Intellectual Freedom of Academic Scientists: Cases of Political Challenges Involving Federally Sponsored Research on National Environmental Policies

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ABSTRACT

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This study contributes to the literature on the academic profession’s intellectual freedom. Drawing significantly on two methodological approaches, comparative case study and grounded theory, this dissertation examines three controversies in which government officials challenged academic scientists’ federally sponsored research, which had implications for national environmental policies. To structure this examination, I used a two-part framework. For the first part, I investigated the evolving interpretations of events and actors’ interests, which revealed the tactics and pressures employed by government officials when challenging the academic scientists’ federally sponsored research. For the second part, I used Freidson’s theory of professional dominance to help us understand how and in what ways institutionalized arrangements within society supported the academic profession’s autonomy and authority over its work. This analysis identified the means by which the academic scientists in my three cases exerted some degree of control over scientific decisions regarding the research assumptions, methods, and analyses of their findings.

The study’s key findings are presented in the form of five research claims: First, the government challengers may try – sometimes successfully – to exercise their influence over indirect participants in the federally funded research in an attempt to control the dissemination of the federally sponsored research findings. Second, the government challengers, though not scientists themselves, relied heavily on their own judgment to declare publicly the kinds of
activities that can and cannot count as legitimate scientific research, rather than relying on the
traditional scientific peer-review process. Third, academic scientists may involve members of
the public in the dispute. When that happens, the public may help decide whether government
officials or academic scientists are better equipped to address the scientific matters associated
with the federal policy. Fourth, academic scientists’ political allies can support academic
scientists’ efforts to defend their research within the policymakers’ setting. Fifth, academic
scientists may assert academic conventions (e.g., peer review) as the standard (or possibly as the
preferred) practice through which to evaluate science, even when government challengers
question the validity of those conventions. Placed in context of the extant literature, these
claims, taken together, suggest that the government officials tried to take actions that exceed
their professional competence, specifically as boundary breakers who attempted to infiltrate the
jurisdictional responsibilities of the academic scientists. In addition, despite the government
officials’ attempts to engage in professional boundary-crossing activities, the academic scientists
asserted institutionalized practices and standards of the profession (e.g., peer review and open
dialogue) and drew on the assistance of external actors (i.e., members of the public and political
allies) as countervailing forces to exert control over their research.
# TABLE OF CONTENTS

## CHAPTER I: OVERVIEW OF THE RESEARCH

- Introduction ................................................................................................................................. 1
- Problem Statement ...................................................................................................................... 4
- Purpose of the Study ................................................................................................................... 9
- The Cases .................................................................................................................................. 10
  - Sewage Sludge Case .............................................................................................................. 11
  - Climate Change Case ............................................................................................................. 15
  - Salvage Logging Case ........................................................................................................... 19
- Significance of the Case Representations .............................................................................. 22

## Conceptual Framework

- Part 1: Government Challenges ............................................................................................. 23
- Part 2: Academic Scientists’ Responses ................................................................................ 26
- Combining the Evolving Interpretations & Interests Frame and Freidson’s Theory of Professional Dominance ........................................................................................................ 30

## Research Questions

- Overarching Research Question ............................................................................................ 32
- Research Sub-questions ............................................................................................................ 32

## Definitions

- Organization of this Study ...................................................................................................... 34

## CHAPTER II: CONCEPTUALIZING “GOVERNMENT CHALLENGES TO ACADEMIC SCIENTISTS’ FEDERALLY SPONSORED RESEARCH”

- Overview ................................................................................................................................. 36
- The Social Contract ................................................................................................................ 37
- Three Frames of Government Challenges ............................................................................. 39
- Unified Interest Frame ............................................................................................................ 39
<table>
<thead>
<tr>
<th>Chapter Title</th>
<th>Page</th>
</tr>
</thead>
<tbody>
<tr>
<td>Summary of Case Selection Criteria</td>
<td>127</td>
</tr>
<tr>
<td>Data &amp; Data-Collection Methods</td>
<td>127</td>
</tr>
<tr>
<td>Rationale for the Data Source</td>
<td>129</td>
</tr>
<tr>
<td>Data-Collection Process</td>
<td>131</td>
</tr>
<tr>
<td>Data Analysis: Approach, Techniques, and Procedures</td>
<td>138</td>
</tr>
<tr>
<td>Individual Case Study Reporting: A Content Analysis with Theatrical Metaphors</td>
<td>138</td>
</tr>
<tr>
<td>Cross-Case Analysis: A Modified Grounded Theory</td>
<td>144</td>
</tr>
<tr>
<td>Trustworthiness, Validity, and Reliability</td>
<td>147</td>
</tr>
<tr>
<td>Credibility</td>
<td>147</td>
</tr>
<tr>
<td>Dependability</td>
<td>149</td>
</tr>
<tr>
<td>Generalizability</td>
<td>150</td>
</tr>
<tr>
<td>Confirmability</td>
<td>151</td>
</tr>
<tr>
<td>Ethical Issues</td>
<td>154</td>
</tr>
<tr>
<td>Compliance and Disclosures</td>
<td>155</td>
</tr>
<tr>
<td>Publicly Accessible Documents</td>
<td>155</td>
</tr>
<tr>
<td>Limitations of the Study</td>
<td>157</td>
</tr>
<tr>
<td>CHAPTER V: THE SEWAGE SLUDGE CASE</td>
<td>159</td>
</tr>
<tr>
<td>Introduction</td>
<td>159</td>
</tr>
<tr>
<td>The Science, the Public Policy Debate, and the Study</td>
<td>159</td>
</tr>
<tr>
<td>The Science Policy</td>
<td>159</td>
</tr>
<tr>
<td>The Controversial Science</td>
<td>161</td>
</tr>
<tr>
<td>Conflict Between the Federally Sponsored Study &amp; the Science Policy</td>
<td>162</td>
</tr>
<tr>
<td>Interactions Surrounding the Challenges</td>
<td>164</td>
</tr>
<tr>
<td>Act 1: Government Challenges</td>
<td>165</td>
</tr>
<tr>
<td>Act 2: Third-Party Reviewers</td>
<td>172</td>
</tr>
</tbody>
</table>
Summary of Challenges............................................................................................................... 177
Responses to the Challenges ................................................................................................. 178
Internal Characteristics ......................................................................................................... 178
External Factors .................................................................................................................... 186
Both Internal Characteristics & External Factors ................................................................. 193
Summary of Responses ............................................................................................................ 197
Summary & Conclusion ........................................................................................................... 198

CHAPTER VI: THE CLIMATE CHANGE CASE ................................................................. 204

Introduction ............................................................................................................................ 204
The Science and the Policy Debate ....................................................................................... 204
The Federally Sponsored Study ........................................................................................... 206

Interactions Surrounding the Challenges ............................................................................ 208
Act 1: Scientific Audits ........................................................................................................... 208
Act 2: Congressional Inquiry ................................................................................................. 217
Act 3: Two Reviews ............................................................................................................... 223
Summary of the Challenges ................................................................................................. 242

Responses to the Challenges ............................................................................................... 243
Internal Characteristics ......................................................................................................... 244
External Factors .................................................................................................................... 251
Both Internal Characteristics and External Factors ............................................................. 256
Summary of the Responses ................................................................................................. 261

Summary & Conclusions ...................................................................................................... 261

CHAPTER VII: THE SALVAGE LOGGING CASE ............................................................... 267

Introduction ............................................................................................................................ 267

The Science, the Public Policy Debate, and the Study ......................................................... 267
CHAPTER IX: ANALYSIS, IMPLICATIONS, AND CONCLUSION ........................................... 358

Introduction ............................................................................................................................. 358

What the Study Findings Contribute ....................................................................................... 359
  Government Challenges ...................................................................................................... 359
  Academic Profession’s Responses ...................................................................................... 366

Synthesis of the Challenges and Responses ........................................................................ 374

Revising the Conceptual Framework ...................................................................................... 376
  Government Challenges ...................................................................................................... 377
  Academic Profession’s Responses ...................................................................................... 379

Implications ............................................................................................................................. 387
  Implications for Academic Scientists Who Have Received or Seek Federal Sponsorship. 387
  Implications for Policymakers ............................................................................................. 388

Recommendations for Future Research .................................................................................. 389

REFERENCES ........................................................................................................................... 392
LIST OF FIGURES & TABLES

Figure 1.1: Evolving Interpretations & Interests Frame ......................................................... 25
Figure 1.2: Internal Characteristics of Professional Control .................................................. 28
Figure 1.3: External Factors to Professional Control ............................................................. 29
Figure 1.4: A Modified Version of Freidson’s Theory of Professional Dominance ............... 29
Figure 1.5: Study’s Conceptual Framework ......................................................................... 31
Figure 2.1: Evolving Interpretations & Interests Frame ......................................................... 76
Figure 3.1: Internal Characteristics & External Factors from the Literature on the Academic Profession’s Responses .................................................................................. 95
Figure 3.2: Internal Characteristics & External Factors Based on Freidson’s Theory of Professional Dominance ............................................................. 102
Figure 3.3: Internal Characteristics of Professional Control .................................................. 111
Figure 3.4: External Factors to Professional Control ............................................................. 111
Figure 3.5: Study’s Conceptual Framework ...................................................................... 114
Figure 4.1: Evolving Interpretations & Interests Frame ......................................................... 116
Figure 4.2: Internal Characteristics of Professional Control .................................................. 118
Figure 4.3: External Factors to Professional Control ............................................................. 118
Figure 4.4: Study’s Conceptual Framework ...................................................................... 120
Figure 9.1: Evolving Interpretations & Interests Frame ......................................................... 379
Figure 9.2: Internal Characteristics & External Factors Based on Freidson’s ......................... 381
Figure 9.3: Revised Conceptual Framework ...................................................................... 386

Table 3.1: Comparison of Terms Used for Internal Characteristics of the Profession .......... 103
Table 3.2: Comparison of Terms Used for External Factors of the Profession .................. 108
Table 4.1: Trustworthiness Standards and Strategies Employed ........................................... 154
Table 5.1: Acronyms for the Sewage Sludge Case ................................................................. 202
Table 5.2: Timeline of Relevant Actions and Events in the Sewage Sludge Case ............... 203
Table 6.1: Differences in Peer-Review Processes ................................................................. 237
Table 6.2: Acronyms for the Climate Change Case ............................................................... 264
Table 6.3: Timeline of Relevant Actions and Events in the Climate Change Case ............. 265
Table 7.1: Acronyms for the Salvage Logging Case ............................................................... 321
Table 7.2: Timeline of Relevant Actions and Events in the Salvage Logging Case .......... 322
Table 8.1: Summary of Finding #1 ...................................................................................... 333
Table 8.2: Summary of Finding #2 ...................................................................................... 339
Table 8.3: Summary of Finding #3 ...................................................................................... 344
Table 8.4: Summary of Finding #4 ...................................................................................... 348
Table 8.5: Summary of Finding #5 ...................................................................................... 354
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CHAPTER I: OVERVIEW OF THE RESEARCH

Introduction

This study explores what it means for government actors to challenge academic scientists’ federally sponsored research and academic scientists to exert control over that research. Drawing significantly on two methodological approaches, comparative case study and grounded theory, this study examines three controversies in which the government challenges academic scientists’ federally sponsored, environmental research. It reveals the government actors’ tactics and pressures employed by examining the circumstances and events surrounding each government challenge, and it identifies the means by which these academic scientists exerted some degree of control over scientific decisions regarding the research assumptions, methods, and analyses of their findings. Further, it evaluates the conceptual framework used in this study.

At the core of this study, I investigate the meaning and application of academic scientists’ intellectual freedom as manifested in how they exert control over their research when there are government challenges to that research. As Lewis and Ryan (1976) remind us, “Academic objectives cannot be fully attained until interference by individuals, or groups, outside the profession is minimized or, in the best of all possible worlds, eliminated” (p. 290). Inherent in the academic profession’s professional liberty is an implied understanding of intellectual freedom, which is intended to minimize lay challenges to its research. Intellectual freedom affords members of the academic profession protection to conduct research within their areas of expertise with impunity from lay individuals or groups, including government actors who are not trained in the particular research area (see generally American Association of University

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1 This study examines scientific challenges from public officials who are not specifically trained in academic science. In other words, the study examines lay, government challenges to academic scientists’ research, but for simplicity in language, I will refer to these actions generally as “government challenges.”
In the United States, intellectual freedom derives from both professional and constitutional sources of authority. The professional recognition started in 1915. The founding members of the American Association of University Professors (AAUP) issued a document that first articulated this professional liberty. Although the professional doctrine underwent several clarifications between 1915 and 1970, the concept has remained fairly consistent since its inception: As a professional liberty, intellectual freedom grants professors, including academic scientists, the ability to work within their research expertise without any penalties or fear of penalties from individuals or groups who are not experts in that area. In addition, in 1954, the U.S. Supreme Court recognized that intellectual freedom has constitutional protections. Largely derived from the First Amendment, the constitutional protection provides legal recourse to the academic profession when a government challenge to a professor’s research becomes an unjustified interference with the professor’s work, such as preventing a professor from publishing her results, absent either a contractual obligation that permits government control of research results or research issues posing a threat to national security. Based on both professional and legal sources, intellectual freedom indicates that professors operate freely to “pursue the investigation, research, … and publication of any subject as a matter of professional

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2 Although public university administrators may also serve as government actors who interfere with professors’ research, the focus of this study rests with government actors outside of the academy who, in their governmental role, sponsor or oversee federally sponsored research. According to those parameters, for purposes of this study, “government actors” typically refers to federal government agencies and members of Congress.
interest, without vocational jeopardy or threat of other sanction,” particularly from the
government (van Alstyne, 1975, p. 71).³

Moreover, when members of the academic profession reasonably perceive an unjustified,
government challenge to a professor’s research, the entire academic profession has a professional
obligation to defend the challenged professor’s intellectual freedom to preserve this liberty for
the whole profession (AAUP, 2001). Given the academic profession’s interest in preserving its
intellectual freedom, this study examines three case studies illustrating government actors’
challenges of academic scientists’ federally funded research and explores what it means for
academic scientists to exert control over that research.

To further clarify and amplify the basic focus of this research study, this chapter provides
a brief overview of the key concepts that shape this study. First, I present the study’s problem.
That is, since 1945, when the federal government established the national policy to fund
academic research as a strategy to spur U.S. scientific innovation, it has added significant limits
to its promise of funding research uninhibited by political influences. Instead, government actors
have identified ways to exercise their authority over academic research. While these actions are
well documented, government officials have introduced many new pressures and tactics since
1945. More important, the extant literature is not well developed in terms of addressing how the
academic profession exerts control over its research, particularly federally sponsored research.
The available literature tends to focus more on how the academic profession is largely

³ The academic profession’s liberty to work without undue, lay interferences is more frequently referred to as
“academic freedom.” Its aim is to afford the academic profession liberty in the areas of teaching, research, service,
and organizational governance. Yet, in the literature, the meaning and application of academic freedom differ
widely about what professional liberties faculty members have in terms of teaching, service, and organizational
governance. In the legal and professional circles, those differences also exist in practice. However, the concept of
academic freedom over research contains fewer debates within legal and professional circles and in the literature.
To distinguish this construct of “academic freedom over research” from other forms of academic freedom, I refer to
research academic freedom as intellectual freedom.
constrained by government actions rather than capable of managing or overcoming the
government challenge. Second, the problem presented leads me to this study’s purpose of
exploring what it means for (a) government actors to challenge academic scientists’ federally
sponsored research (particularly to investigate whether new or modified challenges are
presented) and (b) academic scientists to exert control over that research. Third, in order to
illuminate ways of understanding the concepts of government challenges and academic
scientists’ exerting control over their research, I introduce three cases that illustrate government
challenges involving academic scientists’ federally funded research on environmental issues.
Fourth, to organize the various concepts that guide this study, I briefly describe the study’s
conceptual framework, which, in ways different from prior literature, accounts for both the
government challenges and academic scientists’ responses. Fifth, connecting the cases with my
conceptual framework, which helped construct my study, I state the research questions used in
my initial inquiry to uncover what it means for government actors to challenge academic
scientists’ federally sponsored research and for academic scientists to exert control over that
research. Sixth, clarifying and drawing attention to key terms and phrases used in this study, I
define them. Finally, as a roadmap for this nine chapter study, I present its development and
organization.

Problem Statement

The relationship between academic science and the federal government developed
significantly after 1945. Immediately following World War II, U.S. policy advisors presented
concerns about the nation’s ability to protect itself and gain a competitive advantage in the global
economic market (Smith, 1990). Addressing these concerns, in 1945, Vannevar Bush of the U.S.
Office of Scientific Research and Development proposed a nationally coordinated science policy
in his report, *Science: The Endless Frontier*. The proposal recommended a plan that would focus on expanding the country’s scientific innovation by supporting an uninhibited inquiry into basic research. Basic research refers to the foundational work of science, driven by scientific curiosity, not scientific application. According to Bush (1945/1980), universities were in the best position to carry out this proposal because their purpose aligned with the advancement of basic research and their organizational culture fostered the pursuit of free inquiry (see also Dermeritt, 2000; Graham & Diamond, 2004; Guston & Kenston, 1994; Smith, 1990).

By all accounts, the federal government and American research universities agreed to work together toward this common goal of advancing scientific innovation, and Bush’s report established the basic terms and conditions for the parties (Dermeritt, 2000; Geiger, 1997; Graham & Diamond, 2004; Guston & Kenston, 1994; Smith, 1990). Pursuant to the agreement between the government and universities, government sponsorship of academic science grew substantially (Geiger, 2004; Guston & Kenston, 1994; Kevles, 1977; Smith, 1990). In 1940, federal sponsorship of scientific research at American universities totaled approximately $13 million (Axt, 1952). By 1953, that amount had increased to $138 million. Besides the financial support, this relationship between American research universities and government also rested, in part, on the assumption that government would defer to academic scientists’ expertise regarding scientific decisions connected with their federally sponsored research (see generally, Chomsky, Nader, Wallenstein & Lewontin, 1997; Dermeritt, 2000; Guston & Kenston, 1994; Smith, 1990). As Bush (1945/1980) described, this relationship between American research universities and the federal government was intended to operate with the understanding of “free play of free intellects, working on subjects of their own choice, in a manner dictated by their own curiosity for the explanation of the unknown” (p. 192).
The government’s promise of “free play of free intellects” quickly became an idealized principle. Soon after the adoption of this science policy initiative, the government took actions or adopted policies that placed limits on the principle of “free play of free intellects” when national security was at stake. Besides national security as a limiting the principle of “free play of free intellects,” around the late 1960s, the government began favoring federally sponsored research that met policy priorities and, at times, explicitly conditioned policy goals to research awards; examples of this bias can be found into the late 1970s. Starting around the 1980s, the government began establishing additional accountability measures to address scientific misconduct with federally sponsored projects, particularly misconduct by academic scientists. Although academic scientists and others in the academic community have recognized the need for accountability measures as a form of public intervention when illegal activities, such as research misconduct with public funds, take place, they also acknowledge that the more stringent accountability measures place academic scientists who accept government sponsorship of research at odds with the principle of “free play of free intellects.” In short, over the last 60 years, the government exerted its authority to protect the public by carefully adding new methods of limiting its promise of “free play of free intellects.”

These government actions, which are well documented, raise concerns about the academic profession’s ability to exercise its intellectual freedom. It is well established that intellectual freedom aims to permit the academic profession to “pursue truth unhindered” (Pincoffs, 1975, p. viii; see also Berdahl, 1990; Hofstader, 1955; Maclver, 1955; Metzger, 1955, 1987; Metzger et al., 1969; Rabban, 1990; Slaughter, 1988; van Alstyne, 1972, 1975). Society values intellectual freedom because it promotes a working environment in which the academic profession may perform research in an objective and disinterested manner and that research
potentially contributes to scientific innovation. Despite the purpose and societal benefits of intellectual freedom, “[p]ractical problems arise for academics [–] not as much over the abstract question of the nature of truth,” but rather over the ways in which academics may exert control over research so “they may engage … in pursuit of truth” (Pincoffs, 1975, p. xii). The extant literature offers little guidance.

Unlike the literature that trails government officials exerting its authority, the literature on government challenges to professors’ research often omits or only minimally discusses how the academic profession exercises control over its research. Instead, it typically portrays the academic profession as constrained by government controls (see, e.g., Carleton, 1987; Coker, 1954; Hansen, 1988; MacIver, 1955; Schwab, 1990). Only a modest collection of literature presents an examination of professors’ defenses to government’s challenges (see, e.g., Garrison & Kobor, 2002; Gutfeld, 1970; Hamilton, 1995; Lewis, 1988; Lilienfeld, 2002; Ludlum, 1950; Schrecker, 1980, 1986). Within that collection, a small subset of the literature informs readers of potentially successful strategies and tactics that the academic profession employed; however, these studies contain significant limitations (see, e.g., Garrison & Kobor, 2002; Hamilton, 1995; Lilienfeld, 2002). For example, Garrison and Kobor (2002) and Lilienfeld (2002) were conducted as a preliminary analysis to only one situation regarding a controversy over one academic publication. As the authors indicate, more information regarding ways in which the academic profession may exercise its intellectual freedom remains to be explored.

This message of constraint, rather than a focus on ways to exert professional control, is also represented in the literature on the academic profession. Often, this literature expresses perceived constraints from outside forces, including government, of the profession’s authority and autonomy over research. In fact, for the past several decades, professors have expressed
concerns over increasing limits on their ability to exercise professional discretion (see, e.g., Altbach, 1980, 1997, 2000; Bowen & Schuster, 1986; Cole, 2005; Gutmann, 2005; Newsom & Polster, 2001; Rhoades, 1998). Raising attention about the increasing limits’ leading to an overall decline in the state of the profession, the literature on the academic profession has invoked harshly critical phrases such as “a crisis of the professoriate” (Altbach, 1980), “a national resource imperiled” (Bowen & Schuster, 1986), “uncertainties in the changing academic profession” (Massey, 1997), the “crisis in the academy” (Altbach, 1997), and the “war against the faculty” (Nelson, 1999).

The overarching theme of these lines of research is that the academic profession has experienced a feeling of professional defeatism when faced with government challenges. The literature suggests that the presence of intellectual freedom as a professional liberty is perhaps insufficient to defeat government challenges. While it is possible that intellectual freedom is insufficient when facing the interests put forth by the government and that the academic profession may be experiencing declines in professional authority and autonomy, the reality is that the academic profession maintains some control over its research. Yet, asserting intellectual freedom by itself is not the silver bullet that immediately overcomes a government challenge and translates into the academic profession’s taking control of its research. As Menand (1996) points out, intellectual freedom “is not simply a kind of bonus enjoyed by workers within the system, a philosophical luxury” (p. 4). Instead, constant defense of intellectual freedom is required for its continued viability as a professional liberty. Cole (2005) and Gutmann (2005) even remind the academic community that challenges to intellectual freedom from outside of the academy are likely to persist if the academic profession does not respond with counteractions. Consistent with that view, the AAUP even states that in order for the academic profession to maintain its
intellectual freedom, members of the profession must protect it (AAUP, 2001). The problem, however, is that defenses to intellectual freedom are not well documented in the literature, and that likely contributes to the academic profession’s sense that government constraint over its research is simply a part of professional practice.

**Purpose of the Study**

The purpose of this study is to explore government challenges and academic scientists’ responses to those challenges, so I may introduce a theoretical explanation depicting *what it means for government actors to challenge academic scientists’ federally sponsored research and for academic scientists to exert control over their research*. Since the academic profession’s expressions regarding government constraints appear most visibly with federally sponsored research, I investigate cases involving government challenges to academic scientists’ federally sponsored research. These expressions of government constraints are not surprising, given the government’s evolving definition of “free play of free intellects” for federally sponsored research. However, rather than summarily accepting government constraints as part of the practice for the academic profession, I acknowledge that the government maintains some authority over its own employees (i.e., federal scientists). In addition, the government maintains some authority over projects it funds – an authority that may manifest itself in government challenges to the academic profession’s federally sponsored research. At the same time, I recognize and explore even further that the academic profession, too, maintains some control over research projects it conducts – even when the government sponsors the project.

Because this study involves the government’s challenging academic scientists’ federally funded research and academic scientists’ responses to those challenges in order to protect their intellectual freedom, I first examine circumstances and events surrounding the challenges. I
present three cases to illustrate government challenges of academic scientists’ federally funded research on environmental issues.

I draw on environmental issues because this topical area represents a significant body of complaints found in the literature where scientists, including academic scientists, have recently claimed that government officials challenged their research without sufficient scientific merit (see, e.g., Cole, 1983; Greenberg, 2001, 2007; Jasanoff & Martello, 2004; Pielke, 2007; Schoenbrod, 2005; Wagner & Steinzor, 2006). In addition, the basis for several past government actions (e.g., National Ambient Air Quality Standards, Data Access Amendment, Data Quality Act) that shaped the relationship between the government and the academic scientific community stemmed from environmental research (e.g., CHESS study, Harvard Six Cities study). While examining and analyzing the circumstances and events surrounding government challenges to academic scientists’ research on environmental issues, I also review the strategies, tactics, and other means that the academic community employed to preserve or defend its intellectual freedom. At the end, this study introduces a theoretical explanation of what it means for academic scientists to exert control over research when government officials challenge that research.

The Cases

To investigate this concept of academic scientists’ exerting control over their research, I use three cases with substantially similar characteristics. Most significantly, these cases involve government challenges to academic scientists’ federally sponsored research on environmental issues and include the academic scientists’ responses to those challenges. In addition, the researchers in each case are university-based academic scientists in the United States, working for American universities. Also, the government officials are U.S. federal government
employees who assert their authority to act on behalf of the public’s welfare, which includes actions such as reviewing research, compelling testimony, and requesting documents and other related materials regarding a study’s findings.

The cases also contain some differences. The most evident difference is that each case represents a different environmental issue: sewage sludge, climate change, or salvage logging. The principal government actors, who challenge the academic scientists’ federally sponsored research, also differ. In one case, two federal agencies (i.e., the U.S. Department of Agriculture [USDA] and the Environmental Protection Agency [EPA]) challenge the academic scientists’ research. In another case, members of Congress are the primary challengers, and in a third case, members of Congress and the Bureau of Land Management are the primary challengers. The timing of the controversy also differs. In two cases, the studies’ dissemination and controversy both occurred in the same year – though one study took place in 1997 while the other study was released in 2006. In the third case, the controversy revolved around two connected studies released in 1998 and 1999, respectively; however, the heart of the public controversy (i.e., the claimed government challenges with academic scientists’ research) did not commence until 2005. Despite these differences among the cases, their important similarities make them useful in examining how academic scientists experienced government challenges with their research and how they exerted some control over scientific decisions regarding the research assumptions, methods, and analysis of the findings.

Sewage Sludge Case

The first case addresses government challenges with academic scientists’ research on sewage sludge safety levels. According to the EPA’s Criteria for Classification of Solid Waste Disposal Facilities and Practices, sewage sludge is “solid, semisolid, or liquid residue generated”
from households and industries that is treated through a sewer treatment plant process (see 40 C.F.R. § 257.2). Basically, sewage sludge represents the cumulative materials of treated by-products from various municipal sewer systems and industrial waste plants. These by-products include liquids drained, human excrement flushed, and other substances that enter the sewage lines.

A law passed in 1988 phased out the dumping of sewage sludge in oceans. As an alternative depository, a coalition of municipalities, sludge-management companies, and some farmers advocated land usage of sewage sludge. When properly treated, sewage sludge contains many beneficial nutrients for farming and landscaping purposes. Recognizing land usage of sewage sludge as a substitute for ocean dumping, the EPA adopted new regulations – frequently referred to as “503 rules.” These regulations created new classifications of selected chemicals, metals, and other particles, and they permitted more lax ratios and indicators of microorganisms in acceptable levels of sewage sludge.

With federal and state grant funding, in 1996, a research team of academic scientists consisting of Ellen Harrison, Murray McBride, and David Bouldin of Cornell University’s Waste Management Institute conducted a study that evaluated the effects of land use of sewage sludge. In particular, the team analyzed the impacts of the 503 rules. In 1997, the research team issued a working paper, *The Case for Caution: Recommendations for Land Application of Sewage Sludges and an Appraisal of the U.S. EPA’s Part 503 Regulations*. The study raised serious health and safety concerns over the EPA’s regulations on sewage sludge for land application, and it identified 14 problems with the regulations, including assumptions made to construct the regulations. For instance, according to the report’s authors, the regulations do not account for

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4 The EPA and the sewer management programs determined an acceptable level of industrial waste that may enter the municipal sewage systems. If these regulations are weakened, there is an increased likelihood of higher levels of pathogens and other harmful chemicals entering sewage systems.
regional variations in soil quality. Consequently, the regulatory standards do not factor in those differences when conducting risk assessments. Put simply, the study demonstrated problems with the risk-assessment guidelines in the 503 regulations. To resolve the risk assessments’ problems, the study recommended more stringent regulations over sewage sludge and elaborated on the potentially severe health risks that might emerge without changes to the regulatory language.

The initial draft of the research paper yielded reactions from state and federal officials. To begin, New York State officials and several federal officials from the EPA and USDA issued numerous letters and e-mails to the study’s authors, the president of Cornell University, an associate dean of Cornell’s College of Agriculture, key policymakers including other staff members within the EPA and the USDA, and sludge-management companies. These state and federal officials raised questions about the study authors’ competence and intent. They discredited the authors of the initial draft and criticized the research assumptions, methods, and analyses contained in the report. According to the government officials who authored the correspondence, the intent of the letters was to challenge the research’s value to the scientific community, because it assertedly contained severely flawed assumptions, methods, and findings. Thus, the government officials believed that they acted within their authority to inform various parties who had an interest in the study about the problems with the research study.

In contrast, several observers of the controversy interpreted the government officials’ correspondence as aggressive attacks intended to discredit the study without sufficient bases. In these observers’ view, these actions unduly interfered with academic scientists’ intellectual freedom. In fact, one of the study’s authors, Ellen Harrison, testified before a U.S. House of Representatives hearing, stating:
EPA and USDA have attempted to discredit our science and to ignore the issues we have raised. Their responses have mischaracterized our research and have suggested that we used methods that are not appropriate to answering the scientific questions we seek to address. Some of their assertions about our work (for example that we used metal salts and not sludges to study leaching in a greenhouse study) are simply untrue. This was especially surprising since the same EPA and USDA staff who made the allegation also sat on the advisory board to the project.

In other words, Harrison believed that the government officials’ tactics to discredit the academic scientists’ research went beyond traditional government protocols. Similarly, academic scientists, other environmental scientists, and several policymakers contended that when government officials sent correspondence to the president and an associate dean at Cornell University in an effort to discredit the academic scientists and their studies, those government officials acted inappropriately. Based on the academic scientists’ interpretations of the events, the correspondence inferentially conveyed threats of political retaliation; that is, future government grants and contracts could be jeopardized if Cornell University did not intervene and assist with damage control (Snyder, 2005). As Martin (1999) indicates,

“If someone disagrees with a scientist’s research conclusion or public statements, an accepted method of response is to criticize the argument, for example, by sending a letter to the scientist or to a journal. By contrast, sending a letter of complaint to the scientist’s boss or funding body, attacking the scientist’s credibility or right to speak out, would be seen by many as an attempt to apply pressure on the scientist rather than address the issues under dispute” (p. 110).
In short, the government officials’ letters that challenged the study’s research went beyond merely raising concerns about the study’s reliability: They were likely intended to cause harm to the scientists’ reputations and to call into question their consideration for future funding. Thus, they represented government challenges to the three academic scientists’ research.

Climate Change Case

The second case presents a conflict between academic scientists and government officials over the highly publicized debate about climate change. The climate change controversy, typically referred to as global warming, revolves around the degree to which anthropogenic matter, processes, and effects contribute – if at all – to the increasing emission rates of greenhouse gases, which cause global warming. Stated another way, the debate centers around whether humans contribute to and can control the effects of global warming. Many academic scientists have spoken (see, e.g., Bray, 2010; Dessler & Parson, 2010; Rosenzweig, et al., 2008; Union of Concerned Scientists, 2010a, 2010b). An overwhelming number of them publish, present, and testify in open forums that anthropogenic contributors account for at least some of this global temperature rise. In other words, they contend that humans are causing and generating these greenhouse gas emissions, and these gas emissions are not occurring under natural environmental conditions.

In 2001, the United Nations’ Intergovernmental Panel on Climate Change (IPCC 2001) examined the Earth’s climate changes and declared the matter of rising temperatures a state of international urgency. According to the IPCC 2001 report, the Earth’s temperature is rising, and humans contribute to these rising temperatures. In support of that proposition, the report cited two key studies: a 1998 article in *Nature* and a 1999 article in *Geophysical Research Letters*. Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes co-authored the two peer-
reviewed articles, also known as MBH98 and MBH99, respectively. In a graphical format, the researchers reported in both studies evidence of steady temperature movements throughout the 1600s and 1700s, but their model displayed a sharp increase in temperature around the mid-1800s, with continuous rises today. The line representing temperature on the graph resembled an image of a hockey stick. When controversy arose about the data, the graph that depicts these increasing temperatures became known metaphorically as the “hockey stick.” According to the hockey stick researchers, population growth and industrialization explained the increases in the global temperature. In other words, humans significantly contributed to the rising global temperature.

Because the studies that created the hockey stick were products of federal grants to examine climate change using statistical modeling and the issue linked to public policy matters for the nation as well as to international treaties, Congress got involved. Under congressional authority to act on behalf of the public’s welfare, which includes reviewing federally supported research and gathering more information on climate change for public policy matters, on June 23, 2005, U.S. Representative Joe Barton, chair of the Committee on Energy and Commerce, and Representative Edward Whitfield, chair of the Subcommittee on Oversight and Investigations, sent letters to the three climate researchers of MBH98 and MBH99, the chair of the Intergovernmental Panel on Climate Change (IPCC), and the director of the National Science Foundation. Each letter requested multiple pieces of information to be submitted within three weeks. Barton and Whitfield demanded that each of the climate scientists produce a CV, along with all articles on climate research and sources of funding, a list of all financial support for all research including honoraria, a list of all financial support from federal grants, a document identifying locations for all data archives, including where and when information related to
research was discovered or first identified, information regarding source codes and release of that information, and identification and information regarding requests made for data and responses to those requests. In addition, the congressmen requested that these climate scientists provide a detailed response to an article criticizing their work. The chair of the IPCC and director of NSF received similar, very detailed, requests.

According to Barton and Whitfield, the investigation largely stemmed from concerns raised in a February 14, 2005 *Wall Street Journal* article. The article reported potential scientific problems surrounding the data and the methods used to output the hockey stick graph and the accessibility of that data. Using the information from the article and asserting federal accountability measures as reasons to initiate their inquiry, Barton and Whitfield raised five potential items of interest about the hockey stick study. First, two Canadian researchers, Ross McKitrick, an economist, and Stephen McIntyre, a minerals consultant who worked in industry, questioned the research methods and data used for MBH98 and MBH99. McIntyre and McKitrick asserted that the hockey stick was flawed. Thus, Barton and Whitfield specifically requested Mann, Bradley, and Hughes to comment on McIntyre and McKitrick’s accusations in relation to MBH98 and MBH99. Second, McIntyre and McKitrick had requested the data and the source codes to replicate MBH98 and MBH99, but they reported that they could not get the information. Third and related to the last point, the National Science Foundation supported the research efforts for MBH98 and MBH99, so Barton and Whitfield followed up on data accessibility of federally funded projects and questioned why McIntyre and McKitrick could not access the data from the MBH98 and MBH99 studies. Fourth, if valid questions did exist about studies heavily relied on to craft IPCC 2001, that information would raise concerns over the 2001 report’s legitimacy. Thus, because the studies were products of government funding, Barton and
Whitfield wanted to explore the validity and reliability of MBH98 and MBH99. Fifth, at the time of Barton and Whitfield’s letter, the IPCC Committee had started work on its 2007 report. With notice of potential flaws in the MBH98 and MBH99 studies, Barton and Whitfield’s letter to the IPCC Chair would provide adequate notice to the IPCC of possible problems with future reliance on MBH98 and MBH99, or at least caution the group before the IPCC decided to cite those studies again. Given these five concerns, as stated in the letters from Barton and Whitfield, members of Congress wanted more information about how Michael Mann, Raymond Bradley, and Malcolm Hughes had constructed the hockey stick.

Although Barton and Whitfield asserted legitimate government reasons for their requests, many academic scientists perceived Barton and Whitfield’s actions as unusual requests, because the letters demanded materials that would normally not be asked for or needed, such as (a) copies of all past research using federal funds, even research unrelated to the controversy and (b) the computer code to run the data, which is not necessarily needed for another scientist to replicate the data; furthermore, these critics contended, the code would reveal protected intellectual property. According to several academic scientists, not only were the demands extensive, the requests for data and documents were very detailed (Monastersky, 2005a, 2005b, 2006). Furthermore, the congressmen provided a very short period of time to gather these materials – just under three weeks. Even the critics of the targeted academic scientists wondered if they would be able to comply with the directives to produce the massive amount of documentation and other supporting materials (Monastersky, 2005a, 2005b, 2006).

For many academic scientists, the letters signaled a chilling effect on academic research that is inconsistent with political agenda. Based on news reports, many academic scientists and other observers claimed that these congressional demands would have the likely effect of
silencing, discrediting, and discouraging researchers from entering the global warming debate. Some academic scientists referred to the Barton and Whitfield requests as a “dubious’ inquiry” (Brown, 2005, p. 1) and a form of “harassment” likely intended to silence them (Monastersky, 2005a, p. A1). In addition, others remarked that the events were intended as an “intimidation tactic” to deter other academic scientists from pursuing this research agenda (Daley, 2006, p. A1).

