul, "Julian Simon and the 'Limits to Growth' Neo-Malthusian," *The Electronic Journal of Sustainable* Development 1, 3 (2009)

readers that this was not an attempt to paint an overly rosy future, he asserted that "false bad news is a very real social pollution, and a dangerous one."[64]

His argument essentially boiled down to six main points. First, he believed that the rushed manner in which the report was actually prepared prevented a "careful, thoughtful piece of work."[65] There was some truth to this, as revealed in the final report: "The pressure of short-term tasks often requires the agencies' modeling experts to make expeditious simplifications, which are rarely reviewed in depth and which are difficult to revise once made."[66] Second, he believed that the integration of existing agency models was "inappropriate," and provided little more than "useless hodge-podge."[67] Third, the report privileged "pure logic as a policy guide" rather than historical experience which would show that human ingenuity frequently results in humanity being better off. In his mind, Gillman relied on "technological thinking" which privileges the technical aspects of computer-based models, whereas he privileged "trends of experience in the past." Fourth, he suspected that "organizational self-interest" was at work within the CEQ; by dramatizing environmental threats, Speth and Warren could more easily justify the CEQ's role in environmental affairs. Fifth, he argued that an emphasis on bad news was an attempt to increase its publicity. Lastly, he argued that the report was prejudiced toward the environmental movement's interests, which appeared to emphasis future danger instead of future progress.

While there is some truth to each of his objections, his broader point was less epistemic and more philosophical. Even within a democratic society, he believed, there was no check on what the government could release upon the American people. "There is no censor. Nor is there

[64] Simon, Julian, "Global 2000: A Hard Look at the Global 2000 Report," *Public Interest* (Winter 1981): pp. 3-20, quote on p. 4
[65] Simon (Winter, 1981), p. 4
[66] Global 2000 Study, Vol. 2, p. 483
[67] Simon (Winter, 1981), p. 5

a 'truth auditor' on the government payroll the way there are financial auditors to monitor money irregularities," he noted. "The very best scholar is likely to judge that it is more important to get on with his or her own work rather than try to act as a one-person truth squad. Journalists seldom have the time and patience for deep digging into the scientific literature. So -- who is there to stop them?"[68] For him, the pessimistic tone of the report was disturbing, but he resented more viscerally what appeared to him as the tyranny of the federal government to dictate what the public believes about the future. The effect: the government became the arbiter of knowledge and truth until overturned either by historical experience or the next "GTS lookalike comes along, at which time the new authority will supplant the old without causing any changes at all." The ineffectual nature of government-sponsored pessimism seemed nothing short of tragic.[69]

Having learned of Simon's article in Public Interest, Kitty Gillman, the author of the report's summary volume, believed that a response was warranted. To begin, she admitted that his style as a writer is readable, self-assured, and may even be convincing to the initiated, she characterized him as nothing more than a "professional Pollyanna" that offers "little genuine insight."[70] She criticized his apparent optimism that people will always find a way out of trouble, and reminded Simon -- and the readers of *Public Interest* -- that Global 2000 did not predict future catastrophe as if it were set in stone. Instead, she reaffirmed, it was a set of projections should current trends continue unabated. She dismissed his reliance on a "historical perspective" and privileged instead the "far more sophisticated approach of building up projections from component parts, reflecting analysis of causes and consequences in the real world." Built on "ecological insights" and a more careful treatment of data, she ultimately

[68] Simon (Winter, 1981), p. 19
[69] Simon, Julian, "Global 2000: A Hard Look at the Global 2000 Report," *Public Interest* (Winter 1981): pp. 3-20.
[70] Gillman, Katherine, "Julian Simon's Cracked Crystal Ball," *Public Interest* (Fall 1981): pp. 71-80

concluded that history had little role in guiding future policy decisions; models and ecological understanding of nature were the real arbiters of truth.[71]

In spite of what she saw as Simon's predominantly ill-founded objections, Gillman agreed that the government's projections contained "structural gaps and inconsistencies," and that the agency models used "lack common assumptions, consistent methods, and adequate links." However, this was no cause to interpret Global 2000 -- as Simon had -- as an overstatement of future dangers. To the contrary, she believed that these deficiencies probably "understate the seriousness of the global problems lying ahead." But, she maintained, it was not a gloom and doom report: "Global 2000 has sometimes been labeled, not only by Simon but by others as well, as a 'gloom and doom' report. It is not. Rather, it is a warning and a call to action."[72]

Publishing a rebuttal, Simon felt that the media's interpretation of the Global 2000 Report provided room to question Gillman's latter claim that it was not a gloom and doom report. "Then why did every journal that came into my home interpret it that way?" he retorted. "One can understand Gillman wanting to work both sides of the street -- warning that things are getting worse but at the same time saying that the report is not of gloom and doom. But the facts do not permit her to have it both ways."[73] This was the fundamental question, and he failed to understand the logic driving Gillman's claims. For what he saw was word-play, not analysis. The issue came down to the following: is a claim of future gloom and doom any less so if accompanied by a qualification that things will get better if certain steps are taken? For him, the answer was no.

[71] Gillman, Katherine, "Julian Simon's Cracked Crystal Ball," Public Interest (Fall 1981): pp. 71-80, quote on p. 72
[72] Gillman (Fall 1981), p. 80
[73] Simon, Julian, "False Bad News is Truly Bad News," *Public Interest* (fall 1981): pp. 80-89, quote on p. 80

He was not alone. In his critical review of Global 2000 published in May 1981 in the *Wall Street Journal*, French-born microbiologist Rene Dubos specifically addressed this point. "Time and time again," he began, "statements are preceded by the phrase 'If present trends continue.' What a big 'if' that is. Human beings hardly ever remain passive when faced with dangerous or unpleasant situations. . . the cautionary statement 'if trends remain the same' is completely useless because, wherever human beings are concerned, trend is never destiny."[74] For him, the qualification used by Gillman and the Global 2000 team entailed little to no meaning if only because it appeared so axiomatic. He continued to challenge the assumption frequently used by environmentalists that publicizing bad news is an effective way to alert the public to action. To make his case appear stronger, he referred to Philip Handler's congressional remarks in 1970 that "nations of the world may yet pay a dreadful price" for the hyperbolic claims of scientists. True to the middle ground perspective, he believed that prophecies of doom frequently outshine the strains of genuine truth within each. "There is much truth of course in the publicized expressions of concern about the state of the world, but it might be helpful to publicize also that improvement is taking place in many situations."[75]

In addition, Simon utilized the words of agricultural economist Marion Clawson, who questioned the distinction between forecasts and projections. "If the projections are made from assumptions that have been chosen because they seem reasonable, the result may not be much different from a forecast based on what analysts thought would most probably happen. The projection/forecast distinction thus becomes mostly semantic."[76] What he meant was that no one ever seeks to project into the future based on unreasonable assumptions, and that forecasters

[74] Dubos, Rene, "Half-Truths about the Future," *Wall Street Journal* (8 May 1981): p. 26
[75] Dubos, Rene, "Half-Truths about the Future," *Wall Street Journal* (8 May 1981): p. 26
[76] Clawson, Marion, "Entering the Twenty-First Century -- The Global 2000 report to the President," *Resources* 66 (Spring 1981): pp.19-21

would never seek to base a forecast on what they deem to be the most likely outcome. Projections and forecasts are fundamentally based on what people believe about the future, because the future is ultimately unknowable in the first place.

For Simon, the credibility of the Global 2000 was suspect because it appeared to be methodologically flawed, because it was allegedly premised on people's *a priori* beliefs about future threats, and because it seemed to reinforce what he saw as government using its weight to scare people into submission. But he also thought it was about one side misinterpreting the other side's position. For him, Gillman inaccurately claimed that Simon is a "professional Pollyanna." In his mind, she willfully ignored his own cautious belief -- as evidenced in the article she criticized -- that he was not intending on painting a rosy picture of the future. While he does see human progress and the gradual elimination of problems, "that does not at all imply that things are fine now, or that they will surely be dandy in the future. I have tried to make this clear, but either I have not found a way to say it well enough, or Gillman and others do not want to hear the message."[77] This, as he saw it, was about two individuals who genuinely had the best interests of society at heart but whose philosophical worldviews were fundamentally different. His "historical perspective" against her "technological perspective", his dismissal of the forecast/projection distinction and her acceptance of it -- these issues mattered for those who believed that American society was becoming saturated with claims of future doom.

To stem the apparent wholesale acceptance and influence of bad news, Simon believed that his articles were insufficient. In 1984, he and prominent futurologist Herman Kahn published *Resourceful Earth*, which was conceived as a counterweight to the dire tone of Global

[77] Simon (Fall, 1981), p. 82

2000.[78] Enlisting the efforts of many of the most prominent scientists in their respective fields, Simon believed that something needed to be done that put Carter's effort into what he believed to be the proper perspective.[79]

In contrast to Global 2000, which was authored by government staff, the essays collected under the aegis of *Resourceful Earth* were written by *independent* experts in their respective fields. For Simon, the agency experts who contributed to Global 2000 were subject to "bureaucratic editing" which only served to reduce the range of opinions and "emphasize conventional views" within the environmental movement.[80] Conventionality was considered the antithesis of genuine scientific discussion and debate, and Simon believed that the bringing together of a variety of non-governmental experts would allow room for truth to emerge, even if those truths were deemed unpopular or inconvenient.[81] This criticism was not without merit. In the Global 2000 report itself, the authors described the risk of producing a report via integrated analysis of a centralized team of experts. As they note, if "used as a primary mode of analysis, it would concentrate political power by giving an integrated analysis group a direct line to the policy maker, while making it only marginally answerable to outside analyses. . . It may err due to deficiencies in analytic methodology or to the analysts' personal biases."[82] The rationale: because government experts were not independent from the Carter Administration, and were attempting to cast Global 2000 as an environmental and Cold War statement, it could not be trusted as a genuine reflection of science.

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[78] Simon, Julian and Herman Kahn. *The Resourceful Earth: A Response to 'Global 2000'* (Blackwell Publishing, 1984)
[79] The authors include: Mark Perlman, Julian Simon, D. Gale Johnson, John Wise, Roger Sedjo, Marion Clawson, Aaron Wildavsky, Roger Revelle, Earl Swanson, Earl Heady, Gilbert White, Steve Hanke, John Hart, Helmut Landsberg, Harold Barnett, S. Fred Singer, William Brown, Karl Cohen, Petr Beckman, William Baumol, Wallace Oates, Martin Holdgate, Mohammed Kassas, A.E. Harper, Richard Peto, Bernard Cohen, and Steve Hanke.
[80] Resourceful Earth, Introduction
[81] Simon's concern over convention was not without merit. A close analysis of early guidelines for the Global 2000 study in 1977 suggests concern that individual insights could get lost in "bureaucratic compromise." 1977-08 Chronology, p. 14
[82] Global 2000 Report, Vol. 2, p. 603

Second, the *Resourceful Earth* was believed to be more trustworthy because the reputation of each contributor was on the line. Contrary to Global 2000, the authors could not hide behind a wall of bureaucracy. Third, Global 2000 was produced by government agencies with presumed “axes to grind,” implying that the independent experts who contributed to *Resourceful Earth* did not have such liabilities. Fundamentally, the analyses published in Resourceful Earth were more reliable because they were produced by *independent* and *neutral* experts.