Salvage Logging Case

The third case also recounts academic scientists’ reports of government challenges with federally sponsored research connected to a proposed policy; however, in terms of a timeline of events, the study’s announcement and the policy’s formulation occur in closer proximity than occurred in the climate studies controversy. During the 2005-2006 congressional session, Congressmen Greg Walden (R-Ore.) and Brian Baird (D-Wash.) were negotiating language for a new House bill, The Forest Emergency Recovery and Research Act. In an effort to address forest recoveries from wildfires, the bill would permit a more aggressive approach to salvage logging after wildfires and would relax procedural approvals for logging companies to act.

After forest fires occur, one forest regeneration approach is post-fire salvage logging. Post-fire salvage logging is the practice of cutting down (i.e., “felling”) damaged trees from wildfires, then replanting. Several academic scientists from Oregon State University (OSU) supported the bill’s scientific rationale, including the dean of the College of Forestry and several professors within the college. According to these academic scientists, post-fire salvage logging serves as an effective forest regeneration approach after wildfires.

While the House bill was still pending, on January 5, 2006, a controversial study challenged the widely accepted assumption that post-fire salvage logging significantly aids in
forest regeneration in all instances. The researchers (two graduate students, two forest researchers, and two professors), who worked on a federal grant study housed at Oregon State University, found that post-fire salvage logging actually harms forest regeneration. Based on data collected from a severe wildfire in southwestern Oregon, known as the 2002 Biscuit Fire, the researchers compared data from two locations – where salvage logging had and had not taken place – and they found that the non-invaded areas displayed greater regenerative signs. In other words, the study’s findings contradicted the public policy rationale for the proposed legislation, *The Forest Emergency Recovery and Research Act*.

Drawing attention to the study’s impact on assessing a public policy issue, *Science*, the scientific journal that published the study, referred to the proposed legislation, and the online version of the article even included links to the bill’s language. Then, within a month from the study’s initial release, on February 1, 2006, the Bureau of Land Management (BLM), which oversaw the research grant funding that supported a series of ecosystem studies at Oregon State University including this post-fire salvage logging study, suspended all of OSU’s funds connected with the ecosystem project. According to the BLM, publication of the study’s findings violated two grant conditions, so the BLM’s action was justified. Specifically, the federal funding agreement stipulated that “lobbying with appropriated moneys” is forbidden, and the researchers must consult the program officer or a Bureau representative before publication. The “lobbying” claim is based on the links to the pending bill, *The Forest Emergency Recovery and Research Act*, that accompanied the article in *Science*.

Immediately following that action, several events took place in an effort to convince the BLM to reinstate the funding. Oregon State University and the editor of *Science* each issued letters defending the academic scientists who authored the post-fire salvage logging study. OSU
and Donald Kennedy, then editor of *Science*, indicated that during the copy-editing process, the authors requested removal of the links and any references to the pending bill; nevertheless, the publication retained the links to the House bill. In addition, the BLM, several federal agencies, and numerous federal policymakers received a deluge of letters arguing that the suspension of funds was inappropriate. Furthermore, national and regional media reports also criticized the BLM’s decision to suspend funding.

After reviewing the information presented, approximately a week after the suspension, the BLM reinstated the grant funding, but its public reason for the grant reinstatement was based on evidence that the authors did not lobby and that the authors indicated they were unaware of the requirement that they consult BLM prior to publication. In other words, the BLM rationalized that the authors did not knowingly violate any of the grant’s stipulations and had acted appropriately, so the BLM could authorize the reinstatement of the grant funds. According to observers who supported the academic scientists, the government actions of funding suspension amounted to political pressure. Although it was certainly within the power of the governmental sponsoring organization, as detailed in the contract, to suspend funding for the alleged lobbying activity and for publishing results without clearance, funding suspension without investigating the situation seemed hasty and beyond normal government practices. Furthermore, even though the authors of the study did not initiate the lobbying activities, academic scientists have a duty to inform the public about their findings.

In addition to the BLM funding suspension and reinstatement, there were other ramifications for the academic scientists. Selected members of Congress exercised their investigatory powers. Specifically, Congressmen Walden and Baird, authors of the House bill, conducted a hearing to investigate the findings associated with post-fire salvage logging. The
congressmen invited several academic scientists to testify, in their effort to gather expert scientific information on the subject matter. Supporters of the academic scientists who wrote the controversial paper noted the differences in questioning styles used for the advocates of post-fire salvage logging and for Dan Donato, the lead author of the study that concluded that post-fire salvage logging hinders forest regeneration. According to observers, Donato experienced interrogation-like tactics at the congressional hearing; further, they believe the harsh questioning style was intended to silence the academic scientists and discredit their study, and that the interrogation method at the congressional hearing exceeded traditional norms when members of Congress intend to discover more information about a study. In short, many members of the academic scientific community as well as supporters of the study’s authors expressed concerns about the government’s method of dealing with the study and the academic scientists who authored it.

**Significance of the Case Representations**

Even though the federal government maintains authority that in certain instances permits it to limit academic scientists’ intellectual freedom over research, in order to maintain the professional liberty of the scientists, their institutions, and the academic profession, the limits of freedom need to be more widely understood. Academic scientists still need (a) more clarity about the circumstances and events surrounding government challenges with their research, particularly in terms of what the academic scientists experienced, and (b) a better understanding of how academic scientists exert control over scientific decisions regarding the research assumptions, methods, and analysis of their findings. The examination of these cases is intended to illuminate how academic scientists, who work on federally sponsored research to study environmental matters with national scientific policy implications, experience government
challenges with their research and respond by exerting control over scientific decisions pertaining to their research.

**Conceptual Framework**

In order to conceptualize the problem, in chapters 2 and 3, I review literature from several sources to frame my study. Although I follow the basic structure of prior literature, which frames the challenges and then the responses, unlike frameworks used in this literature, I suggest one that offers a new way to examine academic scientists’ responses to exert control over their research. I briefly present, below, my conceptual framework as two interconnected parts: one part helps understand the government challenges while the other part helps structure the examination of the academic profession’s responses so as to illuminate ways in which professors exert control over their federally sponsored research.

**Part 1: Government Challenges**

Through my review of the literature on government challenges to professors’ research, I identify three distinct frameworks used previously in the literature. I refer to them as the unified interest frame, the competing interests frame, and the evolving interpretations and interests frame. The prior literature does not specifically name the frameworks as I have; however, I believe that these identifiers reflect their distinguishing characteristics. For instance, the unified interest frame examines how the academic profession integrates into society and contributes to societal goals; the competing interests frame examines how the academic profession handles conflict within a divided society; and the evolving interpretations and interests frame examines how the academic profession interacts with individuals and groups within society to shape and define those experiences as a way to illuminate their interests in the contested matter. Each frame provides a distinct contribution to understanding the connection between the role of the
academic profession within society and the circumstances and events surrounding government challenges. That is, each framework presents a different way to think about these studies on government challenges.

Based on my review of the literature on government challenges (Chapter 2), the evolving interpretations and interests frame appears to be the most useful approach to think about the circumstances and events surrounding government challenges to academic scientists’ research. As I explain in Chapter 2, academic scientific research involves highly complex concepts. The meanings associated with those concepts may evoke different interpretations based on both how the information was presented and the person interpreting that information. The evolving interpretations and interests frame presents a way of thinking about how symbols and language, including academic scientific messages, impact interpretations along with subsequent interactions. With those insights, I may better understand the government’s actions to determine the pressures, tactics, and other means that it employed when challenging the academic scientists’ federally sponsored research.

As illustrated in Figure 1.1, the evolving interpretations and interests frame traces the meanings and interactions from the standpoint of the various actors involved in a controversy about contested meanings from messages or other expressions. By contrast, the other two frameworks focus more on the interrelation of educational institutions, government, and class to uncover the circumstances and events surrounding the government challenges. Yet, I believe that examining individual and small-group interpretations and interactions to the extent possible from my data reveals more insights than would the other frameworks, when the issue revolves around academic scientific concepts and the research project involved.
The evolving interpretations and interests frame’s ability to examine situations as individuals report how they experience them, especially regarding potentially different interpretations of academic scientific concepts, serves as the best framework to uncover insights about the circumstances and events surrounding government challenges with academic scientists in particular. As I demonstrate in this study, these insights reveal what kind of government challenges academic scientists are experiencing, why academic scientists perceive recent government actions as beyond the traditional norms of government inquiry, and how academic scientists interpret those government actions as well as actions of other interested parties involved in the controversy.

**Figure 1.1: Evolving Interpretations & Interests Frame**

While the evolving interpretations and interests frame reveals more insights about the circumstances and events surrounding government challenges with academic scientists’ research, those insights respond to only one part of this study’s inquiry (i.e., government challenges to academic scientists’ federally sponsored research). Neither this frame nor the other two employed in past literature on government challenges present a framework to examine how the academic profession exerts control over its research. Therefore, in the next part, I briefly present
how I expanded my review of literature to find an appropriate framework that would support my examination of how academic scientists’ responses to government challenges exert control over their research.

Part 2: Academic Scientists’ Responses

While the evolving interpretations and interests frame offers insights about government challenges, it is less helpful as a conceptual framework in understanding the academic profession’s responses that exert control over their federally sponsored research. As I discuss more fully in Chapter 3, the literature on the academic profession’s responses to government challenges suggests a more detailed consideration of multiple factors to fully appreciate the academic profession’s actions that reflect strategies, tactics, and other means to exert control over its federally sponsored research. Specifically, the literature on the academic profession’s responses suggests considering both internal characteristics of the academic profession and external factors that influence the profession’s degree of autonomy over research. The internal characteristics identify institutionalized features of the academic profession that contribute to its professional autonomy. The external factors refer to conditions outside of the academic profession that influence the profession’s degree of autonomy over research.

While prior research demonstrates that both the internal characteristics and external factors offer a way to understand the academic profession’s responses to government challenges, these studies do not fully explore the significance of the internal characteristics of the profession. Specifically, past studies have not used a framework that explicitly considers the internal characteristics addressing the academic profession’s (a) divisions of labor with project tasks related to research (“divisions of labor”) and (b) academic professional training and development (“professional training”).
To address these conceptual omissions, I propose the use of a modified version of Eliot Freidson’s theory of professional dominance (2001). Although Freidson’s theory has never been used to study the academic profession, his theory fills in the conceptual gaps found in the extant literature. I do, however, make minor modifications to Freidson’s theory, so it better fits the purpose of my study on the academic profession. The modified version of Freidson’s theory considers six internal characteristics of the academic profession and two external factors.

The internal characteristics are: (1) professional knowledge and skills, (2) divisions of labor, (3) labor markets and careers, (4) training programs, (5) ideologies, and (6) professional associations (see Figure 1.2). Professional knowledge and skills are the formal knowledge of the profession that rely heavily on mental judgments over technical details, such as the academic profession’s application of specific research approaches to gather data for a study. The divisions of labor characteristic considers the jurisdictional boundaries related to occupations working on different aspect of a project, such as the academic profession assessing the scientific implications of a policy while policymakers draft the legislation. The labor markets and careers characteristic reflects the profession’s authority in society to determine who qualifies for practice and how one qualifies; one example is the academic profession’s default standard that the Ph.D. is a prerequisite for many college faculty positions. Training programs, usually graduate schools, are the formal educational settings that prepare future professionals and create new knowledge for future professional practice (e.g., a new research methodology). Ideologies reflect the values embedded in the profession’s actions, such as the academic profession’s work to advance the interest of the public good through education. Professional associations are the organized units that represent the academic profession, such as the AAUP and disciplinary organizations.
The two external factors are bodies of knowledge and the state (see Figure 1.3). Bodies of knowledge represent society’s capacity to recognize the value of a profession’s formal knowledge, such as placing market value on research findings. The state serves as a more formalized source; it provides legal authority or protections so the profession can control its work, through mechanisms such as the First Amendment.

According to Freidson (2001), both the internal characteristics and external factors construct how a profession is organized to maintain its economic and political position in society, which allows it to exert control over its work. Similarly, the extant literature on the academic profession views the internal characteristics and external factors as representing elements of an institutionalized arrangement that grants the academic profession autonomy to work freely without undue lay interference. Thus, these internal characteristics and external factors present a structured set of elements for me to consider when I try to understand how the academic profession’s responses might reflect strategies, tactics, and other means to exert control over its federally sponsored research (Figure 1.4).

**Figure 1.2: Internal Characteristics of Professional Control**

![Diagram showing internal characteristics of professional control](image)

Source: Author’s figure based on a modified version of Freidson’s Theory of Professional Dominance (2001)
Figure 1.3: External Factors to Professional Control

State (Legal Authority)  Bodies of Knowledge

Source: Author’s figure based on Freidson’s Theory of Professional Dominance

Figure 1.4: A Modified Version of Freidson’s Theory of Professional Dominance

Legal Authority  Bodies of Knowledge

Knowledge & Skills  Divisions of Labor
Labor Markets & Careers  Training Programs
Ideologies  Professional Associations

Autonomy & Authority Over Academic Profession’s Research

Source: Author’s figure based on Freidson’s Theory of Professional Dominance
Combining the Evolving Interpretations & Interests Frame and Freidson’s Theory of Professional Dominance

In sum, my conceptual framework incorporates both the evolving interpretations and interests frame and a modified version of Freidson’s theory of professional dominance. I draw on the evolving interpretations and interests frame to provide insights about the circumstances and events surrounding government challenges to academic scientists’ research. In particular, these insights structure a way to examine the government challenges (including the actors involved and methods employed). Those insights help me understand the pressures, tactics, and other means that the government employs to challenge the academic scientists’ federally sponsored research. In addition, I use a modified version of Freidson’s theory of professional dominance to examine the academic profession’s responses. The modified version of Freidson’s theory considers the internal characteristics of and external factors relating to the academic profession to understand the profession’s strategies, tactics, and the other means it employed when government challenges take place.

Taking these two parts together, I present my study’s overall framework (see Figure 1.5). With this overall framework, I strive to understand what it means for academic scientists to exert control over their federally sponsored research.
Figure 1.5: Study’s Conceptual Framework

Source: Author’s figure of the integrated concepts drawn from the Evolving Interpretations & Interests Frame
Freidson’s Theory of Professional Dominance
Research Questions

As the cases illustrate, according to academic scientists, government officials challenged academic scientists’ federally sponsored research that involves proposed or existing national science policies. To uncover how academic scientists experience and respond, and ultimately exert control, when government officials challenge the academic scientists’ federally funded research, this study poses the following questions:

Overarching Research Question

How do academic scientists, who are members of a unique but variegated profession, exert some degree of control over scientific decisions pertaining to their federally funded research when government officials challenges that research?

Research Sub-questions

1. Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research,
   a. what kinds of government actors have participated or are now participating in activities that challenged academic scientists’ research on sewage sludge, global warming, and salvage logging?
   b. what kinds of nongovernmental actors have participated or are now participating in activities that challenged academic scientists’ research on sewage sludge, global warming, and salvage logging?
   c. what methods (i.e., pressures, tactics, and other means) did government actors employ to challenge academic scientists’ research on sewage sludge, global warming, and salvage logging?
d. what methods (i.e., pressures, tactics, and other means) did nongovernmental actors employ to challenge academic scientists’ research on sewage sludge, global warming, and salvage logging?

2. Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research,
   a. how did academic scientists, universities, professional associations, and other professional bodies in the Academy respond to the lay challenges with academic scientists’ research on sewage sludge, global warming and salvage logging?
   b. what range of strategies, tactics, and/or other means did academic scientists, universities, professional associations, and other professional bodies employ to preserve or defend the profession’s intellectual freedom?

Definitions

For purposes of this study, the following terms and phrases are defined as follows:

**Academic profession**: The academic profession represents professors, including academic scientists, who are employed at American universities.

**Academic scientists**: Academic scientists are faculty or researchers who are also scientists. They are employed at American colleges and universities, and they work in the United States. The academic scientists’ principal employer is the university – even if the funds for their salary allocations draw heavily from grant money.

**Actors**: Actors represent any entity (including individuals, groups, and organizations) that has the capacity to make meaning of an event and interact.
Federally sponsored research: Federally sponsored research refers to any U.S. federal agency’s financial support for a grant research project administered by academic scientists at an American college or university, where the research takes place within the United States.

Government challenges: Government challenges occur when government actors who have no scientific expertise in the area contest the scientific merits of a federally sponsored academic scientist’s study. Government actors include individuals, groups, or institutions (such as offices, agencies, or branches of government) operating within their professional capacity as employees or arms of the federal government of the United States.

Intellectual Freedom: Intellectual freedom is a subset of academic freedom. Academic freedom is the academic profession’s professional liberty from undue lay interference over matters pertaining to teaching, research, service, and organizational governance. Intellectual freedom refers to the academic profession’s liberty from undue lay interference over matters pertaining professors’ research.

Organization of this Study

This study is divided into nine chapters. This chapter introduced the research study with a discussion that included the research problem, its context, the three cases, the purpose, and its significance. Chapter 2 reviews the pertinent literature on lay, government challenges to professors’ research to explore ways to understand the pressures, tactics, and other means that government employs to challenge academic scientists’ federally sponsored research. Chapter 3 reviews the extant literature on the academic profession’s responses as well as the literature on the academic profession and the sociology of the profession to identify a framework that helps understand the strategies, tactics, and other means that the academic profession employs to exert control over its research. Chapter 4 presents the research questions and describes the research
methodology, the data-collection methods, the data analysis, and elements to ensure research trustworthiness. Chapters 5–7 present the data for each of the case studies. Chapter 8 constructs a cross-case analysis to explore common themes among the cases. Chapter 9 analyzes the study’s findings. In addition, based on the data and the studied phenomena, it presents implications for the conceptual framework and suggests ways to improve the proposed theory. Finally, Chapter 9 also explores the study’s implications and significance for academic scientists’ practice and for scholars who explore topics about academic scientists’ intellectual freedom.
CHAPTER II: CONCEPTUALIZING “GOVERNMENT CHALLENGES TO ACADEMIC SCIENTISTS’ FEDERALLY SPONSORED RESEARCH”

Overview

In Chapter 1, I introduced the purpose of this study: to offer a theoretical explanation depicting what it means for government actors to challenge academic scientists’ federally sponsored research and for academic scientists to exert control over that research. In chapters 2 and 3, I review literature from several sources to develop the framework that guides this study. The extant literature on the academic profession’s responses to government challenges consistently suggests a two-part framework – one part to explore the government challenges (which I discuss in this chapter) and another part to examine the academic profession’s responses (see Chapter 3). Together, these two parts contribute to my study’s overall framework, which will help uncover the means by which government actors have challenged academic scientists’ federally sponsored research and illuminate what it means for these university-based scientists to defend and exert control over that sponsored research.

Accordingly, this chapter reviews the literature on government challenges to academic scientists’ federally sponsored research. The extant literature on government challenges to professors’ research conceptualizes government challenges by uncovering the circumstances and events surrounding each challenge. Specifically, the extant literature applies one of three frames, each of which offers different insights about the pressures, tactics, and other means used by government actors. However, before I discuss these three frames, I briefly revisit the science policy initiative that paved the way for federal sponsorship of academic research. This background offers readers a clearer understanding of why the government challenges alter the
relationship between the federal government and academic scientists who participate in the federally sponsored research.

The Social Contract

As I mentioned in Chapter 1, in response to U.S. concerns about the nation’s ability to protect itself and gain a competitive advantage in the global economic market, Vannevar Bush of the U.S. Office of Scientific Research and Development issued a policy report, *Science: The Endless Frontier*, which proposed a nationally coordinated science policy. The 1945 report presented a national plan that included universities as a key player to advance American science innovation. Outlining the roles of universities and government, it established the terms and conditions of what is often referred to as the *social contract* between academic science and society. For the academic community, participation was premised on an understanding of financial support from the government within a working environment that operated off the principle of “free play of free intellects.”

*Financial Support*

Pursuant to the agreement between government and universities, government sponsorship of academic science grew substantially (Geiger, 2004; Guston & Keniston, 1994; Kevles, 1977; Smith, 1990). In 1940, federal sponsorship of scientific research at American universities totaled approximately $13 million (Axt, 1952). By 1953, that amount had increased to $138 million. In addition to the financial commitments, starting in 1946, President Truman and Congress moved more aggressively on the national science agenda by establishing a federal infrastructure for science policy through the creation of multiple offices to manage federally sponsored research within the academic scientific community. For example, in 1946, Congress established the Office of Naval Research (ONR) as one of the principal scientific research and development
arms for the nation – with universities serving as the primary contractor (Rees, 1987). In that same year, the Atomic Energy Commission (currently known as the Department of Energy) also established basic research initiatives through universities, and it worked with several universities around the country to establish federal labs on college campuses as well as regional labs that engaged multiple universities (Buck, 1983; Holl, Hewlett, & Harris, 1997). In 1950, the federal government established the National Science Foundation (NSF) as the national entity largely responsible for the management of federal sponsorship of basic research (Lomask, 1976). In short, with the federal infrastructure in place to distribute the funds, colleges and universities had access to the federal dollars intended for use to investigate matters of basic research that would contribute to society.

“Free Play of Free Intellects”

Besides the financial support, this relationship between American research universities and government also rested, in part, on the assumption that government would defer to academic scientists’ expertise regarding scientific decisions connected with their federally sponsored research (see generally, Chomsky et al., 1997; Dermeritt, 2000; Guston & Keniston, 1994; Smith, 1990). As Bush (1945/1980) described, this relationship between American research universities and government was intended to operate with the understanding of “free play of free intellects, working on subjects of their own choice, in a manner dictated by their own curiosity for the explanation of the unknown” (p. 192). While members of the academic community expressed fears that reliance on federal support for research might lead to reduced scientific freedoms to carry out their work (see, e.g., Kidd, 1959, 1963; Price, 1954), as a whole, the government appeared to afford many academic scientists and universities the ability to conduct research freely during the early years of the social contract (Geiger, 2004).
Nonetheless, over the past 60 years, the government has also taken steps to redefine and limit this principle of “free play of free intellects.” These limitations are outcomes of government challenges to the academic profession’s federally funded research.

Three Frames of Government Challenges

To explore the emergence of these limitations, in this section I review the extant literature and examine the frameworks (i.e., the Unified Interest Frame, Competing Interests Frame, and the Evolving Interpretations and Interests Frame) used to uncover the pressures, tactics, and other means that government actors have employed to challenge the academic profession’s federally sponsored research.

Unified Interest Frame

One line of research examining government challenges frames the inquiry in terms of how societal institutions and beliefs informed or shaped the actions of the government and the academic profession. These studies have tended to follow or specifically adopt the sociological lens referred to as the functionalist perspective. The functionalist perspective assumes that social institutions, such as the academic profession, act to further the goals of society.\(^5\) According to the functionalist perspective, society consists of interrelated parts that work together as a single system to create social order (Merton, 1957a, 1957b; Muller, 1994). In this system, the interrelated parts consist of institutions such as education, business, and government. Through social organizations such as the academic profession, universities, and government agencies, these institutions cooperate and seek consensus to further societal priorities. Similarly, the

\(^5\) The literature on government challenges to professors’ research that follows a societal interest frame typically adopt a Durkheimian view, a subset of the functionalist perspective of examining society (Rhoades, 2007). Although I note this distinction here, for purposes of this discussion, the specific variation of the functionalist perspective is not critical. The reader should simply recognize that researchers have classified this line of research as capturing societal priorities, or society’s unified interest, to explain the circumstances and events surrounding government challenges of the academic profession’s research.
studies discussed in this section frame matters in terms of actions that further society’s unified interest, specifically to inquire about how social institutions, via representative organizations such as the academic profession, work together to achieve societal order. Thus, I refer to this line of research as studies taking the Unified Interest Frame.

*Application and Contributions of the Unified Interest Frame*

The literature falling under the unified interest frame often examines government challenges during the Cold War, especially the period from the late 1940s through much of the 1950s. During this period of American history and amid allegations that some American scientists handed critical technologies to the Soviet Union, the public feared the Communist ideology and considered members of the Communist party to be a suspect group. Reflecting this societal impression, government actors formed special committees, such as the U.S. House Un-American Activities Committee (HUAC), the U.S. Senate Internal Security Subcommittee (SISS), and the U.S. Senate Permanent Subcommittee on Investigations (PSI), to interrogate individuals suspected of having Communist ties. This cultural phenomenon, in which government efforts sought out and interrogated individuals with Communist connections during the 1940s through the 1950s, became known as the McCarthy era, named after Senator Joseph McCarthy, the most recognized political figure to lead one of the government committees. In the eyes of many, these committees, along with similar state-level committees during the McCarthy era, represented societal interests and championed the efforts to protect the nation from Communist subversion.

Examining the circumstances and events surrounding the government challenges, these studies applying the unified interest frame illustrate government pressures placed on the academic profession to conform with society’s values (see, e.g., Lewis, 1988; Schrecker, 1986).
This frame illuminates several key findings. First, these studies reveal how societal priorities required the academic profession to support anti-Communist initiatives, even if those initiatives contradicted the academic profession’s values and professional liberty. Based on a historical analysis of faculty experiences during the McCarthy era, Schrecker (1986) uncovers evidence that the profession failed to respond and assert its rights of intellectual freedom. Instead, the profession furthered societal values by either standing quietly by or remaining silent while terminations, interrogations, and blacklisting tactics were used against faculty with alleged Communist connections. According to the government, professors who embraced Communism could not exercise unbiased judgment or act within the research or teaching norms of the profession, and their ideology was “incompatible with professional obligations” of the academic profession (Schrecker, 1986, p. 74). Examining the consequences, Schrecker (1986) concludes that the government challenges weakened the profession’s ability to exercise its intellectual freedom.

Second, through this frame, we learn that government actors took measures that restricted hiring of faculty candidates with Communist connections or pressured universities to fire professors with Communist connections. In one instance, even after a faculty committee at Rutgers University concluded that faculty members with former Communist affiliations should be retained, state and federal officials reminded university trustees that keeping these faculty members would tarnish the institution’s reputation and harm the university by jeopardizing government financial support – which at the time could have impacted federal sponsorship of research and building construction awards in the form of grants and loans. Rather than subject the institution to potential lost opportunities for federal financial support, the trustees overruled the faculty committee’s recommendation, an action that, as Schrecker (1986) notes, “infringed
upon the most coveted prerogative of the academic profession: its right to select its own members” (p. 179).

Similarly, Lewis (1988) examines the phenomenon surrounding the anti-Communist movement and its impact on intellectual freedom. Based on a socio-historical study tracing college administrators’ and professors’ reactions to the Communist attacks from 1947 to 1956, Lewis observes that legislators acted on behalf of the social order to justify their tactics in dealing with Communist professors – even when those tactics challenged professors’ research. For example, when legislators interrogated faculty about their possible connections to the Communist party, the legislators publicly justified their actions as inquiries into these faculty members’ professional competence to perform duties such as academic research. Yet, the government’s interrogations did not truly evaluate any faculty member’s capacity to make reasoned inquiries within his or her disciplinary area. The interrogations simply assumed that Communist affiliations made one ineligible to research and teach with independence.

Furthermore, Lewis (1988) suggests that when the media reported interrogations of an institution’s faculty, that publicity harmed the university’s reputation. According to Lewis, the publicity shaped the public’s perception that universities that employed these individuals were harboring suspected Communists. Consequently, to spare the institution negative publicity, many college administrators felt compelled to terminate professors under investigation – even when these faculty members complied with the government inquiries and reported no Communist ties. That is, the institution sought to separate its affiliation with these faculty members. In large part, both the government and the college administrators acted to align institutional values with societal priorities, rather than evaluating the professors based on academic knowledge. The consequences of conforming to societal priorities, however, may
include permitting government to challenge professors’ ability to work, and permitting
challenges to their research activities. As Lewis (1988) and Schrecker (1980, 1986) illustrate, the
unified interest frame helps us uncover those circumstances and events surrounding government
challenges as they further clarify the decision-makers’ actions in relation to societal goals.

Other studies applying a unified interest framework reveal very similar government
pressures and tactics to those the academic profession faced in the studies discussed above (see,
e.g., Eisenberg, 1988; Park, 1986; Shattuck, 1986). These studies report that societal interests,
especially in terms of national security, justified certain challenges by government agencies, such
as reviewing and approving federally funded research prior to publication (i.e., prepublication
reviews), restricting information access levels of academic researchers on federally funded
projects, and subjecting academic researchers to inspections of research data and analyses, when
questions arise about the research integrity.

Having explained the application and contributions of the unified interest frame, I now
turn to the unified interest frame’s limitations.

Limitations of the Unified Interest Frame

The unified interest frame has several limitations. First, it tends to omit or downplay
competing interests that do not serve societal priorities. As I mentioned in the preceding sub-
section, the unified interest frame, as its primary explanatory value, places emphasis on
institutions and structures functioning to further societal goals. Nonetheless, competing interests
often account for conflict between parties. Thus, understanding the competing interests may
clarify or explain the reasoning behind the pressures and tactics employed. For example, Lewis
(1988) and Schrecker (1980, 1986), who follow the unified interest frame, conclude that the
academic profession failed to defend its intellectual freedom to further societal interests when it
bowed to pressures of eliminating Communist influences. Yet, the authors also mention that many members of the academic profession conceded to the government pressures, not to advance societal interests but to manage other concerns. One of these concerns was the academic profession’s ability to control its students. Subgroups within the academic profession believed that supporting the intellectual freedom of alleged Communist faculty demonstrated the profession’s general support of radical groups and viewpoints. Thus, radical student groups such as the American Youth for Democracy might perceive the academic profession’s support for alleged Communist faculty as implicit support for their campus protests, which at times included severe campus disruption. Accordingly, to avoid that misperception, some members of the academic profession wished to distance themselves from radical thought and associations for their own interests.

Overlooking competing interests would present a limitation for my study. The cases presented in this study appear to indicate groups with competing interests that, if explored, might illuminate the case controversies between government and the academic profession. As I described in Chapter 1, the cases in this study describe scientific problems that do not always reflect a clear scientific consensus. They involve environmental policy issues of proposed or pending legislation that generated debates from multiple actors – including industry, environmental groups, legislators, and federal agency officers – about public policies on scientific matters. These cases suggest that competing interests might play a significant part in framing the circumstances and events surrounding the challenges. Therefore, by underscoring the presence of competing interests and the various groups’ expressions, the unified interest frame might not fully uncover the circumstances and events surrounding government challenges to professors’ research, and it might not fully uncover the intentions and motives of key actors in
the situation, yet these intentions and motives may have significant implications for understanding government actions in cases involving government challenges to academic scientists’ federally sponsored research.⁶

Another limitation of the unified interest frame as a way to conceptualize government challenges to academic scientists’ research is its ability to observe individual and small-group interactions. Because the unified interest frame is less concerned with individual and small-group interactions, it tends to overlook or downplay interactions, perceptions, and decision-making processes of individuals or narrowly defined groups in society. Yet, these insights may describe and explain how people negotiate and construct meaning to matters related to a complex social problem, such as the underlying reasons for government challenges to academic scientists’ research (see, e.g., Beauregard, 1988; Gusfield, 1975). For instance, a section of Beauregard’s (1988) book that recounts government challenges to faculty in Ohio during the McCarthy era illustrates the significance of micro-level observations. Beauregard describes how government challenges to the academic profession’s work were not only about Communist party affiliations. Following the interactions among various actors, he reveals that, in numerous instances, the government extended the scope of its investigations when it had concerns about what it deemed

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⁶ Competing interests tend to uncover conflicts, power imbalances, and positions of multiple actors on various matters. Several authors even describe situations in which interest groups, industry representatives, policymakers, and academic scientists have, at some point in time, asserted their interests or policy preferences through the use of unfounded or inaccurate scientific analyses, and their interests prevailed over societal priorities (see, e.g., Landman, Cortese, & Glantz, 2008; Mooney, 2005, 2007; Rosenbaum, 2008).

For example, Landman, Cortese, and Glantz (2008) disclose the tobacco industry’s strategies to combat the U.S. Surgeon General’s 1988 statement regarding the adverse health effects and addictive nature of nicotine. One strategy included employing academics to present a positive campaign about nicotine use without disclosing their industry ties to these announcements. Because the public generally perceives members of the academic profession as legitimate informants when it comes to scientific information, the tobacco industry hired them to disseminate research and other materials that featured individual and social benefits of nicotine use. For this situation, the societal interest frame might examine how the academic profession addressed those academic researchers’ actions that supported the tobacco industry’s goals without full disclosure – since those actions conflict with both the academic profession’s and societal values. Although that analysis is helpful to understand the academic profession’s remedies to align with societal values, it omits or minimizes an analysis regarding the power of the tobacco industry, lobbyists, public relations firms, and the media.
as “radical” behavior. As Beauregard notes, the government did not confine its interpretation of radical behavior to include just academic research activities believed to advance the interests of the Communist party. With their authority, some government officials recognized the opportunity to investigate other radical behaviors – such as interracial dances and labor union activity on college campuses. Nevertheless, the unified interest frame is less concerned with matters at the micro level, so interactions, perceptions, and decision-making processes of individuals or narrowly defined groups in society are largely ignored in its discussion about circumstances and events surrounding a situation such as government challenges to academic scientists’ research.

Similarly, the unified interest frame’s lack of attention to micro-level observations presents a limitation for examining the cases in my study too. These cases present conflicts that are, at least in part, generated from differing individual and group interpretations regarding the meanings about specific academic scientific studies. For example, in the climate change case, the initial summary of facts, which I reported in the previous chapter, raised concerns of possible discrepancies with the two federally sponsored academic studies (i.e., “hockey stick” studies). The research criticisms sparked several exchanges involving academic scientists, government officials, and critics on both sides of the climate change debate. While the initial preview of the case presents only a brief set of facts, data pertaining to the micro-level observations might help us to understand the conflict among various groups involved in the case and to fully appreciate the means (e.g., pressures and tactics) by which the government challenges academic scientists’ federally sponsored research. However, the unified interest frame does not fully capture these micro levels of analysis, and this presents a potential limitation in uncovering the circumstances and events surrounding government challenges to academic scientists’ research.
Summary and Conclusion

In sum, the literature applying a unified interest frame informs readers of how institutions and actors advanced societal priorities as the basis to justify the government challenge of the academic profession’s federally funded research. Generally speaking, these studies conclude that when matters of national security are at issue, the principle of “free play of free intellects” comes with significant limits and, in such circumstances, it is permissible for government to challenge the academic profession’s federally sponsored research. These challenges include determining who qualifies to practice in the academic profession, who may participate in certain federally sponsored research – such as projects involving governmentally sensitive or classified information – and what research materials and findings may be distributed.

While the literature applying the unified interest frame reveals the means by which government actors have challenged the academic profession’s federally funded research, this framework has two significant limitations. It largely downplays or omits (1) discussions regarding competing interests and (2) patterns of behavior drawn from individual or small groups. As I illustrated above, these limitations would likely present significant drawbacks to my study in terms of uncovering the multiple actors and understanding and appreciating their positions and actions in each case dispute. Thus, in the next section, I consider another frame used in the literature to conceptualize government challenges to the academic profession’s research.

Competing Interests Frame

Another line of research examining government challenges frames the inquiry in terms of how structures in society, such as educational level, social prestige, and power, are arranged to advance the dominant class. These studies tend to follow or specifically identify the sociological
lens of conflict theory as its framework. Conflict theory investigates how society benefits groups that fall in the dominant class while depriving other groups that fall in subordinate classes. Most often, these studies regarding government challenges of the academic profession’s government-sponsored research demonstrate how government actions and policies advance the dominant class’s interests. Its significance as a framework is how it examines competing interests and the ways they account for the government’s challenge of the academic profession’s federally sponsored research. Thus, I refer to the literature falling under this line of research as the competing interests frame.

Application and Contributions of the Competing Interests Frame

One strand within this line of research reveals how political priorities limited what qualified as “free play” of academic scientists working on federally sponsored research (Beneke, 1998; Brooks, 1978; Guston & Keniston, 1994; Kash, 1968; Roback, 1968; Salancik & Lamont, 1975). According to Guston and Keniston (1994), from around the late 1960s through the late 1970s, “programmatic directions for research originated not from scientists but from Congress,” and these federal priorities did “not necessarily align[] with those of the scientists” (p. 14). Although federally funded projects have always maintained the “right to impose conditions upon use of funds[] and to stipulate the terms and conditions under which they may be used” (Kidd, 1963, p. 617), starting around the late 1960s, scientific investigations that met political priorities appeared to get more favorable treatment and, at times, advancing political priorities was even a condition to federal sponsorship (Brooks, 1978; Geiger, 2004; Kistiakowsky, 1989; Leslie, 1994).

For example, during the late 1960s and early 1970s, federal policymakers felt compelled to emphasize their interest in funding projects to specifically support the Vietnam War
(Kistiakowsky, 1989). Formalizing the policy priority for federally funded academic research, in 1969, Congress passed the Mansfield Amendment of the Military Reauthorization Act. The law expressly stated that the Department of Defense (DOD) shall not “carry out any research project or study unless such project or study has a direct and apparent relationship to a specific military function” (10 U.S.C. § 2358(c) [1971]; see also, Bell, 1973; Geiger, 1992). Similarly, the research indicates that during this time period of the late 1960s and early 1970s, government funding of environmental matters addressed geophysical topics that served military interests, rather than emerging concerns such as environmental hazards (Doel, 2003). Put simply, some of the studies applying the competing interests frame uncover how the political agenda at times shapes the programmatic directions of what federally sponsored projects would qualify as “free play of free intellects.”