A closer look, however, reveals that the report was not as clean and neutral as supposed. In fact, it was sponsored by the renowned and highly influential conservative think-tank, the Heritage Foundation.[83] Established in 1973, the Heritage Foundation was envisioned as a counter to the liberal establishment’s ability to influence public policy-making.[84] With the rise of what historian Bruce Schulman characterizes as broad-spectrum conservative organizations, the foundation’s overall mission was to promote and safeguard traditional American values and free-market ideology with an astute eye to public relations, in effect translating conservative philosophy into tangible policies.[85] Instead of the New Rights general focus on social issues (e.g. gay rights, abortion), the foundation focused more on ways to influence economic and foreign policy/national security discussions in congress. By the 1970s and early 80s, the Heritage Foundation grew enormously and became one of the leading think-tanks in Washington, enough for President Reagan to remark that the foundation had become a “major

[83] In the Preface of *Resourceful Earth*, Simon expressed his gratitude to a number of prominent members of the Heritage Foundation, namely 1) Burton Yale Pines (VP of Research), 2) Herb Berkowitz (VP of Public Relations), 3) Edwin Feulner, Jr. (President), 4) Richard Holwill (VP for Government Relations), and 5) Phil Truluck (Director of Research).

[84] For a focused history of the Heritage Foundation, see Edwards, Lee. *The Power of Ideas: The Heritage Foundation at 25 Years* (Ottawa, Illinois: Jameson Books, Inc.). For a broader history of think-tanks in American society, see Smith, James. *The Idea Brokers: Think Tanks and the Rise of the New Policy Elite* (New York: The Free Press, 1991)

[85] Schulman, Bruce. *The Seventies: The Great Shift in American Culture, Society, and Politics* (New York: The Free Press, 2001): pp. 196-197

player in national policy."[86] The international ramifications of Global 2000 may explain the foundation's interest.

Since the late 1970s and early 80s, Helmut Landsberg was aware not only of Global 2000 but also Simon's attempts to instill what he agreed was a more valid assessment of environmental risks. For instance, in 1980, Landsberg wrote Simon a congratulatory note for his willingness to speak out on these kinds of issues: "I hope your analysis will shake up some of the misleaders. I have tried for years to set the record straight as far as climate is concerned but it is far more popular to predict an ice age or hell on earth."[87] This was only the beginning, because two years later Simon asked him to contribute to *The Resourceful Earth*. The purpose of Simon's proposal: 1) provide a more "scientifically sound" analysis of climate for policymaking, and 2) "displace the misleading hodgepodge of doomsday projections which constitute the Global 2000 report." Landsberg was chosen specifically for his reputation within the atmospheric science community, which would allow a "respectful reception" of his less pessimistic version of the future. Simon was astutely aware of the political ramifications of having this study funded by the Heritage Foundation, a connection that he deemed to be under threat from the "opposition." Stressing the importance of confidentiality, Simon went so far as to request that any materials pertaining to their correspondence be returned should Landsberg decline the offer.[88] His fears were unnecessary -- Landsberg agreed to participate.[89]

For Landsberg, the Global 2000 Report appeared to cross what was often a fine line between projections to predictions. While much of the climate section of the report was "quite sound," Landsberg criticized the authors' tendency to omit model uncertainties. This was

[86] Smith (1991), p. 196.
[87] Helmut Landsberg to Julian Simon, 24 July 1980, Papers of Helmut Landsberg (University of Maryland Hornbake Library), hereafter known as PHL; Julian Simon to Helmut Landsberg, 28 May 1982, PHL
[88] Simon, Julian and Herman Kahn, eds. *The Resourceful Earth: A Response to Global 2000* (New York: Basil Blackwell, 1984)
[89] Helmut Landsberg to Julian Simon, 4 June 1982, PHL. Each contributor received a $1000 honorarium plus expenses.

especially relevant with regard to the claim that rising atmospheric levels of carbon dioxide would lead to great increases in polar temperatures and subsequent flooding of coastal cities. "This certainly carries us far beyond the year 2000, presumably to the twenty-second century," he attested. "How far into the future should be the project? Is the cut-off point 2000, 2025, 2050, 2100?"[90] His rhetorical critique had both merit and strategic value.

The intention of the report was to provide the most advanced projection of the future to the year 2000 for use by policy makers. As one who consistently believed that the doubling of carbon dioxide would not take place until well-into the 21st century, the mere mention of a carbon dioxide doubling exceeded the report's intended purpose. Likewise, any discussion of the future effects of carbon dioxide without the requisite uncertainties would distort understanding, and undermine the intent to inform policy makers. Agreeing with Simon's methodological commitment to a "historical perspective" and optimism regarding human ingenuity, Landsberg clearly had an eye on reinforcing the self-imposed boundaries of the report's original authors:

> The climate, both globally and locally, has not radically changed in the past few centuries. It is therefore not unreasonable to assume that it will stay within the range of previously observed values and events. To the year 2000 this is a safe expectation. It is likely to hold also to 2025. Should a 0.5C global temperature rise occur (from CO_2 or any other cause), it is unlikely to cause ecological upsets. Similarly, should it get globally 0.5C cooler it would be entirely the realm of experiences of the not-too-distant past. Mankind has been able to cope with such variations. There is little doubt that technology can remedy any difficulties which may arise.[91]

By projecting future harm should present trends continue, Landsberg saw something reminiscent of past concerns. "There seems to be a bit of the 'Jupiter Effect' that has crept into the report," he intoned.[92] This was no small claim. For a decade, the Jupiter Effect had

[90] Resourceful Earth, p. 304
[91] *Resourceful Earth*, p. 299
[92] *Resourceful Earth*, p. 303

represented to Landsberg a mark of prophetic hubris. By presenting the future in such dire tones, he argued, the report directly fed into the media's tendency to sensationalize and manipulate scientific claims.[93] "The dire tone of impending doom has little place in a scientific analysis . . . the conclusions drawn from them in the news media picked only the frightening aspects up for communication to the public: a dangerously heated earth, deluges, and crop-damaging rains." For Landsberg, the climate section of the report represented the continuation of a pattern that had begun in the late 1960s, a pattern he frequently sought to disrupt.

DIVERGENT POLITICAL REALITIES

In 1983, Ned Dearborn, one of the more passionate architects of the report, published a scathing article on the administration's apparent unwillingness to take advantage of the report's insights about the future -- this in spite of widespread public interest. According to Dearborn, the government lacked the ability to plan for the future and that actions were not being taken to ameliorate the situation. Warning the readers of his "highly partisan" perspective on the Reagan administration, he outlined the risks of ignoring the report.

> Imagine, for a moment, a huge oil tanker with a multimillion dollar cargo at sea in a dense fog. According to the ship's radar, which is known to be only marginally reliable, massive threatening shapes seem to be looming in the distance, dead ahead. The ship's captain knows that the tanker requires considerable time to change course. Even though the threatening shapes are only ill-defined, it seems only prudent for him to begin changing course at once to preserve his options, as well as those of the crew and the cargo. in the meantime, urgent work needs to be undertaken to improve the ships radar.[94]

For Dearborn, the Reagan Administration was not looking at the radar.

For others, however, the report was making more than enough progress in political circles. In 1985, Stephen Moore, a researcher at the Heritage Foundation, published an analysis

[93] For the sake of expediency, I refrained from engaging with the hundreds of pages of articles in newspapers and other media pertaining to the Global 2000 Study. Suffice it to say, Landsberg and Simon were correct that Global 2000 did spawn a media frenzy that largely focused on future doom.

[94] Dearborn, Ned, "Global 2000: Radar for the Ship of State," *Futures* 15, 2 (April 1983): pp. 111-125

of what he described as the growing influence of the study in Congress and federal agencies, other nations, education, and within the environmental movement. While noting the trickiness of assessing the influence of a single report on policy, he noted the "flood of costly futures reports" within the federal government. He also cited what he believed to be the increasing link between "foresight legislation" and concerns over centralized planning while depicting federal efforts to promote the report as a "crusade." By framing it as the latest version of the 1972 *Limits to Growth* report, Moore expressed concern that it was vastly more influential because it represented the official position of the federal government. He believed that it was merely a vehicle to inject a "limits to growth mentality" into living rooms across the country, as well as schools and in nations around the world. "Global 2000 is a case study in the abuse of planning and forecasting as science," he concluded.

> To assure immediate, dramatic impact on the course of policy making, alarmists project long-term dire consequences on the basis of apparent trends in short-term data. Typically, the impact of technological change and human ingenuity is ignored. The Reagan Administration and leaders in Congress should ensure that Global 2000's flawed premises and inaccurate forecasts do not become the basis for future federal policy.[95]

Moore's claims perhaps reflected his own perceptions than an accurate assessment of the influence of Global 2000's dire tone on international environmental politics. In November 1981, the NATO Committee on the Challenges of Modern Society (CCMS) met to discuss long-term global forecasting developments, and the role and influence of "prophets of doom" within environmental politics. That they did so was not surprising. The CCMS was founded in 1969 under pressure by President Nixon to promote East-West cooperation during the Cold War. Under the rubric of what soon became known as "environmental diplomacy," the committee itself was envisioned as an opportunity to share knowledge and experience about technical,

[95] Moore, Stephen, "Half-Truths and Consequences: The Legacy of Global 2000," *The Heritage Foundation Institution Analysis* (7 January 1985): pp. 1-16

scientific, and policy aspects of social and environmental matters. Given the complexity of environmental problems, and the geopolitical tensions of the period, the intention was to use the environment as a vehicle for diplomatic collaboration, and jump-start national and international awareness of humankind's impact on the environment. To understand those problems that demanded international cooperation, the committee frequently surveyed the environmental landscape.[96]

The delegations from the United States and various western European countries were particularly interested in the release of President Carter's recent global model in the summer of 1980, entitled *The Global 2000 Report to the President: Entering the 21st Century*. There was good reason. Having sold hundreds of thousands of copies within a year of its publication, and translated into a half-dozen different languages ranging from Spanish to Chinese, contemporaries characterized the study as the "great American forecast" and considered a "genuine bestseller in the world of futurology."[97]

That the CCMS met to discuss the report was not surprising; more surprising is how the delegations framed the report. "The question that faces us now," noted the Belgian delegation, "is whether the recent analysis will go the same way as earlier prophecies of woe, and be replaced by a return to optimism. Or whether we have, at last, reached the point at which such forecasts have to be taken seriously and to form the basis for action at national and international levels." For the delegation from the Netherlands, there seemed to be little role for such rhetoric: "It seems unlikely that the prophets of doom will do much to prevent the developments they predict." The American delegation appeared more restrained, limiting their judgments by describing what appeared to be the influence of such discussions on the study's reception.