Another strand of research using the competing interests frame explained more than the means for government to challenge the academic profession’s federally sponsored research. These studies also revealed the role of the nongovernmental dominant group and its connection with the government’s challenges. These studies illustrate pressures and tactics, often highlighting disparities in political and/or economic power among groups and how the dominant class drives the policy agenda. For example, in one case study that addressed state funding of academic research, state officials suspended an economics professor, Louis Levine, for nearly 10 months because Professor Levine’s scholarly writings and publications proposed a new tax structure that would adversely impact the mining industry’s profits (Gutfeld, 1970). The controversy arose when, in late 1916, the chancellor of the state university in Montana placed the economics professor on a committee to identify new revenue sources for the institution. While analyzing the state’s tax structure, Professor Levine recognized problems with the current tax
calculations: The assessments severely undervalued the mining industry’s tax obligation. As part of his study, Levine devised a new assessment model that would significantly increase the annual tax assessments on the mining industry. Not surprisingly, the mining industry did not respond favorably.

As the dominant group, the mining industry pressured state officials to suppress Levine’s study and silence him. Complying with those pressures, the state engaged in several challenges to the professor’s research expertise. These actions included an investigation regarding the professor’s statements, threats of further scrutiny as a strategy to discredit him, and denied access to the university press as an outlet to publish his report. Based on archival data tracing this event from 1918 to 1919, the state government took the role of active suppressor of the professor’s research expertise. Interestingly, the role of the principal mining company “was nowhere directly demonstrable,” yet as Gutfeld (1970) quotes a secondary source, “‘Everywhere the hand of the [mining] company left its nebulous mark, but nowhere its fingerprints.’” The vast power of the mining interests enabled them to coerce the administration of the state without using directly visible marks” (p. 36). Despite the shadowy proof of the mining industry’s active involvement, the competing interests frame still reveals that the circumstances and events surrounding government challenges to professors’ research serve the interests of the dominant class.

While business is often represented in the dominant class, the dominant class is not a fixed collection of groups. A group may fall in or out of the dominant class. The literature on government challenges to professors’ research that follows the competing interests frame reveals, by examining past events, how a group emerges into social power and into the dominant class. Typically, this literature traces how a group within society (e.g., college students, ideologically
driven organizations, and the media) uses coercive tactics to acquire power and control over the academic profession or another group as a means to position itself in the dominant class (Hamilton, 1995). After the group thrusts itself into dominance, the new, dominant group uses its position to, for instance, initiate government challenges with professors’ research.

Following the competing interests framework, Hamilton (1995) illustrates the movement of a group into the dominant class. In his study, he concludes that ideological “zealots” trampled over professors’ research, often using the government as their agent, in their efforts to reach the dominant class. He argues that about every 15 to 20 years, from 1870 to the early 1990s, a new group emerged to suppress professors’ intellectual freedom. Based on his historical and legal analyses, each of the emerging dominant groups espoused fanatical, ideologically based viewpoints that ran counter to professional norms of an unbiased and reasoned search for knowledge. In order for the dominant group to maintain its position or gain strength over the subordinate group (i.e., professors), the dominant group employed coercive tactics that typically involved government challenges to professors’ research.

Tracing the roles of various powerful groups over time – such as religious fundamentalists, capitalist-minded college boards, nationalists who protected sentiments of patriotism surrounding World War I, anticommunists before World War II, McCarthyist actors, student activists, and radical academic leftists – Hamilton (1995) discusses how each of the prevailing groups leveraged its power through coercive tactics such as accusing, labeling, investigating, and attacking the academic profession (i.e., the subordinate group) in a manner that ostracized some actors and threatened other groups’ positions to emerge as a dominant group. For instance, Hamilton cites examples of the emergence of left-wing radicals as a dominant group during the early 1990s. As professors expressed critical comments in their
research that might be interpreted as politically incorrect statements, the emerging dominant
group (i.e., the left-wing radicals) quickly quashed the legitimacy of these professors’ statements
and framed them as expressions meant to intimidate marginalized classes of people.
Specifically, the emerging dominant group labeled the subordinate group’s actions as harassment
and discrimination.

In one example, a professor’s public comment about an identified ethnic minority group’s
academic preparation for employment, along with selected articles and expressions that analyzed
racial differences in educational opportunities, spurred a U.S. Department of Education Office
for Civil Rights (OCR) investigation that requested all of the professor’s research and writings.
According to Hamilton (1995), the OCR tried to determine whether the professor’s practices at
the institution reflected what she had expressed. If it was found that her practices did reflect
some of her critical comments about selected ethnic minority groups, she might have violated a
federal law that prohibited racially discriminatory practices in higher education. Although the
OCR investigation found no violation, the investigation and accusations created an environment
that members of the academic community would perceive as hostile to open dialogue and
scholarly expression. Thus, taking the competing interests frame, the circumstances and events
surrounding government challenges to professors’ research offer insights about how various
actors employ some dimension of power, in this instance, power exercised through coercive
tactics.

More recently, the literature employing a competing interests framework has also
revealed questionable tactics that pose challenges to the academic profession’s federally funded
research through seemingly scientific sound practices, particularly through a practice known as
regulatory peer review (Shapiro & Guston, 2007). Regulatory peer review involves a review
panel of independent scientists to evaluate relevant studies pertaining to the policy issue, assess the state of the science, and advise policymakers about the policy options. A regulatory peer-review process has the potential to overcome policymakers’ biases and sort out good and bad science. Nonetheless, as Shapiro and Guston (2007) and others explain, in practice, that is not always the case.

Several studies investigate how government challengers have selectively used federal scientists, academic researchers, commissioned groups, lobbyists, industry representatives, and other policymakers to help advise them (the government challengers) as policymakers (Ashford, 1983; Jasanoff, 1985, 2006; Shapiro & Guston, 2007; Steinzor, Wagner, & Shudtz, 2008). According to these previous studies, government officials “stacked” review committees (a technique called committee stacking), with selective experts who would help the government actors fashion a recommendation based on their policy preference. While the use of experts might add credibility to the evaluation process, scientists and other critics of the government challengers have questioned the independence of these reviewers and the balanced perspectives of committees conducting the reviews (Ashford, 1983; Jasanoff, 1985, 2006; Shapiro & Guston, 2007; Steinzor, Wagner, & Shudtz, 2008). Shapiro and Guston (2007) point out that when policymakers have significant discretion in selecting the reviewers, peer review stacking may occur, which involves the selection of committee members with a particular perspective. As Shapiro and Guston (2007) suggest, peer review stacking would likely lead to biased outcomes, such as reaching the conclusions of the government challengers’ policy preferences.

Furthermore, the literature using the competing interests frame has also explored how certain legislation appearing as accountability measures has eased the process for government and other actors to challenge the academic profession’s federally funded research, when the
research topics have policy implications (Couzin & Unger, 2006; Wagner, 2005). The laws emerged after successful lobbying efforts by industry representatives who opposed the findings of several federally funded academic research studies. In the late 1990s, federal policymakers used the academic research projects at issue to help them determine acceptable pollutant levels, which were eventually codified into the National Ambient Air Quality Standards (NAAQS). The adoption of the new standards was expected to cost industry millions of dollars each year to reach the phased-in compliance levels (Esworthy & McCarthy, 2006). Given the change in standards and the costs associated with those changes, during the public discussion phase of the NAAQS, industry scientists and other scientific evaluators sought to analyze the data from the supporting studies. However, the academic scientists of one the studies initially restricted access to the data. They did so because they wanted to comply with their promise of anonymity for the research subjects so as to protect their identities and medical information. Nevertheless, when the academic scientists restricted access to the data, critics of the policy asserted that the academic scientists did not want anyone to review the data or replicate the study. The implication was that access to the data would uncover evidence that the academic scientists blurred the lines between science assessor and science policy advocate.

In reaction to the data access problems and upon encouragement from industry representatives, the government established additional accountability measures. Through these accountability measures, the federal government intended to address scientific research that lacks sufficient scientific support for an advocated policy position (Couzin & Unger, 2006; Mooney, 2005; Wagner, 2005). Rallying to the purpose of protecting the public’s interest, Congress enacted two laws, reportedly to ensure data integrity by giving public access to federally
sponsored research and by establishing a protocol for the public to challenge any scientific research used to inform federal policy.

While these two accountability measures appear on their face to serve the public’s interest, they are significant tools for actors other than the government to discount or minimize the role of federally funded academic research. I will first describe each accountability measure, and then I will explain how they could potentially block the dissemination of federally funded academic research by inviting nongovernmental actors, particularly adversaries of the academic research, to participate in the research challenges.

In 1999, the Data Access Amendment (DAA) to the Freedom of Information Act, also known as the Shelby Amendment, mandated accessibility of federally sponsored research data that recipients of federal grants maintain (Couzin & Unger, 2006; Wagner, 2005). This provision allows the public to access the data from publicly funded research for independent reviews. On its face, this appears to be a law that serves the public’s interest. For example, under the DAA, one beneficial feature is that an independent reviewer may uncover whether severe scientific misconduct occurred and/or whether the scientific recommendations reflect undue reliance on insufficient evidence, perhaps to advance a policy preference. Interestingly enough, in 2000 – after the passage of the DAA – an independent review supervised by the Health Effects Institute concluded that the federally sponsored academic research studies that helped inform policymakers to determine the National Ambient Air Quality Standards were sound scientific examinations with reasonable conclusions (Krewski et al., 2000). Despite that, the law is still perceived by the public and policymakers as a reaction to academic research used to advocate a policy with insufficient data (Hornstein, 2006; Wagner, 2003).7

7 Similarly, in 2001, the National Research Council (NRC) reviewed the research on this topic of air particulates in the environment and concluded that both the Six Cities and ACS studies contained reliable information. Also,
Even though the reanalysis of sponsored research studies vindicated the academic scientists of any scientific wrongdoings, in 2001, Congress also enacted the Data Quality Act (DQA), which represents another accountability measure. The DQA requires the establishment of uniform guidelines for federal agencies to ensure consistent data evaluation procedures in research that federal agencies rely upon to create or influence policy decisions (Couzin & Unger, 2006; Wagner, 2005). It also permits anyone to challenge data and request an agency evaluation to correct or remove studies used to help craft a policy when disputes exist over the data. This measure could delay important new research and the adoption of related policies, but the extent of this accountability measure is best observed when considering the uses of both the DAA and DQA.

Both the DAA and DQA permit actors, other than government, to limit the principle of “free play of free intellects.” These laws used together present significant tools to discount or minimize the role of federally funded academic research. For example, when a party wishes to contest federally sponsored academic research that is used to influence or create science policy, that party can, pursuant to the DAA, demand release of the federally sponsored data. The interested party may then examine the data with the intent of crafting another study that purports to present seemingly contradictory data. As Hornstein (2006), Wagner (2003, 2005), and Wagner and Michaels (2004) suggest, the interested party may use private funds to conduct a study that produces the party’s desired results, including data that conflicts with the federally sponsored academic research. Unlike the federally sponsored academic research, the interested party’s privately funded research is not subject to scrutiny under the DAA. Furthermore, it does not have to comply with the same levels of scientific rigor, such as peer review, which are standard
practices for academic research as well as federally sponsored research. With conflicting data from the interested party’s study, under the DQA, the interested party can then challenge the federally sponsored academic research’s data. That challenge potentially leads to a re-examination of the academic study and likely requires the study to be temporarily or permanently removed from the regulatory, decision-making process. In the end, it is possible that under these accountability measures, the government places greater reliance on the privately funded research and affords the scientists of the privately funded project more deference than it does the academic scientists engaged in the federally sponsored research (Wagner, 2005; Wagner & Michaels, 2004).

On their face, the DAA and DQA represent accountability measures that potentially offer significant contributions to advance the public’s interest. The laws provide public access to data and scrutiny of the study methodology and results. These laws, however, also present opportunities for potential negative effects, such as ignoring the peer-review process and allowing different treatment for studies, depending on the funding source. For academic scientists who receive federal funding, these laws create legally justifiable means for actors other than the government to limit “free play of free intellects” through serious delays and ad hominem attacks. For policymakers, these laws offer greater opportunity to rely on industry research, which is not subject to the same levels of scrutiny.

In sum, the competing interests frame offers insights about the conflicts in society as groups compete to advance their interests but ultimately the dominant group prevails. This section discussed how political priorities, industry interests, and members of the public may potentially convince or pressure government officials to challenge the academic profession’s federally sponsored research.
Limitations of the Competing Interests Frame

While the competing interests frame offers a way to examine government challenges to professors’ research, it also has two significant limitations. One limitation is that the competing interests frame underscores or ignores the presence of shared societal interests that promote social order. As applied to the literature on government challenges to the academic profession’s research, the competing interests frame assumes that institutions within society, including universities and government, feed into social arrangements – such as educational attainment and social prestige – which are unfairly distributed in society. The imbalances of these social arrangements reflect or exacerbate competing interests and group conflicts.

Nevertheless, as I discussed earlier and mentioned in the case summaries regarding government challenges to academic scientists’ federally sponsored research on environmental topics, it is well established that universities and the academic profession act to benefit societal interests – particularly when it advances its research in pursuit of the social good (see, e.g., Frank & Gabler, 2006; Geiger, 1986). Furthermore, other social institutions also work with universities and the academic profession to further societal interests such as advancing science. Several past studies even examine how universities, business, and government have successfully collaborated to advance national science initiatives that serve the public good (see, e.g., Crow & Tucker, 2001; Etzkowitz, Webster, Gebhardt, & Terra, 2000; Mowery, Nelson, Sampat, & Ziedonis, 2001). Similarly, for this study, the competing interests frame’s focus on conflict and competing interests may overshadow a general pattern of societal cohesion, such as collective actions that further societal interests. That focus on conflict and competing interests over consensus and social integration presents a limitation of this framework to examine the cases for my study.
Besides underscoring shared, societal interests, the competing interests frame has another limitation. Just like the unified interest frame, the competing interests frame places emphasis on broad patterns drawn from large social structures and classes of society, to examine group conflicts within a divided society. This framework generally overlooks the individual or small-group interactions as the source to explain a group’s interests and actions. Nevertheless, when the situation involves highly complex matters such as academic scientific terms and processes, this macro lens (i.e., focusing on large social structures rather than individual or small-group interactions) may oversimplify what is reported as the circumstances and events surrounding government challenges, which in turn potentially limits what the study reveals in terms of pressures, tactics, and other means employed. Kroll (2001) illustrates this limitation in an article that examines how individuals and groups construct different meanings about one scientific study. The study that generated these different meanings is Rachel Carson’s 1962 groundbreaking work on the environmental harms from pesticide use. The U.S. Department of Agriculture challenged Carson’s study and attempted to discredit it. Kroll traces three media forms that told the Rachel Carson story. Each media form resulted in different public impressions about the study and subsequent actions. Kroll concludes that the contextual presentation of the Carson study altered people’s interpretation of the study. Yet, Kroll’s examination required breaking down the actors to smaller groups and tracing their interactions – that is, using a micro level of analysis.

Under the competing interests frame, interpretations and interactions surrounding a social conflict are primarily captured from the standpoint of significant social actors, or they are based on the relationship among broad social structures and forces in society. That is, this framework examines social situations using a macro-level analysis. Consequently, the competing interests
frame largely ignores or minimizes the effect of interpretations and interactions from individuals or small groups as a way to conceptualize government challenges – which for purposes of this study presents a limitation as a lens that would help me understand the circumstances and events surrounding government challenges to academic scientists’ research.

**Summary and Conclusion**

In sum, the competing interests framework presents a lens that follows the actions of key groups in society confronted with conflict and competing interests. As the literature illustrates, the competing interests frame examines society in terms of social arrangements that foster inequality and constraint, and groups in society such as the academic profession and government often participate in this societal environment to advance the interests of the dominant group. This section informs us of various ways in which government actors have represented or have been the dominant group, which led to government challenges of the academic profession’s government-sponsored research. Specifically, we learned that government challenges have occurred by (1) setting political priorities that define what areas of research the government will financially support, disseminate, and entertain for policy consideration; (2) intimidating or weakening the position of marginalized groups; and (3) adopting policies or processes (e.g., regulatory peer review and accountability measures) that appear to be credible systems but as applied, compromise the scientific review system through committee stacking or inviting adversaries to participate in the attacks on federally funded academic research.

While the studies employing the competing interests framework help illuminate the circumstances and events surrounding government challenges to professors’ research and thus can contribute to my understanding about the pressures and tactics employed, this framework has two significant limitations when applied to my study. It underscores or ignores the presence of
shared societal interests that promote social order, and it places too much emphasis on broad patterns drawn from large social structures and classes of society, rather than capturing the nuanced relationships. As I discussed above, these limitations risk not fully examining the relationships between actors and not appreciating the overarching social values that drive certain behaviors. My study’s cases involve the potentially shared interests of serving the public good and also involve highly complex matters. Both of these contexts might be oversimplified through a framework driven by larger social factors, rather than individual and small-group interactions. Given these limitations, I examine the third and final framework used in the literature addressing government challenges of the academic profession’s federally sponsored research.

Evolving Interpretations and Interests Frame

A third line of research suggests that uncovering the circumstances and events surrounding the government challenges is best accomplished by tracing the evolving interpretations of key actors’ interactions with others in the controversy. This type of examination would reveal the different actors’ interests and prompt a better understanding of how the actions led to the pressures, tactics, and other means employed to challenge the academic profession’s government-sponsored research (Beauregard, 1988; Garrison & Kobor, 2002; Gutfeld, 1970; Lilienfeld, 2002). These studies examine symbols or messages conveyed through one-on-one and group interactions to help explain subsequent interactions. The observable interactions ultimately capture the various means by which lay actors challenge the academic profession’s research. While the literature using this framework resembles principles of the symbolic interactionism,8 which is one form of sociological inquiry, the literature on

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8 Symbolic interactionism is a sociological perspective that views society through interactions that individuals and small groups have. Three premises ground the symbolic interactionist perspective (Blumer, 1969/1986). First, humans act according to the meanings associated with things such as symbols or messages, which may come in the form of texts, pictures, or live events. Second, meaning occurs through social interaction, such as conversational
government challenges does not identify a name of this framework, but I will refer to it as the Evolving Interpretations and Interests Frame.

**Application and Contributions of the Evolving Interpretations and Interests Frame**

To explore the application of the evolving interpretations and interests frame, I discuss two key pieces of literature that illustrate the primary features of this framework (Garrison & Kobor, 2002; Lilienfeld, 2002). These two studies, Garrison and Kobor (2002) and Lilienfeld (2002), uncover meanings that various individuals and groups use to examine a controversial study’s findings about child sexual abuse (hereafter known as the “Rind study”) (see Rind, Tromovitch, & Bauserman, 1998). The controversy revolved around a peer-reviewed article in *Psychological Bulletin*, an American Psychological Association journal. The authors of the Rind study concluded that, based on their meta-analysis of 59 studies with a combined sample size of over 15,000 college students, child sexual abuse did not always result in severe harm. This study directly challenged the common assumption that the effects of child sexual abuse included severe psychological harm. In addition, the authors of the Rind study also found that the impacts of child sexual abuse were significantly less harmful for male respondents who considered their sexual encounters as children to be “consensual” acts (Lilienfeld, 2002).

While Garrison and Kobor (2002) and Lilienfeld (2002) report that the Rind study contains scientifically valuable contributions, the authors of the controversial article used words and terms whose meanings lay readers misinterpreted. These differences in meaning served as exchanges between two parties about the subject matter of the symbols or messages. Third, through that social interaction, an interpretive process illuminates the meaning for the recipient of the symbols or messages. Simply put, this perspective captures how individuals and small groups interpret messages and interact based on those interpretations, as a way to conceptualize situations in society such as government challenges to scientists. While aspects of the evolving interpretations and interests frame appear to represent aspects of the symbolic interactionist perspective, I acknowledge that the evolving interpretations and interests frame does not carry out this sociological perspective in the same depth of inquiry as does symbolic interactionism. Instead, the guiding principles of symbolic interactionism seem to inform or coincidentally reflect many parts of the framing and analysis of studies that I discuss in the evolving interpretations and interests frame.
significant barriers to proper understanding and ultimately to the academic profession’s exercise of its intellectual freedom. Garrison and Kobor (2002) and Lilienfeld (2002) suggest that legislators, the media, and other individuals interpreted certain words and phrases contained in the Rind study differently from what the authors intended, and those different interpretations significantly explain the circumstances and events surrounding the controversy, particularly the government challenge to the academic research. For example, the phrase “adult-child sex” in the Rind study provoked arguments about the value of this research topic. The differences in meanings become clear when one compares how various actors – the Rind study’s authors, other academic-psychologists, members of Congress, representatives from the North American Man/Boy Love Association, and the media – used the phrase.

Garrison and Kobor (2002) and Lilienfeld (2002) illustrate how the framing of individual and group interpretations about the Rind study’s findings generated justifications for government challenges. Initially, the Rind study received little attention; however, approximately four months after its publication, a nationally syndicated radio host, Dr. Laura, criticized the article’s findings on-air numerous times over the course of several months. Dr. Laura’s lay interpretations of the study’s conclusions, that the authors had supposedly found adult-child sex to be acceptable, reached the masses. Then, socially deviant groups such as the North American Man/Boy Love Association (NAMBLA), which promote legalization of adult-child sexual encounters, used the Rind study for an unintended purpose: to legitimize their position on pedophilia. These events drew public criticisms about the study from multiple media outlets, including radio and television talk shows, blogs, and newspapers.

As word spread about this study’s conclusions and its uses to advocate illegal and immoral activities, several state legislatures and members of Congress publicly rebuked the
authors for releasing the study, called into question the authors’ scientific expertise, and expressed their disbelief that such a study could be published. Although their comments derived from lay interpretations of the article, the legislators, as public officials, feared that the study’s findings might advocate, or at least have the effect of further legitimizing with other individuals and groups as it had with NAMBLA, socially deviant behaviors such as child abuse. Recognizing the need for public action, government officials across the country criticized the study’s authors for their statements and the APA for publishing the article. Members of Congress even held a press conference condemning the study and insisting that the APA denounce the study’s legitimacy as scientific research. Collectively, the lay interpretations of the Rind study, along with other circumstances and events surrounding the study, raised questions about both the value of certain academic research and the ability of the academic community to self-regulate.

The academic community responded to these events. Through public statements and letters to state and federal legislators, the APA expressed its support of the study’s scientific approach, noted the study’s contribution to the literature (i.e., the study challenged the common assumption that the effects of child sexual abuse always result in severe psychological harm later in life), and initially defended the process of peer review, by which professionals in the field had determined that the article had scientific merit and was worthy of publication. In addition, through academic and public venues, the authors of the Rind study continued to publicly defend their article’s methodology, analysis, and conclusions. Despite these attempts, as public pressure to condemn the article grew, state and federal legislators proposed resolutions to reject the study’s conclusions.
According to Garrison and Kobor (2002) and Lilienfeld (2002), the legislators proposed these resolutions for two reasons. First, the resolutions were intended to publicly denounce any support of child abuse. The legislators wanted to send a strong message that they did not condone what they truly perceived as the study’s findings. Second, the resolutions were intended to indirectly scold the American Psychological Association for its decision to publish this article when, as some legislators expressed, its staff likely knew or should have known that the article’s findings would cause the public outcry that it did. In the minds of many legislators, the APA should not have printed an article, which, as interpreted, essentially condoned activities that would harm minors. Despite these legislators’ public intentions for supporting the resolutions, many in the academic community interpreted the effect of these resolutions, particularly the one passed by Congress, with different meanings. They believed that the resolutions symbolized more significant consequences to academic research. In the opinion of many academics, these resolutions would have a “chilling effect on research concerning controversial scientific topics” (Lilienfeld, 2002, p. 181). Yet, as Lilienfeld (2002) notes, at least one member of Congress did not realize that members of the academic community would view the Congressional Resolution in that manner. He, like many other legislators, was concerned only about the lay interpretations surrounding the Rind study and the APA’s perceived irresponsibility for publishing the article, which legislators understood as advocating or at least legitimizing child sexual abuse.

After several months of public pressure, which was heightened by commentaries from members of Congress and the media, the APA changed its position about the article and on its peer-review process. Sensing that the public believed the APA was endorsing the lay interpretation of the article (i.e., pedophilia is not harmful in all instances), the Chief Executive Officer of the APA, Dr. Raymond Fowler, issued a public letter to Republican Congressman
Tom Delay, who was the House Majority Whip and played a significant role in formulating the Congressional Resolution that admonished the Rind study authors and the APA. In his letter, Fowler indicated that the article “included opinions of the authors that are inconsistent with APA’s stated and deeply held positions” that sexual relations between children and adults are not harmful acts (cited in Lilienfeld, 2002, p. 181). Furthermore, Fowler intimated that the peer-review process, which was in place at the time, might have been flawed. Because the APA has always stood against sexual abuse of children, the Rind study’s conclusions should have initiated an evaluation “based on its potential for misinforming the public policy process (cited in Lilienfeld, 2002, p. 181). Fowler admitted that this consideration of the study’s impact on the public did not occur, but the APA “will do [so] in the future” (cited in Lilienfeld, 2002, p. 181).

The APA statement spurred on further debate from academics and nonacademic psychologists. These individuals were troubled by the APA’s recanting of support for the Rind study and the APA’s public remarks that suggested changes to the journal review process, which in their minds might mean changing the peer-review process. To manage the impressions of these academics, the APA attempted to clarify its position. Tracing evolving interpretations, Lilienfeld writes: “In subsequent policy statements, however, the APA made clear that its statement should not be construed as implying a policy change in the peer-review process for controversial articles. Instead, journal editors would have the responsibility of alerting the APA to articles that might be especially likely to incite controversy so that the APA could adopt a more proactive stance with the media, politicians, and others” (Lilienfeld, 2002, p. 181). Nevertheless, as Garrison and Kobor and Lilienfeld observe, these academics and nonacademic psychologists felt that the APA’s initial statements about the changes to the journal review process were not clear, and some individuals interpreted the initial statements to mean that the
peer-review process would be changed for controversial articles. The individuals who complained to the APA about its proposed change to the review process believed that the APA’s initial statements placed the academic peer-review process in a vulnerable state, especially since legislators, the media, and other nonscientists might not appreciate the role of peer review, and moreover, some of these outside critics thought the process did not work.

In sum, Garrison and Kobor (2002) and Lilienfeld’s (2002) framework illustrates how (1) individuals attach meanings to symbols and messages, (2) meaning making takes place through interactions with other actors, and (3) the evolving interpretations and interactions capture the circumstances and events surrounding the government challenges to academic scientists’ research and elucidate the pressures and tactics that the lay actors employed.

**Limitations of the Evolving Interpretations and Interests Frame**

There are two limitations to the evolving interpretations and interests frame when used to uncover circumstances and events surrounding government challenges to academic scientists’ research. In this section, I elaborate on both.

One limitation of the evolving interpretations and interests frame is that it tends to downplay the influences of larger social factors, such as social arrangements (e.g., educational attainment and social prestige), institutions (e.g., universities, business, media), and forces (e.g., political, economic, or technological). Minimizing the larger social factors potentially overlooks how these social factors contribute to our identification and understanding of the circumstances and events surrounding the government challenges. Several prior studies on government challenges to professors’ research illustrate the significance of connecting the various social factors with the government challenges as a way to identify and understand the circumstances and events surrounding the government challenge. For example, Lewis (1988) and Schrecker
investigate the role of social institutions, including universities, government, and the academic profession, to help uncover and examine the circumstances and events surrounding the government challenges. They explore how these institutions functioned to advance the asserted public interest in eliminating (or at least minimizing) Communist influences within American colleges and universities. Accordingly, they uncover pressures and tactics drawn from these social institutions’ functions, particularly those that sought to serve the social good. Beneke (1998), Gutfeld (1970), and Hamilton (1995), in their studies about government challenges to professors’ research, also explore the influences of larger social factors. They focus on how social arrangements divided groups in society in terms of financial resources and political power. In light of these resource and power differences, they trace the circumstances and events surrounding the government challenges to discuss how the groups with limited resources and political power, such as the academic profession, behaved in a way that served the dominant groups’ interests.

Another limitation of the evolving interpretations and interests frame is that it relies too heavily on subjective interpretations in uncovering the interactions that collectively define the situation (i.e., the circumstances and events surrounding government challenges to academic scientists’ research). As I noted earlier, an individual may interpret an encounter or message differently from another person who also participates in the encounter or is presented with the same messages. While one of the contributions of this framework is that it captures individual interpretations of messages and the interactions that follow, that individualized meaning-making process also illuminates a limitation of this framework. The individualized meaning-making process allows an individual to incorporate personal beliefs, attitudes, and perceptions to influence the meaning one makes about a situation. Further, the individual interpretation is
dependent on whose observations a researcher captures. In other words, the interpretations and interactions present highly subjective constructions of reality that depend largely on whose viewpoint is taken and what the individual interprets to be the situation, given his beliefs, attitudes, and perceptions.

For instance, Garrison and Kobor (2002) and Lilienfeld (2002) identify multiple realities about how others perceived the APA’s role in the Rind study publication. One interpretation that many critics of the Rind study, including politicians and media personalities, outwardly expressed was that the APA endorsed the article when it published it. Another interpretation that other critics of the Rind study held was that the APA neglected its responsibility to control unethical research projects that its members conduct. According to these critics, the authors should never have explored the issue of harm to children who experience child sexual abuse, because children cannot consent to sex and adult sex with children is illegal. Because of the unethical and illegal nature of the subject, these critics believed that as matter of professional oversight, the APA should never have published the study. A third interpretation, which reflected the understanding of many academic scientists, is that the APA followed the norms of the scientific community, including considering the peer-review process, and that process led to peers in the field judging the article as worthy of publication. As I explained earlier, each interpretation led to different interactions, including public objections to the study through proposed legislation and published criticisms from other academics who accused the Rind study researchers of ethical violations. That is, the individuals interacted based on their subjective interpretations – which in some cases may have been driven by personal beliefs, while in other cases, by professional standards. But, subjectivity helps highlight different understandings of highly complex scientific issues.
Individual interpretations of messages and symbols are inherently subjective. The literature on social cognition identifies several reasons for individual differences with message interpretation when individuals encounter (e.g., by oral, written, or other visual cues) the same words and symbols. Since the 1970s, social psychologists, who study social cognition, have suggested that individuals may present differing interpretations from others, even when these individuals encounter the same language or symbols. For example, social psychologists identified an observable behavior known as the perseverance effect, in which some people hold onto beliefs about themselves and their environment even when others discredit those beliefs (Ross, Lepper, & Hubbard, 1975). This concept explains how, to use one example, individual factors present the possibility that people who hear or experience the same message may articulate different interpretations of that message. Indeed, many other factors influence or shape the way people process language and symbols, including what they use to help anchor the meaning, which ranges from past events to seemingly similar events that they or another person experienced (Pronin, Puccio, & Ross, 2002). While these anchor points help an individual make meaning of the language or symbols, one person’s anchor may be different from another’s, and those differences lead to the possibility for a wide range of interpretations of the same language or symbols. Therefore, as a way to uncover the circumstances and events surrounding the government challenges to academic scientists’ research, this framework is vulnerable to subjective interpretations.

*Summary of the Contributions and Limitations of the Evolving Interpretations and Interests Frame*

To summarize, the evolving interpretations and interests frame has several strengths and limits to conceptualize the circumstances and events surrounding government challenges to
academic scientists’ research. In terms of this framework’s strengths, there are at least three. One contribution is that the evolving interpretations and interests frame recognizes and captures human capacity for thought and action. It does not overlook an individual actor’s purposeful interactions as a way to understand the circumstances and events surrounding government challenges. Another contribution of the evolving interpretations and interests frame is that it recognizes the significance of language and symbols. Because individuals interact in a purposeful manner, the language and symbols surrounding an incident also represent meaningful messages that help uncover the circumstances and events relating to the government challenge. A third contribution of the evolving interpretations and interests frame is that it captures the ongoing process of interactions. It operates off the premise that a situation develops over a series of interactions. By tracing these interactions, we can uncover the circumstances and events surrounding the government challenge to academic scientists’ research.

While the evolving interpretations and interests frame presents several conceptual strengths as a way to uncover the circumstances and events surrounding the government challenge, it also has two limitations. As a framework that offers a micro level of analysis, it relies much more on individual and small-group interpretations and interactions and less on larger social factors, such as social structures and institutions. By downplaying the effect of social factors, the evolving interpretations and interests frame largely overlooks key societal features, such as how groups behave based on social roles or as a consequence of societal conflict. Further, it relies too heavily on subjective interpretations of reality as a way to construct meaning about the role of the academic profession as well as to account for the circumstances and events in society, such as government challenges to academic scientists’ research.
Despite these limitations, when compared to the societal interest and competing interests frameworks, I propose in the next section that the evolving interpretations and interests frame still offers the best framework to capture circumstances and events surrounding government challenges to academic scientists’ federally sponsored research.

Proposing the Evolving Interpretations and Interests Frame

As I indicated earlier, the purpose of this chapter is to establish one part of my study’s conceptual framework. While my overall study explores what it means for government actors to challenge academic scientists’ federally sponsored research and academic scientists to exert control over that research, in this chapter, I set out to identify a framework to understand the circumstances and events surrounding government challenges to academic scientists’ research. The circumstances and events lead to uncovering the lay actors’ pressures, tactics, and other means employed, which are necessary for me to consider as I conceptualize what it means for academic scientists to experience government challenges.

While any one of these frameworks offers me a way to conceptualize government challenges, I searched for the most useful framework for me to investigate a central characteristic of my cases: how various individuals construct different understandings about academic scientific research that involves highly specialized or complex scientific terms and processes. As I noted earlier, my cases revolve around controversies in which various individuals present differing opinions about a federally sponsored academic research project that pertains to an environmental policy topic. Because these academic scientific research projects involve highly specialized or complex scientific terms and processes, it is not surprising that the cases illustrate conflicts arising from different understandings between and among nonscientists, academic scientists, government officials, the media, and interest groups. One way to capture these
interpretations and interactions, which would help me uncover the circumstances and events surrounding the government challenges, is by using the evolving interpretations and interests frame.

The evolving interpretations and interests frame explains the government challenge through a series of interpretations and interactions. Seen within this framework, individuals create meanings based on their interpretations of symbols and language, and based on the meanings each constructs, each individual interacts with others. For example, Garrison and Kobor (2002) and Lilienfeld (2002) recount how differing interpretations about the Rind study publication generated a series of interactions. In that case, regardless of the Rind study’s authors’ stated intentions, several actors (i.e., individuals from the media, Congress, and members of the North American Man/Boy Love Association) attached meanings to the article’s research approach, findings, and effects on society. In some instances, even though the article contained highly specialized or complex terms and processes, individuals not trained in the specialty commented on the study by describing research flaws in terms of design, methods, and analysis. Others who questioned the research approach and findings sought out individuals, whom they deemed as credible experts, to evaluate the research in order to address the highly specialized or complex terms and processes. These identified experts, some but not all of whom had relevant social and behavioral science training, evaluated the study – typically conveying messages to discredit the Rind study authors. A group of scientists and nonscientists also criticized the study for violating research ethics. In their minds, the specialized or complex terms and processes of the study were insignificant, because a study addressing illegal behaviors against minors violated research ethics. Collectively, these various interpretations and
interactions at the micro level constructed the circumstances and events surrounding the
government challenges to the academic scientists’ research.

By contrast, the unified interest and competing interests frameworks tend to focus on
patterns based on broader social factors, such as social structures and institutions, as well as
social arrangements. Viewed as offering a macro-level lens to the situation, both the unified
interest and competing interests frameworks regard arrangements in society, such as economic
and political mechanisms, as well as social institutions, such as universities and business, as
shaping the circumstances and events surrounding government challenges to professors’
research. As indicated earlier, the *unified interest frame* emphasizes the academic profession’s
function to further societal goals. Accordingly, past studies that used a unified interest frame
conceptualized the role of the academic profession within society in its pursuit of a clearly
established societal priority, such as ridding society of Communist affiliations. Using this
framework, the interpretations of circumstances and events are anchored to societal norms and
values as well as how the academic profession fits into its setting. Further, it ignores how the
individuals who engage in the controversy define the situation. Likewise, the *competing
interests frame* pays attention to broader social arrangements, such as class and educational
access. It emphasizes that society is already designed in an unequal environment to benefit the
dominant group and deprive the subordinate group; therefore, groups such as the academic

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9 Here, I cast the distinction between macro and micro levels of analysis as theoretical constructs that rely more
heavily (i.e., a theoretical focus) on certain social units, rather than conveying “micro-macro extremism” (Ritzer &
Goodman, 2004, p. 485). That is, the unified interest and competing interests frames, much like the sociological
frames of functionalism and conflict theorists, tend to *focus on the macro level* by examining social institutions and
structures, rather than placing weight on individual interactions to understand the circumstances and events of each
case. By contrast, the evolving interpretations and interests frame, which resembles several aspects of the symbolic
interactionist perspective, tends to *focus on the micro level* by examining interactions between and among
individuals along with the human agency considerations (i.e., individuals having the capacity to act independently to
understand circumstances and events of each case). This distinction is not intended to convey micro-macro
extremism. None of the frames presented in this study reflect a pure application of social institutions, social
structures, or individual/small-group interactions as the exclusive examination to determine the circumstances and
events of the government challenges.
profession and government act to serve the dominant group’s interests. Under the competing interests frame, the interpretations of circumstances and events revolve around the inequalities of society and how groups, including the academic profession, advance the goals of the dominant group.

In conclusion, rather than focusing on the social institutions and societal arrangements as a way to conceptualize what it means for academic scientists to experience government challenges, I propose using the evolving interpretations and interests frame, because it offers me an understanding of how individuals and small groups create and influence reality. This framework can help me uncover the circumstances and events associated with individual interpretations and interactions that are based on specialized or complex terms drawn from the academic research study in question. By doing so, this framework provides me a way to uncover how individuals and small groups interpret language and symbols, such as public statements about the controversial research from various individuals, including government officials, the media, academic scientists, professional organizations, lobbyists, industry leaders, and interest group representatives. Put simply, as a way to understand the pressures, tactics, and other means that government used to challenge academic scientists’ federally sponsored research, the evolving interpretations and interests frame offers me the best conceptual approach to uncover and examine the circumstances and events surrounding government challenges. Specifically, it details the interactions, especially those deriving from varying interpretations of academic scientific concepts, including processes such as peer review. Given its value as a way to think about the circumstances and events surrounding each challenge, I propose its application to this study.
Figure 2.1: Evolving Interpretations & Interests Frame

Source: Author’s depiction of the application of the Evolving Interpretations & Interests Frame

Chapter Summary & Conclusion

This chapter proposed a way to conceptualize what it means for academic scientists to experience government challenges to their federally sponsored research, which represents one part of my overall conceptual framework, the other part being how the scientists respond to exert control. I wanted a framework to help me conceptualize what it means for government to challenge professors’ research by uncovering the circumstances and events. Thus, I reviewed literature on government challenges to professors’ research, to search for the way that best uncovered the circumstances and events surrounding government challenges to academic scientists’ research.

Based on this review, I identified the three frames used in the extant literature (i.e., unified interest, competing interests, and evolving interpretations and interests) to uncover the circumstances and events surrounding government challenges. For example, the literature following the unified interest frame inquired into the functions of the academic profession to
determine how it integrated with other social institutions to meet societal interests. The literature following the competing interests frame inquired into the academic profession’s intentions and motives to explore how it tried to serve its own interests, as well as the interests of the dominant class, in an environment with a societal arrangement that maintained differential resources and power. The literature using the evolving interpretations and interactions frame inquired into the various actors’ interactions, and the meanings made by individuals involved in each of the encounters, to generate a more general understanding about what happened. Since each framework focused on different aspects of the government challenges to the academic profession’s federally funded research, each of them leads a researcher to uncover different aspects of the circumstances and events identified – which in turn modifies how one conceptualizes the government challenges to professors’ research.

While each perspective presents a legitimate frame to investigate the circumstances and events surrounding government challenges, I conclude that one of the three frames, the evolving interpretations and interests frame, offers the most useful way to uncover the circumstances and events surrounding government challenges to academic scientists’ research. The evolving interpretations and interests frame offers me a way to examine how individuals interact in society when confronted with differing understandings about highly specialized or complex scientific terms and processes, which are critical elements of my cases. Academic scientific concepts often evoke different meanings for people in the lay community, and these interpretations may not be generated from social institutions working together to further societal priorities or from conflicts purposefully intended to serve the interests of a certain group at the expense of another (see, e.g., Garrison & Kobor, 2002; Hunt, 1999; Lilienfeld, 2002). The evolving interpretations and interests frame does not assume social consensus or conflict. Instead, it presents a way to
examine a situation that is open to the possibility of either consensus or conflict – or perhaps something in between consensus and conflict. That is, the evolving interpretations and interests frame captures individual interpretations and interactions to uncover how individuals or small groups negotiate their way in society, based on their understanding of the situation – particularly one that involves highly specialized or complex scientific terms and processes. Using this frame and viewing these interpretations and interactions that present detailed information drawn from various individuals, I will construct the circumstances and events surrounding the situation, which in turn will provide me with a way to conceptualize what it means for academic scientists to experience government challenges to their research. I wish to emphasize that my selection of this framework does not mean I will ignore social institutions or structures that are at play. Instead, I recognize that the evolving interpretations and interests frame provides me guidance in examining my data, not a rigid set of rules with which I must comply. Given these considerations, I believe that the evolving interpretations and interests frame responds to one part of this study’s research question (i.e., who participates in the circumstances and events surrounding the government challenges to academic scientists’ research and how do the various individuals, including academic scientists, interact in the situation?) and offers the most inclusive examination to address this part of the study.

Finally, while the evolving interpretations and interests frame offers insights about government challenges, it is less helpful as a conceptual framework in understanding the academic profession’s responses that exert control over academic scientists’ federally sponsored research. Instead, as I discuss more fully in Chapter 3, the literature on the academic profession’s responses to government challenges suggests a more detailed consideration of multiple factors to fully appreciate the academic profession’s actions that reflect strategies,
tactics, and other means to exert control over its federally sponsored research. Thus, I propose the use of the evolving interpretations and interests frame to uncover the circumstances and events surrounding the government challenges, while in Chapter 3, I propose another part of my study’s overall conceptual framework to consider the academic profession’s responses, which strive to exert control over academic scientists’ federally sponsored research.
CHAPTER III: CONCEPTUALIZING “EXERTING CONTROL OVER RESEARCH”

Overview

This chapter presents a framework to help us understand the academic profession’s responses to government challenges over research that represent strategies, tactics, and other means for the academic profession to protect its intellectual freedom. More specifically, it offers a way to conceptualize “exerting control over research” when government challenges take place. To explain how I derived at my proposed framework, in this section, I preview the chapter.

The literature on the academic profession’s responses to government challenges generally suggests a framework that examines both internal characteristics and external factors of the academic profession. The internal characteristics identify institutionalized features of the academic profession that contribute to its professional autonomy. For instance, one internal characteristic of the academic profession is its expert knowledge. It contributes to societal recognition that a member of the academic profession has the expertise to carry out a particular research project in one’s disciplinary specialty without undue lay interferences. The external factors refer to conditions outside of the academic profession that influence the profession’s degree of autonomy over research. For instance, the courts, as an external entity, have recognized a constitutional form of intellectual freedom, which generally protects the academic profession from undue government interference over its research, even when the research is government-sponsored. This legal authority represents an external factor. Both the internal characteristics and external factors contribute to the multiple institutionalized arrangements that make it possible for the academic profession to maintain autonomy over its work. Those institutionalized sources of autonomy offer various considerations when one examines the academic profession’s responses to government challenges. That is, the internal characteristics
and external factors likely contribute to our understanding of the academic profession’s strategies, tactics, and the other means it employed when government challenges take place.

While prior research demonstrates that both the internal characteristics and external factors offer a way to understand the academic profession’s responses to government challenges, these studies do not fully explore the significance of the internal characteristics of the profession. Specifically, past studies have not used a framework that explicitly considers the internal characteristics addressing the academic profession’s (a) divisions of labor with project tasks related to research ("divisions of labor") and (b) academic professional training and development ("professional training"). As I discuss below, the omission of these characteristics misses other sources of institutionalized arrangements through which society has granted the academic profession autonomy. In other words, the omission of these two internal characteristics potentially overlooks additional strategies, tactics, and other means for the academic profession to protect its intellectual freedom.

Rather than ignoring these internal characteristics, I suggest considering them, because they may provide additional insights as I explore what it means for the academic profession to exert control over its federally sponsored research. To address these conceptual omissions, in this chapter, I review how past studies on the academic profession’s work autonomy have been structured. That literature suggests using frameworks from the sociology of the professions. Accordingly, I review the research on the sociology of the professions, which leads me to Eliot Freidson’s theory of professional dominance (Freidson, 2001).

Although Freidson’s theory has never been used to study the academic profession, it fills in the conceptual gaps found in the extant literature. As I explain later in this chapter, Freidson’s theory presents several compelling reasons to use it as a framework to guide this study’s
examination of the academic profession’s responses to government challenges over academic scientists’ federally sponsored research – though I make two slight modifications to the theory. With these minor modifications, which I explain in greater detail below, Freidson’s theory of professional dominance presents a more accurate and comprehensive set of internal characteristics of and external factors influencing the academic profession.

In sum, this chapter discusses several sources of relevant literature. First, it presents the prior frameworks used to study the academic profession’s responses to lay challenges. That literature draws out the conceptual omissions of past frameworks. Second, to locate a framework that considers those conceptual omissions, this chapter also explores the literature on the academic profession’s work autonomy and the sociology of the professions. The purpose of those reviews is to identify a framework that both aligns well with the past literature and fills in the conceptual gaps. That review leads me to propose using Freidson’s theory of professional dominance, though with slight modifications, as a conceptual framework to guide my understanding of the academic profession’s responses to government challenges. Then, at the conclusion of the chapter, I briefly discuss how (a) the evolving interpretations and interests frame from Chapter 2, which I use to understand the government challenges, and (b) a modified version of Freidson’s theory of professional dominance, which I use to understand the academic profession’s responses, work together as the overall conceptual framework of this study.

Prior Frameworks

As I indicated earlier, the literature on the academic profession’s responses to government challenges typically uses a framework that examines both internal characteristics of and external factors influencing the academic profession. This section reviews the concepts considered in past frameworks – first by examining the internal characteristics, then by
examining the external factors. Following that review, I point out conceptual gaps from the prior frameworks, which may lead to overlooking potential strategies, tactics, and other means for the academic profession to protect its intellectual freedom. Now, I begin by discussing the internal characteristics identified in the literature on the academic profession’s responses to government challenges.

Internal Characteristics

The extant literature on the academic profession’s responses applies some mix of four possible internal characteristics to frame studies on the academic profession’s responses to government challenges to their work. They are expert knowledge, qualifications for practice, professional associations, and professional ethos (see, e.g., Bloom, 1990; Gutfeld, 1970; Saltmarsh, 1991). Discussing two key studies, I illustrate, below, how the consideration of these internal characteristics helps one understand how the academic profession’s responses identify strategies, tactics, and the other means it employed when government challenges take place.

*Expert Knowledge, Qualifications for Practice, and Professional Associations*

In Chapter 2, I examined a study by Gutfeld (1970). Here is a brief summary of that case: In the early 20th century, the government tried to silence Professor Louis Levine. Levine, an economics professor at the state university in Montana, published a report criticizing the state’s tax structure and pointing out the state’s generous tax assessment on the mining industry. In reaction to the report, business leaders and government officials quickly attacked Professor Levine’s methodology and findings, prevented the publication of his subsequent research, and pressured his employer to suspend him.

Structuring his study on the academic profession’s responses to government challenges, Gutfeld (1970) considers three internal characteristics of the academic profession: (1) its expert
knowledge, (2) its qualifications for practice, and (3) its professional associations. Using this framework, he reveals how considering these three internal characteristics helps uncover the means by which the academic profession exerted control over its research in this case. Specifically, that framework helps reveal how the profession launched three significant responses around different forms of professional self-regulation.

In one form of self-regulation, the academic profession reinforced the notion that expert knowledge required qualified individuals to assess the validity of the work. Accordingly, the academic profession sought two Columbia University academics with expertise in economics and tax to evaluate Levine’s report. The economists concluded that Levine’s report was professionally sound for publication. They also noted that their university’s press would even consider a manuscript of that quality. In a second form of self-regulation, the academic profession demonstrated its social role as a major participant to determine who could practice, or remain in practice, within the profession. Members of the academic profession served on the internal review committee, and they judged whether the employing university proceeded fairly when it suspended Levine. The university committee, which was composed of well-known faculty from the institution, concluded that the suspension was inappropriate, and referred to the suspension as a “farce,” an “arbitrary … intolerable” act. The committee believed that the events did not provide a valid reason for suspension, and its members pressured the university to reinstate Levine. The third form of self-regulation resembled the second one. This time, however, rather than drawing from faculty inside the university, the reviewing committee consisted of faculty from around the United States. Specifically, the American Association of University Professors (AAUP), a nationally recognized professional organization, evaluated the
situation and supported reinstatement of Levine. These three forms of self-regulation eventually led to Levine’s reinstatement and back pay to compensate him for the period of suspension.

Levine’s case demonstrates that expert knowledge, the academic profession’s practice of determining who is qualified to be a professor, and the collective action of its professional association amounted to self-regulation. In addition, the Levine case shows how multiple actors potentially supported self-regulation in different ways. Specifically, we learn of three types of self-regulatory measures that the academic profession used: peers within the field to evaluate the scholarship’s content, peers within the university to evaluate the suspension process, and the professional organization as members of the Academy who are not employed at the institution to review the process.

*Expert Knowledge, Qualifications for Practice, and Professional Ethos*

Like Gutfeld’s study, Bloom’s research (1990) also explores how “academic institutions respond to public crises about the freedom of the intellectual” during times of social conflict (p. 19). Bloom, however, considers another internal characteristic, namely its professional ethos, which Gutfeld does not explicitly identify.

In Bloom’s study, the government questioned Professor Bernhard Stern’s fitness as a university researcher and teacher. The case unfolded during the 1940s and 1950s, a time of significant social conflict generated from public fears that Communist ideology would influence the political-economic structure of the United States. Between the late 1940s and early 1950s, the government interrogated Stern about his political associations and activities. The government conducted these inquiries intending to connect Stern to the Communist Party, which, at the time, would have constituted unfitness as a university researcher and teacher. In 1953, at his final hearing, Stern testified that he had no interaction with the Communist Party in 1947 or
beyond. He remained silent about his associations and activities prior to 1947. When members of the congressional committee probed further, Stern asserted his Fifth Amendment privilege against self-incrimination.

To investigate the academic profession’s responses in this case, Bloom (1990) focuses on the following internal characteristics of the academic profession: expert knowledge, authority to judge who qualifies for practice, and the profession’s ethos. He examines how these internal characteristics contributed to the academic actors’ efforts to exert control over Stern’s qualifications to research and teach at the University. For example, Bloom reveals how Stern’s responses to the government interrogations reflected a professional ethos that valued protecting the profession’s interests of promoting knowledge over succumbing to political forces.

Illustrating this point, during the 1953 congressional hearing, Stern responded truthfully about his nonparticipation in the Communist Party between 1947 and 1953, and he asserted a Fifth Amendment defense of self-incrimination for the period prior to 1947. Bloom notes that Stern upheld his commitment to trustworthiness, which is an understood value within the academic profession. That is, society relies on the honest disclosures of university researchers and teachers, so Stern elected to uphold his professional integrity and spoke honestly at the hearing. Stern also demonstrably valued the academic norms fostering a professional culture of freethinking. He believed that disclosing names of other academics who had Communist affiliations would only fuel the government challenges against the academic profession and stifle the profession’s inquiries into controversial topics for fear of reprisal. Thus, he chose not to disclose any names.

Stern’s unresponsiveness, however, presented a problem for him at the workplace. Prior to Stern’s 1953 hearing, his employer, Columbia University, adopted a policy to investigate the
professional fitness of faculty who refused to testify at government hearings about their political associations. The university administration implemented the policy primarily to handle matters that would reflect poorly on the institution’s reputation. In accordance with that policy, Columbia initiated an internal investigation of Stern – an inquiry that stemmed from the government challenge.

Bloom also examines the academic profession’s responses to Stern’s employer’s investigation, along with the events following that investigation. He considers several internal characteristics of the academic profession, namely expert knowledge, authority to judge who qualifies for practice, and professional ethos. Using those internal characteristics as part of the study’s framework, Bloom uncovers how members of the academic profession responded much as Stern himself had. Specifically, a group of senior faculty at Columbia defended Stern’s intellectual freedom, just as Stern had done for others at Columbia and the broader academic profession when he did not reveal names of faculty who may have had Community Party affiliations. This professional collegiality took two forms.

First, rather than placing Stern under a traditional, internal investigation committee, the leaders at Columbia conducted their investigation under the auspices of a special review board known as the Committee on Conference. The university established the Committee on Conference as an organizational “device to insulate faculty members from attack by groups and individuals outside of the University community” (Bloom, 1990, pp. 26-27). Presumably, the committee functioned to review matters that involved a unique understanding about the academic profession, which lay observers would not fully appreciate. In this case, the administrators balanced the interests of the public need to investigate the reasons behind Stern’s refusal to testify and the academic profession’s authority to decide who it believes is qualified to research
and teach. Factoring those two interests, they determined that the Committee on Conference, rather than a public forum, was the appropriate venue to hear this case.

Following the investigation, the Committee on Conference issued its report. The committee found that while Stern had prior affiliations with the Communist Party, he was no longer a member of the Communist Party by the time of the government investigation. Further, he had not surrendered his rights to intellectual freedom because of influence from group affiliations, did not violate any professional integrity when he failed to fully respond at any of the congressional hearings, and was, at the time, an independent, rational thinker. Given their findings, they recommended that the university not discipline Stern, but instead, renew his contract.

Several university trustees expressed concerns about the recommendation. These discussions led to the second response, illustrating again the academic profession’s reciprocated collegiality. Rather than leaving Stern to fend for himself, the university leaders, along with senior faculty in Stern’s department, met with the trustees who were uncomfortable with simply renewing Stern’s faculty appointment. At that meeting, the university representatives assured the trustees that Stern no longer had Communist affiliations, but they admitted that Stern had actively participated in the Communist Party from 1934-1943 and occasionally interacted with the Communist Party between 1944 and 1946. The university representatives also noted that Stern had not distorted information or used his professional position to indoctrinate the scholarly community or his students to advance the Communist Party’s agenda. Further, the representatives emphasized that Stern had been a reputable scholar and teacher, who formulated professional judgments without external influences. The trustees accepted the representatives’

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10 Stern asserted his Fifth Amendment rights, so this information had not been revealed during the government hearing.
arguments, and, as a compromise, the university representatives modified the investigation report to justify the committee’s recommendation that the university renew Stern’s contract.

In sum, Bloom’s consideration of the profession’s internal characteristics (i.e., expert knowledge, authority to judge who qualifies for practice, and the profession’s ethos) reveal several key findings in the Stern case. First, Bloom’s study identifies how the academic profession responded in a way that reinforced academic values through efforts that protected knowledge creation and promotion rather than a political agenda. Second, the study demonstrates how members of the academic profession responded in a manner that reciprocated Stern’s professional collegiality, such as by placing him under a special review committee, defending him to the trustees, and modifying the report – which serves as a permanent record or artifact about Stern’s employment qualifications. These findings reflect strategies, tactics, and other means that the academic profession employed when a challenge took place.

Summary of Internal Characteristics

These two studies illustrate several internal characteristics of the academic profession, which are frequently considered in the literature on the academic profession’s responses to government challenges (see also Garrison & Kobor, 2002; Hamilton, 1995; Lilienfeld, 2002). Specifically, the literature on the academic profession’s responses typically identifies expert knowledge, qualifications for practice, professional associations, and professional ethos as internal characteristics to consider.

Now, in the next section, I present the external factors influencing the academic profession.
External Factors

The literature on the academic profession’s responses also considers social factors external to the academic profession (“external factors”) to understand how the academic profession strives to exert control over its research (see, e.g., Rabban, 1990; Saltmarsh, 1991). These external factors reflect conditions, which are external to the academic profession, that influence the profession’s degree of autonomy over its research. Typically, these studies observe how society places value on the academic profession’s research, and that value either establishes social support or warrants special government privileges. Examining two frequently cited external factors, this section explores how public literacy and legal authority are considerations that help us understand how the academic profession, attempting to exert control over its research, might respond and what strategies, tactics, and other means it might employ.

Public Literacy

The literature addressing government challenges to the academic profession’s federally sponsored research has presented public literacy as a potential factor to help defend the academic profession. One line of research describes this construct of public literacy in terms of how the academic profession educated the public about the technical aspects of the research that government actors challenged (see, e.g., Bailey, 2002; Garrison & Kobor, 2002; Keller, 1996; Saltmarsh, 1991). For instance, Saltmarsh (1991) examines public literacy as an external factor. His study addresses the government challenges and the academic profession’s responses surrounding Scott Nearing’s dismissals – first, while a professor at the University of Pennsylvania (Penn), then afterward, at the University of Toledo. Nearing’s research and teaching proposed a controversial economic theory, one that neither industry nor government appreciated. In fact, Saltmarsh traces several instances in which industry and government
pressured Nearing’s employing university to terminate him. As Saltmarsh shows, Nearing’s conflicts with industry, government, and his employing university led to “intellectual repression” and “academic asphyxiation.”

Nevertheless, by educating the public in meaningful ways (i.e., generating public literacy), Nearing gained support from various groups outside of the academic community. As Saltmarsh notes, Nearing advanced his research by creating messages appropriate to his audiences (considering their backgrounds, especially in terms of educational level). In addition, he tailored his comments to address social, political, and economic conditions of the time, in order to illustrate the value of his research. Specifically, to address concepts of “new economics,” he used street-level language and connected his points to current and more practical events, such as business exploitation of child labor, employer dominance in the workplace, social stratification, and political power. By educating the public in a meaningful manner, he managed to gain support from local labor unions, the Toledo chapter of the National Association for the Advancement of Colored People, and other organized groups to rally against the government intrusions on his research. While Nearing never regained employment at Penn or Toledo, he did garner some support from members of the public, who understood his economic theory. That support took the form of organizations engaging him in public talks and offering him public backing during his brief foray into politics.

Considering public literacy is particularly useful for my study, too. Mooney and Kirshenbaum (2009) posit that scientific literacy might decrease the likelihood of government challenges to scientists’ research. They contend that if the public had scientific literacy,

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11 Nearing is a well-known figure in the American intellectual freedom literature. In the early 1900s, when his terminations occurred, the academic profession sought to establish a professional right from undue lay interference on a professor’s teaching and research. Since his terminations occurred during this movement and represented the kinds of lay intrusions that intellectual freedom would have protected, supporters of the professional liberty cited events surrounding Nearing’s dismissals to justify their position.
scientific researchers could garner support from the public and overcome nonscientifically justified government interferences. Similarly, if I consider the role of public literacy in my study, it could help me understand whether academic scientists’ efforts to educate the public on their research are equally, more, or less important than the academic scientists’ efforts to address the purely scientific tasks. Stated another way, how should academic scientists balance their time between working on the scientific inquiry and providing lay translations about their work? That knowledge potentially clarifies for me which responses present more effective strategies, tactics, or other means for the academic profession to protect its intellectual freedom.

Another line of research discusses how professors may draw on the public’s understanding of scientific research findings and policy choices when confronted with government actors’ challenging the academic profession’s research (Guston & Sarewitz, 2006; Jasanoff, 2005; cf. Nowotny, Scott, & Gibbons, 2003). These studies argue that public participation generated knowledge regarding the cultural context of the challenged study, especially in terms of how members of the public felt the challenged study affected them. For example, when government officials challenged academic scientists’ research sponsored by the National Institute of Environmental Health Sciences regarding the adverse health effects from an area’s pollution levels, the university-based research team worked with the local community to help the academic scientists better understand their research findings in terms of how severe the health effects were from concentrated air pollutants (Boffey, 1976; Frank, 1983). Thus, as the literature reports, the connection between science and the community offers academic scientists an opportunity to learn about the cultural context of their findings, which helps generate additional arguments of the study’s value. Indeed, these arguments contribute to the defenses of the challenged study.
Legal Authority

Numerous works examine another external factor, the academic profession’s reliance on legal authority, in attempting to understand how the academic profession might exert control over research when government challenges occur (see, e.g., Eisenberg, 1988; Hamilton, 1995; Metzger, 1987; O’Neil, 1997; Rabban 1990; van Alstyne, 1972, 1990). In some professions, legal authority consists of state or federal legislation protecting a profession’s practice through law, such as by state licensure (e.g., social worker, medical doctor). In the academic profession, the literature frequently suggests that legal authority is manifested in professors’ legal right of intellectual freedom, which is derived from the First Amendment. While not a right limited to federally sponsored research, intellectual freedom grants the academic profession autonomy to exercise its professional discretion in research without undue government interference.

For example, in Rabban’s (1990) extensively cited law review article on the constitutional recognition of intellectual freedom, he considers how the academic profession uses the law as an external source of authority to exert control over its research. Specifically, even when members of the academic profession conduct federally sponsored research within their disciplinary specialties and government unduly interferes, Rabban (1990) argues that members of the academic profession, while “serving the public’s First Amendment interests in fostering critical inquiry and knowledge vital to democracy and civilization,” maintain a special legal privilege (p. 254). That special legal privilege, called intellectual freedom and derived from the First Amendment, is what members of the academic profession can assert against the government actors.12

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12 Rabban actually examines the general principle of academic freedom as a professional liberty over teaching, research, service, and organizational governance. Here, I focus on what I call intellectual freedom, a subset of academic freedom, which applies to academic freedom only from undue governmental interference over the academic profession’s research.
Rationalizing his argument, Rabban presents a three-part legal analysis. First, he considers the foundational cases of intellectual freedom that eventually led to its legal recognition on the basis of First Amendment law. Second, he demonstrates how the social interest in academic work justifies a legal protection – a form of societal privilege. Third and finally, he presents how intellectual freedom as a legal right is consistent with other First Amendment principles that protect certain actors who perform their duties with professional liberties. For example, he cites how certain professionals working within a specific institutional context and performing “distinctive job functions,” such as legislators during a legislative debate and judges during a court proceeding, maintain professional liberties recognized under the First Amendment. These three parts formulate his argument of a constitutional protection that the academic profession might invoke when government unduly interferes with a professor’s federally sponsored research.

This article and others like it support the notion that legal authority is an external factor, the study of which helps one understand how the profession might exert control when it is confronted with government officials’ challenging its federally sponsored research. That said, I did not use this line of research to determine whether the academic profession actually has a legal right of intellectual freedom in the three cases presented in this study. Instead, the literature here informs me that legal authority represents an external factor for me to consider in my study.

As I noted in Chapter 1, academic freedom refers to the academic profession’s professional liberty in teaching, research, service, and organizational governance. In the literature, the meaning and application of the term academic freedom differ widely when applied to the professional liberties faculty have in terms of teaching, service, and organizational governance. In the legal and professional circles, those differences also exist in practice. However, the concept of academic freedom over research contains fewer debates within legal and professional circles and in the literature. To separate this construct of “academic freedom over research” from other forms of academic freedom, I refer to research academic freedom as intellectual freedom.
**Summary of External Characteristics**

In this section, I presented consideration of two external factors influencing the academic profession to understand how academic scientists respond to government challenges in an attempt to exert control over their research. Public literacy reflects the extent to which the public or certain groups can appreciate and support the academic profession’s research. Legal authority represents a more formalized external factor to consider: the extent to which societal privilege exists to further the academic profession’s ability to exert control over its federally sponsored research.

**Conceptual Omissions**

So far, I have discussed how the extant literature took into account internal characteristics and external factors to reveal the academic profession’s strategies, tactics, and the other means it employed when the government challenges occurred. This literature identifies internal characteristics, including expert knowledge, qualifications for practice, professional associations, and professional ethos. In addition, the literature identifies social factors external to the academic profession, including public literacy and legal authority.

**Figure 3.1: Internal Characteristics & External Factors from the Literature on the Academic Profession’s Responses**

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<th><strong>INTERNAL CHARACTERISTICS</strong></th>
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<tr>
<td>• Expert Knowledge</td>
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<td>• Qualifications for Practice</td>
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<td>• Professional Ethos</td>
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<td>• Professional Associations</td>
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Source: Author’s figure based on a modified version of Freidson’s Theory of Professional Dominance (2001)
While these internal characteristics and external factors offer a sufficient framework for this study, the broader literature on the academic profession suggests that additional internal characteristics of the profession also reflect institutionalized arrangements that support the profession’s autonomy. For instance, Altbach (1980) notes that the academic profession maintains “considerable autonomy in controlling working conditions and time, and can collectively make key decisions concerning who is permitted to enter the profession, the curriculum, degree requirements and the like” (pp. 10-11). Similarly, Schuster and Finkelstein (2006) report that even though the academic profession may perceive its loss of professional control (as evidenced from national data indicating declines over the past 40 years), the profession still maintains quite a bit of autonomy over who enters the profession and what the future professionals’ graduate education will consist of.

Researchers on the academic profession point out internal characteristics, such as dictating working conditions and deciding who can study and learn the knowledge of the practice, as institutionalized arrangements that permit the academic profession to act with professional discretion. Nevertheless, the literature on the academic profession’s responses to government challenges does not appear to consider these internal characteristics, which fall into two concepts – divisions of labor and professional training. Therefore, in the next section, I review related literature to search for a more comprehensive framework.

**Searching for a More Comprehensive Framework**

**Academic Profession’s Work Autonomy**

As I indicated above, the literature on the academic profession alludes to other possible internal characteristics that represent institutionalized arrangements granting the profession autonomy. In this section, I review the literature on the academic profession’s work autonomy to
augment my knowledge about the internal characteristics identified from the academic profession’s responses literature.

During my review of the literature, I noticed that some of this literature uses theories within the sociology of the professions to examine the relationship between the academic profession and its control over its work (see, e.g., Hutcheson, 2000; Parsons & Platt, 1968; Rhoades, 1998; Slaughter & Leslie, 1997; Slaughter & Rhoades, 2004). This literature largely captures how the academic profession has some social capacity to negotiate or exchange its services. For instance, Rhoades (1998) notes that work on a project is typically divided up, and the academic profession’s specialized knowledge makes it possible for the profession to negotiate jurisdictional boundaries over its specific work tasks, especially when it comes to research. Rhoades uses Abbott’s (1988) theory of the professions to examine these jurisdictional boundaries of work. According to Abbott (1988), disputes about jurisdictional boundaries occur between two professions over the type of tasks that each profession performs. Thus, the theory might explain the jurisdictional boundaries over work tasks between tax accountants and lawyers in terms of giving tax planning advice (see generally, Dezalay, 1995) or between doctors and nurses regarding the jurisdictional boundaries of medical treatment (see, e.g., Allen, 1997). Rhoades has adapted the theory to the jurisdictional boundaries of authority between professors and administrators regarding which contract terms of work are negotiable under collective bargaining agreements. Examining Abbott’s theory from that context (i.e., professors and administrators negotiating terms of work), Rhoades concludes that the conditions for the academic profession have changed and that the terms of its work are heavily managed.

Rhoades’s study also leads me to consider that the academic profession could negotiate its jurisdictional boundaries with other occupations as a way to leverage social power.
Consistent with that observation, Lee, Cheslock, Maldonado-Maldonado, and Rhoades (2005) indicate that today’s global economy values specialized knowledge, and the academic profession has carved out its own area (i.e., exclusive jurisdiction), which yields better returns in the market. Similarly, Slaughter and Leslie (1997) highlight how the academic profession maintains some social capacity to exchange its services. In their study, they observe that faculty and other academic staff participate in academic capitalist behavior (i.e., use their bodies of knowledge in a market-like manner) because that enables them to gather additional resources they can use to “protect their autonomy, prestige, and expertise” (p. 179).

While these studies show that the academic profession has the social capacity to negotiate or exchange its services as a means to gain resources, they fall short in that they do not provide a comprehensive framework of the internal characteristics and external factors to help conceptualize “exerting control” over the profession’s federally sponsored research. For this reason, these works (i.e., Rhoades, 1998; Slaughter & Leslie, 1997) and others that examine the relationship between the academic profession and its control over its work (see, e.g., Hutcheson, 2000; Parsons & Platt, 1968; Slaughter & Rhoades, 2004) led me to explore the literature on the sociology of the professions. The purpose of that review was to locate a framework that offers a comprehensive list of internal characteristics and external factors to help me understand more fully how the academic profession strives to exert control over its federally sponsored research.

Sociology of the Professions

One line of research in the sociology of the professions, known as the traits school, identifies a series of attributes or traits of the occupation that help distinguish a profession from other occupations. For example, this line of research typically lists knowledge, expertise, competence within a field, service to society, preparation through formal training with input
from members of the profession, and societal recognition of the occupation (see, e.g., Abbott, 1988; Barber, 1963; Goode, 1957; Greenwood, 1966; Hall, 1968; Johnson, 1972; Larson, 1977; MacDonald, 1995); taken together these traits help define and qualify the occupation as a profession. While these traits seem to describe the academic profession, the literature does not explain how considering these traits would help one understand how they were linked to the academic profession’s capacity to exert control over its federally sponsored research.

Another line of research, the processual school, presents the process as one in which the profession negotiates or exchanges its services with other entities outside of the profession or achieves a series of professionalizing events to elevate its status in society (Caplow, 1954; Daniels, 1967; Stricker, 1988a, 1988b; Wilensky, 1964). I find the processual school informative. It traces the process of several professions as they develop social legitimacy, and it demonstrates how the professions, including the academic profession, could use their services to gain additional resources (Slaughter & Leslie, 1997; Slaughter & Rhoades, 2004; Stricker, 1988a, 1988b). The drawback, however, is that this line of research does not specifically identify specific internal characteristics of the academic profession to consider. Instead, it relies more heavily on the external factors, or social forces, that often shape and influence the profession’s capacity to exert control over its federally sponsored research. While I plan to include the social forces as a consideration, I do not wish to ignore the academic profession’s internal characteristics, because they likely contribute to my understanding of the academic profession’s responses.

In a third line of research, the literature introduces a common theme among the professions: power itself. The power school, as it is commonly called, typically presents elements of the profession that grant it societal privileges, such as dominance over other
occupations, autonomy to exercise professional discretion, and economic and social rewards. In addition, the literature under the power school typically explains the relationship between (a) the internal characteristics of and external factors influencing the profession and (b) its capacity to gain and maintain power in society.

In particular, Freidson’s theory of professional dominance (2001) describes the social organization of a profession that gives it the political and economic support it needs to exert control over its work. That is, it tells me what internal characteristics and external factors to consider, so I know what elements make it possible for a profession to have power (i.e. dominance over other occupations, autonomy to exercise professional discretion, and economic and social rewards). Given the insights from the power school, notably Freidson’s theory of professional dominance, I propose adopting it as my study’s conceptual framework – with two minor modifications, which I will discuss below.

Freidson’s Theory of Professional Dominance

Though it has not yet been used to study the academic profession, Freidson’s theory of professional dominance presents a viable framework to conceptualize how the academic profession’s responses to government challenges exert control over academic scientists’ federally sponsored research. To explain its value, in this subsection, I describe Freidson’s theory, articulate its significance to my study, and indicate slight modifications that I make so it properly accounts for certain aspects of my study.

*Overview of Freidson’s Theory*

As with the literature on the academic profession’s responses, Freidson’s theory considers both the *internal characteristics* of and *external factors* influencing the profession (Freidson, 2001). In Freidson’s theory, the internal characteristics, which he refers to as a set of
defining components that make it possible for the profession to control its work, include five static, interrelated parts. These components are: (1) professional knowledge and skills, (2) divisions of labor, (3) labor markets and careers, (4) training programs, and (5) ideological commitment to the profession itself (Freidson, 2001). Professional knowledge and skills are the formal knowledge of the profession that rely heavily on mental judgments over technical details such as the academic profession’s application of specific research approaches to gather data for a study. The divisions of labor component considers the jurisdictional boundaries related to occupations working on different aspect of a project, such as the academic profession assessing the scientific implications of a policy while policymakers draft the legislation. The labor markets and careers component reflects the profession’s authority in society to determine who qualifies for practice and how one qualifies; one example is the academic profession’s default standard that the Ph.D. is a prerequisite for many college faculty positions. Training programs, usually graduate schools, are the formal educational settings that prepare future professionals and create new knowledge for future professional practice (e.g., a new research methodology). Ideology reflects the values embedded in the profession’s actions, such as the academic profession’s work to advance the interest of the public good through education.

In addition to the internal characteristics, Freidson (2001) also identifies several external conditions that moderate the extent and nature of a profession’s control over its work. He focuses on two factors, bodies of knowledge and the state. Freidson describes how bodies of knowledge serve as a potential source to generate more resources. He explains that bodies of knowledge have the capacity for society to recognize the value of a profession’s formal knowledge and grant resources based on that understanding. Similarly, the state serves as a potential source to provide legal authority or protections, so the profession can formally control
its work. He cites government-controlled licensing boards as an illustration of this external factor.

**Figure 3.2: Internal Characteristics & External Factors Based on Freidson’s Theory of Professional Dominance**

According to Freidson (2001), both the **internal characteristics** and **external factors** construct how a profession is organized in society to maintain its economic and political position in society, which allows it to exert control over its work.

**Contributions of Freidson’s Theory**

Since Freidson’s theory addresses what I set out to investigate (i.e., what it means for academic scientists to exert control over their federally sponsored research), I explore how this theory helps me understand the academic profession’s responses that represent strategies, tactics, and other means used to overcome government challenges.

Specifically, I discuss how Freidson’s theory offers three significant contributions as a framework to examine the academic profession’s responses.

1. **Consistent with the Extant Literature’s Internal Characteristics**

   Freidson’s theory is generally consistent with the extant literature’s identified internal characteristics that researchers have used to understand how the academic profession exerts control over its federally sponsored research when government challenges take place. Earlier, I discussed that the extant literature identified expert knowledge, qualifications for practice, and
professional ethos as conceptual considerations. Freidson suggests three very similar internal characteristics, but he calls them professional knowledge and skills, labor markets and careers, and ideology, respectively (see Table 3.1).

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<thead>
<tr>
<th>Freidson’s theory</th>
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<tr>
<td>Professional Knowledge and Skills</td>
<td>Expert Knowledge</td>
</tr>
<tr>
<td>Labor Markets and Careers</td>
<td>Qualifications for Practice</td>
</tr>
<tr>
<td>Ideologies</td>
<td>Professional Ethos</td>
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2. Considers Other Internal Characteristics

I noted earlier in the chapter that the extant literature on the academic profession’s responses tends to overlook the consideration of two internal characteristics of the academic profession: divisions of labor and professional training. Yet, as I describe in greater detail below, these two characteristics are important factors that may help us understand how the academic profession exerts control over its research. Freidson, however, does include these two internal characteristics. I explain in some detail why these two internal characteristics are important to consider.

   a. Divisions of Labor

   The extant literature does not consider the profession’s “divisions of labor” as an internal characteristic that helps one understand how the academic profession might exert control over its federally sponsored research. Freidson, however, includes this characteristic. According to Freidson, the divisions of labor address the organization of professional work, that is, the “way work is coordinated and controlled when individuals are performing different but related tasks”
(Freidson, 2001, p. 36). This characteristic is important when we think about how the “human division of labor is by its nature socially organized through the exercise of power” (p. 59).