[96] See, Duignan, Peter. *NATO: Its Past, Present, Future* (Stanford: Hoover Institution Press, 2000): p. 63
[97] Testimony of Thomas Pickering, Hearing before the Subcommittee on International Economics of the Joint Economic Committee, 4 September 1980, p. 14

"Global 2000 has also been sharply criticized from various points of view," it noted. "Some have argued that it is too 'doomsday' in tone and conclusion, and unrealistically alarmist since it all but ignores the important strides that technology and institutions have been making of late in addressing many of the problems."[98]

CONCLUSION

Global 2000 intensified deep philosophical divisions within American culture over how to properly communicate what environmentalists considered serious dangers. On the one hand, Kitty Gillman articulated a vision that said that Global 2000 was not a "doom and gloom" report because of the inclusion of a caveat that allowed the possibility to implement policies to prevent future catastrophe. She, along with the Global 2000 team, represented a new kind of environmentalism that could showcase the value of the federal government to preventing future catastrophe. Global 2000 embodied what she believed to be an effort by the federal government to create an ecological perspective of global environmental problems, and believed that model-based analyses could allow better planning in the future.

On the other hand, critics perceived Global 2000 in an entirely different way. While they agreed that problems exist, figures like Julian Simon vehemently chastised the report not only as unscientific but indicative of a broader campaign by "prophets of doom" within the Carter Administration to stifle debate about the environment. In contrast to Gillman's perspective, Simon believed that the report embodied all of the characteristics of a gloom and doom report. In addition, he believed that Gillman's use of modeling and an "ecological perspective" provided a false understanding of the future. Instead, he privileged what he considered to be a "historical perspective" that took into account how human civilization has become more healthy and safe.

[98] Speaking Notes from 1981 CCMS Meeting, Gerald Barney Papers

While Gillman emphasized what appeared to be a pessimistic view of the future, he believed that present trends could more accurately be characterized in an optimistic way.

But, Simon's perspective was not entirely aligned with Landsberg and Singer. When Landsberg and Singer created the AGU CEQ in 1970, they sought to counter what they believed to be the uptake of catastrophic predictions by those they usually referred to as "alarmists" or "prophets of doom." Overly dire predictions of the future, they believed, were not justified because of significant uncertainties within the science. Additionally, such claims by scientists themselves could undermine expert authority over the long-turn, and inflame already existing suspicions toward the American government, expert scientists such as themselves, and science as a whole.

When they agreed to contribute to Julian Simon's project, *The Resourceful Earth*, their campaign -- perhaps unwittingly -- evolved. On one level, Simon was not like Singer or Landsberg because he did not fit the prototype a member of what they considered the scientific elite. Whereas Simon was an academic economist with very few ties and influence outside of his own discipline, Singer and Landsberg had a set of credentials and experience that far outweighed Simon's. This is not to say that Singer was as experienced or credentialed as Landsberg, particularly given the twenty year age difference, but they were tied into experts at the highest levels of academia and the federal government. Nonetheless, the three did have a common cause in their attempts to minimize what they all believed to be "alarmist" rhetoric within environmental politics and popular discussion. All three were suspicious of the environmental movement and they all agreed that engaging the general public was important. They agreed in the virtue of being proactive in the defense of what they thought was a more sober assessment of environmental challenges.

On another level, Simon's distaste for what he saw as "prophets of doom" -- as exemplified by those who authored Carter's Global 2000 Report -- was motivated by a separate set of issues when compared to the earlier efforts of Landsberg and Singer. While Singer and Landsberg could in their minds represent what they believed to be a middle ground (i.e. not politically partisan) during the late 1960s and 1970s, their alignment with Simon introduced a kind of conservative politics into their agenda. While they may have had a common cause toward what they considered exaggerated rhetoric toward environmental threats, Simon's affiliation with the far right via the Heritage Foundation and outright support of the Reagan Administration cast a political and ideological connotation on the middle ground agenda that had not existed before in any coherent and explicit form.[99]

The middle ground agenda advocated by Singer and Landsberg had become interwoven with conservative politics; their agenda had become mainstream enough to represent an opportunity to criticize the federal government's role in environmental politics and thereby mobilize those on the far right who were 1) critical of the Carter Administration and 2) supportive of the Reagan Administration. While Singer and Landsberg did carry an aversion to what they believed to be ad hoc policies in response to rhetoric on the far left, they were not outspoken critics of government nor government-funded science. They were not as vociferously driven by free-market ideology as Simon, and believed that government did have a place in regulating industry and businesses when circumstances permitted. With the release of Global 2000, it appeared that the "middle course" of the AGU CEQ had evolved into a conservative

[99] While it is not explored to the fullest extent possible here, Simon frequently published in the Heritage Foundation's *Issue Bulletin*, particularly in the wake of the release of the Global 2000 Report in 1980. See, Simon, Julian, "The 'Global 2000' Juggernaut," *Heritage Foundation Issue Bulletin* 24 (13 May 1983); Simon, "Global Foresight Bills: the Danger of Centralized Data Sources," *Heritage Foundation Issue Bulletin* 103 (24 February 1984): pp. 1-12; Simon, "Treating People as an Asset," *Heritage Foundation Backgrounder* 367 (13 July 1984): pp. 1-8

movement not only against what they believed to be "prophets of doom" but also those on the political left.

CONCLUSION

As partially explored in Chapter Two, renowned climatologist Helmut Landsberg stated his belief in 1973 that there was little reason to be alarmed about the role that humans were playing on the global climate. Directed to those whom he frequently characterized as prophets of doom, he seemed particularly concerned that vigilant alertness was being conflated erroneously with alarmism in popular discussions of humanity's role on the climate system. "A total assessment of the global situation of man's possible influence on climate," he suggested, "leads one to conclude that there is no present substantive evidence to be alarmed, but there is every reason to be alert to the possibilities."[1] By this, he meant that scientists had every reason to suspect that humans could potentially cause global changes in the long term by continuing to emit greenhouse gases into the atmosphere, but that the current state of climate science did not justify what he perceived to be public panic or hysteria about its implications in the short-term.

Six years later, Landsberg repeated his distinction between alarmism and alertness while presenting a paper at the Center for Philosophy and Public Policy at the University of Maryland in 1979 on what he called the "CO_2 Problem." For him, the scientific evidence seemed credible enough to warrant a concerted effort by scientists to understand the nature and scope of any human-induced climate changes, but that extant knowledge was insufficient to justify large-scale national and international decisions beyond further research. As he noted,

> Obviously our knowledge is very deficient. Therefore the decisions, both on a national and international level, to find as many of the missing pieces of the puzzle as promptly as possible is the correct one. It is equally important to keep close watch on the atmosphere, its composition and its physical condition. Being alert is wise, being alarmist is foolish.[2]

[1] Landsberg, Helmut, "Man's Effect on the Atmosphere," Series 3, Box 6, PHL
[2] Landsberg, Helmut, "Energy Use and the Atmosphere," Series 6, Box 2: Working Group on Energy Policy, 1979-80, PHL

To be clear, Landsberg's apparent restraint was not an attempt to marginalize or construct unnecessary doubt about what seemed like a serious possibility that humankind could, over the long term, influence the global climate. Like many other atmospheric scientists, he increasingly felt that that carbon dioxide was the "only serious anthropogenic threat to global climate."[3] His concern was with those whom appeared unrestrained in their characterizations of the climate problem.

Landsberg's problem with those he deemed to be advocates and prophets of doom was what he envisioned as the apparent disconnect between their expertise in climate-related matters and their level of authority to publicly speak on scientific matters -- especially those matters that pertained to political decision making (e.g. climate change). For him, as shown in chapters one and two, ecologists like Lamont Cole, physicists like Howard Wilcox, astrophysicists like John Eddy, and science writers like Nigel Calder appeared to breach a kind of disciplinary etiquette. They publicly spoke on science-based matters that were highly uncertain, appeared to veer off into exaggerated statements that occasionally conflicted with the statements of others, and therefore appeared to risk the credibility of his own professional ideals of reticence and restraint.

This dissertation has traced how he and a few of his colleagues -- Abelson, Handler, and Singer -- attempted to monitor and regulate how such discussions were framed within the public sphere. Their claims suggest that more was at stake for them, however; the professional reputation of the scientific community seemed to be at stake in light of what appeared to be overly sensationalized claims of future doom without equal care to stating the uncertainties embedded in such visions of the future. Identifying and communicating uncertainty was one of the foremost responsibilities of a scientist. For Landsberg, especially, scientists had a core set of

[3] Landsberg to James Norwine, 3 June 1977, Series 2.2, Box 8, PHL

responsibilities that, if carelessly or haphazardly ignored or unwittingly overlooked, could result in science becoming increasingly politicized and biased. As he noted in 1978,

> For the scientist it is important to do research and publish his results. If a technical matter in his purview has humanitarian or socio-economic implications he should share his knowledge. But he also has the obligation to state the degree of certainty of this knowledge. If it is inadequate he must not advocate for courses of action which may be heavily influenced by personal bias. He should advocate for further research to improve knowledge.[4]

As revealed in chapter three and four, these issues were instrumental in guiding the decisions of Singer and Press as they attempted to guide how policymakers understood the state of science. For Singer, public claims about the future impact of a fleet of supersonic transports on human welfare seemed premature. MacDonald, for instance, frequently noted with urgency -- in spite of his own admission that the scientific evidence had not yet been formally vetted -- that supersonic transport emissions could not only cause global climate changes but also cause cancer rates to increase substantially within the United States. For Singer, these statements appeared to misrepresent the available evidence and recklessly contribute to what he saw as emotional side of anti-technologists within the environmental movement. For McDonald, however, the issue was clear: SST's -- if built -- posed a long-term problem for humanity. It was best, according to him, to avoid starting on down the road of SST production before humankind unwittingly produces the problem in the first place.

For Landsberg, his role appeared to be more hidden within the broader context of SST debates. As someone who firmly believed that the reputation and credibility of scientists was in the manner in which they control and regulate information, he questioned others if they should appear to contribute to what he perceived to be the emotional environmental politics of the SST controversy. While he never testified in front of Congress about the matter as Singer and

[4] Landsberg, Helmut, "Energy Use and the Atmosphere," Series 6, Box 2: Working Group on Energy Policy, 1979-80, PHL

McDonald had done, he sought to reprimand the apparent carelessness of epidemiologist Gio Gori for releasing what appeared to be unreliable assessments of cancer rates to Senator Proxmire. This, he believed, directly illustrated the fragile relationship between science advising and political partisanship. While Gori appeared to follow a reasonable protocol when attempting to advise Proximire (i.e. speaking to other professionals prior to responding to Proxmire's request for more information, gathering relevant information from existing scholarship, conveying the state-of-science as he knew it), Landsberg expressed great concern that these actions merely contributed to political brinksmanship and risked the credibility of professional scientists to speak on matters with important implications for the future welfare of humanity. Indeed, it appeared as if Gori had indirectly engaged the general public via Proxmire, and therefore allowed what he saw as un-vetted science to leak into the public sphere as political weaponry.