The divisions of labor represent an institutionalized arrangement by which occupational groups maintain jurisdictional boundaries of their work. That is, divisions of labor identify the tasks associated with a project that a particular occupation oversees and executes. For example, as I mentioned in Chapter 1, academic scientists and science policy advisors maintain different societal roles over the science policy process. Academic scientists discover and create new scientific knowledge as well as assess policy choices. Science policymakers, such as federal legislators, establish policies that advance the nation’s interest in science, particularly in terms of funding projects and proposing laws. In other words, when it comes to the science policy process, the divisions of labor establish the role differentiation among occupations that society has accepted – and even institutionalized (Smith, 1992).

Role differentiation is a factor that has been used to support government challenges. As I indicated in Chapter 1, industry scientists, politicians, and several science policy researchers have criticized academic scientists for blurring the lines between science assessor and science policy advocate (see, e.g., Lackey, 2007; Rosenbaum, 2008; Stine, 2009). These critics contend that academic scientists have, at times, overstepped their boundaries in order to drive (or significantly influence) the content or direction of science policies. Some critics believe that academic scientists have no place in the science policy process, even as science policy assessors (see, e.g., Pielke, 2007; Primack & von Hippel, 1974). In the cases they describe government officials challenged members of the academic profession for exceeding the jurisdictional boundaries that society accepts to divide the labor force.
Division of labor may also present an internal characteristic to consider when trying to understand ways through which the academic profession might exert control over its federally sponsored research. For example, going back to the science policy process, Pielke (2007) argues that scientists, including those in the academic profession, indeed have a critical role in this process. According to Pielke, academic scientists may properly advocate a science policy when science determines the researcher’s position. He reframes the criticisms to consider the academic scientists’ roles in the science policy process as honest brokers. Piekle suggests that if we consider how academic scientists serve society as honest brokers who use science to inform policy issues and decisions, then society might understand how academic scientists could justifiably advocate specific policy (i.e., the science dictates such a position). In other words, considering the divisions of labor among various occupations (e.g., academic professionals and policymakers involved with the science policy process) presents another potential internal factor that may help us understand how the academic profession might exert control over its federally sponsored research.

b. Professional Training

Another gap in the extant literature is that it does not consider the academic profession’s role in professional training and development (“professional training”). However, as Freidson (2001) notes, this internal characteristic, is important in order to consider how society grants the profession discretion to determine who studies, what they study, and how they study.

Unlike most professions, the academic profession controls the professional training environment for its future professionals. The profession is thus uniquely situated in society to determine who receives professional preparation, what students study, and how they study. As Slaughter and Leslie (1997) observe, “in some ways[,] [professors] are the paramount
professionals because they have monopolies on advanced degrees and train and credential all other professionals” (p. 5). That is, to a large extent, the academic profession has control over establishing the norms of acceptable academic inquiry, identifying appropriate methodologies, and determining how future members of the profession will acquire the requisite knowledge and skills (Schuster & Finkelstein, 2006). This control over professional training also carries over as an important consideration for understanding how the academic profession might also exert control over its federally sponsored research. For example, through professional training programs, the academic profession can advance a specific concept or teach a particular technique that becomes normative in the practice of science and influences students and practitioners by not legitimizing other concepts and methodologies.

For my study, the professional training and development is particularly relevant, because colleges and universities educate not only the academic scientists whose research government sometimes challenges, but they educate the industry scientists and other non-university-based scientists. For these professionals, too, colleges and universities largely determine who studies, what they study, and how they study the scientific controversies my research explores. Thus, I recommend considering professional training as an internal characteristic to understand how the academic profession might exert control over its federally sponsored research.

3. Consistent with the Extant Literature’s External Factors

For the most part, Freidson’s consideration of external conditions aligns with prior research on external factors. As noted earlier in this chapter, the literature on the academic profession’s responses to government challenges traces how public literacy and legal authority represent two external factors that help one understand the extent and nature of the profession’s
ability to exert control over its federally sponsored research. Freidson (2001) suggests two somewhat similar factors, which he calls “bodies of knowledge” and “the state.”

Freidson’s “bodies of knowledge” is essentially the same as what I described earlier in this chapter as public literacy, an external factor cited in the academic profession’s responses to challenges. According to Freidson, bodies of knowledge represent society’s capacity to recognize the value of a profession’s formal knowledge. Similarly, public literacy refers to the public’s comprehension of specialized knowledge that leads it to support the academic profession’s research. I illustrated this latter concept above, when I discussed Scott Nearing’s public talks to educate multiple groups about his economic theory. This response to government challenges led several organizations to openly support Nearing’s research and teaching through actions such as vocal protests.

As Freidson emphasizes and we recognize in the Scott Nearing example, bodies of knowledge refers to the level of status that the profession attains and the profession’s capacity to gain resources through its bodies of knowledge. This factor is also brought out in the works of Sheila Slaughter and colleagues (see, e.g., Silva & Slaughter, 1984; Slaughter, 1988; Slaughter & Leslie, 1997; Slaughter & Rhoades, 2004). Although most of these works do not fall within the literature on the academic profession’s responses to government challenges, her research often illustrates ways in which professors gain power in the market when they use their bodies of knowledge as a valued resource. For instance, Slaughter and Leslie (1997) observe that faculty and other academic staff participate in academic capitalist behavior (i.e., use their bodies of knowledge in a market-like manner) because that enables them to gain additional resources so they can “protect their autonomy, prestige, and expertise” (p. 179). In light of this discussion, Freidson’s identification of “bodies of knowledge” as an external factor fits well as a way to
understand what it means for the academic profession to exert control over its federally sponsored research.

Under Freidson’s theory (2001), the state (or governmental body) also serves as an external factor (i.e., an outside entity that influences the academic profession’s degree of autonomy over research). Freidson suggests that the degree to which the state takes action to advance a profession is highly dependent upon the government’s structure and receptivity to establishing policies over a profession, which includes granting or removing privileges to a profession, typically through some legal recognition. For instance, as O’Neil (1997) and Rabban (1990) observe, the legal construct of academic freedom presents a legal protection under the First Amendment to shield the academic profession from undue lay interferences over its research. The “state” under Freidson’s theory is similar to the external factor of “legal authority” within the literature on the academic profession’s responses to challenges. Both reflect formalized controls established through government authority over citizens in its jurisdiction as an external factor that potentially grants the academic profession some degree of professional autonomy over its research (see Figure 3.4).

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<th>Freidson’s theory</th>
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<tr>
<td>Bodies of Knowledge</td>
<td>Public Literacy</td>
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<tr>
<td>The State (i.e., government entity)</td>
<td>Legal Authority</td>
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Conceptual Modifications

While Freidson (2001) presents what I consider to be the best theoretical framework for this study, I modify his framework in two ways to address concepts specifically identified in my study.

The first modification pertains to how I consider external factors that impede the academic profession’s attempt to exert control. Freidson (2001) indicates that the external factors both support and impede the profession’s attempts to exert control over its work. Rather than relying solely on the external factors that Freidson identifies to consider how they impede the academic profession’s control over its federally sponsored research, I draw significantly from the evolving interpretations and interests frame, which I explained in Chapter 2. The evolving interpretations and interests frame allows me to consider the circumstances and events surrounding government challenges, giving me a way to reveal the pressures, tactics, and other means that lay actors employ to challenge the academic profession’s research. That framework is therefore valuable in exploring factors that impede on the profession’s control. However, to consider external conditions that support the academic profession, I still intend to explore the two factors (i.e., bodies of knowledge and the state) that Freidson identified. In sum, this minor modification is consistent with the literature on government challenges to the academic profession’s research as well as with the literature on the academic profession’s responses to government challenges.

My second modification pertains to the placement of professional associations. Freidson (2001) places professional associations as an external condition. He suggests that professional associations have a limited role, and that the role is typically to convince the state to grant additional legal authority. He also observes that professional associations do not necessarily
represent the larger corporate body of the profession. In that context, he describes occasions when professional associations advocate for policies that support the interests of only certain segments of the profession and end up compromising the profession’s overall goals and interests. Indeed, Freidson’s observations might be true in certain instances. For example, works by Schrecker (1980, 1986) and Lewis (1988) support his treatment of professional associations. They uncovered evidence that the academic profession’s associations, most particularly the AAUP, did not uniformly defend the profession during the McCarthy era, when government officials challenged professors who allegedly were members of the Communist Party (see also Hutcheson, 2000).

I contend, however, that professional associations still reflect a significant internal characteristic of the academic profession. The AAUP is the organizing body that represents the academic profession. It defined the profession’s concept of intellectual freedom (American Association of University Professors, 2001). Today, it investigates claims of intellectual freedom, and it defends unjustified violations of a professor’s intellectual freedom (Eisenberg, 1988; Finkelstein, 1984; Rabban, 1990). In addition, the AAUP engages in expressions of protest through actions, such as censuring and public denouncements, when it finds clear evidence of intellectual freedom violations. Given the significant roles that the AAUP has had in developing and protecting the academic profession’s intellectual freedom, it appears more appropriate, for this study, to place professional associations as an internal characteristic.

There is another reason for this study to reject Freidson’s placement of professional associations as an external condition. Freidson’s theory is a general theory of the professions. His observations of professional associations and his justifications for placing them as an external condition rely on the behaviors and stated purposes of professional associations across a
wide range of professions. Quite possibly, his placement of professional associations as an external factor might be more fitting for other professions. Yet, for the reasons that I described above, professional associations within the academic profession more appropriately reflect an internal characteristic.

**Illustrating the Framework for the Academic Profession’s Responses**

Integrating my explanations from this section of the chapter, I present a depiction of the internal characteristics (Figure 3.3) and external factors (Figure 3.4).

---

**Figure 3.3: Internal Characteristics of Professional Control**

- Knowledge & Skills
- Divisions of Labor
- Labor Markets & Careers
- Training Programs
- Ideologies
- Professional Associations

Source: Author’s figure based on Freidson’s Theory of Professional Dominance

**Figure 3.4: External Factors to Professional Control**

- State (Legal Authority)
- Bodies of Knowledge

Source: Author’s figure based on Freidson’s Theory of Professional Dominance
Chapter Summary and Conclusion

This chapter has proposed a way to frame the academic profession’s responses to government challenges. To develop my study’s conceptual framework, in this chapter, I reviewed the literature on the academic profession’s responses to identify the internal characteristics and the external factors that consider how the academic profession strives to exert control over its federally sponsored research. Then, after realizing that the literature on the academic profession’s responses did not fully consider the possible internal characteristics, I reviewed the literature on the academic profession. That review has led me to survey three schools of thought within the sociology of the professions – the traits, processual, and power schools.

From that review, I discovered Freidson’s theory of professional dominance. Freidson’s theory, which falls within the power school, offers several advantages. It is aligned with the literature of the academic profession’s responses in terms of the internal characteristics and external factors that it describes. It also adds two internal characteristics (i.e., divisions of labor and professional training), which the literature on the academic profession omits. Although I made two slight modifications to Freidson’s theory, as a whole, it offers the most comprehensive set of internal characteristics and external factors to help us understand what it means for the academic profession to exert control over its federally sponsored research.

Now, to present my overall conceptual framework – which includes exploring the circumstances and events surrounding government challenges and the internal characteristics and external factors associated with the academic profession’s responses, I illustrate, below, (a) the evolving interpretations and interests frame and (b) the modified version of Freidson’s theory of professional dominance. The evolving interpretations and interests frame helps me uncover the
circumstances and events surrounding government challenges to academic scientists’ federally sponsored research. With that information, I will have a better understanding of the pressures, tactics, and other means that challengers – which may include actors other than the government – use to contest academic scientists’ federally sponsored research. In addition, the modified version of Freidson’s theory of professional dominance considers the internal characteristics of and external factors influencing the academic profession to understand the profession’s strategies, tactics, and the other means it employs when the government challenges take place. Taking these two parts together, I present my study’s overall framework (see Figure 3.5). With this overall framework, I strive to understand what it means for academic scientists to exert control over their federally sponsored research.
Figure 3.5: Study’s Conceptual Framework

Source: Author’s figure of the integrated concepts drawn from the Evolving Interpretations & Interests Frame
Freidson’s Theory of Professional Dominance
CHAPTER IV: STUDY DESIGN AND METHODS

Overview

The aim of this study is to explore the meaning of a critical concept within the academic profession: *exerting control over research* as a manifestation of the profession’s intellectual freedom. Specifically, I explore what it means for academic scientists to exert control over their federally sponsored research when the government as a lay actor challenges that research. In this chapter, I outline my study design and methods, which I anticipate will lead me to better understand (a) the pressures, tactics, and other means employed in the government challenges and (b) the academic profession’s responses in terms of strategies, tactics, and the other means it employs when the government challenges take place. Elaborating on the qualitative design and methods, I present the following: (1) I state my research questions; (2) I present my research design and methodology; (3) I describe my data and the data-collection methods; (4) I summarize the data-analysis techniques and procedures; (5) I describe my study’s strategies for trustworthiness; (6) I review the steps that I took to ensure compliance with ethical standards; and (7) I disclose the limitations of this study.

Conceptual Framework and Research Questions

The overarching research question for this study asks: How do academic scientists, who are members of a unique but variegated profession, exert some degree of control over scientific decisions pertaining to their research when government officials challenge that research? To explore this question, I reviewed the literature on government challenges to professors’ research (Chapter 2) and the literature on the academic profession’s responses (Chapter 3) as sources to help guide how I think about this study.
As discussed in Chapter 2, the literature on government challenges to the academic profession’s research suggests that I might consider how individuals and small groups interact in society when confronted with different understandings about highly specialized, or complex, scientific terms and processes. By examining the interactions of key actors in the situation, I might better understand the circumstances and events surrounding government challenges to academic scientists’ federally sponsored research. After a detailed review of the frames used in the extant literature, I proposed using the evolving interpretations and interests frame, because it appears to be the most useful lens to address the complex nature of academic science, to uncover the different meanings associated with the science, and to trace the interactions among the various actors involved in each of these cases (see Figure 4.1).

**Figure 4.1: Evolving Interpretations & Interests Frame**

![Evolving Interpretations & Interests Frame](image)

Source: Author’s depiction

Then, in Chapter 3, I searched for a way to examine the academic profession’s responses to government challenges. This literature presents various frameworks that consider both internal characteristics of the academic profession and external factors that influence the profession’s degree of autonomy and authority over its work, especially in terms of its research. The frameworks are useful to an extent; nevertheless, as the literature on the academic profession suggests, the prior frameworks used to examine the academic profession’s responses to
government challenges may not fully consider the profession’s internal characteristics. Most notably, these earlier frameworks omit two internal characteristics: (a) divisions of labor, by which scientists maintain jurisdictional boundaries over their research and (b) academic professional training and development.

After reviewing several lines of research, I discovered Freidson’s theory of professional dominance (Freidson, 2001). This framework offers three conceptual benefits. First, it considers the internal characteristics and external factors identified in the extant literature on the academic profession’s responses. Second, it includes the two omitted internal characteristics mentioned in the literature on the academic profession. Third, it explains the relationship between the internal characteristics and external factors in a more explicit manner than was laid out in previous literature.

Although I did make slight modifications to Freidson’s theory so it addressed specific aspects that apply to the academic profession, at the end, this framework presented a more comprehensive view of the internal characteristics and external factors. Specifically, the framework considers six internal characteristics that reflect institutionalized arrangements that support the profession’s autonomy. They are: (1) professional knowledge and skills, (2) divisions of labor, (3) labor markets and careers, (4) training programs, (5) ideologies, and (6) professional associations. In addition, the framework considers actions derived from two social institutions: bodies of knowledge and government. These institutions serve as external factors that influence the degree to which the profession has autonomy and authority over its research.
Below, in Figures 4.2 and 4.3, I depict the internal characteristics and external factors.

**Figure 4.2: Internal Characteristics of Professional Control**

[Diagram showing the internal characteristics: Knowledge & Skills, Divisions of Labor, Labor Markets & Careers, Training Programs, Ideologies, Professional Associations.]

Source: Author’s figure based on Freidson’s Theory of Professional Dominance

**Figure 4.3: External Factors to Professional Control**

[Diagram showing the external factors: Legal Authority, Bodies of Knowledge.]

Source: Author’s figure based on Freidson’s Theory of Professional Dominance

The overall framework for this study examines both the government challenges and the academic profession’s responses. To recap, the evolving interpretations and interests framework guides my process of uncovering the circumstances and events surrounding government challenges to academic scientists’ federally sponsored research. With that information, I have a
better understanding of the pressures, tactics, and other means used to challenge the academic scientists’ federally sponsored research – which includes identifying the roles of nongovernmental actors, who, in my cases, participated actively in each controversy. In addition, the modified version of Freidson’s theory of professional dominance provides a structure to understand the academic profession’s strategies, tactics, and the other means it employed when the government challenges took place. Taking these two parts together, I present my study’s overall framework (see Figure 4.4). With this overall framework, I sought to understand what it means for academic scientists to exert control over their federally sponsored research.
Figure 4.4: Study’s Conceptual Framework

Source: Author’s figure of the integrated concepts drawn from the Evolving Interpretations & Interests Frame

Freidson’s Theory of Professional Dominance

Government

Evolving Interpretations & Interests Frame

Academic Profession

Other Actors in the Controversy

Knowledge & Skills

Divisions of Labor

Labor Markets & Careers

Training Programs

Ideologies

Professional Associations

Legal Authority

Bodies of Knowledge

Autonomy & Authority Over Academic Profession’s Research

Source: Author’s figure of the integrated concepts drawn from the Evolving Interpretations & Interests Frame

Freidson’s Theory of Professional Dominance
With the literature offering guidance on the areas of inquiry and the use of my conceptual framework as an initial structure to start my inquiry, I pose more specific questions in order to contribute to the overarching examination of academic scientists’ exerting some degree of control over their research. My research sub-questions seek to uncover data that reveal patterns of human behavior, examining the identities of the lay challengers (i.e., the pressures, tactics, and other means that these groups used), and the academic profession’s responses to these pressures and tactics. Specifically, these sub-questions pose the following lines of inquiry:

1. Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research,
   a. what kinds of government actors have participated or are now participating in activities that challenged academic scientists’ research on sewage sludge, global warming, and salvage logging?
   b. what kinds of nongovernment actors have participated or are now participating in activities that challenged academic scientists’ research on sewage sludge, global warming, and salvage logging?
   c. what methods (i.e., pressures, tactics, and other means) did government actors employ to challenge academic scientists’ research on sewage sludge, global warming, and salvage logging?
   d. what methods (i.e., pressures, tactics, and other means) did nongovernment actors employ to challenge academic scientists’ research on sewage sludge, global warming, and salvage logging?

2. Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research,
Collectively, these questions seek to elucidate the meaning of academic scientists’ exerting control over their research. In more practical terms, the findings corresponding to these questions clarify the parameters of the academic profession’s intellectual freedom.

**Case Selection Criteria**

This study follows the paradigm of a qualitative research design (see, e.g., Hatch, 2002; Marshall & Rossman, 2006; Maxwell, 2004). To theorize this concept of government officials’ challenging academic scientists’ federally sponsored research and academic scientists’ exerting control over that research, I draw significantly from two methodological approaches, comparative case study and grounded theory. Initially, I present this research project using a multiple case approach, also known as a comparative case study. Here, I describe case study as a methodology, including its value to represent concepts and phenomena. In addition, illustrating its value for this study, I discuss the case boundaries and sample-selection criteria of each case contained in this research project.

A case study research design requires the establishment of the case. A case study investigates a single unit, such as an incident, an event, a program, a person, or a particular setting. The case represents the boundaries surrounding the study’s matter. Earlier, in chapters 1 and 2, I mentioned in general terms that each case study for this research project illustrates
instances in which government officials challenged academic scientists’ federally sponsored research, along with the academic profession’s responses; however, the cases contain additional boundaries that home in on the research focus. For Stake (1995, 1997, 2005), a case is an integrated system with an outline or some set of boundaries. Similarly, Merriam (1998) describes a case “as a thing, a single entity, a unit around which there are boundaries” (p. 27). The particulars for each case address the “bounded context,” which eventually contributes to the uncovering of the phenomenon (Miles & Huberman, 1994; Merriam, 1998; Yin, 1994).

Furthermore, as a methodological approach, multiple case studies present “more compelling” and “more robust” data when the researcher clarifies the case boundaries and the appropriateness of the case selection (Yin, 1994, p. 45). Therefore, in order to uncover what it means for academic scientists to exert control over their research, I identify below additional parameters regarding the cases, including what type of government entity, professors, and research categories qualify each case that illustrates government challenges to academic scientists’ research and the academic profession’s responses to those instances.

**Federal Government Challenges**

To provide consistency among the characteristics of the lay challengers and avoid potentially complicated legal matters, this study focuses on the U.S. federal government as the primary challenger to academic scientists’ research. With a boundary that the case context may represent only federal actions, the cases will offer some degree of consistency and reduce potential conflicts between state and federal priorities and legal interpretations. For example, since intellectual freedom may be protected when government actors infringe on professors’ constitutional rights, the boundary to focus on challengers from the federal government generally means that only the U.S. Constitution or federal rights are at issue. If this boundary were not in
place and cases with state-government challengers were allowed, the controversy might entail both the U.S. Constitution and the respective state’s constitution (cf. State of New Jersey v. Schmid, 1980). Therefore, to minimize the legal complications to be attended to, this study limits the cases to federal government employees as the acting challengers to academic scientists’ federally sponsored research.

University-based Academic Scientists Who Work for American Universities in the United States

Although this study is about professors, that class of professionals encompasses a large, varied group (Clark, 1987a; Schuster & Finkelstein, 2006). Therefore, this study examines a sub-population of professors, academic scientists, because as a sub-population, they share some common characteristics, such as normative practices of scientific research (Anderson, Ronning, DeVries, & Martinson, 2010; Merton, 1973). Furthermore, because some academic scientists are not primarily appointed by the university, that status complicates the study and the profession’s relationship to the institution. For instance, many Carnegie-classified “very high research activity” universities maintain government-owned, contractor-operated (GOCO) labs, and the academic scientists who work in them typically have the status of affiliated professors to the university. In many instances, the federal government actually employs the academic scientists who work at GOCOs, so government challenges to their research would represent acceptable employer directives and would not involve intellectual freedom as afforded to professors. Therefore, to avoid inclusion of that special class of academic scientists, this study bounds the case further to professionals who are university-based academic scientists; that is, the academic scientists’ principal employer is the university – even if the funds for their salary allocations draw heavily from grant money.
The final boundary for these professionals is that they are in the United States and working for an American university. Since many academic scientists who are employed at U.S. universities also work abroad, this boundary limits the case to situations in which the academic scientists work in the United States. Furthermore, this study is limited to faculty employed by an American university, because it simplifies any jurisdictional matters that may exist with the federal government as research sponsor.

**Federally Sponsored Research that Addresses a Common Public Policy Issue**

This study also limits the case sample to selected research conditions. In particular, this study selects cases in which the government challenged academic scientists’ federally sponsored research addressing a public policy issue that is common among the three cases. University-based academic scientists receive funding from multiple sources, including business, nonprofit foundations, and government. By limiting the cases to research projects that received federal funding, the funding source remains constant. In addition, federal government sponsorship affords members of Congress and other federal employees greater opportunities to publicly question the research, because the federal government is paying for the research. Since federal employees have that wide latitude to debate the merits of research the government has funded, this study likely better illustrates how academic scientists exercise control over their federally sponsored research. Equally important, this study focuses on a common public policy issue as a thread that keeps the issues topically constant. Like the funding source, this condition intends to hone in on the case context. By narrowing the case representations to a common public policy issue, the study focuses on the overarching phenomenon, rather than other spurious effects from multiple research issues.
Cases Selected

I set out to identify three cases to examine. I needed at least three cases, so I could have enough comparison points. To locate cases meeting my selection criteria, I searched news articles from higher education (e.g., *The Chronicle of Higher Education, Inside Higher Ed, UWIRE*), general science news journals (e.g., *Science and Nature*), and environmental news via LexisNexis and E&E Publishing Service. Because these sources served as leads to identify cases, I conducted broad searches using key words that would capture news stories indicating government involvement in academic research.

Each time I located a relevant case, I gathered some preliminary data. My goal was to ensure that sufficient data existed. To conduct the initial data collection, I relied on the same sources that I used to locate the cases. Once I identified three cases, I stopped the process.

Through these rounds of case searches, I read more than 3,000 entries of news stories and editorials. Not surprisingly, many of the sources provided unrelated information. In addition, numerous articles led me to possible government challenges, but they did not fit my case selection criteria. For instance, approximately two dozen articles reported on fears that government officials might challenge academic researchers who were engaged in federally sponsored research, rather than demonstrating or referring to instances of actual challenges. Several articles referred to potential cases addressing government challenges of federally sponsored academic research, but these incidents did not involve environmental research. One identified a case addressing a state-funded project that government officials in Michigan challenged, but as I explained earlier, my case boundaries eliminated challenges primarily dealing with state actors and funding. Several leads directed me to disputes between government
officials and academic scientists in which strong evidence supported research misconduct. In short, the identification of the three cases required a significant database investigation.

**Summary of Case Selection Criteria**

In sum, the purpose of this study is to examine what it means for government officials to challenge academic scientists’ federally funded research and to describe how academic scientists react to exert some degree of control over that research. Absent clearly articulated case boundaries, the study might not have analyzed the intended unit of analysis or have contained sufficiently robust data to explain the interactions among the variables (Merriam, 1998; Stake, 1995, 1997; Yin, 1994). Thus, I defined additional boundaries of the case sample criteria. Specifically, the additional case boundaries required the U.S. federal government as the challenger of the academic scientists’ research. The professors involved were to be university-based, academic scientists in the United States. At the time of the challenge, they had to be working for American universities. These academic scientists must have received federal sponsorship for their challenged research, and their research topic had to address a common public policy issue. After a review of several news sources, I located three cases illustrating that conflict-defense interaction, and each case involved an environmental research problem (e.g., sewage sludge, climate change, and salvage logging).

**Data & Data-Collection Methods**

To gather data on these three cases, I employed documents as my data source. The qualitative research literature presents a wide range of what constitutes “documents” for research. A narrow conception refers exclusively to written texts (Hodder, 2000; Lincoln & Guba, 1985). For example, Lincoln and Guba (1985) categorize documents as written texts used for personal records, whereas officially issued materials such as a drivers’ license and building
permits are not documents, but instead, they are considered records. A broader recognition of what constitutes documents for qualitative research includes all written texts, such as public records, news articles, reports, research articles, books, transcripts of hearings, e-mails, blogs, letters, and diaries as well as material images such as maps, paintings, photographs, and videos (Hookway, 2008; Merriam, 1998; Prior, 2003). For purposes of this study, I adopted the broader definition of documents, which included all written texts and material images. However, I differentiated among the various types of documents and images, and noted several other distinguishing characteristics, such as the document’s degree of scientific review (e.g., peer-reviewed status), source origination (e.g., posted on organization’s Web site with a clear agenda), and solicitation status (e.g., a government-commissioned report on an issue or an industry-based study).

More specifically, I used publicly accessible documents as my primary data source. The value of publicly accessible documents is that the data materials are available to members of the public. In contrast, private documents, such as non-distributed diaries, e-mails not available under open records laws, documents blocked by confidentiality agreements, and photos not posted or referenced through any public source, are not accessible to the public. Generally speaking, publicly accessible documents are readily available through a variety of sources, including the Web (e.g., reports, videos), libraries (e.g., articles and books), and special archives (e.g., newsletters, newspaper articles). Such documents sometimes require nominal processing and copying fees (e.g., government agency records and materials), but despite the fees, these documents are still publicly accessible.
Rationale for the Data Source

To limit my data source to publicly accessible documents offered two significant contributions to support my study’s design and methodology.

Capturing Societal Positions

The most important contribution is that, as a data source, they meet the goals of capturing societal positions about the respective cases – which is consistent with both the modified version of Freidson’s theory and the evolving interpretations and interests frame. According to Freidson (1986, 2001) and other theorists of the professions (see, e.g., MacDonald, 1995; Starr, 1982), society plays an integral part in moderating a profession’s ability to exercise its expert judgment over work-related tasks. The literature on the sociology of the professions and on the academic profession conveys the underlying concept that societal legitimacy influences the conditions for the extent to which the academic profession may exercise its control over work (e.g., the autonomy and authority it has to make discretionary decisions about how to conduct its teaching and research tasks as manifested through intellectual freedom), or in terms of academic scientists in this study, their ability to exert control over research, which includes making scientific decisions based on expertise. The openness of the data to society aligns well with the evolving interpretations and interests frame. As Denzin (2001) notes, the study of interpretations based on interactions illustrates the “interrelationships between private lives and public responses to personal troubles” (p. 2). In other words, the public must gain some access to the case controversies, so it can determine whether the academic profession receives support through societal legitimation. Since publicly accessible documents sufficiently connect the case interactions among the various actors and institutions with the requirement that the public have
some knowledge to determine societal legitimacy, I used publicly accessible documents as my data source for this study.

Inherent Benefits of Using Documents

A second, significant contribution to this study’s design and methodology that accounts for the use of publicly accessible documents as my data source relates to the inherent benefits of documents (see, e.g., Handy & Ross, 2005). According to Merriam (1998), the use of documents for data sources offers three significant benefits, plus I note a fourth. First, documents are nonreactive. “[T]hey exist independent of the research agenda; [therefore], they are … unaffected by the research process” (Merriam, 1998, p. 126). That is, the focus of the research centers around the reactions and interactions engaged in the study, and not the subject’s reaction with the researcher (Lee, 2000). Second, documents are grounded within the context source. That point is particularly important in a research project that employs the evolving interpretations and interests frame within a case study. Both the evolving interpretations and interests frame and case study methodology emphasize the relation between the context and observation. Specifically, they emphasize that the properties of the observation include the context and the meanings from those experiencing the interaction within that context. Thus, documents, as a means to draw out the context and meanings, support both the methodology and a portion of the conceptual framework (specifically, the evolving interpretations and interests frame, which is used to draw out the circumstances and events surrounding the government challenges). Third, documents “cost little or nothing and are often easy to obtain” (Merriam, 1998, p. 127). Thus, the data sources are accessible, and the study is easy to replicate.

Finally, a fourth benefit relates to several of Merriam’s points mentioned in the last paragraph. Premised on a belief that “research impinges on powerful persons or the exercise of
coercion or domination,” documents are more open accounts and tend to capture an accurate and realistic view of a situation (Renzetti & Lee, 1993, p. 6). According to Hodder (2000), several studies report that what research participants state versus what they actually do are not always the same. Likewise, Lazarsfeld and Thielens (1958), Lewis (1988), and Schrecker (1980, 1986) highlight the distinction between faculty rhetoric and action. Based on data from the era of the anti-Communist movement, these authors note that professors expressed that they would support faculty members’ rights to due process, even those faculty who were members of the Communist party. However, the actual practices of the time, traced from historical archives, demonstrate that faculty members’ actions did not conform to their earlier reported responses, given in surveys and interviews. In other words, evidence exists about the dissonance between rhetoric and reality. Thus, documents as nonreactive measures have a significant benefit to the data, because the observations drawn from that textual analysis tend to represent the true reality over many other data sources, particularly self-reported measures such as semi-structured interviews.

In light of the above reasons, I used publicly accessible documents as my data source for this study.

Data-Collection Process

Before starting a more systematic data-collection process, I conducted several searches using news sources to gather preliminary case information. These initial searches served four purposes. First, the process of finding initial data gave me a better sense of the type of available documents, and it helped me construct a “memo” template of how to analyze my documents. Second, through the searches, I drafted a skeletal timeline of events that represented significant markers or decision points for each case. Third, I was able to identify a preliminary list of key actors and their roles. Fourth, I needed some guidance as to the beginning and end of the case.
The initial searches provided me sufficient data to identify the event that initiated the controversy, along with evidence that signaled an end of the case, at least in terms of the significant interactions regarding the case matter.

_Document Treatment_

Because my data sources were publicly accessible documents, I followed a multi-step process to determine the treatment and significance of each document. I identified the form (e.g., newspaper, blog, congressional testimony), purpose (e.g., news reporting, official memo), source and author (e.g., news reporter, industry representative, federal scientist), degree of content permanency (e.g., user able to delete and modify) and consistency (e.g., fixed with identifiable time marker, changes with some identifiable time marking trail, changes without any original date/time stamp or marker), and type of review that the document undergoes (e.g., journalistic standards, peer reviewed). In addition, I conducted a document authentication process as suggested by several researchers (see, e.g., Lincoln and Guba 1985; Love, 2003; Merriam, 1998). The process included posing questions about the documents, such as, “What is the history of the document?” “What guarantee is there that it is what it pretends to be?” and “Has it been tampered with or edited?” (see Merriam, 1998, p. 127).

Having established a protocol about how to determine the treatment of my documents, in the next sub-section, I describe my initial organizing principles governing my data-collection process.

_Organizing the Data and Constructing a Timeline_

With general background about each case, my systematic data-collection efforts began with uncovering the circumstances and events surrounding the government challenges. As indicated in my conceptual framework (see Figure 4.4 above), this information is important for
understanding the pressures, tactics, and other means used to challenge academic scientists’ federally sponsored research and sets the stage for exploring what it means for academic scientists to exert control over their research. Since the data collection was intended to uncover the interactions that follow from government challenges and also trace each controversy’s historical antecedents, I first asked: “What led up to this government challenge?” In each instance, the initiating case event took place when the federal government challenged the academic scientists’ research decision, a decision that required significant scientific expertise. I looked for interactions described in the documents that represented critical facts about the case development. I also looked for a way to recognize a conclusion of the case. I concluded the cases when I had observable data signaling sufficient evidence of changed events or when several sources indicated the case controversy had ended (cf. Wells, Hirshberg, Lipton, & Oakes, 1995).

For example, in the climate change case, the appropriate period to end the case came when multiple sources noted a change in position of several government actors, who participated in this initial controversy. These government actors at a certain point either dropped their challenges or refocused their attention on other scientific studies to challenge. In the salvage logging case, members in the specific subfield of the controversial study referred to the case as some event in the past simply for historical notation, because the controversy had died or subsided significantly. In the sewage sludge case, reports indicated that another event had trumped the investigation about government challenges to the academic scientists’ research, and those data points signaled that the case had halted or become overshadowed by another intervening event. In short, my initial construction of the data concentrated on defining key elements of the case based on a timeline, so I could examine the process through sequential order.
Databases and Searchable Resources

After identifying each case’s temporal parameters (i.e., when the case started and ended), I gathered additional information in a more orderly, systematic manner. I conducted searches on LexisNexis Academic through its news database. In my first round of searches, I imposed no date restrictions, to ensure a wide scope of possible data inclusion. For search terms, I entered the last names of each of the academic scientists involved in a case sample. For instance, for the climate change controversy, I entered Mann, Bradley, and Hughes, the three authors of the controversial climate change studies. After reviewing numerous documents, I realized that in each case, the first author was consistently referenced as the key person, and at times, the other academic scientists who participated in the controversial study were not mentioned. Thus, I modified the search and limited the word searches to the last name of the first author and a short phrase to capture the controversial study’s title and the subject matter (e.g., sewage sludge, climate change, and salvage logging). In addition, I searched for variants, but each time I kept the last name of the first author and the subject matter in the search terms.

I conducted these searches through multiple databases within the LexisNexis Academic news section, which contains both national and international sources. The number of sources in each category was fairly large, and the international sources, with the exception of those based in the United Kingdom, did not contribute to the data. At times, a U.K. article served as an informant that helped me locate U.S. equivalent data or highlighted a point in a U.S.-based document, but no U.K. articles presented any new data or leads that I could not retrieve from U.S.-based sources. Because the databases contained large numbers of international sources
with relatively low value, and the case study dimensions focused on U.S.-based academic scientists who work at American universities, I omitted non-U.S. sources.\textsuperscript{13}

In addition to the databases within LexisNexis Academic’s news section, I searched science-specific sources, general news sources that capture events within higher education, and broad-based sources that include government events and scientific news. For instance, within LexisNexis Academic, I conducted searches in the scientific materials section, which contains 139 U.S. sources, including \textit{American Journal of Law and Medicine}, \textit{Modern Healthcare}, and full-text medical databases that aggregate sources like \textit{Public Health Reports}. I also searched the LexisNexis Environment database. Although many of these sources repeat data located in the LexisNexis Academic Universe, this database includes non-news sources in case law, regulations, and government reports. Consequently, I believed this database would augment my findings from LexisNexis Academic.

I repeated the initial searches (i.e., last name of the first author and the subject matter) on several other general media databases, such as ProQuest (which includes, for example, \textit{The Chronicle of Higher Education}). In addition, I conducted searches through specialized news outlets within higher education, such as InsideHigherEd.com, and I repeated my efforts with

\textsuperscript{13} By restricting my searches to only articles published within the United States, I eliminated many irrelevant articles, and the number of sources became more manageable. For instance, the newspaper database drew from 1,343 sources internationally. With a focus on U.S. newspapers, the population of available sources reduced to 544 U.S. newspapers. With a focus on trade association journals, when I eliminated the non-U.S. journals, the numbers also declined significantly. This action reduced the population from 1,144 to 646 sources. However, not all the databases declined so significantly. For instance, when eliminating newsletters outside of the U.S., that database reduced its total number from 828 to 608.