How science was communicated to policy makers was crucial to maintaining a sense of status and authority among some elite members of the scientific establishment. As examined in chapter four, President Carter's science adviser Frank Press frequently expressed his concerns over how climate knowledge was being implemented within national policy making. Within the context of deliberations regarding the National Climate Program Act, Press frequently expressed concern that policy makers were developing legislation that privileged user needs over the need of scientists to conduct a national climate research agenda. While Press believed that the primary responsibility of a science advisor was to serve the interests of the president, he also believed that the state of climate science was insufficient to justify legislative activities beyond further research funded by the executive branch. Contrary to the interests of Congress, he believed that accommodating user-needs would lead to unduly raising the expectations of the general public, and not only risk the credibility of the federal government but also scientists themselves.

Rather than using the language of doubt to mask an ideologically partisan agenda, my interpretation of the archival evidence suggests that Press was acting in good faith and in strict accordance with his role as the president's assistant as well as his stature as an esteemed geophysicist. He frequently advocated what he considered caution and restraint to both Congress and the President, and his concerns over the science of climate forecasting appeared to extend even to his own field of specialization -- seismology. His confidential correspondence to the President appears consistent with his congressional testimony, and there appears little indication that he was disingenuous when he sought what he considered a middle ground between complacency and panic when addressing the future implications of climate change. Indeed, he was one of the growing community of geophysicists and atmospheric scientists who believed that carbon dioxide was a serious problem, but believed that that scientists had an obligation to appear more conservative and cautious when addressing both the public and policy makers. For Landsberg, Singer, and Press, conducting science behind closed doors was fundamental to acquiring genuine understanding of the scope and nature of environmental threats.

Historian of science James Fleming may help historians explain why some of the most esteemed members of the American scientific establishment frequently advised for further climate research and cautious restraint when addressing the public about the severity and urgency of climate change. Within his seminal 1998 book, *Historical Perspectives on Climate Change*, Fleming examines what appears to be the apprehensiveness of humans toward the climate, a term he defines in three distinct ways. First, he defines apprehension in terms of an individual's understanding of the global climate system. By using technologies such as rocketry, balloons, satellites and computer-based modeling, human networks of observers, historical sources like diaries and logbooks, and experimentation on the upper and lower atmospheres, natural philosophers and scientists have committed themselves to demystifying the climate system over the last several centuries. Second, he defines apprehension in terms of anxiety and fear towards climate. For centuries, climate has been perceived as a potential hazard that not only threatens human life, but may undermine entire civilizations. Lastly, he defines apprehension as an attempt to apprehend or control the global climate. If humankind could find a away to harness the climate, or prevent the negative effects of climate, then positive growth could ensue.[5]

As argued in this dissertation, Handler, Abelson, Singer, Press, and Landsberg believed that greater understanding of the climate system was at risk of being supplanted by what appeared to be premature responses based on fear-laden claims of the future. For them, science was principally about data collection and analysis to understand the natural world -- the first definition of apprehensiveness. Scientists had a responsibility to understand the global atmosphere first before engaging with the general public and implementing policies meant to benefit the public, and only until all the facts were reliably understood could they sanction scientists stepping out from behind the curtain of scientific deliberation. For them, scientists were a community, a family, and the general public had little business understanding what goes on within the family until the deliberations have already occurred and disagreements remedied.

Within the context of environmental politics of the 1970s, the stakes were quite high given what appeared to be an over-production of paranoia and hype -- Fleming's second form of

[5] For a historical account of anxieties about climate change, see Fleming, James. *Historical Perspectives on Climate Change* (New York: Oxford University Press, 1998). For an extended historical treatment of attempts to intervene and alter the global atmospheric system, see Fleming, *James. Fixing the Sky: The Checkered History of Weather and Climate Control* (New York: Columbia University Press, 2010)

apprehension. Scientists were at their highest aspirations truth and knowledge-tellers, they believed, not so-called prophets of doom. Their optimism about the role of science and technology in societal affairs was not because they were naive of future risks, as if they advocated sticking one's head in the sand and waiting to see what happens. To the contrary, they were aware of the risks but instead advocated for the importance of establishing what they imagined to be credible and robust foundation upon which decisions could be based. Further research before engaging the public was apparently very useful.

Given what appeared to them as serious scientific uncertainties about a range of important global threats, their aspiration to pave a middle ground appeared increasingly at odds with more 'visible' scientists, or advocates who did not hesitate to inject their own subjective values into national decision making. On the one hand, many credentialed American scientists felt that warning the public of what appeared to be credible threats to human civilization was responsible and warranted. They subscribed to what scholars call a precautionary ethos, according to which uncertainties within science were insufficient to justify postponing meaningful and long-term strategies to prevent or mitigate future threats.[6] For them, what critics referred to as "alarmist" claims were not irresponsible, but rather entirely justified because the threats themselves were genuinely alarming. On the other hand, a core group of elite scientists within the American science establishment subscribed to what sociologists of science call "organized skepticism," whereby scientists properly vet scientific claims until robust scientific truths are produced. This line of thought lead to a suspicion of what may appear to be premature engagement with the general public; without a fairly rigid barrier between the scientific community and the general public, the credibility of experts over the long term may be put at

[6] For a general discussion of the precautionary principle, see Riordan, Timothy and James Cameron, eds. *Interpreting the Precautionary Principle* (London: Earthscan Publications, 1995)

risk, the role of expert authorities may be devalued, federal funding may be cut to science if policy makers believe that scientists have a partisan agenda apart from research, and scientists may end up exacerbating public hostility and skepticism toward science and the federal government itself. Stepping onto the public stage was just as risky as not stepping on stage at all.

During the 1970s, elite scientists really believed that a debate was raging beneath the surface of what appeared to be excessively adversarial environmental politics. As climate modeler Stephen Schneider within his 1976 book, *The Genesis Strategy*, observed,

> For those who were willing to listen, there was a debate raging, worthy of notice. As in other modern debates about aspects of the 'world predicament,' such as population, environment, and resources, articulate 'prophets of doom' were presenting strong evidence to support their theories of imminent catastrophe. Meanwhile, with equal passion, choruses of 'Pollyannas' were countering that history is replete with dire predictions of doomsday, none of which has yet come to pass.[7]

The key phrase -- "For those who were willing to listen" -- suggests that the apparent construction of a middle ground should not be easily dismissed as merely a politically partisan attempt to dismantle a maturing environmental movement. To the contrary, constructing a middle ground appeared entirely rational within the context of serious scientific uncertainties. As described in chapter one, Landsberg and Singer used the institutional authority of the AGU to define what they claimed to be a more reasonable discourse about the urgency of environmental threats. Their claims suggest that they were not Pollyannas nor prophets of doom; they believed that such views were merely two sides of the same coin. Pollyannas appeared excessively optimistic about the future and overly dismissive of concerns over the environment calm public urgency, while prophets of doom appeared to exaggerate the urgency of environmental threats and induce public fear. In effect, both groups appeared to contribute to a simplistic back-and-forth between advocates of the status quo and advocates of a fundamental restructuring of

[7] Schneider (1976), Preface.

American society -- all of which, they reasoned, threatened the integrity of the scientific establishment.

Their advocacy of what appeared to them as a middle ground is not to say that these elite scientists were dispassionate arbiters of knowledge. They frequently became embittered and frustrated by their experiences, and occasionally revealed their true opinions not only toward "prophets of doom" but also the environmental movement in general. Landsberg was highly suspicious of what he saw as the emotionalism of some sectors of the general public, and grew concerned with what he imagined to be America's slide into an "unenlightened age." Singer was also very critical of the environmentalists as a whole, and appeared to acquire a positive reputation among many in the industrial and business communities that were frequently seen by environmentalists as entirely partisan and anti-environmentalist. They were passionate, and harbored jealousies when they felt that their perspectives as experts were either underrepresented or ignored entirely.

If their goal was to prevent the further deterioration of environmental politics, their efforts to construct what they considered a reasonable middle ground became more difficult within the release of Jimmy Carter's Global 2000 Report in 1980. As examined in chapter five, this report appears to have widened the fissure between so-called prophets of doom and Pollyannas, and make a middle ground appear even less reasonable as a way to guide how the general public understood environmental politics. For many environmentalists and who many pejoratively called alarmists, Carter's signature environmental achievement was seen as a validation of their concerns. The American federal government had put together what many claimed to be the most sophisticated model of the future ever-produced, and its gloomy forecast only reaffirmed what many believed all along -- the world really did appear to be on the verge of collapse if nation's avoided their responsibility to deal with the environment in a holistic and collaborative way. However, critics charged that the report was a federally-sponsored vehicle for who they saw as prophets of doom. The danger, according to critics like free-market economist Julian Simon, was that genuine expertise was at risk of being stifled by the American federal government. Freedom of inquiry, centralized bureaucracy, the apparent codification of what he characterized as doomsaying -- the American federal government, it appeared, had become complicit in pushing an extreme environmentalist agenda.

The production of this report appeared to introduce a subtle transformation in the kinds of issues discussed by Landsberg, Singer, and others during the late 1960s and 1970s. While critics like them had long expressed concern with those whom they considered to be relatively extreme elements within the environmental movement (i.e. prophets of doom and scientific advocates), archival evidence suggests that their concerns never involved the kinds of explicit anti-government rhetoric as espoused by Julian Simon. Indeed, his Heritage Foundation-sponsored campaign to discredit the Global 2000 Report seemed to introduce a new rhetorical dimension to the kinds of issues that led to the creation of the AGU CEQ. Consequently, as this dissertation argues, the political meaning of the middle ground as envisioned a decade prior had become more associated with radical right politics via Singer and Landsberg's association with Simon's anti-Global 2000 crusade. The result was a gradual dissolution of the middle ground within environmental politics, and what appeared to be a more adversarial and embittered environmental politics after President Ronald Reagan's inauguration.

EPILOGUE

While it would be premature to conclude that the fundamental motivations discussed in this dissertation are causally related to contemporary debates over climate change, disagreements over what it meant to properly communicate future threats seems to bridge the past with the future. Indeed, how scientists -- especially climate scientists -- perceive the value of reticence bears on how scientists evaluate their roles within American society. The practice of making cautious claims -- Stephen Schneider once called 'erring on the side of least drama (ESLD)' -- has been a persistent tendency among climate scientists. Careful to avoid over-interpreting the data, climate scientists have consistently privileged what they describe as unemotional and cool-headed claims in order to avoid or reduce questions about their motivations -- what may be called a conservative bias. However, this bias may be more risky in that it may be misapplied to threats that genuinely deserve public urgency. As noted by Oreskes, et al.,

> ESLD provides a context for interpreting scientists' assessments of risk-laden situations, a challenge faced by the public and policy-makers. In attempting to avoid drama, the scientific community may be biasing its own work -- a bias that needs to be appreciated because it could prevent the full recognition, articulation, and acknowledgment of dramatic natural phenomena that may, in fact, be occurring. After all, some phenomena in nature are dramatic.[1]

According to this characterization, reticence and conservatism may protect the reputation of the scientific community, but may be detrimental to humanity over the long-run since scientists are typically the only source of relevant information about future threats like climate change.

Of course, timing is everything; history does not provide an objective metric by which to judge whether one is appropriately cautious. Part of the uncertainty in communicating urgency arises from the inherent complexity of the climate system. Since the 1960s, atmospheric

[1] Brysse, Keynyn, Naomi Oreskes, Jessica O'Reilly and Michael Oppenheimer, "Climate Change Prediction: Erring on the Side of Least Drama?" Global Environmental Change, 23 (2012): pp. 327-337, quote on p. 335.

scientists have been very aware of the difficulty of forecasting future climatic conditions, primarily due to the fact that the atmosphere is a quasi-stochastic (random) system. What this means is that small changes in initial conditions, as mathematician Edward Lorenz revealed in an influential article published in 1968, may lead very large outcomes that are inherently difficult to predict.[2] The consequence is that atmospheric scientists must use a certain level of subjective judgment and intuition in determining future conditions of the atmosphere -- what meteorologist Carl-Gustaf Rossby once called the "horrible subjectivity."[3]

This does not mean that scientists are obligated to pursue a reticent course if the situations appear dire enough to warrant public claims of future catastrophe. This is particularly salient in contemporary climate change debates. "Well, scientists are naturally reticent, I think," spoke James Hansen on January 8, 2008 during an interview with Terry Gross on National Public Radio's (NPR) show, Fresh Air.[4] Even so, he argued, society would benefit from scientists raising the alarm in a way that led to the implementation of policies that would stem the rise of greenhouse emissions. Having served as the Director of the NASA Goddard Institute of Space Studies (GISS) since 1981, Hansen has gone to great lengths to cast reticence as more harmful than scientists realize given what appears to be the amassing of crucial evidence that supports an emerging scientific consensus about the threat-level of climate change. "We may rue reticence, if it serves to lock in future disasters," he remarked a year earlier.[5]

Having experienced what he interpreted to be censorship at the highest levels of government during the Bush Administration, he felt that a self-subscribed commitment to

[2] Lorenz, Edward, "Climatic Determinism," *Meteorological Monographs* 8, 25 (1968): pp. 1-3
[3] Rossby's reference to the "horrible subjectivity" is contained in a letter to George Platzmann written on May 8, 1949. Cited in Harper, Kristine. *Weather by the Numbers: The Genesis of Modern Meteorology* (Cambridge: MIT Press, 2012): p. 262, cit. 34
[4] Interview with Mark Bowen and James Hansen, Fresh Air, 8 January 2008.
[5] Hansen, James, "Scientific Reticence and Sea Level Rise," *Environmental Research Letters* 2, 2 (April-June 2007).

reticence can blind scientists to their more fundamental obligations to humanity.[6] Recently, Hansen has recently employed the threat to his grandchildren as a way to stir what he deems to be necessary corrective action to steer humanity away from future danger. Individual scientists, he argues, "can choose to stay within a comfort zone, and not worry that they may say something that proves to be slightly wrong. But perhaps we should consider our legacy from a broader perspective."[7] Like many environmentally-conscious scientists during the 1970s, Hansen quite simply wanted scientists to consider the harm of their professional norms, and to expand their roles within society by taking the necessary risks to save the humanity.[8] He did not come to these conclusions easily.

Indeed, Hansen has had a long career in the risks of reticence within climate politics extending back to the 1980s. During the 1980s, global warming was becoming a major media topic of interest, and Hansen began to garner attention as being one of the foremost authorities on the matter.[9] Eric Pooley, Senior Vice President of the Environmental Defense Fund, has

[6] Bowen, Mark. *Censoring Science: Inside the Political Attack on Dr. James Hansen and the Truth of Global Warming* (New York: Penguin Group, Inc., 2007)

[7] Hansen, James, "Climate Catastrophe," *New Scientist* 195, 2614 (28 July 2007): pp. 30-34. For his most recent views on the matter of scientific reticence, see Hansen's *Storms of My Grand Children: The Truth about the Coming Climate Catastrophe and Our Last Chance to Save Humanity* (New York: Bloomsbury USA, 2009)

[8] Hansen was arrested on August 29, 2011 outside of the White House for demonstrating against the Keystone Pipeline, and again for the same reason on February 13, 2013.

[9] There is an expansive literature on the media's depiction of global warming. See, Lichter, S. Robert and Linda Lichter, "The Great Greenhouse Debate: Media Coverage and Expert Opinion on Global Warming," *Media Monitor*, vol. 6, (December 1992), pp. 1-6; Nacos, Brigitte, et al., "News Issues and the Media: American and German News Coverage of the Global-Warming Debate," within the edited work, *Decisionmaking in a Glass House: Mass Media, Public Opinion, and American and European Foreign Policy in the 21st century* (New York: Rowman & Littlefield Publishers, Inc., 2000); Trumbo, Craig, "Longitudinal Modeling of Public Issues: An Application of the Agenda-Setting Process to the Issue of Global Warming," *Journalism and Mass Communication Monographs*, Issue 152 (August 1995): 1-41; Ungar, Sheldon, "The Rise and Relative Decline of Global Warming as a Social Problem," *The Sociological Quarterly*, vol. 33 (Winter, 1992): pp. 483-501; Mazur, Allan and Jinling Lee, "Sounding the Global Alarm: Environmental Issues in the US National News," *Social Studies of Science*, vol. 23 (November 1993): 681-720; McComas, Katherine and James Shanahan, "Telling Stories about Global Climate Change: Measuring the Impact of Narratives on Issue Cycles," *Communication Research*, vol. 26 (1999): 29-57; Nissani, Moti, "Media Coverage of the Greenhouse Effect," *Population and Environment*, vol. 21 (September 1999): 27-43; McCright, Aaron and Riley Dunlap, "Challenging Global Warming as a Social Problem: An Analysis of the Conservative Movement's Counter-Claims," *Social Problems*, vol. 47 (November 2000), pp. 499-522; McCright, Aaron and Riley Dunlap, "Defeating Kyoto: the Conservative Movement's Impact on U.S. Climate Change Policy," *Social Problems*, vol. 50 (August 2003), pp. 348-373; Gelbspan, Ross. *Boiling Point: How Politicians, Big Oil and Coal, Journalists, and Activists are Fueling the Climate Change Crisis* (New York: Basic Books, 2004); Gelbspan, Ross. *The Heat is On: The High Stakes Battle Over Earth's Threatened Climate* (Reading, Mass: Addison-Wesley Publishing Co., 1997); Hoggan, James. *Climate Cover-Up: The Crusade to Deny Global Warming* (Greystone Books, 2009); Pooley, Eric. *The Climate War: True Believers, Power Brokers, and the Fight to Save the Earth* (Hyperion, 2010);

recently noted that global warming "was just then enjoying its first tour of the public consciousness, thanks to Jim Hansen's testimony."[10] Ross Gelbspan, one of the earliest investigators of the climate debates, mirrors this idea: "Although it has lurked in the dim margins of public attention for the past few years, global warming first emerged on the public stage during the brutally hot summer of 1988, when Dr. James Hansen . . . warned a congressional panel that it was at hand."[11] This was a crucial moment in the history of American climate politics, and it was during this time that Hansen began to understand the gravity of one's conclusions when presented in public.

On June 23, 1988, with cameras buzzing and sweat beading on his forehead during what many believed to be the hottest day on record, climate modeler James Hansen outlined what he called three principal conclusions, all of which indicated to the world that industrial emissions were *presently* influencing the global atmospheric system.[12] A human-induced global warming was not ten, twenty, or a hundred years away, it was happening now. In light of prevalent droughts throughout the United States, his testimony appeared all the more relevant to those who were interested in using climate forecasts to ameliorate the effects of climate fluctuations on agricultural production. As he testified,

> the earth is warmer in 1988 than at any time in the history of instrumental measurements. Number two, the global warming is now large enough that we can ascribe with a high degree of confidence a cause and effect relationship to the greenhouse effect. And number three, our computer climate simulations indicate that the greenhouse effect is already large enough to begin to effect the probability of extreme events such as summer heat waves.

Peterson, Thomas, et al., "The Myth of the 1970s Global Cooling Scientific Consensus," *Journal of the American Meteorological Society* (2008): 1325-1337.

[10] Pooley, Eric. *The Climate War: True Believers, Power Brokers, and the Fight to Save the Earth* (New York: Hyperion Books, 2010), p. 77

[11] Gelbspan, Ross. *The Heat is On: The Climate Crisis, the Cover-Up, the Prescription* (New York: Basic Books, 1998), p. 16

[12] Senator Timothy Wirth recounts in a *Frontline* interview in April 2007 how he and his staff left the windows open the night prior so that Hansen's testimony would highlight the heat of the room combined with the temperature of the multitude of television cameras. For a transcript of the interview, http://www.pbs.org/wgbh/pages/frontline/hotpolitics/etc/script.html, last accessed April 9, 2014

With ninety nine percent certainty, he concluded, the "the greenhouse effect has been detected, and it is changing our climate now."[13]

For many in the media, Hansen's testimony appeared unique in that it represented the first time that a climate scientist of standing had publicly come out so forcefully on the matter. "Hansen is the first," wrote the *Washington Post*, "to concretely link warming and greenhouse gas emissions and to authoritatively proclaim the phenomenon's arrival."[14] Part of the apparent force of his testimony was due to the fact that others were attempting to use the summer heat-wave of 1988 to maximize public concern over global warming. As Roger Pielke, Jr. stated, "The hearing that day was carefully stage-managed to present a bit of political theater."[15]

While attractive to the media, many within the atmospheric science community objected to Hansen's testimony because it not only appeared inconsistent with available science but also appeared too political. Alan Hecht, Director of the National Climate Program under the aegis of the National Oceanic and Atmospheric Administration, noted that droughts were a "part of our climatology. So the mere fact that we have a drought now is really part of what has been the historic pattern." Michael E. Schlesinger, an atmospheric physicist at Oregon State University, also noted that the data was just too limited to surmise the influence of fossil fuel emissions on the global climate system. "My concern," he noted,

> is if we say now that this is the beginning of the greenhouse effect, and then it cools off because of natural variability, people will say, 'Well, you fellows didn't know what you

[13] Testimony of James Hansen, Hearing Before the Committee on Energy and Natural Resources (Washington, D.C.: U.S. Government Printing Office, 1988): pp. 39-41

[14] At least four national newspapers covered the story, though the Washington Post and the New York Times seems to have produced the stories copied by other newspapers. "'Greenhouse Effect' Called Reality," *Chicago Tribune*, 24 June 1988, p. 6; "Greenhouse Effect Cited for Hot '88," *The Sun*, 24 June 1988, p. 1A; "Scientist Says Greenhouse Effect is Setting In," *The Washington Post*, 24 June 1988, p. A4; "Sharp Cut in Burning of Fossil Fuels is Urged to Battle Shift in Climate," *New York Times*, 24 June 1988, p. A1.