I used the following LexisNexis Academic Universe databases in the news category:

\begin{itemize}
  \item[a.] Newspapers: total of 544 in the database population, including the \textit{Chicago Tribune}, \textit{Los Angeles Times}, \textit{The New York Times}, \textit{USA Today}, and \textit{The Wall Street Journal};
  \item[b.] Newsletters: total of 608 in the database population, including \textit{Environmental Policy Alert}, \textit{Inside EPA Weekly Report}, and \textit{Water Policy Report};
  \item[c.] Web-based publications: total of 56 in the database, including AgWeb.com, CNN.com, USNews.com;
  \item[d.] Industry Trade Press: total of 646 in the population, including \textit{Environment and Energy Daily}, \textit{Inside Green Business}, and \textit{Waste Treatment Technology News}.
\end{itemize}

These figures represented the sources available to generate my sample of documents for each case.
broad-based scientific magazines of general review and wide circulation: *Science*, a news magazine published by the American Association for the Advancement of Science, and *Nature*.

Besides the media outlets and academic science journals, I conducted searches through additional public forums, including the Internet, and additional specialized databases. My goal here was to ensure inclusion of Web sites and blogs from specialized news outlets, environmental interest groups, corporations, faculty groups, scientists, think tanks, research centers, and other organized units. When available, I placed date restrictions, which one can do in some search engines – for instance, by using the advanced features on Google. This step typically generated more useful documents in a reasonable time period than by combing through 1,000 links, which I had done during my initial searches. I also performed searches on the Government Printing Office Web site ([http://www.gpoaccess.gov/cerecord/index.html](http://www.gpoaccess.gov/cerecord/index.html)) and the THOMAS database through the Library of Congress ([http://thomas.loc.gov/](http://thomas.loc.gov/)) to locate congressional reports, bills, records, statements, and committee reports. I repeated these steps through Westlaw and the LexisNexis government databases, particularly the congressional sources, so I had additional data-gathering reliability checks.14 Furthermore, as references to documents arose, I noted those documents and located them.

*Theoretical Sampling*

Although knowledge of the case initiation and conclusion dates helped me devise the temporal account of the academic scientists’ experiences and responses to government challenges, the data collected in this fashion was not the only guide to determine sufficiency of information. Since the aim of my study is to explore concepts that offer theoretical generalizations about what it means for government officials to challenge academic scientists’ federally funded research and for academic scientists to exert control over that research, I

14 Binder (2002) also relies heavily on documents to construct her study.
followed the principles of theoretical sampling as applied in grounded theory, so that my data-collection efforts would sufficiently account for the “relevant concepts and their properties and dimensions” (Corbin & Strauss, 2008, p. 144). Following theoretical sampling principles within my conceptual framework, my data-collection efforts focused on gathering sufficient data to uncover whether and to what extent (a) the evolving interpretations and interests frame captures the pressures, tactics, and other means used by lay challengers to the academic scientists’ research and (b) the modified version of Freidson’s theory of professional dominance (which contains both the internal characteristics of the academic profession and the external factors that influence the degree to which the profession has autonomy) account for the strategies, tactics, and other means that the academic profession employs to protect its intellectual freedom. When the concepts were sufficiently explained or I reached a point of saturation, I moved forward with the data analysis. As I explain in the next section in greater detail, my steps also dealt with treatment of discrepant data and/or alternative explanations of my data until I was able to identify a clear data pattern (see, e.g., Binder, 2002).15

In conclusion, I followed the suggested guidelines established in the qualitative literature to adhere to social science norms (see, e.g., Lee, 2000). After several months of conducting online searches using the steps outlined above, I generated and stored 3,245 documents. The length of the documents ranged from one page to 872 pages. Because my documents included non-text-based materials, including pictures and graphs, I also report my document trove in terms of the kilobyte (kb) size. The documents ranged from 9 kb to 150,066 kb. While I used more than 3,000 documents to inform me about the cases, not all documents were equally helpful. I relied more heavily on 149 documents for the sewage sludge case, 411 documents for

15 My data-collection process echoes what Binder (2002) stated: “While I cannot be certain that I exhausted the universe of articles … for each challenge using this search method, I am confident that I unearthed the vast majority of [documents] [archived] about the challenge…” (p. 50).
the climate change case, and 246 documents for the salvage logging case. Many documents served as additional support by restating, confirming, or clarifying earlier statements or information. As I explain in the next sub-section, my theoretical sampling is what determined if I had sufficient and appropriate data and whether I needed more documented data. That consideration led me to determine that my core data rested in 806 documents. These accounted for much of my data reporting of the cases in chapters 5-7.

**Data Analysis: Approach, Techniques, and Procedures**

Since this study explores what it means for government officials to challenge academic scientists’ federally funded research and for academic scientists to exert control over their work, I strived to connect concepts, including their properties and dimensions, with generalized knowledge or what social science researchers often refer to as theoretical generalization (see, e.g., Seale, 1999; Shulman, 1997). My data-analysis approach, techniques, and procedures followed a two-stage process. First, I analyzed each of the cases through a method infusing theatrical metaphors to explain the case interactions. Second, I followed the conventions of a comparative case study methodology and the logic used to construct a variant of grounded theory, which relies heavily on the data but is also partially guided by an existing theoretical framework. Below, I explain these two parts of my data analysis.

**Individual Case Study Reporting: A Content Analysis with Theatrical Metaphors**

In this first stage of the data analysis, I examined each case of government challenges with academic scientists’ research as an initial investigation. Relying on publicly accessible documents as my data source, I inquired into academic scientists’ experiences and interactions with others when government challenges take place. As a starting point, to analyze the
documents collected in this study, I conducted a content analysis under the conventions of a comparative case study methodology. That is, I analyzed within a case and across cases.

Content analysis employs a textual translation of the words, phrases, symbols, images, and other visual items used in the data. Consistent with the evolving interpretations and interests frame, as a researcher, I interpreted the meanings associated with observed objects. My interpretation of the relationship between the text and its source impacted the meanings associated with the documents used as data sources. In the literature about content analysis as a data-analysis approach, a debate exists regarding how researchers should interpret the relationship between the text and its source. Some researchers contend that the text contains an objective meaning; therefore, the common understanding of the text speaks for itself (see, e.g., Berelson, 1952). Other specialists in content analysis assert that the analysis cannot separate the content and its contextualized source, since the meanings derive from the messenger and the parties connected with that message making (see, e.g., Holsti, 1969; Krippendorff, 2004). The latter approach to content analysis (i.e., interpreting the contextualized source) reflects this study’s evolving interpretations and interests frame to data analysis. As I mentioned in Chapter 2, this framework suggests that meanings associated with objects should derive as authentically as possible from the parties engaged in the interaction. Consistent with that approach to content analysis, Krippendorff (2004) states that “[c]ontent analysis is a research technique for making replicable and valid inferences from texts (or other meaningful matter) to the contexts of

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16 Holsti (1969) actually moves beyond just connecting the overall context to a more elaborate and rigid schema of coding of the textual communication. His analytic approach requires responding to a series of information about the text such as “who,” “what,” “how,” “to whom,” and “why,” with explicit details; however, answers to those questions did not always exist from selected data I collected. Krippendorff’s approach also informed my research practices. Krippendorff’s takes a bit broader and gives the researcher some degree of freedom to determine the meanings of the data when a contextual one cannot be drawn for each data point. In other words, the researcher uses her/his “lever” (i.e., researcher-based, informed knowledge) when the context is silent.

17 I do not use discourse analysis because there are some documents that are clearly not intended for discourse, and I am using entirely nonreactive measures, so many of the documents have different purposes than intended. Themes might be better recognized under content analysis.
their use” (p. 18, emphasis in original). That is, content analysis relies heavily on the context to reveal the meanings and is appropriate “for describing and interpreting the artifacts of a society or social group” as is intended in this study design (Marshall & Rossman, 2006, p. 108).

Since Krippendorff’s approach to content analysis aligns with the evolving interpretations and interests frame, I briefly describe Krippendorff’s analytic approach, which I employed for my initial round of data analysis. Krippendorff presents six steps:

**Step 1: Unitizing**

Unitizing refers to the unit of analysis for the data analytics. It is the unit of meaning or those textual expressions that represent a concept or theoretical element. I anticipated that my unit of meaning would initially begin with very narrow units, such as a line in a blog entry. As more data is analyzed, the patterns of behavior become easier to recognize and the unit of meaning may expand.

**Step 2: Sampling**

Sampling draws a subset of data analysis from the population. One sampling technique of interest is relevance sampling. Relevance sampling “aims at selecting all textual units that contribute to answering given research questions” (Krippendorff, 2004, p. 119). For purposes of this study, which attempts to theorize what it means for academic scientists to exert control over their research, I employed a theoretical sampling technique from grounded theory. Rather than inspecting all news articles that discuss Congress’s demands for research data on climate change, I examined key articles that capture
representations of various concepts and eliminated articles or excerpts of articles that did not add to the analysis.

Step 3: Recording/Coding

“Recording takes place when observers, readers, or analysts interpret what they see, read, or find then state their experiences in the formal terms of an analysis” (Krippendorff, 2004, p. 126). In other words, recording involves the initial jotting down of notes about the textual organization and meaning. The researcher records a set of relationships and rules that originate from the theory. So, recording serves as the starting point, and coding becomes the process of actually assigning some meaning to the organized text.

Step 4: Reducing

Reducing simply refers to the editing of the data in a more meaningful manner to start identifying the phenomena of interest. Krippendorff (2004) suggests that the researcher use the established codes and connect that scheme to scales or other categories. For instance, professional discretion in research approaches may be one of the codes for this study. By reducing, I may take the list of textual data under this code and reduce the data into categories, or measures, such as high, medium, and low.

Step 5: Inferences to Contextual Phenomena

Inferences to contextual phenomena represent the connections between the basic textual analyses to the meaning of the text in context. “It bridges the gap between descriptive amounts of texts and what they mean, refer to, entail, provoke, or cause” and it “points to unobserved phenomena in the context of
interest to an analyst” (Krippendorff, 2004, p. 85). This step is the process that begins the analytical construction; it supports, modifies, or ignores the existing theoretical framework and contributes to the study from the observable phenomena. In addition, Krippendorff notes that the latent meanings oftentimes emerge in the inferences stage.

*Step 6: Narrating*

Narrating refers to the data-analysis reporting; it incorporates both the description and inferences. Narrating usually includes a recounting of the steps taken, with the inclusion of the coding process and notes about the patterns and themes that emerged. In addition, it responds to the research questions.

I conducted Steps 1 to 4 (unitizing, sampling, recording/coding, and reducing) with each case; then, Steps 5 and 6 (inferring and narrating) occurred initially with each case, followed by a collective analysis across the cases.

In addition, during an initial review of the data, I examined the academic profession’s responses using a modified version of Freidson’s theory of professional dominance – as I described in Chapter 3. Content analysis methodologists suggest that the structure of the initial analysis depends on whether or not it draws largely from an existing theory (Hesse-Biber & Leavy, 2006; Hseih & Shannon, 2005). The researcher’s decision to analyze the data substantially as an outgrowth from existing theory potentially directs or redirects the meanings drawn from the data. According to Hseih and Shannon (2005), a substantial reliance on a theory to account for the relationships among concepts guides the first round of coding, whereas a study designed absent theory-driven constructs does not start with the same amount of guidance with the initial coding scheme. For instance, in this study, I worked with the theoretical
underpinnings represented in my conceptual framework, such as the examination of the internal characteristics of the academic profession (i.e., professional knowledge and skills, divisions of labor, labor markets and careers, training programs, ideological commitment, and professional associations), and the external factors (i.e., bodies of knowledge and the state) as considerations to understand what it means for the academic profession to exert control over its federally sponsored research.

The content analysis led me to focus significantly on each actor’s role in the controversy. I divided actors into teams. For instance, for government officials, I had primary and secondary political instigators as well as primary and secondary political allies. As I explored the actions of each team, I noticed that team members were behaving much like actors in a play. This performance effect was not surprising, given the high-profile nature of environmental policies and the public scrutiny that government officials endure. Furthermore, I used publicly accessible documents, so actors involved in these three cases would have scripted their statements, staged their actions, and taken some matters offstage. Given the performance of each case, I examined my documents with inquiries about (a) the role expectations of the teams, (b) timing of sub-events, (c) interaction effects, such as whether the events transpired into cooperation or conflict between and among teams, (d) event staging, such as whether it took place onstage (e.g., congressional hearings) or backstage (e.g., meetings mentioned in a report but never reported in any other observable manner), and (e) significance of props such as policy tools and reports, (f) effect of scripts (e.g., press releases and memos) as well as improvisational conversations (e.g., phone conversations and quick, unplanned exchanges).

Building off the data analysis, I used a chronology along with a theatrical metaphor to narrate my presentation of the data (see chapters 5-7). The theatrical metaphor was particularly
helpful when examining politicians’ messages and actions, since they frequently crafted language (e.g., sound bites) or behaved in a manner intended to gain support from their constituents and other groups. To reiterate, these considerations did not change my framework; however, I was probably more attuned to the significance of certain communications, such as the effect that some reports have as policy tools, and this analytic lens led me to dig deeper into relations between the government officials and university administrators who had oversight but no direct research responsibilities for the challenged study. Further, it more clearly articulated the academic scientists’ efforts to defend their research.

Cross-Case Analysis: A Modified Grounded Theory

Although this study presents a theoretical explanation that might generate the “key concepts or variables as initial coding categories” (Hseih & Shannon, 2005, p. 1281), I used an iterative review of the data, which involved multiple rounds of coding and recoding, to illuminate interactions not recognized by the theoretical sources that initially informed my study (see, e.g., Neumann, 2006, 2009). Therefore, for the second stage of my data analysis, I conducted additional analyses that required sifting, sorting, coding, and recoding exercises beyond the theoretical framework – especially one so conceptually defined as the modified version of Freidson’s theory of professional dominance, which I used to understand the academic profession’s responses to challenges.

I followed an analytic approach similar to Neumann’s (2006, 2009). Like Neumann, I too applied key analytic principles of grounded theory, a qualitative methodology that builds theory from data, in order to better understand what it means for government officials to challenge academic scientists’ federally funded research and for academic scientists to exert control over their research. In her works, she referred to selected literature as a way to inform
her understanding about selected themes, but she largely built her explanations regarding the relationships among various concepts without an established frame. For example, Neumann (2006) set out to study recently tenured professors’ personal expressions of “love” of their scholarly endeavors as articulated under her concept of “passionate thought in scholarship.” Initially, drawing from the extant literature, she constructed her analysis of professors’ expressions of passionate thought based on their experience, and she articulated frames of experience as outgrowths of the professors’ awareness and emotion. These theoretical explanations “shaped and shaded” her understanding of the primary concepts in her study (p. 385).

At the same time, she drew heavily from her data. She coded, bounded, constructed new codes, recoded, and rebounded. Through each iteration, she examined the data to address “incongruous and disconfirming signals” and recoded and rebounded until she reconciled the data into workable codes; then she crafted her themes accordingly (p. 391). By moving beyond the initial framework and becoming enmeshed in the data, she expanded the scope of what she discusses about professors’ experience (i.e., something more than awareness and emotion); she explored the idea of “how passionate thought is contextualized in professors’ larger experiences, intellectually and emotionally, of their scholarly work and lives” (p. 391). In the end, she recognized that her data revealed a refined theoretical explanation of “passionate thought as a distinctively contextualized experience,” and her modified grounded theory approach led her to that conclusion (p. 392).

Like Neumann (2006), I revisited the value of the theoretical explanation to help me understand what it means for government officials to challenge academic scientists’ federally funded research and for academic scientists to exert control over their research. While my data
largely affirmed the proposed theoretical underpinnings (i.e., the modified version of Freidson’s theory of professional dominance and the evolving interpretations and interests frame), I kept open the possibility that my data might lead me to present an alternative explanation, so I had to use the theoretical concepts “lightly.” Throughout this process, my analysis referred to the literature and theoretical explanation in a manner that informed me about the phenomenon that I sought to explain. To stay true to my findings, I was “sensitized” to the relations among concepts, yet my knowledge of those relationships also offered me a skeletal structure to envision the possible connections without driving or dictating the analysis (Corbin & Strauss, 2008).

Using an iterative process, I coded, bounded, recoded, and rebounded until I constructed a theory from the data generated. While taking these steps, I searched for and accounted for discrepant data in order to properly craft my circumscribed grounded theory (cf. Popper, 2002). Keeping in mind that this analytic process is a fluid one, I was open to what Neumann characterizes as an analytic approach that explores “ways to see beyond” the existing theoretical concepts. I allowed the possibility that the data might reveal a refined or significantly different, though circumscribed, theoretical explanation about what it means for academic scientists to exert control over their research when government challenges take place. By virtue of this analytic approach, I inquired into the usefulness of my conceptual framework, which combines the modified version of Freidson’s theory and the evolving interpretations and interests frame, as a way to understand academic scientists’ exerting control over their research. As I explain in Chapter 9, my findings did not reveal a substantially different theoretical explanation from Freidson’s model of professional dominance; it did, however, lead me to suggest some refinement to my conceptual framework.
Trustworthiness, Validity, and Reliability

In qualitative research, to ensure the study’s value, the researcher establishes a set of strategies that increase the trustworthiness or authenticity of the research process and product (Lincoln & Guba, 1985). In this subsection, I briefly discuss each of the research standards (i.e. credibility, dependability, generalizability, and confirmability) and the strategies that I employed to meet those standards.

Credibility

Credibility examines the internal validity of the research. It evaluates whether the researcher used appropriate evidence to justify the asserted claims from the study’s findings (Lincoln & Guba, 1985; Merriam, 1998; Prior, 2003; Stake, 2005; Yin, 2003). For this study, I examined whether the connection between the observations and phenomenon. In order to meet the credibility standard and reduce error from improper inferences, I followed the procedures of triangulation, repeated observations, and disclosure of the researcher’s standpoint (Lincoln & Guba, 1985; Merriam, 1998). In addition, I sought feedback from other researchers to seek further insights about my research’s internal validity.

- Triangulation: This is the “process of using multiple perceptions to clarify meaning, verifying the repeatability of an observation interpretation” (Stake, 2005, p. 443). For this study, I used multiple documents from various sources to capture each construct (e.g., codes, sub-codes, dimensions, properties). This approach is also consistent with Lewis’s (1988) study on intellectual freedom, as he, too, emphasized the requirement of multiple data frames for each descriptive data point as a strategy to address research credibility.
• Repeated Observations: I conducted repeated observations of the same phenomenon, which included “gathering data over a period of time in order to increase the validity of the findings” (Merriam, 1998, p. 204). I reviewed the documents multiple times through repeated data analysis events to ensure research credibility.

• Researcher’s Standpoint: Disclosure of the researcher’s standpoint involves the clarification of the “researcher’s assumptions, worldview, and theoretical orientation at the outset of the study” (Merriam, 1998, p. 205). This information gives the reader a sense of how the researcher constructed the “systematic connections among the observable behaviors, speculations, causes, and treatments” (Stake, 1997, p. 408). Accordingly, in this chapter, I disclosed my standpoint, such as my assumptions and theoretical orientation. In addition, I noted that I am a professor, who strongly believes in intellectual freedom to protect professors’ rights over research that is germane to their expertise. Also, I am a lawyer, so I bring my legal lens to the research, when applicable.

• Other Researchers’ Examination: Merriam (1998) refers to peer examination as a process that incorporates the use of colleagues in the discussion of the findings. For purposes of this study, I drew on other researchers’ examinations through several forums, including academic colloquia and an advisory council. These events took place on the campus of a university in the Upper Midwest as well as via online conferences (i.e., Skype and Google+). I presented my data and comparative case analyses at three academic colloquia (one in-person and two virtual sessions) to receive feedback regarding my data analysis and presentation of the findings. In addition, I established a group of three scholars in the social sciences; this group served as my research advisory council to
discuss research concepts and data analysis. I spoke to two members of the council about ideas and written statements that eventually led to chapters 5-9.

**Dependability**

Dependability determines the research reliability. It ensures that “the results are consistent with the data collected” (Merriam, 1998, p. 206, emphasis removed; see also Lincoln & Guba, 1985; Yin 1994). Dependability focuses attention on the “extent to which research findings can be replicated” (Merriam, 1998, p. 205). In order to meet the dependability standard so replication of the study is possible at some later point in time, I followed Merriam’s (1998) suggested steps to disclose the investigator’s position, triangulate the data, and establish an audit trail.

- **Investigator’s Position:** The investigator’s position reveals the “assumptions and theory behind the study” through a discussion about the selection process on the data sources, data gathering, and data analysis (Merriam, 1998, p. 206). This information supports the consistency of the findings throughout the research process. Accordingly, I have detailed my assumptions and theories for this study as a means to ensure research dependability.

- **Triangulation:** Triangulation (see also, strategies for credibility, above) is the “process of using multiple perceptions to clarify meaning, verifying the repeatability of an observation interpretation” (Stake, 2005, p. 443). This strategy also ensures research dependability.

- **Audit Trail:** An audit trail offers a descriptive set of notes about the research data-collection and analysis processes. Thus, “the investigator must describe in detail how data were collected, how categories were derived, and how decisions were made
throughout the inquiry” (Merriam, 1998, p. 207). As part of my research process, I recorded my steps within four lab books to ensure research dependability.

**Generalizability**

Generalizability refers to external validity of the study. Because this study accounts for the theoretical construction of what it means for government officials to challenge academic scientists’ federally funded research and for academic scientists to exert control over their research, the generalizability refers to theoretical generalization of the case representations of government challenges to academic scientists’ federally sponsored research. That is, within the boundaries of the cases, I asked myself whether my data analysis captures the inferential treatment of the conclusions drawn (Shulman 1997). Although Geertz (1973) contends that “theoretical formulations hover so low over the interpretations they govern that they don’t make much sense or hold much interest apart from them” (p. 25), nevertheless, other researchers suggest that an examination of events or tasks “around common problems or themes” potentially characterizes theoretical concepts that generate theory, though perhaps one with contextual particularity (Rueschemeyer, 1984, p. 160). In order to work toward theoretical generalization, I used methodology, especially in terms of my data-analysis techniques, largely derived from grounded theory. Accordingly, I followed the principles of theoretical sampling (Corbin & Strauss, 2008) and I presented multiple cases with relatively detailed descriptions. I wanted to present sufficient information to draw out the points of commonality among the cases (Miles & Huberman, 1994). These strategies contributed toward my design of a qualitative study that presented a theoretical generalization from the cases.

- **Theoretical Sampling:** This refers to the collection of data that “will maximize opportunities to develop concepts in terms of their properties and dimensions, uncover
variations, and identify relationships between concepts” (Corbin & Strauss, 2008, p. 143).

Consistent with that principle, I collected data until the point of saturation in order to articulate properties and dimensions of concepts and explain relationships among the concepts, which collectively contribute to our understanding of the refined theory that I discuss in Chapter 9.

- Multiple Cases with Detailed Descriptions: Multiple cases support the refinement of the connections among concepts, which reduces the likelihood of odd or outlying events affecting the theory construction. Thus, my case data (chapters 5-7) present rather rich, detailed descriptions so that I had in-depth information about the cases to draw inferences about the data (Geertz, 1973). Taking these two study elements (i.e., multiple cases and detailed descriptions), Miles and Huberman (1994) suggest that generalizability drawn from the empirical data likely deals with variations and captures the commonalties in order to craft a theory:

  One aim of studying multiple cases is to increase generalizability, reassessing yourself that the events and processes in one well-described setting are not wholly idiosyncratic. At a deeper level, the aim is to see processes and outcomes across many cases, to understand how they are qualified by local conditions, and thus to develop more sophisticated descriptions and more powerful explanations (p. 172).

In short, my process of presenting three cases with detailed descriptions supported my efforts for theoretical generalization.

Confirmability

Confirmability refers to the extent that other researchers can confirm or corroborate my research results (Lincoln & Guba, 1985). Although researchers bring their own perspectives and
knowledge to a research project, researchers’ sensitivity to insights added to the study and disclosure about what insights contributed to the study provide sufficient information so an observer can confirm the findings in light of the researcher’s disclosures. In order to meet the confirmability standard, I disclosed my position within this investigated topic, provided an audit trail, searched and accounted for discrepant data, and highlighted the benefits of the study’s data sources.

- Investigator’s Position: The investigator’s position reveals the “assumptions and theory behind the study” through a discussion about the selection process on the data sources, data gathering, and data analysis (Merriam, 1998, p. 206). With this disclosure, others may more easily confirm or corroborate my research findings. While I have already presented many of the assumptions and descriptions that crafted my theoretical framework to construct this study and the data sources that informed this study, I have also described my data gathering and analysis to inform readers of my analytic logic, so as to increase confirmability of my conclusions.

- Audit Trail: An audit trail offers a descriptive set of notes about the research data-collection and -analysis process. As mentioned earlier, an audit trail permits another researcher to replicate the study to achieve confirmation or corroboration of the study’s findings. Accordingly, I created an audit trail of my research process as a strategy that supports data confirmability.

- Discrepant Data: By searching and accounting for discrepant data, I discovered that certain assumptions I made during the process of my data analysis were at times incorrect. As I discussed earlier, I employed a technique that Neumann (2006) used, which required me to undergo several rounds of coding, recoding, bounding, and
rebounding of my data until I could satisfactorily address the discrepant data. As Miles & Huberman (1994) suggest, these steps contribute to researcher bias reduction and support the construction of robust theoretical knowledge that eventually led to my refinement of Freidson’s theory of professional dominance.

- Data Sources: The data sources used in this study were publicly accessible documents. This type of data has two benefits for confirmability. First, the document sources are easy to obtain relative to other data sources, so confirmation of findings becomes easier than with many other data sources. Second, documents are nonreactive measures, so they are “unaffected by the research process,” since the information on the documents was collected for other purposes (Merriam, 1998, p. 126). Consequently, my review of documents offered me a more open review than would be possible with other data sources (Hatch, 2002; Prior, 2003).
Table 4.1: Trustworthiness Standards and Strategies Employed

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<tr>
<th>Trustworthiness Standard</th>
<th>Description</th>
<th>Anticipated Strategies</th>
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<td>Credibility or Internal Validity</td>
<td>Whether appropriate evidence exists to justify the asserted claims from the study’s findings</td>
<td>Triangulation</td>
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<td>Repeated Observations</td>
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<td>Researcher’s Standpoint</td>
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<td>Other Researchers’ Examination</td>
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<tr>
<td>Dependability or Reliability</td>
<td>Whether the study’s results are consistent with the data and capable of replication</td>
<td>Investigator’s Position</td>
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<td></td>
<td></td>
<td>Triangulation</td>
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<td></td>
<td>Audit Trail</td>
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<tr>
<td>Theoretical Generalizability or External Validity</td>
<td>Whether the study’s findings apply to concepts that generate or refine theory</td>
<td>Theoretical Sampling</td>
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<td></td>
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<td>Multiple Cases with Rich, Detailed Descriptions</td>
</tr>
<tr>
<td>Confirmability or Objectivity</td>
<td>Whether the study’s results can be confirmed or corroborated by others</td>
<td>Investigator’s Position</td>
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<td></td>
<td></td>
<td>Audit Trail</td>
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<td></td>
<td>Search and Account for Discrepant Data</td>
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<td></td>
<td></td>
<td>Data Sources, Use of Documents</td>
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**Ethical Issues**

To construct and carry out a study that met ethical standards, I disclosed the process followed to ensure compliance and proper disclosures pursuant to the Institutional Review Boards at two universities. In addition, I described how the use of publicly accessible documents and the steps that I took to obtain the documents meet ethical research standards.
Compliance and Disclosures

Throughout the data-collection and -analysis process, I complied with the Institutional Review Board (IRB) policies at two educational institutions. Because I am a doctoral student at Teachers College, Columbia University, I completed the documents and comply with the IRB process at Teachers College. In addition, although I do not conduct this study within my capacity as a faculty member at the University of North Dakota, to avoid any question about my role and the capacity under which I conducted the research, I complied with the IRB standards at the University of North Dakota. Also, to avoid any suggestion of impropriety or conflicts of interest, I disclose that I do not have a direct interest in any of these case studies. But because, indirectly, every person has some interest in this research study, I disclose here that during the time of this study, I was not and have not engaged directly in work related to the national policies or research agenda for sewage sludge, climate change, or salvage logging.

Furthermore, during the period of this study, other than conversations that helped interpret my data, in order to reduce bias about matters related to the environmental topics and issues pertaining to the research process, I limited my discussions with others on topics related to sewage sludge, climate change, and salvage logging. In addition, to ensure data integrity and authenticity, I archived the data: one hardcopy and one electronic copy of each of the documents retrieved. Furthermore, pursuant to my assurances of data trustworthiness, I documented my research trail within four lab books. This step served as my research audit trail.

Publicly Accessible Documents

Because the case boundaries for this study (i.e., university-based academic scientists who are employed by American universities, working in the United States, and receiving federal sponsorship for their work; U.S. federal government actors as challengers to the research; the
case controversies over research within environmental policies) limit potential subjects and institutions as available sources for information, I relied exclusively on publicly accessible documents; any other data-collection method might have made subjects susceptible to employment retaliation or other forms of scrutiny. To avoid possible ethical dilemmas potentially associated with the identification of the actors and institutions, I intentionally used publicly accessible documents to represent my data sources. Similarly, the public nature of the data, which identifies the actors and institutions as well as their interactions, was the basis for this study’s research questions, design, and methodology. Such openness avoided the use of voluntary subjects and any potential harm to them.

In addition, as I noted earlier in the discussion of the data sources, documents are nonreactive data, so documents reduced the potential for ethical dilemmas associated with the identification of the actors and institutions. Documents do not require direct contact with human subjects in investigations, as interviews do. Instead, they are created for purposes other than the research presented. In contrast, reactive data-collection methods could bring selected actors or institutions to harm by jeopardizing future funding. Consequently, since reactive data-collection methods could serve as obtrusive forms, informants who experienced the case interactions described in this study might well elect not to participate. To avoid these potential barriers to data, this study used documents.

My limitation of sources to publicly accessible documents potentially overcomes many questions of ethics. As the phrase indicates, publicly accessible documents are not personal, private items, which, depending on the data-collection approach, might raise questions of research ethics. For this study, I used only data sources in the public domain or accessible
through public acquisition, such as Internet postings of news reports, blogs, and web page document depositories.

**Limitations of the Study**

The study’s questions, design, methodology, methods, and analysis as well as the literature reviewed support each other and meet the purpose of the study – to explore what it means for government actors to challenge academic scientists’ federally sponsored research and for academic scientists to exert control over their research. Despite the study’s construction, there is one identifiable limitation to this study: This study relied exclusively on documents. Several research methodology texts explicitly indicate or suggest that documents should not serve as the exclusive form of data collection (see, e.g., Hatch, 2002; Hodder, 2000; Merriam, 1998; Russell & Kovacs, 2003; Webb, Campbell, Schwartz, Sechrest, & Grove, 1981).

Although earlier I justified the rationale behind the study’s exclusive use of documents as data sources, here, I present the four potential drawbacks to the use of documents as exclusive data for the study.

First, documents are nonreactive. This has benefits, but there are also drawbacks. As a researcher, I was unable to directly ask how the actors or those representing social institutions made meaning of the interactions. Second, because I do not have reactive informants, I also could not ask for leads to other data sources. Thus, my snowball sampling for data sources depends entirely on other documented sources. Third, some of the events and interactions could not be captured in a documented form. Although advancements in technology have increased instances of recording of events and communications, there may be selected interactions that are not captured on any documents. Fourth, by exclusively using publicly accessible documents, my study has another limitation. In particular, I excluded private documents, such as diaries and
other non-publicly accessible materials, that could have contributed to the refinement or
generation of a theory to represent these cases.

Despite these drawbacks, I wish to emphasize that publicly accessible documents as
exclusive data sources also respond to society’s need for information about the cases to
determine societal legitimation (see Rationale for the Data Source, in this chapter). Furthermore,
in terms of data integrity, documents are nonreactive, grounded within the context source,
accessible, and easy to replicate. These factors support the strength of this study’s capacity to
construct a theoretical generalization capable of withstanding public scrutiny. Thus, although
publicly accessible documents come with some study limitations, as a whole, the benefits of
using publicly accessible documents outweighed the drawbacks. Nevertheless, I disclose the
limitations that arise from such documents.
CHAPTER V: THE SEWAGE SLUDGE CASE

Introduction

This chapter presents the findings of the sewage sludge case. First, I provide a brief overview of the science and the public policy debate about land-applied sewage sludge. I also introduce an academic scientists’ government-sponsored study that raises concerns about the science policy. Second, I describe several challenges to the academic scientists’ government-sponsored research. Examining the circumstances and events surrounding these challenges, I capture the interactions among individuals and groups involved to explain the actors’ reported perceptions and interests. Third, I present my data on the strategies that academic scientists and their supporters employed. I explain, below, how these strategies helped the challenged researchers exert control over their government-sponsored work. Specifically, drawing on my theoretical framework in Chapter 3, I examine the internal characteristics of and external factors influencing the academic profession to help us understand how certain responses from supporters of the challenged research aided the academic scientists of the contested study. Fourth, I summarize the key findings.

The Science, the Public Policy Debate, and the Study

The Science Policy

Since 1993, the U.S. Environmental Protection Agency (“EPA”) has permitted the use of certain grades of sewage sludge for agricultural or consumer usage.18 According to the EPA, sewage sludge, also known as a biosolid, is “solid, semisolid, or liquid residue generated” from

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18 The EPA identifies two types of sewage sludge – Class A and Class B. Generally speaking, Class A sludge involves the most thorough wastewater treatment processing and usually originates from nonindustrial, municipal sources. By contrast, Class B sludge accepts significantly higher levels of pathogens and heavy metals that could be dangerous if ingested. Because Class B sludge may not be handled by humans without proper equipment and taking other steps to minimize human contact, the EPA prohibits its use for agricultural or consumer usage.

In this case, the controversy involved both Class A and Class B sludges; however, the most severe adverse health reactions appear to have originated from Class B sludge.
households and industries that is treated through a sewer treatment plant process. Basically, sewage sludge represents the cumulative materials of treated by-products from various municipal sewer systems and industrial waste plants. These by-products include liquids drained, human excrement flushed, and other substances that enter the sewage lines.

In the United States, land application of sewage sludge became more prominent after Congress passed the Ocean Dumping Ban Act of 1988, a law that phased out sewage sludge dumping in oceans by 1992. As an alternative to dumping in the ocean, a coalition consisting of several municipalities, wastewater treatment plants, sludge-management companies, and farmers advocated land usage of sewage sludge. When the sludge is properly treated, the science is quite clear: Sewage sludge contains many beneficial nutrients for farming and landscaping purposes. Recognizing land usage of sewage sludge as a substitute for ocean dumping, the EPA adopted new regulations — frequently referred to as “503 rules.” These regulations spelled out what chemicals, metals, and other particles would be treated and tested to determine acceptability of the processed sludge. However, some environmental groups, citizen groups, farmers, academic scientists, and even federal scientists objected to the use of the treated sludge. Many of them believed that the federal standards failed to protect the public from harmful metals and pathogens. According to sludge opponents, the federal regulations maintained ratios that were too lax and permitted microorganisms (including harmful pathogens) in sewage sludge.

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20 The EPA and the sewer-management programs determined an acceptable level of industrial waste that may enter the municipal sewage systems.
21 See, e.g., Carlisle, J. K., & Centrone, M. J. (2001, Jan. 28) EPA may be killer agency, some say. Charleston Gazette, 1C). John Carlisle and Michael Centrone, two environmental policy analysts, reported that the EPA had been defending its rules without sufficient scientific support. They suggested that political interests trumped citizen health interests. Based on their observations, “Americans have long been forced to pay a high price for EPA’s bureaucratic arrogance and scientific ineptitude in the form of lost economic opportunity and costly regulations” and they recommended environmental policy changes that adequately considered health effects from sludge applications.
The Controversial Science

Starting in 1994, critics of the 503 rules raised even more questions about the adverse health effects of land-applied sewage sludge. Two well-publicized events seemingly escalated the public concerns. First, in 1994, an 11-year-old boy, Tony Behun, died, and his family claimed that Tony’s exposure to sewage sludge contributed to his illness and led to his death. About a week before his death, Behun rode his dirt bike around a field blanketed with sewage sludge in Osceola Mills, Pennsylvania. Later that evening, he became ill with a fever and lesions formed on his arm. Several days later, he went into a coma. Then, about a week later, he died.

In 1995, similar symptoms were reported. That illness also led to death, but this time, it took place in Greenland, New Hampshire. In that same year, approximately 650 tons of sewage sludge covered one area of the town. The Greenland sludge surrounded the Conner family home. Several months later, residents of the town, including Shayne Conner, a 26-year-old, complained of physical reactions to the sludge. Shayne reported severe, adverse physical reactions, including vomiting, fever, and distressed lungs. Several weeks after the symptoms emerged, Shayne died in his sleep. His family claimed that the sewage sludge killed him or alternatively contributed to his death.

For both the Tony Behun and Shayne Conner cases, the media reported that the autopsy investigations concluded that there was no linkage between sewage sludge and each individual’s


complications leading to death. Despite these findings, the deaths of Tony Behun and Shayne Conner still worried many people who lived in communities with land-applied sewage sludge and communities that contemplated such use. Many in these communities attributed these deaths to Tony and Shayne’s exposure to the nearby sludge. They also asked for an EPA investigation, but based on news reports, the EPA initially just stood behind its rules and was slow to act.

Conflict Between the Federally Sponsored Study & the Science Policy

In 1996, with federal and state grant funding, Ellen Harrison, Murray McBride, and David Bouldin of the Cornell Waste Management Institute (“CWMI”) conducted a study that evaluated the human health effects from land-applied sewage sludge. Specifically, the CWMI research team analyzed the impacts of the EPA’s risk-assessment procedures (i.e., the 503 rules). The CWMI research, which is at the center of this controversy, appeared in three different forms, but each paper was essentially the same.