[15] For a brief account of how figures like Colorado Rep. Timothy Wirth "staged" the testimony to coincide with the hottest day on record, see Pielke, Roger. *The Climate Fix: What Scientists and Politicians Won't Tell You About Global Warming* (Philadelphia: Basic Books, 2010), p. 1

were talking about,' and would turn to other issues and do nothing about what is a very real problem.[16]

S. Fred Singer, who was by now Professor of Environmental Sciences at the University of Virginia, also chimed in by suggesting that Hansen's testimony renewed long-standing ideas of "doomsday scenarios" that provided little more than a "mixture of fact and fancy." Very much similar to the kinds of statements he made two decades earlier with regard to the claims of ecologist Lamont Cole, he laid out several technical and historical reasons that "should induce a certain amount of skepticism and make us somewhat more humble about the ability of theory to predict the future of the atmosphere and of climate."[17] Because carbon dioxide was not the only industrial byproduct to influence the greenhouse effect, and there were many other factors that may directly and indirectly influence the global climate system (e.g. oceans), Singer believed that Hansen was bordering on irresponsibility. Believing that a "cottage industry" had become committed to a "problem that may or may not be real," he believed that climate change was becoming another example of polemics gone awry -- with Hansen at center stage.[18]

Hansen's 1988 testimony appeared all the more remarkable to his colleagues given testimony he provided only months earlier, and his own admission that a middle ground perspective was the best way to interpret available evidence. On November 2, 1987, Hansen received a request from the chairman of the Senate Committee on Energy and Natural Resources, J. Bennett Johnston (D-LA), to provide his best estimates on the "likely pace and regional implications of the greenhouse effect and global climate change." Citing his credentials and experience, Hansen provided what he hoped to be an "appropriate middle ground between the preference of scientists to stress all caveats in detail, and the desire of non-technical parties for

[16] "Scientists Discount Drought Tie to 'Greenhouse' Warmth Trend," *Los Angeles Times*, 25 June 1988, p. 24
[17] Singer, S. Fred, "Fact and Fancy on Greenhouse Earth," *Wall Street Journal*, 30 August 1988, p. 22
[18] Singer, S. Fred, "Fact and Fancy on Greenhouse Earth," *Wall Street Journal*, 30 August 1988, p. 22

an understandable practical statement of the status of scientific understanding." This was remarkable if only because Hansen -- in this testimony -- appears to agree with those like Singer who had long advocated for a more accurate assessment of existing uncertainties. While he was generally confident that a greenhouse warming was expected to occur at sometime in the future -- "we can confidently state that major greenhouse climate changes are a certainty" -- Hansen was very uncertain of when. The best he could do was to conclude that "the climate model results indicate that greenhouse effects on near-term global temperature trends should be apparent within the next several years."[19]

Consistent with what he called the "middle ground," Hansen testified about his own skepticism toward his model results, and what they meant. After conducting a model simulation of fossil fuel emissions from 1958 to 2030, he noted that a projecting warming was not at present "large enough relative to the natural variability of climate," and therefore it was not clear whether human activities were influencing the global climate system. While the warming revealed within the model was statistically improbable and thus of some concern -- it was three times the standard deviation of natural variability -- he publicly cautioned that the greenhouse effect had yet to be visible in the climatological and statistical record.[20] While archival evidence is present unavailable to examine why Hansen shifted his confidence level between November 1987 and June 1988, Hansen began to garner an unfavorable reputation among many of his peers.

In 1989, science writer Richard Kerr wrote a piece in *Science* about a recent gathering of climate scientists in Massachusetts entitled "Greenhouse Gas-Induced Climatic Change."[21]

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[19] Testimony of James Hansen, Hearing before the Committee on Energy and Natural Resources (Washington, D.C.: U.S. Government Printing Office, 1987): pp. 51-54
[20] Testimony (1987), p. 64
[21] Kerr, Richard, "Hansen vs. the World on the Greenhouse Threat," *Science* 244, 4908 (June 2, 1989): pp. 1041-1043

Prompted by "a furor" in Washington, D.C. over whether Hansen was being censored by policymakers, Kerr sought to understand why Hansen "was once again at loggerheads with his colleagues in the climate community over how to speak to outsiders" and why some felt resentment toward their "now famous colleague." While reminiscent of the reaction by some of his peers after his 1988 testimony, Kerr's piece depicts Hansen's behavior in a more provocative light. What really bothered his colleagues, he reported, was not that Hansen may have been wrong but rather Hansen's "failure to hedge his conclusions with the appropriate qualifiers that reflect the imprecise science of climate modeling." This was the fundamental problem: the apparent confidence with which he spoke in public.

Indeed, the problem with appearing overly confident about "seeing" a greenhouse effect -- especially in public -- was the risk that such an effect may not have ever been there in the first place. While sympathetic to Hansen's overall message of concern given what were genuine problems, Tom Wigley, a climate scientist at the University of East Anglia, believed that Hansen's claim of 99% certainty was "not justified theoretically." Schlesinger, the same modeler interviewed a year earlier, noted how Hansen's testimony gave greater certainty than is warranted given current understanding the global climate system -- instead of 99%, he noted, "confidence in detection of the greenhouse is now down near zero." Stephen Schneider, a climate modeler with the National Center for Atmospheric Research, justified his own skepticism given the "pretty hokey" coupling of the ocean and atmosphere in Hansen's modeling efforts. "To say that we've seen the greenhouse signal is ridiculous," remarked Tim Barnett, an oceanographer at the California-based Scripps Institute of Oceanography.

While many believed that the it was inappropriate to publicly declare the "reality" of a global warming signal given scientific uncertainties, that did not mean that Hansen was necessarily wrong about the underlying physics of the greenhouse effect nor his concern with what genuinely appeared to be a growing problem with greenhouse gas emissions. Stephen Schneider, a colleague of Hansen, sympathized with the difficulty of applying one's judgment in making public claims about the future risks of climate change. Scientists were being asked to testify and make judgments that could inform national policy making, and they were being asked to balance their scientific knowledge with their own professional but potentially fallible assessments of the future. As Schneider articulated,

> I'll be surprised if it doesn't happen, but how do you assign a probability to something when you have no objective means of doing so? You base it on physical intuition and then state your assumptions. By my intuitive reasoning, the greenhouse signal has been detected at an 80% probability. My faith is based on the principle of heat trapping by greenhouse gases and the billions of observations that support it. All that objective stuff rests on assumptions. The future is not based on statistics, it's based on physics. Objectivity is overplayed.

And, this is why Hansen's 1987 testimony appeared so different from his 1988 testimony -- the latter appeared *too* objective, *too* certain. "What bothers a lot of us is that we have a scientist telling Congress things we are reluctant to say ourselves," noted Alan Robock, a climate modeler with the University of Maryland.

Given the experience of having been criticized by his peers and closest colleagues within the climate modeling community, Hansen himself recounts how he became more reticent after his 1988 testimony to protect the integrity of the discipline. "It soon became apparent, though, that my testimony, combined with the weather, was creating a misimpression," he wrote. After an failed attempt to clarify his 1988 testimony in 1989, Hansen decided to retreat after realizing the power of the media: "After my testimony at Gore's hearing, I was firmly resolved to go back to pure science and leave media interactions to people such as Steve Schneider and Michael

Oppenheimer, people who were more articulate and seemed to enjoy the process."[22] This was no small admission.

Indeed, most scientists had little trouble with Hansen's personal feelings of concern and urgency. Behind closed doors, they agreed that alertness was appropriate and that scientists had a responsibility to determine the scope and severity of the threat before it became a serious problem. Instead, they believed that Hansen had traversed what appeared to them as an important boundary. Danny Harvey, having just recently a received his Ph.D. in atmospheric science from the University of Toronto, remarked that Hansen "crawled out on a limb" and in so doing risked the potential that decision making could be derailed if a warming did not happen in the 1990s and 2000s. Climate was a long-term issue, and Hansen apparently took to the airwaves too soon and -- at least in Singer's eyes -- appeared overly alarmist.[23]

While many authors place great importance on Hansen's 1988 testimony, the issue of appearing over confident about the urgency of climate change was nothing new. During the early 1980s, policy makers (as illustrated in the 1970s with regard to the National Climate Program Act) began to push for legislative efforts to understand and response to the threat of climate change and whether policies should be enacted to counter the growth of greenhouse emissions. One of the most active participants in these renewed debates was the chairman of the House Subcommittee on Investigations and Oversight, a young Albert Gore (D-TN), who organized hearings on July 31, 1981 regarding the effects of carbon dioxide on the global climate system."[24]

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[22] Hansen, James. *Storms of My Grandchildren: The Truth about the Coming Climate Catastrophe and our Last Chance to Save Humanity* (New York: Bloomsbury USA, 2009); Hansen, "Scientific Reticence and Sea Level Rise," *Environmental Research Letters* 2 (2007): pp. 1-6

[23] Kerr, Richard, "Hansen vs. the World on the Greenhouse Threat," *Science* 244, 4908 (June 2, 1989): pp. 1041-1043

[24] "Carbon Dioxide and Climate: The Greenhouse Effect," Hearing before the Subcommittee on Investigations and Oversight, 31 July 1981 (Washington, D.C.: U.S. Government Printing Office, 1981),

Within his introductory remarks, Gore recounted how he first learned of the issue and why he felt it was important enough to do something about. "Quite frankly, my first reaction to it several years ago was one of disbelief. Since then I have been waiting patiently for it to go away, but it has not gone away. The evidence continues to indicate that this Nation may be on the way to a natural disaster of unprecedented proportions." Carbon dioxide, he claimed, had the potential to "radically alter" the climate, upset agricultural production, and fundamentally change society for the worse. "Ironically, though the Greeks might call it the result of our civilization's hubris, it appears that this effect is caused by the flowering of our industrial society." The crossroads of human desire for growth and the restraining forces of the natural environment had finally arrived, and Gore felt the time had come to inspire fundamental change in how humans lived.

Many prominent witnesses were asked to testify, including atmospheric scientists from the National Center for Atmospheric Research, the Geophysical Fluid Dynamics Laboratory, the National Climate Program Office, administrative officials from the Reagan Administration, and economic policy specialists. While most believed that the threat more than warranted further research, Gore's exchange with N. Douglas Pewitt, the acting director of the Office of Energy Research in the Department of Energy, is particularly useful in contextualizing what appeared to be the importance of not overstating the threat level of carbon dioxide and climate change. Indeed, Pewitt appears to have rekindled the very issues that appeared when Frank Press testified four years earlier regarding his belief in the need for a balance between complacency and panic.

After showing two graphs which showed what appeared to be a clear-cut correlation between rising carbon dioxide levels and increasing surface temperatures, Gore asked Pewitt if this was not sufficient to justify urgency. "No," Pewitt responded. For him, raising the alarm required a great deal of care so as not to induce what he considered excessive public alarm. As he described,

> I think that in running a scientific research program, we have a responsibility to the Congress and the American people to act in a fashion that is not alarmist. We have been down this road in similar areas before; for example, in aerosols and in the SST's disturbing the atmosphere. It clearly is the prerogative of the Congress and policymakers to set a higher priority than the scientific programs can produce research results. There is a natural approach to scientific research that probably would not justify much of the more alarming carbon dioxide statements. I absolutely refuse as an official in a responsible position to engage in the type of alarmism for the American public that I have seen in these areas time and time again, and I do not think that I can responsibly encourage that sort of alarmism.[25]

Importantly, Pewitt and gore were not arguing over the truth value of the claims, but rather how they should be interpreted as either an urgent message to implement strong policies or as a sign for concern and vigilance to justify further research. According to Pewitt, scientists' opinions -- no matter how well articulated and urgent -- must be balanced with other interests and concerns within the federal government. As he testified,

> We try to reach out and get reasonable advice, and use peer review and advisory bodies to tell us how to proceed in this area. One should make sure that their advice is solidly based scientific evidence. . . You can't have bureaucrats dictating science, but at the same time you can't have scientists using alarmism in order to justify bigger research budgets. That is irresponsible, too. We are just trying to approach this issue reasonably. Nobody predicts anything to happen in less than 50 years. It is important not to waste the next decade, but it is also important not to jump off and get the American public concerned, stop synfuels, stop everything in the world, on the basis of misinformation. We intend to pull together information in the timeframe we are talking about. We will have a reliable basis to know what is reasonable to anticipate. You never know precisely in this business; all you do is narrow the bounds of uncertainty. We will have narrowed the bounds of uncertainty very considerably in 2 or 3 more years in this program.[26]

[25] Pewitt Testimony, p. 87
[26] Pewitt Testimony, p. 88

In response, Gore repeated his agreement that one should avoid alarmism but that one cannot merely "stick one's head in the sand."[27] Indeed, this was a crucial divide that seemed to only get bigger as time passed: the more one advocated for patient scientific investigation to reduce uncertainties, the more one appeared to advocate for a policy of inaction and complacency. In their own way, they were both trying to appeal to the middle ground in that they did not want to project undue alarmism. In this, they were aligned. However, they diverged in exactly what it meant to avoid the perception of alarmism. For Gore, avoiding alarmism meant an expansion of budgets directed to carbon dioxide research, making urgent but presumably not alarmist public declarations about the future consequences of climate change, and use the weight of politics to stir action in spite of existent uncertainties. For Pewitt, however, maintaining a reasonable posture meant appropriating sufficient research funds commensurate with national priorities and what is likely to be learned, avoiding what may appear to be overly public declarations until more was known, and restrain the goals of politics until science could reduce existing uncertainties.

This disagreement was about making their respective positions appear not only reasonable but entirely reflective of the state of climate science. While Gore believed that charts amply demonstrated the influence of humankind on the global climate system, Pewitt was arguing from a position where charts were useful but not the overriding concern. Indeed, his sentiments were mirrored by the Reagan-appointed director of the Department of Energy Climate Dioxide Program, Frederick Koomanoff. Testifying to the technical uncertainties regarding the relationship between oceans and carbon dioxide, Koomanoff claimed that his job as director was to create what he called a "sound, scientific program" by moving ahead in a "logical, prudent, and as rapid a way as possible." As he continued,

[27] Gore, p. 89

> So yes, the carbon dioxide concern is an extremely important one; we have to get answers because it is important to all of us. But to try to make a conclusion at this point with our lack of knowledge and the great uncertainties that exist and jump into the future is a very dangerous thing to do. We can lead ourselves down paths and try to make decisions before we have the data required to make scientifically based, sound decisions.[28]

In light of what appeared to be a wholesale dismantling of climate research for political reasons under the Reagan Administration, Koomanoff's goals appeared to contrast with what he saw as an equally political strategy by figures like Gore to prevent such reductions by using the rhetoric of urgency.[29] He felt that the scientific uncertainties were robust enough to warrant more appreciation for what he considered the social and political realities involved in the implementation of climate policy beyond further research, and that messages of urgency were not necessarily beneficial. As he cautioned,

> Realistically, you wouldn't be able to get significant social changes made on the basis of scientific information, anyway, until the event became real to people. That is the basic problem with this. More enlightened public leaders would try to shape public opinion. Nevertheless, it would take a long time before you could get the dramatic social changes accepted that would be required.[30]

As if dealing with a culture unknown to him, Gore appeared confused by the inability of who appeared to be trained experts advocating a more cautious political response to the question of climate change. "I used the word 'urgency' hoping it would not connote an alarmist tone. What do you think the proper tone of our response should be?" he asked of modeler Stephen Schneider. This was an intriguing question if only because Gore was looking for an objective metric to gauge how alarmist one's claim to be. For Schneider, the answer was simple: scientists were unable to provide a metric because the issue was not a matter that could be resolved using

[28] Testimony, p. 89
[29] Weart, Spencer, "Money for Keeling: Monitoring CO2 Levels," *Historical Studies in the Physical and Biological Sciences* 37 (2007): pp. 435-452
[30] Testimony, p. 91

appeals to epistemic claims. As he described at length in response to Gore's apparent frustrations,

> Whether one is an alarmist or considers the CO_2 problem urgent isn't based on any scientific information. It is a value judgment. It depends upon how you personally fear potential risks versus how you personally fear the costs of mitigating them, versus your own political philosophy about whether individuals should be free to do what they want, or whether we have a collective responsibility. The whole question of urgency or alarmism really is not something that I think a group of experts can define. All we can do, as experts, is try to list what are the possibilities and, in particular, what the uncertainties are. . . I do not believe that change will necessarily be catastrophic, although the issue of catastrophe is almost irrelevant. The issue is, if we have done something, and if we can know who did it, there is the question of equity as to who should be responsible or how we can minimize the damages so that it is not a catastrophe. It is like many other pollution issues. I have a sense of urgency in the view that we need to consider now those actions which increase our options. In that sense, it is urgent. In the sense it will be an urgent catastrophe which will eliminate all life, that kind of statement is absolutely unwarranted.[31]

Everyone felt that appealing to a middle ground was appropriate, but Gore, Koomanoff, Pewitt, and Schneider all had their own understanding of what that meant. These were legitimate disagreements that not only involved the assessments of future risks but, as Schneider attested, how one allowed their personal value systems to guide their policy deliberations that will inevitably influence the lives and behaviors of others. Whether a claim was "alarmist" or not was never really about the truth value of epistemic claims, but rather the manner in which threats were communicated to the general public and what it meant for policy makers to restrain themselves in a way that seemed more commensurate with scientific understanding.

The question must be asked, then, is whether this doctoral research provides a basis to ask meaningful questions about the role of a middle ground in recent climate debates, and whether doubts expressed about the reality and urgency of global climate change motivated by a genuine concern for scientific truth? Indeed, over the last thirty years, the issue of climate

[31] Testimony, p. 91

change has become one of the most divisive scientific and political issues of the 21st century. Disagreements frequently arise due to how the threat of climate change is depicted in the general public. Alex Epstein, Founder and President of the conservative-leaning Center for Industrial Progress, believes that the question is not whether a global warming consensus exists, but rather how the issue is framed to heighten public anxieties about the future. As he notes,

> It makes a big difference if there is a consensus that there is some global warming vs. a consensus that there will be catastrophic global warming. . . Most consensus statements are very unclear on who agrees with what and why. They are unscientific consensuses -- misrepresentations of the state of scientific opinion designed to further a political agenda.[32]

By "unscientific consensus," Epstein means three things. First, such a consensus appears to be the product of scientists willing to supplant their technical expertise and instead rely on personal values and judgment. Once a scientist advances beyond his or her scientific training and knowledge, he argues, he or she risks their credibility and reputation. Dramatizing the meaning of technical claims -- a technical claim of warming vs. an alarmist claim of *catastrophic* warming -- may confuse an already susceptible general public, and inject bias into the debates. Second, an unscientific consensus is one that attempts to prematurely shut down discussion; it has particularly undemocratic overtones. Third, "catastrophic" depictions of the future has the tendency to reduce the inherent complexities not only of the science, but also the social elements that constitute the consensus. By this, he means that the consensus may ignore or overlook the range of disciplines involved, and glosses over any realms of disagreement that may exist between and within disciplines.[33]

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[32] Epstein, Alex, "The Unscientific Consensus," *Forbes,* 27 February 2014

[33] Preliminary reviews of scientific literature in 2010 suggested that 97-98% of climate scientists support the tents of anthropogenic climate change. See, Anderegg, William, et al., "Expert Credibility in Climate Change," *Proceedings of the National Academy of Sciences* 107, 27 (6 July 2010): pp. 12107-12109; Doran, P.T. and M.K. Zimmerman, "Examining the Scientific Consensus on Climate Change," EOS Transactions of the American Geophysical Union 90, 3 (2009): p. 22; Oreskes, Naomi, "Beyond the Ivory Tower: The Scientific Consensus on Climate Change," *Science* 306, 5702 (3 December 2004): p. 1686

While it seems fairly clear that Epstein is driven by a conservative value-system, some highly credentialed scientists also speak out about what they see as the risks of framing environmental concerns in alarmist ways. As MIT atmospheric scientist Richard Lindzen expressed in 2010, "One can only hope the climate alarmists will lose so we can go back to dealing with real science and real environmental problems such as assuring clean air and water."[34] Critics like Lindzen charge that influential individuals like former U.S. presidential candidate Al Gore appeal to what he interprets as exaggerated hyperbole to convince both policymakers and the general public that the dangers of climate change are real and urgent enough to limit carbon dioxide emissions. Alarm, he argues, is also used to maintain a funding structure built on the threat of climate change -- what he calls the "iron triangle of alarmism" between climate scientists, advocates, and policymakers.[35] Science is threatened by what he sees as alarmist proclamations because it shuts down debate and produces hostility against more skeptical views like his own.[36] Whether he is right or wrong, illogical or not, the influence of Lindzen's statements appears to have a great deal of weight for those who question the validity of the global warming consensus within the public sphere.[37]

How do historians adjudicate the motivations of Epstein and Lindzen? Do the deserve to be placed in the same camp in light of their antipathy toward what they see as alarmist claims? Are they responsible for politicizing the debates to the point that a middle ground seems less and less reasonable? Indeed, those who fundamentally disagree with individuals like Lindzen and

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[34] Lindzen, Richard, "Alarmists Keep Ringing the Bell," *The Australian*, 24 April 2010.
[35] Lindzen, Richard, "Climate of Fear: Global Warming Alarmists Intimidate Dissenting Scientists into Silence," *Global Research*, 31 March 2013
[36] Jacoby, Jeff, "Invoking 'Consensus' to Shut Off Debate on Climate Change is Authoritarian and Anti-Scientific," *Boston Globe*, 4 December 2013.
[37] Rogers, Norman, "Global Warming Dogma and the New Iron Triangle," *American Thinker*, 15 April 2012, http://www.americanthinker.com/2012/04/global_warming_dogma_and_the_new_iron_triangle.html; "Climate Science Exploited for Political Agenda: Lindzen," *Greenie Watch*, 30 August 2013, http://antigreen.blogspot.com/2013_08_01_archive.html; Hayward, Steven, "Climate Science: Time for 'Team B'?" American Enterprise Institute for Public Policy Research, January-February 2005

Epstein also contribute to my suspicion that the middle ground is gradually becoming impossible to maintain -- if it ever was. Rajendra Pachauri, Chairman of the Intergovernmental Panel on Climate Change (IPCC), likens doubters like Lindzen to members of the Flat Earth Society.[38] Prominent atmospheric scientist Michael Mann mirrors Pachauri's sentiments by dismissing anyone who disagrees with the climate consensus in equally derogatory terms:

> The overwhelming consensus among climate scientists is that human-caused climate change is happening. Yet a fringe minority of our populace clings to an irrational rejection of well-established science. This virulent strain of anti-science infects the halls of Congress, the pages of leading newspapers and what we see on TV, leading to the appearance of a debate where none should exist.