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23 Gibb, 2000; Rutter, 2000; Snyder & Kunkle, 2001; USA Today Staff, 1999; Tuohy, 2000.
24 Gibb, 2000; Rutter, 2000; Snyder & Kunkle, 2001; USA Today Staff, 1999; Tuohy, 2000.
25 Shayne Conner’s mother sued the company that applied the Greenland sludge. In 2002, Synagro Technologies, the company that acquired the organization responsible for applying the sludge in Greenland, settled with Shayne’s mother. While a legal settlement does not necessarily mean fault, the settlement likely demonstrated Synagro’s evaluation of the odds that it would lose the case. Agreeing with what many others thought, David Lewis, a former EPA scientist, said: “Synagro obviously avoided a lawsuit that they must have felt they stood a reasonable chance of losing, or else they wouldn’t have settled the case.” (Truini, 2002)


26 USA Today Staff (1999, Oct. 7). Faced with evidence of health threat, EPA looks the other way USA Today, 18A (“these deaths call for an aggressive investigation … [but] the Environmental Protection Agency has been circling the wagons.”); Tuohy, J. (2000, Jul. 13). 2 mothers, 2 deaths, too many questions: Sickness was in the air, but officials wouldn’t blame sludge. USA Today, 13D (According to this USA Today article, Shayne Conner’s mom commented that the “Environmental Protection Agency made a mistake with its sludge program and doesn’t know how to back away without admitting culpability”); Snyder, D., & Kunkle, F. (2001, Aug. 1). Health fears over sludge spur quest for controls: EPA stand challenged after suspicious deaths. Washington Post, B1 (According to the Washington Post article, “the EPA remains largely unconvinced that ‘biosolids’ cause health problems.”)

27 I refer to Ellen Harrison, Murray McBride, and David Bouldin as the CWMI scientists and the CWMI researchers.
First, in March 1997, an early version of the CWMI study appeared as a draft paper, *Land Application of Sewage Sludges: Recommendations and Appraisal of the US EPA’s Part 503 Sludge Rules*. That initial report included a scientific safety assessment about the federal 503 rules and included recommendations for increased public protections for the New York State sludge rules. Second, in August 1997, the CWMI scientists issued a working paper, *The Case for Caution: Recommendations for Land Application of Sewage Sludges and an Appraisal of the U.S. EPA’s Part 503 Regulations* (“Case for Caution”). That paper raised more concerns about the 503 rules and proposed stricter standards, which aligned with standards in several European countries. Third, the CWMI researchers published their *Case for Caution* paper in a peer-reviewed journal, *International Journal of Environment & Pollution*, with the title, “Land Application of Sewage Sludges: An Appraisal of the U.S. Regulations.” For all three papers, the CWMI scientists examined the scientific implications of the EPA’s regulations (though the final two papers contained slightly more detail and an international comparison of sludge regulations, noting that other countries have adopted stringent guidelines).

Each of the papers essentially restated the same findings. In these papers, the CWMI scientists alleged that serious health and safety concerns existed, given the current EPA regulations on land-applied sewage sludge. The papers identified and discussed what the authors believed to be 14 problems with the regulations, including assumptions made to construct the regulations. For instance, according to the CWMI scientists, the regulations did not account for regional variations in soil quality. Consequently, the regulatory standards did not factor those differences in when conducting risk assessments, which meant that some soil could have had

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concentrated levels of metals or other harmful substances. Further, the CWMI scientists contended that the regulations failed to examine many metals and pathogens, which were possibly overlooked in 1993 or did not exist as prominently in 1993. The CWMI scientists also warned that the regulations had not properly considered harm caused from more indirect human contact. In particular, the regulations did not adequately consider adverse health impacts from (a) certain agricultural land applications, (b) infected sources caused by events such as water runoff, and (c) ingestion of harmful materials from other sources, such as grazing animals. Given these possible risks, the CWMI researchers recommended numerous policy changes, including higher cutoff levels for acceptable substances in the sludge, consideration of additional metals and other by-products for soil testing, and more stringent oversight and enforcement practices of sewage-sludge application.

Interactions Surrounding the Challenges

The Cornell Waste Management Institute (“CWMI”) papers generated a lot of attention among academic scientists, state and federal policymakers, municipality administrators, environmental groups, wastewater treatment industry representatives, and citizen groups concerned about sludge application. While no one ever demonstrated that the CWMI scientists engaged in scientific misconduct, such as data fabrication, manipulation, or suppression, many individuals and groups questioned the study’s methodology and analyses as well as speculating that the researchers might have been interested in advancing a policy that the science did not justify. Given these concerns, critics of the CWMI research dismissed the study as not credible or sufficiently flawed. Thus, in their minds, the CWMI research had little or no value.

In this section, I focus on the significant challenges to the CWMI research. These challenges drew the attention of the CWMI scientists and other observers, and the challengers
and supporters of the CWMI scientists interacted in a sufficiently meaningful way to explain the situation. Like a play, the interactions surrounding the challenges appeared in Acts. In this metaphorical play, there are two Acts. In Act 1, I recount events involving several governmental actors (i.e., federal agency administrators and scientists) as the initiators of the challenges. In Act 2, I describe events in which a state government agency and a corporate entity took two separate actions to challenge the CWMI scientists. When viewed as a whole, these two Acts dramatize fully the storyline about the multistage challenges to the CWMI scientists’ government-sponsored research.

**Act 1: Government Challenges**

Act 1 introduces several governmental actors who led the challenge against the CWMI scientists. They also happened to be the same government officials who had oversight of the 503 rules and/or helped craft the original regulations. As I present below, these governmental actors employed a multi-step challenge, hoping that the CWMI’s governmentally funded research would go away.

*Scene 1:*

Scene 1 opens in 1997. In late March of that year, the CWMI scientists, Ellen Harrison, Murray McBride, and David Bouldin, completed the first paper criticizing the EPA’s 503 regulations. Immediately following the release of that paper, Harrison sent a copy of it and another paper to David Sterman, the deputy commissioner of the New York State Department of Environmental Conservation (“NYSDEC”). Harrison conveyed to Sterman that the papers explained the scientific reasoning behind the CWMI scientists’ recommendation that NYSDEC

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should adopt higher standards for what qualifies as acceptable sewage-sludge application, particularly for agricultural purposes. In April 1997, Sterman forwarded the materials to Robert Perciasepe, the assistant administrator for the EPA’s Office of Water. In his letter, Sterman questioned the science policy implications and requested assistance from the EPA to defend the federal regulatory standards, because that response would also determine the sufficiency of the state standards.31

Sterman’s correspondence to Perciasepe led to a chain of letters from the EPA and the U.S. Department of Agriculture (“USDA”) in which the federal agencies contested many of CWMI’s scientific observations.32 These federal actors also claimed that the CWMI’s recommendations failed to assert sufficient scientific evidence to justify any changes to the federal or state regulations. For example, Alan Rubin, one of the original authors of the 503 rules, sent a letter to Sterman and copied two other government officials and Ellen Harrison of CWMI. Rubin’s letter criticized the CWMI paper. According to Rubin, the CWMI scientists made many assertions and recommendations to the effect that “no scientific basis” could support the need for the proposed guidelines.33 In addition, Rubin suggested that the CWMI scientists examined the wrong issue. Rubin recommended that the CWMI scientists should explore the health impacts of animal manure products, not sewage sludge.

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These correspondences referred to communications involving federal scientists Rufus Chaney of the USDA and John Walker of the EPA. Both Chaney and Walker participated in crafting the original 503 regulations, and the correspondences referred to both these individuals as actors involved in discounting the CWMI research.

See also, Anonymous (1997, Dec.). Neither scientific facts nor meeting can bring biosolids comfort to Cornell Institute. Biocycle, 38(12), 10-12. (The article states: “Walker’s memo was accompanied by several letters from Bob Perciasepe … written in response to questions about the scientific basis for the EPA Part 503 regulation concerning biosolids use”).

33 Rubin, May 9, 1997, paragraphs 3, 7, 14, 18, & 19.
Like Rubin’s letter, the other correspondences followed the same pattern of discrediting and distracting. That is, several government actors initiated a series of communications to start a collective campaign to discredit the CWMI study. In addition, the correspondences served as an opportunity for these government actors to distract observers about the CWMI scientists’ reported concerns by directing these observers to other science policy matters (i.e., use of animal manure). While these letters reflected the initial challenges against the CWMI scientists, in Scene 2, a federal administrator, who was responsible for the 503 rules, introduced a third step to the challenge.

**Scene 2:**

In Scene 2, which begins in July 1997, besides discrediting and distracting observers about the issues raised in the CWMI research, Robert Perciasepe of the EPA sent his objections about the study to two Cornell administrators. Perciasepe wrote in July 1997 to David Sterman of the NYSDEC to express the EPA’s position about CWMI’s first paper. The four-page letter conveyed three clear messages. First, throughout the letter, Perciasepe criticized the CWMI paper for containing many “scientific inaccuracies.” According to Perciasepe, the paper presented several data source misstatements, failed to consider special land conditions, and asserted recommendations without scientific support. He framed the CWMI’s paper as the academic scientists’ attempt to unnecessarily alarm the public about a scientifically safe practice. In nearly every paragraph of the letter, Perciasepe also characterized the CWMI recommendations as “unnecessarily over-restrictive regulatory policies.”

that their [sic] regulatory decision is policy-based rather than science-based.”\(^{37}\) Put simply, Perciasepe clearly conveyed his beliefs about the CWMI paper and attempted to discredit the paper as a policy-advocacy piece that contained inadequate scientific justifications for the authors’ findings and recommendations.

Second, Perciasepe contended that the CWMI scientists placed “undue focus on biosolids” as a health concern.\(^{38}\) He claimed that the CWMI’s research “divert[ed] attention from problems that [were] occurring … from the use of animal manures and other agricultural inputs,” which academic scientists had not fully examined.\(^{39}\) However, the CWMI scientists along with David Lewis, a scientist for the EPA, and several grass-roots organizations opposing sludge application, disagreed with Perciasepe’s characterization of the CWMI scientists’ research contribution.\(^{40}\) They interpreted Perciasepe’s comment about the animal manure issue as an attempt to distract sewage sludge critics.

Third, Perciasepe sought assistance from others who might have influence over the CWMI researchers. As one might expect, Perciasepe sent copies of the letters to individuals who were directly connected with the CWMI study, such as Ellen Harrison, one of the CWMI authors, and Richard Rominger, the Deputy Secretary of the USDA, since the study addressed

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Also, the National Sludge Alliance issued more than 20 short papers referred to as “National Sludge Alliance Fact Sheets” that also question the EPA’s position and support the CWMI scientists’ research contribution. Furthermore, throughout this time (i.e., the late 1990s to the early 2000s, EPA scientist David Lewis participated in research projects examining the adverse health effects of sewage sludge and supporting the CWMI research. In 2001, Lewis filed a federal whistleblower claim against the EPA for allegedly silencing him and other federal scientists. According to Lewis, the EPA covered up the scientific evidence demonstrating adverse health effects from sludge. The EPA settled with Lewis for the sum of $205,000. In 2003, the EPA fired Lewis.

land-applied sludge for agricultural uses. However, Perciasepe also sent his letter to two administrators at Cornell, University President Hunter Rawlings III and Ronnie Coffman, the Director of Research and Associate Dean of the College of Agriculture. The CWMI researchers and several observers supporting the CWMI paper suspected that involving the two Cornell administrators reflected Perciasepe’s attempt to encourage these university administrators to assist with silencing the CWMI researchers.41 Brian Martin, a professor of social sciences at the University of Wollongong (Australia) who studied political dissent and scientific suppression, commented that:

If someone disagrees with a scientist’s research conclusion or public statements, an accepted method of response is to criticize the argument, for example, by sending a letter to the scientist or to a journal. By contrast, sending a letter of complaint to the scientist’s boss or funding body, attacking the scientist’s credibility or right to speak out, would be seen by many as an attempt to apply pressure on the scientist rather than address the issues under dispute.42

Similarly, other observers believed that Perciasepe’s actions sent an implicit message that the CWMI researchers harmed Cornell University’s reputation through their allegedly poor research and that the institution’s future federal grant awards might be jeopardized if the institution continued to disseminate this research. While Perciasepe never revealed his reason for sending

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41 Michael Vatalaro of the Environment News Service, an international news wire service, characterized Perciasepe’s letter as an effort to “denounc[e] the science in the Cornell paper to both the New York State Department of Environmental Conservation and the President of Cornell University” (Vatalaro, 2000, paragraph 16).


the two administrators a copy of those letters, many observers interpreted the action as an attempt to silence the CWMI scientists on matters about the 503 rules.43

Perciasepe’s letter represented a symbolic government challenge rather than one that actually yielded significant negative consequences for the CWMI researchers. The Cornell administrators and federal agency officials appear to have taken no action in response to Perciasepe’s July 1997 letter. The CWMI scientists continued their work. In August 1997, the CWMI scientists released their second paper, Case for Caution. Rather than suppressing the second paper, Cornell issued a press release. The press release headline read, “EPA rules for land application of sewage sludge are too lax.” In addition, the university highlighted the CWMI scientists’ research in the Cornell Chronicle, an internal news service for the university community.44 Also, in September 1997, Ellen Harrison sent Robert Perciasepe a copy of the Case for Caution paper.45 In Harrison’s cover letter, she acknowledged that the EPA Office of Water did “not agree with many of [the CWMI researchers’] arguments and conclusions,” but she offered to engage in “continued dialogue” with his staff – if that would help each side understand the other’s position and scientific questions.46

On October 31, 1997, Perciasepe sent Harrison a letter challenging CWMI second paper, Case for Caution.47 Perciasepe responded in a manner that suggested he was more interested in closing the discussion than engaging in an open dialogue. As in his July 1997 letter, Perciasepe

46 Harrison, Sept. 19, 1997, paragraph 2.
47 Perciasepe, R. (1997, Oct. 31), [Letter from Robert Perciasepe, Assistant Administrator of the EPA, to Ellen Z. Harrison, Director of CWMI].
repeated his actions to challenge the new CWMI paper. First, he criticized the CWMI research as a flawed study with many misstatements. Among his allegations, Perciasepe wrote that the CWMI paper erroneously asserted that the 503 rules failed to consider “safety and uncertainty factors.”48 He also believed that the paper inappropriately “implie[d] that a 1 in 10,000 cancer risk [was] dangerous, that soil ingestion factors considered in the risk assessment may be too low, and that plant uptake coefficients and dietary evaluations” were misstated.49 According to Perciasepe, the CWMI scientists presented their data and recommendations without sufficient scientific support. Second, Perciasepe suggested that the CWMI scientists had examined the wrong issue. He considered the CWMI scientists’ research insignificant because he concluded the science already determined “low risks associated with land application of biosolids.”50 Perciasepe advised that the CWMI drop this inquiry, and instead, explore another related, but important, issue. Specifically, he cited a lack of research about “the human health and environmental impacts of animal manures and other agriculturally recyclable by-products,” not sewage sludge.51 Besides redirecting the CWMI scientists to a different research topic, Perciasepe advised the CWMI scientists to “undertake a more holistic problem-solving approach in the management and utilization of nutrients and residuals in agriculture.”52 His comment appeared as a subtle criticism that the CWMI research failed to address an issue in an appropriate systemic manner – a necessary approach for science policy decisions. Finally, just as with his July 1997 letter, Perciasepe also notified the Cornell University President, Hunter Rawlings III, and Ronnie Coffman, the Director of Research and Associate Dean of Cornell’s College of

Agriculture. That is, Perciasepe once again alerted actors who could influence or exercise organizational authority over the CWMI scientists.

*Act 1 Summary*

Act 1 presented certain government actors (i.e., various EPA and USDA administrators and scientists who had oversight of the 503 rules and/or helped craft the original regulations) taking steps to close the conversation about the CWMI scientists’ concerns about the 503 rules. I presented the actors’ use of three tactical steps to carry out this strategy. In Scene 1, they attempted to *discredit* the academic scientists’ research and *distract* observers by leading them to another science policy matter. Then, in Scene 2, they repeated the first two steps and added a third. In that third step, a government official alerted actors who had *influence* over the CWMI scientists that the government agencies disapproved of the CWMI scientists’ research; this, I suggest, was an attempt to silence the CWMI scientists through other actors. While none of the government’s tactics actually stopped the CWMI scientists from disseminating their research, the challengers still employed pressures and tactics intending to quiet the debate.

*Act 2: Third-Party Reviewers*

In Act 2, I introduce two new actors to this play. These actors are organizations, one public and one private. As we will learn, they have no official responsibility in overseeing or writing the EPA’s 503 rules challenged by Cornell Waste Management Institute (“CWMI”) research. By some accounts, they appear as neutral parties who can comment on the CWMI research as independent (i.e., disinterested) parties.
Scene 1:

In November 1997, the New York State Department of Environmental Conservation ("NYSDEC") issued a technical report criticizing the Case for Caution paper. A technical report usually evaluates the scientific assumptions, methods (including data sources), analyses, and conclusions. However, rather than conducting an independent review, NYSDEC’s technical report largely reiterated statements made earlier by several federal administrators and scientists, such as Rufus Chaney, Robert Perciasepe, Alan Rubin, and John Walker. These individuals had oversight of the 503 sludge rules and/or helped write the rules back in 1993. NYSDEC justified why it contacted both the EPA and USDA for research information and adopted much of what the federal agencies had already stated. According to the NYSDEC, because it “did not participate in the risk assessment process,” its responses were “based on information from [the EPA and USDA] concerning the risk assessment.”

According to several observers who supported the CWMI scientists, the NYSDEC’s use of these federal actors to argue against the CWMI research did not actually represent an external review of the sludge policies. Caroline Snyder, professor emerita of science, technology, and society at the Rochester Institute of Technology and vocal opponent of sludge application, painted a slightly different picture. In an article that she published in the International Journal of Occupational and Environmental Health, a peer-reviewed journal, she described the New York state agency as having “worked closely with Alan Rubin, John Walker, EPA’s Assistant Administrator, and Rufus Chaney of USDA, on a response to the Cornell paper.”

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Reflecting back on these close working relationships, Ellen Harrison, the lead author of the challenged CWMI study, even referred to these government tactics as simply ignoring the federal actors’ biases.\textsuperscript{56} David Snyder and Fredrick Kunkle of the \textit{Washington Post} quoted Harrison explaining her observation. According to Harrison, “\{w\}hen a regulatory agency becomes a promoter of something, it makes it difficult for them to be objective.”\textsuperscript{57} Nonetheless, several trade organizations, such as the Mid-Atlantic Biosolids Association and New England Biosolids and Residuals, viewed the report as another data source with a seemingly independent scientific review by these state regulators. Yet, for the CWMI scientists and their supporters, the NYSDEC’s report simply reiterated the federal agencies’ biases and served as another document contesting the CWMI research. Viewed another way, the report appeared as a third-party review for others to cite as evidence that the CWMI scientists disseminated flawed research.

\textit{Scene 2:}

Leaping forward, Scene 2 took place primarily between 2001 and 2002. In 2001, Synagro Technologies, Inc., the largest sewage recycler in the United States, hired CPF Associates, Inc. (“CPF”), a science research and consulting firm with a specialty in environmental science. Synagro commissioned CPF to review the CWMI paper, \textit{Case for Caution}. Based on that review, CPF identified two “significant deficiencies.”\textsuperscript{58} First, CPF raised questions about the CWMI’s efforts to overcome research bias. Illustrating possible

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\textsuperscript{56} Ellen Harrison also mentioned in several other settings, including her congressional testimony and several news reports, that the EPA had biases. In 1992, the EPA hired the Water Environment Federation (WEF) to lead a campaign to promote the “beneficial use of biosolids,” with the goal of making the matter noncontroversial by the year 2000. In 2001, the EPA renewed the WEF contract for several more years, so the campaign continued. See also, 2001: Environmental Protection Agency (2001). Cooperative Agreement CR-820725-01-1 with the Water Environment Federation; 1997: Water Environment Federation (1997). \textit{National outlook: State beneficial use of biosolids activities}. Washington, D.C: Author.


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problems with research bias, CPF asserted that the CWMI paper (a) had not undergone peer review, (b) had relied on selected scientific literature (or sections of papers) that favored the CWMI arguments and appeared to have omitted those that presented unfavorable data, and (c) had failed to follow certain scientific protocols, such as presenting calculations, data, and source of research funding. According to CPF, the alleged evidence of research bias violated “generally accepted standards for publication of scientific research.”

For the second significant deficiency, CPF alleged that “most of the scientific conclusions [in the CWMI paper] … [were] erroneous and/or misleading.” CPF claimed that the CWMI scientists made errors by improperly comparing metal concentrations in different settings, mistakenly referencing regulatory standards or criteria, and incorrectly describing environmental risk assessments. Given these assertions of “scientific errors and disregard for standard scientific principles,” the report concluded that “the Cornell document is fatally flawed.”

In light of the negative review, it was not surprising that Synagro approved the release of the CPF findings. In February 2002, CPF issued its report and presented its findings at the annual meeting for the New York Water Environment Association, a wastewater industry-friendly environment. The CWMI researchers and other observers, such as the National Sludge Alliance, questioned the objectivity of CPF. Although CPF considered itself an independent researcher and disclosed its connection with Synagro, not everyone perceived the arrangement as one without pressures or influences over its evaluation of the CWMI study. Ironically, the CPF review team even noted in its report that disclosure of funding sources alerts the reader to

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59 Chrostowski, Foster, & Preziosi, 2002, p. iii.
60 Chrostowski, Foster, & Preziosi, 2002, p. iii.
possible influences from the funding source (this, in connection with questioning the funding sources of the CWMI study). Indeed, for the CWMI authors and some of their supporters, the CPF contract with Synagro presented a possible conflict. In their response, the CWMI authors, for their part, insinuated the possibility that Synagro, as the CPF funding source, might have presented a skewed perspective. They emphasized that “[s]tudies have shown that research findings are strongly influenced by the source of funding for that work.”

Although the level of Synagro’s influence (other than paying for the report) was not clear from the available data, what was clear from this scene was that the CPF report served as another challenge against the CWMI scientists’ research. Several critics viewed the report as a third-party scientific evaluation intended to discredit the CWMI research.

*Act 2 Summary & Conclusion*

Act 2 discusses two technical reports used to challenge the CWMI research, one from a state environmental agency and another from a private environmental-science consulting firm. Both reports criticized the CWMI research for lacking scientific support, relying on flawed assumptions, and following allegedly incorrect protocols. Yet, as several CWMI supporters pointed out, these reports might have been influenced by actors with an interest in the outcome. The state report largely repeated the statements from the federal agencies that adopted and oversaw the controversial regulations, and Synagro Technologies, Inc., a significant corporate entity in the sludge business, hired the private environmental-science consulting firm to write its report. Regardless of whether the influences on NYSDEC and CPF actually took place, these

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63 Harrison, McBride, & Bouldin, 2002; See also, Snyder, 2005.
64 Harrison, McBride, & Bouldin, 2002, paragraph 3.
technical reports still served as additional challenges to the CWMI research. Some critics of the CWMI research, such as sludge trade organizations, sludge companies, and municipalities, viewed these two reports as additional evidence to overcome the CWMI research, while supporters of the CWMI study questioned whether these technical reports actually represented impartial scientific reviews. Put simply, these reports became the policy tools to discount the CWMI research.

Summary of Challenges

The circumstances and events surrounding the challenges highlighted a common theme: The challengers intended observers to discount the CWMI scientists’ concerns about the 503 rules.

In Act 1, federal administrators and scientists, who oversaw and/or wrote the 503 rules, employed a three-step approach to challenge the CWMI’s government-sponsored research. These actors (a) engaged in a collective campaign to discredit the CWMI research, (b) proposed an alternative research topic to distract observers from the sludge issue, and (c) alerted other individuals, who at the time had influence over the CWMI scientists, about the federal agencies’ concerns with the CWMI research, possibly hoping to effect the silencing of the researchers.

In Act 2, two organizations created seemingly independent reports, which served as policy tools to essentially discount CWMI’s research. As noted earlier, the NYSDEC and CPF, an environmental consulting hired by Synagro, issued separate technical reports to challenge the CWMI research. Even though these reports essentially restated many of the same messages as the federal agency memos, critics of the CWMI research identified these reports as additional, independent studies that concluded the CWMI study contained serious scientific flaws. Several sludge trade organizations used these documents to discount the CWMI research.
The CWMI scientists and their supporters did not sit idly during the challenges from the federal actors and the third-party scientific reviewers. As I discuss below, the CWMI scientists and their supporters responded to each significant challenge in hopes of defending the research. The CWMI scientists also took steps so they could exert control over their government-sponsored research.

**Responses to the Challenges**

Responding to the challenges, the CWMI scientists and their supporters employed three primary strategies to defend the research and exert control over it. Following my conceptual framework (Chapter 3), in this section, I show that these three strategies relied heavily on (a) internal characteristics, (b) external factors, and (c) both internal and external factors of the academic profession. Further, I demonstrate how these three types of strategies made it possible for the CWMI scientists to exert control over their governmentally sponsored research.

**Internal Characteristics**

One strategy relied on internal characteristics of the academic profession. The CWMI scientists used academic traditions as behavioral guides to address the government challenges. The CWMI scientists and their supporters rolled out this strategy in two parts. First, the CWMI scientists articulated certain academic traditions as guidelines for practice or rules to follow. Second, the CWMI scientists, their supporters, and other observers appear to have adopted the academic traditions as behavioral “guides” or “rules” to address the contested research. As I demonstrate below, these guides afforded the CWMI scientists control over their research by shifting the rules of engagement from government policies and statements that challenged the CWMI research to certain academic traditions that favored research independence, a continuing dialogue about the CWMI’s concerns, and academic scientists’ participation in science policy.
Articulating Academic Traditions as Behavioral Guides

The CWMI scientists declared that certain academic traditions should guide the actors’ behaviors in addressing this controversy. One of these occasions took place in late summer 1997 (i.e., immediately following the release of the Case for Caution). In a letter to Robert Perciasepe of the EPA, the CWMI scientists explicitly requested an open dialogue between them and the government agencies standing behind the 503 rules. On behalf of the CWMI scientists, Ellen Harrison offered to engage in “future discussions” with the EPA to address differences in their perspectives, particularly in terms of their research assumptions and methodology.\textsuperscript{66} For the CWMI scientists, an open dialogue reflected the exercise of two internal characteristics of the academic profession as means to leverage control over the situation.

First, the CWMI scientists intended the dialogues as equivalents to academic forums in which they could exercise their professional knowledge and skills to guide the discussion. On behalf of the CWMI scientists, Harrison wrote Robert Perciasepe that: “We believe that open debate is useful and hope that the spirit of our future interactions will be based on that premise.”\textsuperscript{67} Harrison also offered a suggestion on what to investigate. She suggested that the CWMI and the federal agencies start by examining their differences, especially in terms of the “appropriate uptake coefficients, leaching and groundwater impacts, [and] dietary assumptions.”\textsuperscript{68} Some observers and the CWMI scientists believed that clarifying these terms and the study methodology would allow the CWMI scientists an opportunity to publicly justify their work as well-reasoned and appropriate steps.\textsuperscript{69}

\textsuperscript{67} Harrison, Sept. 19, 1997, paragraph 8.
\textsuperscript{68} Harrison, Sept. 19, 1997, paragraph 8.
\textsuperscript{69} See, e.g., Mar. 22, 2000: EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106\textsuperscript{th} Cong. at 179 (2000) (Testimony of Ellen Harrison) (“we have been unsuccessful in working with EPA to address the scientific issues”); Apr. 6, 2000: Segelken, R. (2000, Apr. 6), Utilize academic
Second, the CWMI scientists adopted an academic view that as academic scientists, they would serve society through their research. They outlined a division of labor in which academic scientists and policymakers work as complementary actors supporting each other in the solution, rather than competing over who gets jurisdiction over the matter. As noted above in the Interactions Surrounding the Challenges section, Robert Perciasepe and other government challengers criticized the CWMI scientists for trying to set regulatory standards, a responsibility that fell within the policymakers’ professional jurisdiction. However, academic scientists also have a responsibility to expand the scientific knowledge about matters such as chemical and metal reactions and impacts to human health.

Relying on the academic tradition of service, the CWMI scientists and their supporters tried to redefine the roles of academic scientists in science policy. While they discussed the divisions of labor concept in terms of how each actor conducted his or her tasks in distinct ways and often with different perspectives, they also asserted that each actor is charged with the social responsibility to propose solutions to the science policy issue (i.e., whether the 503 rules adequately protected individuals from adverse health effects when exposed to the sludge).


70 According to the governmental challengers, the CWMI scientists overstepped their boundaries by promoting policy advocacy, not science. This claim rested in large part on the governmental challengers’ belief that the CWMI scientists argued for regulatory changes allegedly based on nearly nonexistent probabilities that harm would arise from sewage sludge application.

71 For example, assistant professor of agronomy Richard Stehouwer (1999), in Land application of sewage sludge in Pennsylvania: What is sewage sludge and what can be done with it? (University Park, PA: Pennsylvania State University), suggested that the CWMI scientists contributed to the education of landowners and policymakers through their service in sludge research. Similarly, Professors Krogmann, Gibson, and Chess (2001) of Rutgers University conducted a study that concluded that university extension services significantly contributed to the understanding of surveyed farmers regarding the science sludge policy as well as the science of sludge. Krogmann, U., Gibson, G., & Chess, C. (2001). Land application of sewage sludge: Perceptions of New Jersey vegetable farmers. Waste Management & Research, 19(2), 115-125.
division of labor serve as the working conditions for the academic scientists and the policymakers.\textsuperscript{72} Accordingly, the CWMI scientists and their supporters advocated for a \textit{division of labor} in which both groups would work together. Further, as the CWMI materials indicated and Ellen Harrison stated on numerous occasions, the CWMI service commitment was particularly important for Cornell University as a land grant university.\textsuperscript{73} The CWMI scientists felt an obligation to inform policymakers when a policy presented actual or potential harms to New York citizens.

To summarize, in this section, we observed how the CWMI scientists established several academic traditions as behavioral guides (or academic rules) for addressing the contested research. In the next section, I present the actions that followed these academic rules.

\textit{Using Academic Rules}

In this section, the application of academic traditions as behavioral guides in addressing the controversial research becomes more noticeable. Many observers of the controversy, including federal scientists, academic scientists, citizens who complained about sludge application, and wastewater treatment staff, followed these academic norms to guide their actions or influence their thoughts when responding to the situation. For example, when Perciasepe of the EPA challenged the CWMI scientists and notified two Cornell administrators (i.e., the

\textsuperscript{72} Also during the question period of the March 2000 congressional hearing, Ellen Harrison pointed out a problem when the EPA failed to divide the science and oversight beyond the EPA’s Office of Water, the office responsible for both the sludge science and oversight. Describing the problem, she noted: “[T]he fact that the regulatory function, the risk assessment and science function and the funding of wastewater treatment plants … are all rolled up in one place within EPA is a problem” (Harrison, 2000, p. 250). According to Harrison, a solution is division of labor. She explained, “I believe the risk assessment and science needs to be separated from those who are developing the policies and risk management choices so that it can remain scientific and objective” (Harrison, 2000, p. 250). Testimony of Ellen Harrison, Mar. 22, 2000. EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106\textsuperscript{th} Cong. at 250 (2000) (Testimony of Ellen Harrison).

\textsuperscript{73} See, e.g., EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106\textsuperscript{th} Cong. at 179-181 (2000) (Testimony of Ellen Harrison); Segelken, R. (2000, Apr. 6), Utilize academic researchers, don’t discredit them. \textit{Cornell Chronicle}. Retrieved from \url{http://www.news.cornell.edu/chronicle/00/4.6.00/Harrison-EPA.html}
the records at Cornell indicate no administrative action was taken to investigate or silence the CWMI scientists. On the contrary, several reports show that Cornell administrators respected the CWMI scientists’ practice of research independence from sources that might have undue influence over their results.75

Under Freidson’s concept of ideology, a professional’s independence from external sources reflects an internal characteristic of a profession to exercise control over its work. Here, the CWMI supporters generally upheld the academic tradition of research independence from undue influence and afforded the CWMI scientists their ability to continue their research and to disseminate their work products. In March 2000, Ellen Harrison even testified at a congressional hearing that “we were confident that academic freedom and integrity would protect us from any recriminations at the University, which they did.”76

While academics, such as the CWMI scientists and the Cornell administrators, respected the academic traditions, the federal actors who challenged the CWMI research rarely did. Despite their non-adherence to academic norms, some observers still perceived academic traditions as the designated rules of engagement for these federal actors to use. For example, when several government actors (e.g., Robert Perciasepe of the EPA, David Sterman of the NYSDEC) failed to comply with the academic norms, some of these observers raised questions about the federal actors’ intentions.77 They wondered whether the federal actors who challenged

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76 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 180 (2000) (Testimony of Ellen Harrison).
77 For example, in 1999, Dr. Greg Evanylo of the Virginia Polytechnic Institute and State University presented both sides of the debate, but he indicated that the CWMI study aligned with the scientific knowledge in the field for
the CWMI research acted in the best interests of the public. According to Sheila Cherry, reporter for a politically conservative newspaper, citizens exposed to the sludge believed that “the EPA has gotten behind a sludge program allegedly to protect the environment and, when people began to sicken and die, became as intransigent as any other accused polluter.78

By contrast, some observers, particularly academic scientists, perceived the CWMI scientists’ compliance with the academic rules as evidence of their good faith efforts to move along the conversation in a structured manner.79 These events represented the CWMI scientists’ efforts to advance the academic profession’s ideology of an open exchange along with their ability to exercise their professional knowledge and skills. Furthermore, asserting the academic traditions of open dialogue, the CWMI scientists tried to reverse the effect of the government actors’ attempts to close off debate.80 The CWMI scientists defended and disseminated their controversial research even as they kept working. For example, using traditional academic

that time period (i.e., the “best professional judgment”). Nevertheless, he indicated that a re-evaluation of the 503 rules was required as the EPA had not conducted a review since before 1993. Evanylo, G. K. (1999). Agricultural land application of Biosolids in Virginia: Risks and concerns (Report 452-304). Blacksburg, VA: Virginia Cooperative Extension, Virginia Polytechnic Institute and State University.

In addition, two academic scientists at the College of William & Mary’s Virginia Institute of Marine Science, Robert Hale and Mark La Guardia, conducted a study on chemicals and metals present in sewage sludge. Supporting the CWMI research, they found high concentrations of harmful materials in the sludge and suggested that the U.S. needed to adopt tougher standards, just as the CWMI scientists had recommended. In their article, Hale and La Guardia (1999) reported that “[n]o U.S. regulations exist limiting organic pollutant burdens. The diversity of organics in sludge is tremendous. While some are easily degraded, others are persistent” (p. 373). Nonetheless, they pointed out, the EPA had not acted on the CWMI recommendations even after the CWMI scientists pointed out that the U.S. has very lenient standards when compared with many European countries.


79 Snyder (2005) explained how sludge critics and academic scientists interpreted the academic traditions of peer review and open access as reflecting the good faith efforts of the CWMI scientists to provide sound scientific research.


80 For example, Robert Perciasepe of the EPA sent letters that conveyed that the sludge science was clear and not open for further discussion.

means to report their research findings, in 1999, the CWMI researchers published their third paper examining the scientific effects of sewage sludge and criticizing the 503 rules. That paper, which was essentially identical to the *Case for Caution*, appeared in the *International Journal of Environment and Pollution*, a peer-reviewed journal. Publication in a peer-reviewed journal was a further effort to gain academic recognition. It also helped the CWMI scientists gain more scientific credibility among academic scientists and with some state and local policymakers. Specifically, as states and municipalities debated changing their policies and/or practices regarding the use of land-applied sewage sludge, several policymakers and opponents to sewage sludge use cited the peer-reviewed publication as an expert analysis.

The CWMI researchers also used their affiliation with the agricultural extension services at Cornell to disseminate their work. As a land grant university, Cornell’s mission includes a public service commitment to the agricultural community within New York State. The CWMI scientists reported that they had a social responsibility to assist “farmers, policymakers and citizens by performing relevant research and publication of results in peer-review journals, as

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82 For example, according to John Lucas, a reporter for the *Evansville Courier & Press* (Evansville, IN), Kentucky’s Hopkins County developed an ordinance that “was based on studies by Cornell University” that recommended banning sludge application to certain lands.


83 For example, the congressional record referred to Ellen Harrison as an academic scientist, who engaged in peer-reviewed research to evaluate the 503 rules. The implication was that her testimony reflected her compliance with an academic tradition of independent review.

EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 3 (2000) (Statement of Representative James Sensenbrenner, Jr., committee chair); EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 4 (2000) (statement of Representative Sherwood Boehlert, committee member.)

Also, CWMI scientist Ellen Harrison and then Cornell law student Malaika Eaton co-authored a paper that explained the legal policies surrounding local regulations for sludge application. The authors presented various types of municipal control measures such as sludge restrictions in terms of the amount, quality, and source.

well as through outreach” about agricultural issues such as the possible effects of sewage sludge on farm lands. However, the CWMI scientists viewed their services to the university extension as more than an exercise of their professional expertise. They also described their extension services work much like a *training program* for the agricultural community and policymakers.

As a training program, the CWMI delivered professional-development seminars, disseminated educational booklets and papers, and contributed new knowledge to farmers and policymakers about their respective practice areas of farming and policymaking. Uta Krogmann, Virginia Gibson, and Caron Chess – three academic scientists at Rutgers University – also viewed the strategy of training end users about the scientific implications of sludge as a useful way for farmers to learn about the benefits and drawbacks of the substance. They investigated New Jersey vegetable farmers’ understanding about sludge, and among the authors’ findings, they identified university extension services as significant information sources for these farmers.