For Mann and Pachauri, a consensus has been established, the science is clear, the warning could not be louder, and doubters like Lindzen are threatening humanity. Given what appears to be the complacency of skeptics, figures like modeler James Hansen and Michael Mann argue that scientists have an obligation to extend their activities beyond traditional boundaries of conducting scientific research insulated from the general public: "it is no longer acceptable for scientists to remain on the sidelines."[39] By equating those who reject or cast doubt on human-caused climate change to an anti-science fringe group, and those who support what appears to be a scientific consensus as "alarmists," the debate has only evolved to charges of pseudo-science and conspiracy.

In recent years, historian of science Naomi Oreskes and Erik Conway have sought to understand the role political affiliation and ideological rigidity in guiding these increasingly adversarial debates. Over the last six years or so, they have argued that a few prominent scientific elites -- Robert Jastrow, S. Fred Singer, William Nierenberg, and Frederick Seitz -- have been able to manipulate the media in a way that confuses the general public into believing

[38] Interview with Rajenda Pachauri, "Blunt Answers about Risks of Global Warming," *Chicago Tribune*, 3 August 2008
[39] Mann, Michael, "If You See Something, Say Something," *New York Times Sunday Review*, 17 January 2014.

that a legitimate scientific debate exists within the scientific community about global warming. Those who they call "denialists" -- those who adhere to a free-market, conservative, and anti-communist ideology and thus object to the implications of reducing fossil fuel emissions -- have relied upon this doubt to stall legislative action and combat an apparent effort by liberals to destabilize the free market and the national sovereignty of the United States. They have also examined at length the institutions that were born from these efforts (e.g. the Marshall Institute), and have shown how these institutions were designed to influence the debate not by promoting the publication of scientific and peer-reviewed articles, but rather engaging the media under the guise of fair practice for a balanced presentation of "the facts." The scholarship of Aaron McCright and Riley Dunlap provides another important and revealing sociological perspective about the role of conservatives on contemporary public perceptions of global warming, which tends to buttress what appears to be a genuine and fundamental disagreement based on political beliefs and conservative value-systems.[40]

While the historical interpretation of Conway and Oreskes sheds important light on the ideological considerations and rhetorical strategies of those on the far right, this dissertation argues that appeals to left-right binaries may hide as much as reveals about the history of climate politics. For instance, by analyzing one of the most divisive individuals in contemporary climate politics -- S. Fred Singer -- from the late 1960s and early 1980s, one sees an individual who claimed to represent what he and many of his colleagues envisioned as a more reasonable and tempered middle ground during the late 1960s and 1970s.

[40] McCright, Aaron and Riley Dunlap, "The Politicization of Climate Change and Polarization in the American Public's Views of Global Warming, 2001-2010," *The Sociological Quarterly* 52, 2 (Spring 2011): pp. 155-194; McCright, "The Effects of Gender on Climate Change Knowledge and Concern in the American Public," *Population and Environment* 32, 1 (September 2010): pp. 66-87; McCright and Dunlap, "Social Movement Identity and Belief Systems: An Examination of Beliefs about Environmental Problems within the American Public," *The Public Opinion Quarterly* 72, 4 (Winter 2008): pp. 651-676; McCright and Dunlap, "Challenging Global Warming as a Social Problem: An Analysis of the Conservative Movement's Counter-Claims," *Social Problems* 47, 4 (November 2000): pp. 499-522

For instance, one of the elements with the increasingly heated debates is why the most esteemed climate scientists of the 20th century and political advocates to curb fossil fuel emissions, James Hansen, also appealed to what he considered a "middle ground" in his congressional testimony of 1987. How could it be that the figure most responsible for contributing to a modern climate change consensus in the 1990s and 2000s sounded -- if for a brief moment -- like the kind of scientist individuals like Singer and Landsberg envisioned as the ideal? This dissertation provides one piece to the puzzle by arguing that Hansen was not speaking rhetorically; this notion of a "middle ground" has a robust history that is rooted in the environmental movement of the 1970s. My suspicion is that his statement -- if ever so brief -- was not an accident. Hansen quite literally embodied two different kinds of scientist during the mid to late 1980s, and he slowly came to grips with two streams of thought over how best to engage the general public about highly uncertain but very real threats to humanity.

In spite of what appears to be very little room to argue for a middle perspective that abandons the vitriolic rhetoric that has become contemporary climate debates, some evidence suggests that a resurgence is taking place. In an April 2014 *New York Times* op-ed, environmental strategists Ted Nordhaus and Michael Schellenberger expressed their concerns about the effect of a documentary, *Years of Living Dangerously,* may be having on contemporary debates about climate change. Produced by James Cameron, the program follows celebrities (e.g. Arnold Schwarzenegger, Don Cheadle, Harrison Ford, among others) as they attempt to not only reveal how climate change is currently altering human civilization but to act as advocates to reduce global greenhouse gas emissions. To convey urgency, the documentary highlights extreme natural weather events and the effects they are having on citizens around the world.[41]

By linking natural disasters with climate change, Nordhaus and Schellenberger believe that public skepticism toward the climatological community and further inflame partisan politics about climate. While they note the producers' and actors' noble intentions, the documentary's clear intention to advocate for the implementation of policies to reduce greenhouse emissions may ultimately "backfire." Nordhaus and Schellenberger were not saying that global warming is something to ignore, but they do reinforce what appears to be growing scholarship that shows how "dire messages" may actually undermine efforts to prevent future harm.[42] As they explain,

> Claims that current disasters are connected to climate change do seem to motivate many liberals to support action. But they alienate conservatives in roughly equal measure. . . While the urgency that motivates exaggerated claims is understandable, turning down the rhetoric and embracing solutions like nuclear energy will better serve efforts to slow global warming.[43]

Frustrated by the lack of political progress in dealing with the future effects of climate change, Nordhaus and Schellenberger advocate what they envision to be a new way of talking about environmental problems. In 2003, they founded the Breakthrough Institute, which, according to their mission statement, is committed to "modernizing environmentalism for the 21st century." By this, they reject "outmoded orthodoxies on the Left and Right, and are dedicated to new ways of thinking, new political frameworks, and new policy paradigms." From

[41] One may visit the homepage, http://yearsoflivingdangerously.com/
[42] For further discussion on the effects of fear-inducing rhetoric in contemporary debates over climate change, see Nicholson-Cole, Sophie and Saffron O' Neill, "'Fear Won't Do It': Promoting Positive Engagement with Climate Change Through Visual and Iconic Representations," *Science Communication* 30, 3 (March 2009): pp. 355-379; Howell, Rachel, "Investigating the Long-Term Impacts of Climate Change Communications on Individuals' Attitudes and Behavior," *Environment and Behavior* 46, 1 (January 2014): pp. 70-101; Feinberg, Matthew and Robb Willer, "Apocalypse Soon? Dire Messages Reduce Belief in Global Warming by Contradicting Just-World Beliefs," *Psychological Science* 22, 1 (January 2011): pp. 34-38; Das, de Wit, Stroeb, "Fear Appeals Motivate Acceptance of Action Recommendations: Evidence for Positive Bias in the Processing of Persuasive Messages," *Personality and Social Psychology Bulletin* 29 (2003): pp. 650-664; Ferguson and Branscombe, "Collective Guilt Mediates the Effect of Beliefs about Global Warming on Willingness to Engage in Mitigation Behavior," *Journal of Environmental Psychology* 30, 2 (2010): pp. 135-142; Hoog, N., et al., "The Impact of Fear Appeals on Processing and Acceptance of Action Recommendations," *Personality and Social Psychology Bulletin* 31 (2005): pp. 24-33
[43] Nordhaus, Ted and Michael Shellenberger, "Global Warming Scare Tactics," *New York Times* Op-Ed, 8 April 2014. For a response to their claims, see "Public Attitudes about Climate Change," *New York Times*, 14 April 2014.

this perspective, their *New York Times* editorial about the dangers of what they see as exaggerated rhetoric reflect their institutional mission to minimize dramatic language of crisis often associated with global warming discussions. For them, modernizing the debate over climate means "turning down the rhetoric."[44]

While they see an opportunity to create a new environmentalism, this dissertation should prevent future historians from evaluating whether the ambitions of the Breakthrough Institute are wholly original. Indeed, I believe the historical evidence is quite strong that this desire for a new kind of dialogue about the environment -- defined as a separation from Left/Right dichotomies, as well as rhetoric that appears overly exaggerated and pessimistic -- is rooted in a much broader history about how individuals in certain historical circumstances seek to create what they deem to be a more rational perspective between two existing ones. Like my argument that the anti-environmental statements of Singer, Landsberg, and other members of the American scientific establishment should not be merely interpreted as an attempt to maintain the status quo, I do not believe future historians should presume that the work of Nordhaus and Schellenberger are driven by the same kinds of motivations behind the efforts of Epstein and Lindzen.

Indeed, after having seen a historical parallel between two individuals that are often framed as standing on opposite ends of contemporary debates -- James Hansen and S. Fred Singer as well as parallels between two institutions committed to establishing a new kind of environmentalism -- the Breakthrough Institute in the 21st century and the AGU CEQ in the 20th -- I suggest that further research can shed a great deal of light on the continuities that exist between contemporary climate debates and the environmental debates of the 1960s and 1970s. Indeed, one should not allow contemporary perspectives to limit one's understanding of important historical continuities. As American historian Arthur Schlesinger, Jr. once noted,

[44] For a brief summary of the Breakthrough Institute's mission, see http://thebreakthrough.org/about/mission/

> History haunts even generations who refuse to learn history. Rhythms, patterns, continuities, drift out of time long forgotten to mold the present and to color the shape of things to come. Science and technology revolutionize our lives, but memory, tradition and myth frame our response. . . The dialectic between past and future will continue to form our lives.[45]

How Americans have conceived -- and continue to conceive -- environmental challenges appears to be an example of such processes. There is room for historians to speak about a middle perspective without being perceived as an apologist for either side. Whether this dissertation successfully represents the virtue of such a perspective within history is for others to judge.

[45] Schlesinger, Jr., Arthur. *The Cycles of American History* (Boston: Houghton Mifflin Company, 1986): p. xii.

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