In sum, many supporters of the CWMI research, along with other observers, came to believe that the academic traditions of open debate were the best way to address the research controversy. In other words, this strategy shifted the rules of engagement *from* government policies and statements that challenged the CWMI research *to* certain academic traditions that favored research independence, a continuing dialogue about the CWMI’s concerns, and academic scientists’ participation in science policy.

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84 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 180 (2000) (Testimony of and supporting documentation from Ellen Harrison).
External Factors

The CWMI scientists and their supporters also relied heavily on external factors influencing the academic profession, such as individuals and venues that fall outside of the Academy. In particular, the CWMI scientists and their supporters relied on political forums and other mechanisms that influence the public or aid them in understanding the science. These actions helped the CWMI scientists gain control over their governmentally funded research.

Raising Another Academic Voice

Even though the EPA and USDA officials tried to close the conversation about the CWMI research, the CWMI scientists managed to gain access to political forums, such as congressional, state, and county hearings, as a means to have a voice in the sludge debate. More significantly, the CWMI scientists used these forums (a) to inform the public about the challengers’ tactics of discrediting their sludge research and (b) to disseminate and defend their work. Illustrating this strategy, I draw attention to a U.S. House of Representatives Committee on Science hearing, the EPA’s Sludge Rule: Closed Minds or Open Debate? The House hearing investigated citizen reports that the EPA tried to silence critics of the 503 rules. According to Congressman James Sensenbrenner, Jr. (Wisconsin, Republican), the chair of the House Committee on Science, the purpose of the hearing was to determine “whether the EPA in its

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87 Throughout the 1990s, sludge opponents publicly questioned whether the 503 rules sufficiently protected the public from pathogens and other materials contained in sludges. In the late 1990s, several of these sludge critics allegedly received “threatening and harassing” communications from Dr. Alan Rubin, a sludge research scientist at the EPA (Mann, 1999, p. 26). One of the critics, Jane Beswick, reported receiving more than 10 threatening and harassing communications from Dr. Rubin. Beswick was a farmer and grass-roots organizer opposing sludge use. In March 2000, she testified that Rubin threatened retaliation if she did not stop her anti-sludge campaign. According to Beswick, Rubin confronted her and asked: “[W]hen was the last time either a federal or state inspector was out at your farm to inspect your dairy manure lagoon for integrity? I do not mean this as a threat” (Beswick, 2000, p. 223). However, Beswick reportedly interpreted the message and others like it as Rubin’s efforts to silence her. After receiving several complaints about Rubin and other EPA officials trying to stop anti-sludge groups, the House Committee on Science scheduled this hearing to investigate the matter.


EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 223 (2000) (testimony of Jane Beswick, farmer and coordinator of the Coalition for Sludge Education).
management of the Part 503 Sludge Rule … [failed] to foster sound science with an open exchange of ideas.”

In the fall of 1999, during the preparatory stages of the hearing, the CWMI scientists worked with a political ally, Congressman Sherwood Boehlert (New York, Republican), to gain access to this political venue. As Congressman Boehlert stated at the House hearing, he worked with CWMI scientist Ellen Harrison to get her a witness slot. Boehlert felt strongly that the academic scientists’ perspective needed to be heard, specifically so Harrison, on behalf of the CWMI scientists, could “air their legitimate concerns about how [the] EPA has responded to criticism of its sludge rule.”

At the hearing, Harrison provided examples of how the EPA had attempted to discredit the CWMI research. For example, she testified that the EPA “mischaracterized our research and … suggested that we used methods that [were] not appropriate to answering the scientific questions we [sought] to address. Some of [the EPA’s] assertions about our work (for example that we used metal salts and not sludges to study leaching in a greenhouse study) [were] simply untrue.” According to Harrison, the EPA’s challenging of the CWMI’s research methods “was particularly surprising” because the EPA office, which had been the party most critical of the CWMI research, had a representative on this research project’s advisory board. Harrison noted that the representative even “knew the research methods being used.” In another example, Harrison pointed out that the government challengers characterized the CWMI research as

88 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 3 (2000). Statement of Representative James Sensenbrenner, Jr., committee chair.
89 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 4 (2000). Statement of Representative Sherwood Boehlert, committee member.
90 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. 4 (2000) (Statement of Representative Sherwood Boehlert, committee member.).
91 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. at 179 (2000) (testimony of Ellen Harrison).
92 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. at 179 (2000) (testimony of Ellen Harrison).
overstating the sewage sludge impacts to groundwater. Yet, according to Harrison, “in fact and ironically, EPA and New York State’s own cleanup guidelines for hazardous waste sites suggest[ed] that groundwater concerns may require significantly more stringent standards for acceptable soil levels of contaminants than those allowed under the sludge rules, an inconsistency brought about by the fact that different parts of the agency did those different risk assessments and wrote those rules.”93

Harrison’s testimony afforded the CWMI scientists a voice in a major public forum. At this hearing, she contested several of the EPA’s claims and demonstrated how the EPA had attempted to discredit the CWMI study.94 Further, she presented parts of her research for the public to hear. In some cases, certain members of the public reasserted these arguments to raise concerns about the 503 rules and to battle municipalities that intended to apply sludge in the complaining parties’ cities.95

*Academic Research Affirmed and Public Rallying*

Around the same time frame as the events surrounding the House hearing (i.e., late 1990s and early 2000s), several environmental groups issued reports disputing the safety of sewage sludge.96 Supporting their claim, the groups cited to the CWMI research as one of the primary academic studies presenting an independent examination of the 503 rules. Despite these

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93 EPA’s Sludge Rule: Closed Minds or Open Debate?: Hearing before the House Committee on Science, 106th Cong. at 179 (2000) (testimony of Ellen Harrison).
95 Snyder and Kunkle (2001) presented several examples in which citizens used the CWMI research to help contest local application of sludge and tried to pass ordinance to ban such application. Furthermore, Harrison and Eaton (2001) published an article that surveyed strategies that citizens used to justify municipal bans.
assertions, the EPA’s Office of Water continued to refute claims that the 503 rules were not sufficient protections; in addition, the office reiterated that the CWMI research was scientifically flawed. Then, coincidentally, the EPA and the U.S. Centers for Disease Control and Prevention started receiving a series of reports that staff at wastewater treatment facilities and citizens who lived nearby recently applied sludge had encountered a variety of suspicious health reactions, including headaches, skin ulcers, nausea, vomiting, nosebleeds, and respiratory problems.97

Those health incident reports, which took place from 1998 to 2000, led to two investigations: one by the EPA’s Office of the Inspector General (OIG) and another by the National Institute for Occupational Safety and Health (NIOSH).98

In 2000, the EPA’s Office of the Inspector General (OIG) issued an internal investigation questioning the sufficiency of the EPA’s oversight measures to protect the public from harmful sewage sludge.99 The EPA OIG found that sewage sludge may actually contain “toxic pollutants and disease-causing organisms.”100 The EPA reiterated that the “EPA cannot assure the public that current [sewage sludge] land application practices are protective of human health and the environment.”101 Echoing the exact issue that the CWMI scientists had raised, the EPA OIG questioned whether the EPA’s Office of Water, which oversaw the sludge policy, maintained

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101 EPA, 2000, p. ii.
sufficient oversight mechanisms to protect the public from undetected, yet harmful, sewage sludge.\(^\text{102}\)

The NIOSH report also echoed significant concerns about the 503 rules. That investigation began after the CDC had received numerous complaints of wastewater treatment worker illnesses. In August 2000, after having conducted inspections of several wastewater treatment facilities, NIOSH determined that additional protective measures for individuals working in waste treatment plants to process Class B sludge, the more potent sludge for commercial application, were essential.\(^\text{103}\)

In light of the 2000 OIG and 2000 NIOSH reports, the EPA faced increasing public pressure to investigate concerns about the adequacy of the 503 rules. In September 2000, the EPA issued a task order to the National Research Council (NRC).\(^\text{104}\) In late 2000, the NRC solicited feedback for recommendations of scientists to add on its panel. CWMI and its researchers overwhelmingly dominated the list of recommendations.\(^\text{105}\) Of the ten letters recommending an individual or institution with the appropriate expertise, six of them identified CWMI or specifically one of the *Case for Caution* researchers. Five of the letters specifically mentioned Ellen Harrison. Three also identified Dr. David Lewis, a researcher at the University

\(^{102}\) See EPA, 2000. *N.B.:* The second investigation reiterated the same concerns.


of Georgia and former EPA scientist, who also publicly supported the CWMI research. Beyond Harrison and Lewis, no other individual or institution received more than one recommendation.

Based on comments from the public, the justifications for supporting Harrison’s recommendation emphasized her expertise, her ability to “represent the public interest,” and her reputation as a “qualified independent scientist.” These descriptors captured the impact of the CWMI scientists’ efforts to engage the public and win broad recognition as experts in the field. In early 2001, the NRC appointed Ellen Harrison to the panel as one of the 16 scientists, who included other representatives from academia as well as industry and state agency sludge administrators.

The EPA limited the scope of the NRC examination to just a few issues. Accordingly, the panel had a very specific focus, although the agency acknowledged that “some readers” might have expected the report “to cover all aspects of biosolids and determine whether EPA should continue to promote its use.” Even with the limited scope of review, the NRC generally concluded that the 503 risk-assessment standards required a significant update to account for newer science. For instance, the NRC team reported that the EPA had not conducted a “substantial reassessment … to determine whether the chemical or pathogen standards promulgated in 1993 [were] supported by current scientific data and risk-assessment methods.”

107 Daly, T. (2001, Feb. 15), [Letter from Tina Daly of the Pennsylvania Environmental Network to Susan Martel of the National Academy of Science] Comments on Provisional Appointments to the Committee, paragraph 15.
108 The EPA identified three specific areas to evaluate: (1) the “risk-assessment methods and data used to establish concentration limits for chemical pollutants in sludge” to determine appropriateness of the approach, (2) “the current standards for pathogen elimination in sludge and their adequacy for protecting public health,” and (3) “whether approaches for conducting pathogen risk-assessment can be integrated with those for chemical risk-assessment.”

110 NRC, 2002, p. 5.
These statements generally supported the CWMI scientists’ findings and recommendations that the 503 rules were outdated and did not assess the effects of certain metals and pathogens. For example, Sections 1.3, 7.11, and 7.12 of the CWMI scientists’ 1999 paper outlined several related arguments about the inadequacy of the containment standards and the assessment of pathogen risks, which the NRC also mentioned.\textsuperscript{111} Further, the NRC observed that the EPA did not have “an adequate program to ensure compliance with the biosolids regulations and has not documented the effectiveness of its prescribed management practices.”\textsuperscript{112} Likewise, in Section 7.13 of the 1999 paper, the CWMI scientists dedicated a section to arguing that the EPA maintained “inadequate enforcement and oversight.”\textsuperscript{113}

While the NRC report raised many of the same concerns that the CWMI scientists had, the NRC team did not advocate standards as strict as what the CWMI scientists had recommended. Nevertheless, placing Harrison on the NRC panel gave the CWMI scientists an opportunity to assert their findings and recommendations as well as openly defend their science before the other 15 experts. Indeed, the common thread throughout this overarching strategy has been the CWMI scientists’ actions to leverage support from actors external to the academic profession. These external actors, particularly a political ally who placed Harrison on a congressional witness list and public supporters who recommended Harrison to the NRC panel, made it possible for Harrison to disseminate and defend the CWMI research in two public settings of the national sludge policy debate.

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\textsuperscript{111} Harrison, McBride, & Bouldin, 1999, §§1.3, 7.11, & 7.12.

\textsuperscript{112} NRC, 2002, p. 5.

\textsuperscript{113} Harrison, McBride, & Bouldin, 1999, §7.13.
Both Internal Characteristics & External Factors

There was another strategy. I refer back to events that started in Act 2, Scene 2 when sludge recycler Synagro Technologies hired CPF Associates, an environmental consulting firm, to review and ultimately challenge the *Case for Caution*. Several months after CPF issued its report, the CWMI scientists responded to the CPF Associates review by reversing the challenge. That is, the CWMI scientists took the challenger’s questions or allegedly discrediting evidence and turned them back on the challenger. Below, I explain how this strategy relied on two internal characteristics of the academic profession (i.e., *professional ideology* and *knowledge and skills*) and one external factor (i.e., *public understanding/interpretation*), with the effect of helping the CWMI scientists exert control over their research.

*Changing Places: From the Party Being Challenged to the Role of Challenger*

In the opening section of the CWMI scientists’ response to the CPF report, the academic scientists drew boundaries around the matters they would address in the report, just as the government challengers had done earlier when they contested the CWMI papers. The CWMI scientists justified their actions, stating “a detailed rebuttal” would not “help to further the science or quiet the debate.” Accordingly, the CWMI scientists issued a limited response to the CPF report. The CWMI scientists only addressed matters about which they believed CPF had made inconsistent or erroneous statements.

For example, to weaken the credibility of the CWMI research, CPF had claimed that the CWMI scientists failed to comply with a basic academic norm of disclosing research funding sources. In response, the CWMI scientists disclosed their government sources. Additionally, the

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CWMI scientists acknowledged that “[s]tudies have shown that research findings are strongly influenced by the source of the funding for that work.”116 Characterizing their government sponsors as neutral parties to the research, the CWMI scientists reported that their financial streams had derived from no sources “with a financial stake in their work or from any private sector source.”117 In other words, they demonstrated a professional ideology of research independence. But the CWMI scientists claimed that CPF could not claim research independence under this same standard (i.e., using no private sector funding source that might compromise objectivity). Drawing on public interpretations and understandings, the CWMI scientists emphasized that “readers should have information about the source of funding.”118 Contrasting their financial sources with CPF’s financial source, the CWMI scientists pointed out that CPF Associates had acted “at the request of Synagro Technologies, Inc., a firm whose business includes the land application of sewage sludges.”119 Though the CWMI scientists never explicitly stated that Synagro actually influenced the CPF report, their message did the job of conveying that CPF might have been influenced to meet the interests of its sponsor, a corporate entity with direct financial ties to the sewage sludge industry.

The CWMI scientists took the same approach to address CPF’s challenges about the Case for Caution’s scientific content. CPF had reported scientific errors in the CWMI scientists’ research methods and data treatment.120 Relying on the academic scientists’ professional knowledge and skills, the CWMI scientists offered their rationale for their methods and data treatment. For instance, the CPF Associates report alleged that the CWMI scientists had used the

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118 Harrison, McBride, & Bouldin, 2002, paragraph 3.
120 Chrostowski, Foster, & Preziosi, 2002.
incorrect soil ingestion rate.\textsuperscript{121} The CWMI scientists explained that “calculations of the concentration of contaminants … rely on both concentration of contaminants in the sludge and the amount of sludge applied.”\textsuperscript{122} Another discrepancy concerned the application rate. The CWMI scientists noted that in its report CPF Associates had relied on a measure of three tons/acre.\textsuperscript{123} However, the CWMI scientists indicated that the EPA used a rate of ten tons/acre to determine risk assessments of land application for agricultural purposes.\textsuperscript{124} The CWMI scientists contended that their experience in New York State supported the calculation of “10 tons/acre [as] a reasonable figure” to determine soil concentrations of contaminants.\textsuperscript{125} After explaining the different scientific assumptions that the CWMI scientists and CPF Associates had used, the CWMI scientists reframed the controversy to draw on \textit{public interpretation} of which measurement to rely upon – the one used by CWMI or the one used by CPF. The CWMI scientists suggested that “… an important reason there is disagreement about the US EPA 503 rules is the debate about what constitutes an acceptable risk. What level of crop yield reduction is considered acceptable, for example? How many additional cases of human disease are acceptable?”\textsuperscript{126} Further, they pointed out that certain federal policymakers, industry representatives, and some academic scientists cannot agree on how “to deal with [scientific] uncertainty.”\textsuperscript{127} However, the CWMI scientists reported that they relied on their past experience to construct “a very cautious approach,” better than the one their challengers used.\textsuperscript{128} Thus, these

\textsuperscript{121} Chrostowski, Foster, & Preziosi, 2002, p. 8.
\textsuperscript{122} Harrison, McBride, & Bouldin, 2002, paragraph 9.
\textsuperscript{123} Harrison, McBride, & Bouldin, 2002, paragraph 9; see also Chrostowski, Foster, & Preziosi, 2002, pp. 12-14.
\textsuperscript{124} Harrison, McBride, & Bouldin, 2002, paragraph 9.
\textsuperscript{125} Harrison, McBride, & Bouldin, 2002, paragraph 9.
\textsuperscript{126} Harrison, McBride, & Bouldin, 2002, paragraph 12.
\textsuperscript{127} Harrison, McBride, & Bouldin, 2002, paragraph 13.
\textsuperscript{128} Harrison, McBride, & Bouldin, 2002, paragraph 13.
events once again demonstrated the CWMI scientists’ efforts to turn the tables to raise concerns about CPF Associates’ evaluation of risk and uncertainty.

This strategy offered two significant benefits to the CWMI scientists. First, for some members of the public, the CWMI scientists’ response made it easier to discount the CPF report and consider the findings and recommendations contained in the CWMI paper. For example, in 2002 and 2003, the Center for Progressive Regulation and the Sierra Club both highlighted the value of Ellen Harrison’s expertise and the problems associated with the industry-funded statements.129 In 2005, Caroline Snyder, professor emerita of science, technology, and society at the Rochester Institute of Technology and vocal opponent of sludge application, published a paper in the *International Journal of Occupational and Environmental Health* characterizing CPF as biased and CWMI as the scientific voice of sludge. In that publication, Snyder referred to CPF as “a group of sludge-friendly scientists [hired] to attack” the CWMI’s *Case for Caution*, a paper that she described as a “comprehensive science-based critique of the 503 rule[s].”130

Second, the CWMI scientists’ response directed observers to review the NRC report, which was issued on July 2, 2002 (i.e., approximately six weeks after the CPF report became public). The CWMI scientists even stated, “Rather than rehash [the risk-assessment] arguments here, we urge all interested persons to refer to the National Research Council report on biosolids….”131 Following the NRC report’s publication, members of the media and other

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130 Snyder, 2005, p. 418.
commentators also linked the CWMI papers with the NRC recommendations raising concerns about the adequacy of the 503 rules to protect the public.  

Summary of Responses

Drawing on my theoretical framework, which I summarized in Chapter 3, I examined the data and revealed that the CWMI scientists and their supporters employed three principal strategies. First, relying on the internal characteristics of the academic profession, the CWMI scientists identified academic rules as the basis for determining how actors in the controversy should behave. Symbolically, the adoption of these academic norms reflected a shift in the rules of engagement from government policies to academic traditions as the behavioral guide to address science policy matters.

Second, the CWMI scientists and their supporters relied on external factors influencing the academic profession to give themselves, via Ellen Harrison, a voice in two important areas. First, they worked with a political ally so Harrison could participate in a congressional investigation regarding questions about certain EPA officials’ behaviors toward sludge opponents. Besides disclosing the governmental pressures to silence the CWMI scientists, Harrison’s testimony provided another public venue for the CWMI scientists to defend and disseminate their research.

Finally, for the third strategy, the CWMI scientists reversed the positions of the challenger to the party being challenged. Most notably, when CPF Associates challenged the

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These articles make the connection between the CWMI research and the NRC publications without any reference to the CWMI’s May 2002 response as a source. It is possible that the CWMI’s response did not help the media and other observers make that connection; however, it is also possible that the CWMI’s response directed the public’s attention to an examination of the NRC report rather than paying any more attention to the CWMI-CPF disagreements. Further, it is likely that the CWMI scientists believed that the NRC report would make a more detailed and compelling case that the 503 rules were problematic.
CWMI scientists’ *Case for Caution* paper, the CWMI scientists responded to several of the CPF claims, then turned the question (or a reframed version of the question) back to CPF in a manner that raised concerns about the CPF review.

In short, the CWMI scientists and their supporters employed three strategies – using academic norms as the rules of engagement, gaining a public stage from which to comment on political behavior and the scientific assessment of sludge, and reversing the positions of the challenger and the party being challenged – as ways to defend the CWMI’s government-sponsored research and exert control over it.

**Summary & Conclusion**

In this chapter, I explored the challenges to a government-sponsored research study and the strategies that helped the authors of the challenged study exert control over it and fend off attacks. In this case, the contested study, a state- and federally sponsored research project (i.e., Cornell Waste Management Institute [CWMI] research) questioned the sufficiency of the 503 sludge rules. According to the CWMI scientists, the 503 rules failed to take into account many metals, chemicals, and pathogens that presented potential harm to individuals exposed to the sludge. Consequently, the CWMI scientists proposed a more stringent risk-assessment standard and called for improved government oversight of the sludge application.

Several policymakers at the Environmental Protection Agency (EPA) and U.S. Department of Agriculture (USDA) did not welcome the CWMI research findings or recommendations. Besides disagreeing with the CWMI research, these government actors, as well as other actors, challenged the CWMI study. Reconstructing the interactions among the primary actors in the conflict as a dramatic play, I recounted the circumstances and events surrounding the release of the study. I then traced the multistage set of challenges that followed.
In Act 1, several EPA and USDA administrators and scientists who had oversight and/or helped write the 503 rules challenged the CWMI scientists. These federal actors constructed a collective campaign to discredit the CWMI research, distract observers from the sludge issue by raising another waste management topic to explore, and to alert individuals, who at the time had influence over the CWMI scientists, about the government’s concerns with that study. In Act 2, the challengers shifted to non-federal actors, specifically, the New York State Department of Environmental Conservation (NYSDEC), CPF Associates, and Synagro Technologies. Both the NYSDEC and CPF Associates performed reviews of the CWMI scientists’ *Case for Caution* paper and each issued a technical report. In 1997, the NYSDEC report basically relied on many of the scientific challengers that the EPA and USDA had mentioned earlier. CPF Associates conducted a more thorough review; however, as the CWMI scientists pointed out, Synagro Technologies, the leading U.S. sewage recycler, paid CPF to conduct the study, so observers questioned the report’s credibility. Moreover, the CWMI scientists highlighted several problems with that report (i.e., several omissions, misstatements, and mischaracterizations). Even though one of these reports largely repeated earlier federal documents and another report was commissioned by a sludge company and contained several errors, for some sludge proponents, these two technical reports still served as additional scientific sources to challenge the CWMI research.

The CWMI scientists and their supporters responded to these challenges. Drawing on my theoretical framework (see Chapter 3), I grouped the responses in three categories: (a) actions that relied heavily on the internal characteristics of the academic profession, (b) actions relying on the external factors influencing the academic profession, and (c) actions based on both
internal and external factors. Framing the actions into these three groups, the data revealed that the CWMI scientists and their supporters employed three principal strategies.

Under the internal characteristics frame, the CWMI scientists adopted several academic traditions as the rules or behavioral guides to address the contested research. These individuals turned to the academic traditions, not government policies and practices, as normative responses to make science policy decisions. Referring to these academic rules, numerous observers, especially many CWMI supporters, interpreted certain federal officials’ actions as being inconsistent with the established academic rules, and thus, counter to openness and research independence. By contrast, these observers viewed the CWMI scientists’ actions as being consistent with several academic traditions – and thus, open to examination for data quality and control checks – as well as advancing other behaviors to achieve research independence.

Under the external factors frame, the CWMI scientists and their supporters established a strategy of obtaining a voice for Ellen Harrison, who spoke on behalf of the CWMI scientists, in two important settings. In the first setting, the CWMI scientists worked with a political ally, so Harrison would receive an invitation to testify in a congressional investigation regarding the EPA’s attempts to silence sludge opponents. By testifying at that hearing, Harrison had an opportunity to describe the EPA’s pressures and tactics and to present information supporting the study’s claims. Likewise, in the second setting, a group of CWMI supporters recommended Harrison for appointment to the NRC panel. That venue also gave Harrison a chance to defend the CWMI research and persuade the panel that several of the CWMI findings and recommendations should be included in the NRC report. Although, from the data it was not clear to what extent Harrison had to persuade the panel about the value of the CWMI research findings and recommendations, the NRC report included many findings and recommendations
similar to those in the CWMI research. For example, both the CWMI scientists’ paper and the NRC report recommended (a) updating the 503 assessment approaches to include additional chemicals, metals, and pathogens to test and (b) adopting more stringent oversight and enforcement practices.

When combining both the internal and external factors influencing the academic profession, a third strategy emerged. The CWMI scientists reframed questions and accusations that one challenger used on the academic scientists and reversed the questions or accusations onto the challenger. In essence, the position of the challenger changed to the position of the party being challenged. For some observers, this strategy raised concerns about the scientific accuracy and sufficiency of the original challenger’s claims and positioned the CWMI research as more credible than the CPF report.

In closing, I note that this chapter captured the primary conflicts between the government actors and the CWMI scientists from 1997 to 2002. The sludge controversy did continue beyond 2002, but these challenges-responses were rather minor relative to the 1997-2002 time frame. Although the conflicts between the government actors and the CWMI scientists subsided by 2002, the challenges between EPA administrators and EPA scientists continued. In fact, in 2010, the EPA and Dr. David Lewis, a former EPA scientist, were still in litigation over his 2003 termination.133

133 In his suit, Lewis contended that the EPA terminated him for blowing the whistle on the agency when he reported that the EPA buried research revealing adverse health effects from sludge exposure.
### Table 5.1: Acronyms for the Sewage Sludge Case

<table>
<thead>
<tr>
<th>Acronym</th>
<th>Full Form</th>
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<tbody>
<tr>
<td>CWMI</td>
<td>Cornell Waste Management Institute</td>
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<tr>
<td>EPA</td>
<td>[U.S.] Environmental Protection Agency</td>
</tr>
<tr>
<td>NIOSH</td>
<td>National Institute for Occupational Safety and Health</td>
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<tr>
<td>NRC</td>
<td>National Research Council</td>
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<tr>
<td>NYSDEC</td>
<td>New York State Department of Environmental Conservation</td>
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<tr>
<td>OIG</td>
<td>Office of the Inspector General</td>
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<tr>
<td>USDA</td>
<td>United States Department of Agriculture</td>
</tr>
<tr>
<td>Date</td>
<td>Action/Event</td>
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<tr>
<td>--------------</td>
<td>-----------------------------------------------------------------------------</td>
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<tr>
<td>1993</td>
<td>Environmental Protection Agency (EPA) promulgates 503 rules.</td>
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<tr>
<td>Mar. 21, 1997</td>
<td>Cornell Waste Management Institute (CWMI) sends papers to New York State Department of Environmental Conservation (NYSDEC).</td>
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<tr>
<td>Apr. 14, 1997</td>
<td>David Sterman of NYSDEC contacts Robert Perciasepe of the EPA for assistance on responding to CWMI papers.</td>
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<tr>
<td>Apr. to Dec. 1997</td>
<td>NYSDEC works with the EPA and U.S. Department of Agriculture (USDA) administrators and scientists to work on a response to Cornell.</td>
</tr>
<tr>
<td>Jul. 24, 1997</td>
<td>Robert Perciasepe responds to David Sterman. Perciasepe carbon copies several people, including Ellen Harrison of CWMI, the Cornell University president, and an associate dean at Cornell.</td>
</tr>
<tr>
<td>Aug. 1997</td>
<td>CWMI issues the <em>Case for Caution</em>.</td>
</tr>
<tr>
<td>Sept. 19, 1997</td>
<td>Ellen Harrison sends a copy of the <em>Case for Caution</em> to Robert Perciasepe of the EPA.</td>
</tr>
<tr>
<td>Oct. 31, 1997</td>
<td>Robert Perciasepe responds to Ellen Harrison. Again, Perciasepe carbon copies several people, including the Cornell University president and an associate dean at Cornell.</td>
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<tr>
<td>Nov. 1997</td>
<td>NYSDEC issues its technical report of the <em>Case for Caution</em>.</td>
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<tr>
<td>1999</td>
<td>CWMI scientists publish the <em>Case for Caution</em> under a different title in the <em>International Journal of Environment &amp; Pollution</em>.</td>
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<tr>
<td>Mar. 20, 2000</td>
<td>U.S. House Committee on Science Hearing, <em>Sludge Rule: Closed Minds or Open Debate?</em></td>
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<tr>
<td>Mar. 2000</td>
<td>EPA’s Office of Inspector General issues report, which is critical of the 503 rules.</td>
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<tr>
<td>Jul. 2000</td>
<td>CDC’s National Institute for Occupational Safety and Health (NIOSH) issues report that additional safety precautions are needed to handle sludge.</td>
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<tr>
<td>Sept. 2000</td>
<td>EPA issues a task order to conduct a National Research Council (NRC) review of the 503 rules.</td>
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<tr>
<td>Late 2000 to Feb. 2001</td>
<td>NRC solicits recommendations for additional panelists, and Ellen Harrison is subsequently appointed to NRC panel.</td>
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<tr>
<td>Feb. 3, 2002</td>
<td>CPF Associates, Inc. issues its review of the <em>Case for Caution</em></td>
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<tr>
<td>May 20, 2002</td>
<td>CWMI scientists respond to CPF Associates’ report.</td>
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<tr>
<td>Jul. 2, 2002</td>
<td>NRC releases report study on sludge.</td>
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CHAPTER VI: THE CLIMATE CHANGE CASE

Introduction

In this chapter, I present my findings of the climate change case. To help us understand the environmental issues of this case, I first provide context about the science behind the climate change controversy and federally sponsored academic research that governmental actors publicly challenged. Second, I identify and describe the tactics and pressures that the challengers employed. This discussion elucidates the circumstances and events surrounding the challenge. In addition, I capture the interactions among individuals and groups involved to explain the actors’ reported perceptions and interests in the scientific dispute. Third, I discuss the strategies and tactics that the academic profession used in defense of its intellectual freedom. Drawing on my theoretical framework (see Chapter 3), I examine the internal characteristics and external factors common in the academic profession to help show how certain responses from members of the profession and other allies aided the researchers of the challenged study. I explain how these strategies and tactics helped the challenged researchers exert control over their federally sponsored work. Fourth, I summarize the key findings.

The Science and the Policy Debate

Policymakers and scientists generally accept the notion that the Earth’s temperature has experienced periods of warming over the past 100 years.\textsuperscript{134} What remains a debatable issue among policymakers and scientists is whether human actions contributed to this global warming.\textsuperscript{135} In more scientific terms, the global warming debate may be framed as: Do


anthropogenic matter, processes, and effects contribute to the increasing emission rates of greenhouse gases, which become trapped in the Earth’s atmosphere, causing temperature increases? On one side of the debate, policymakers and scientists have contended that the Earth’s rising temperature reflects an ecological cycle.\textsuperscript{136} They believe that natural radiative forcings such as volcanic eruptions are alone causing the increases in temperature. On the other side of the debate, a different group of policymakers and scientists have argued that besides the natural radiative forcings, anthropogenic radiative forcings such as carbon dioxide emissions and fluorocarbons trapped in the atmosphere are producing heat.\textsuperscript{137}

Each scientific position presents a potentially different plan of action. If the former science prevails, policymakers can do nothing or adopt policy choices that would not harm the environment yet reduce the warming trend.\textsuperscript{138} If the latter science prevails, policymakers and scientists have a social obligation to identify mitigation efforts that would reduce human activities that emit heat-trapped greenhouse gases. If humans are contributing to global warming, the consequences of not acting may severely harm the ecosystem. Many scientists believe that the unnatural warming will eventually lead to significant ice melts in mountain glaciers and Arctic seas. If they are right, the melting will raise sea levels and contribute to greater precipitation. Furthermore, the unnatural warming will alter the ecosystem in other ways, such as by compromising the viability of certain animals and plants.


\textsuperscript{138} While it might be possible for humans to control the warming generated by natural CO\(_2\) and methane emissions, human intervention might actually disrupt the ecosystem and cause more long-term harm.
To date, no scientific study presents findings that prove beyond all doubt that anthropogenic radiative forcings contribute to global warming. However, a number of studies suggest that human influences may indeed play a role. Since 1990, the United Nations Intergovernmental Panel on Climate Change (IPCC) has issued assessment reports updating policymakers and other interested parties about the state of climate change research. These assessments have changed significantly from report to report as paleoclimate science improves.

In 2001, while U.S. legislators were debating the adoption of policies to curb emissions of greenhouse gases, the IPCC issued its Third Assessment Report on the global warming concern (“IPCC 2001”). It concluded that the science presented stronger evidence than reported in the Second Assessment Report that humans likely do contribute to global warming. In support of that proposition, the report’s chapter addressing observed climate change cited more than 500 studies. One particular study, Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes’s 1999 article in *Geophysical Research Letters*,\(^ {139}\) stood out for readers, among them policymakers, scientists, academics, members of the media, and environmental interest groups. An accompanying graph depicted a sharp increase in global temperatures after the industrialization era. As David Appell, a journalist, noted, the graph became an “iconic symbol of humanity’s contribution to global warming” and a controversial image for policymakers, scientists, and other individuals, who did not attribute global warming to human causes.\(^ {140}\)

The Federally Sponsored Study

Funded through a National Science Foundation grant, Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes (“MBH”) reconstructed temperature records back to the year 1000 A.D. In their 1999 peer-reviewed article (“MBH99”), the three academic scientists


examined climate variations in the Northern Hemisphere from 1000 to 1998 A.D., estimating the
temperatures before 1850 using data proxies from nature-based recordings such as tree rings, ice
cores, and coral.\textsuperscript{141} Earlier climate research had also used proxies, but MBH99 and one of the
authors' earlier studies, known as MBH98,\textsuperscript{142} were the first comprehensive studies to use
multiproxies of nature-based temperature recordings to model the temperature levels for such a
long period of time (i.e., nearly 1000 years).

MBH’s temperature reconstructions displayed a pattern, which caught international
attention and the interest of the IPCC 2001 authors. The MBH99 data showed evidence of
steady temperature increases from 1000 to just before the 1900s. Around the turn of the 20\textsuperscript{th}
century, their data displayed a sharp increase in temperature, and from that point continuous rises
until 1998. According to the authors, the natural forcings could not fully account for the
significant increase in temperature. They posited that industrialization and other human
activities likely explained the rise in temperature beyond what could be attributed to the natural
forcings.

The authors charted the data in a graphical format. The graph’s line, a fairly straight line
with a slight curve and then a dramatic spike upward, resembled an image of a hockey stick.\textsuperscript{143}
The graph appeared in one of the IPCC 2001 report’s chapters and in an abridged version of the
report, which was targeted at policymakers. The eye-catching and easy to understand illustration
of anthropogenic global warming became the center of a series of challenges against its authors.

\textsuperscript{141} Starting in 1850, scientists used instrumental temperature measures.
\textsuperscript{142} Mann, M. E., Bradley, R. S., & Hughes, M. K. (1998). Global-scale temperature patterns and climate
forcing over the past six centuries. \textit{Nature}, 392, 779-787
\textsuperscript{143} See, e.g., Regalado, A. (2005, Feb. 14). Global warming: In climate debate, the “hockey stick” leads to a
Retrieved at \url{http://motherjones.com/environment/2005/04/man-behind-hockey-stick}
Interactions Surrounding the Challenges

Many policymakers, scientists, lobbyists, and others interested in the global warming debate in some way challenged Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes and their 1998 and 1999 federally sponsored research. However, it is outside the scope of this paper to detail each interaction and list all the actors. Instead, I draw attention only to the challenges that capture the primary actors and interactions involved in this case study.

As in a play, the challenges appear in Acts. In this metaphorical play, there are four Acts. In Act 1, I recount a series of events in which two individuals, Stephen McIntyre and Ross McKitrick (“M&M”), use their background in statistics and mathematics to challenge MBH’s research. This Act uncovers how M&M’s criticisms increasingly captured the attention of the scientific community and other members of the public. In Act 2 we see how several government actors used M&M’s criticisms as the primary basis to challenge MBH’s research, and how several scientific organizations, climate scientists, and political allies protested the government challenge. In Act 3, I reveal steps that these governmental challengers took to build a case against MBH. In addition, I report an action that political allies of MBH took to investigate the contested scientific studies. In Act 4, I highlight the interactions of a congressional hearing, which essentially repeated the challengers’ primary arguments to discredit MBH98 and MBH99 as well as the supporters’ primary arguments to defend those studies.

Act 1: Scientific Audits

Taking place between 2003 and 2005, Act 1 lays the evidentiary foundation for the government challenges that eventually occur in Act 2. Specifically, Act 1 recounts a series of events in which M&M allegedly uncover several problems with MBH98 and MBH99. As M&M examine these studies, they report them to MBH, the publisher of MBH98, other academic
journals, mass media, and interest groups that have been known to cast doubt on human-induced global warming.

**Scene 1:**

Scene 1 opens in 2003. In that year, Stephen McIntyre, a retired minerals consultant, and Ross McKitrick, an economics professor at the University of Guelph, Ontario, began collaborating to examine the MBH temperature reconstructions. In April 2003, after experiencing problems retrieving the MBH98 and MBH99 data, McIntyre corresponded with Michael Mann, the M in MBH, requesting access to the MBH temperature-reconstruction data.\(^{144}\) A chain of e-mails ensued.\(^{145}\) When McIntyre e-mailed Mann in September 2003 and posed questions about missing data and the data analysis approach, Mann replied by explaining the data treatment and noting the location of the requested data. He also added, “Owing to numerous demands on my time, I will not be able to respond to further inquiries. Other researchers have successfully implemented our methodology based on the information provided

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\(^{145}\) The following items of e-mail correspondence are archived at Stephen H. Schneider’s site through the Woods Institute for the Environment, Stanford University, Stanford, CA, [http://www.climatechange.net/](http://www.climatechange.net/)

See, e.g., McIntyre, S. (2003, Apr. 8), [E-mail correspondence from Stephen McIntyre to Michael Mann]; Mann, M. (2003, Apr. 9), [E-mail correspondence from Michael Mann to Stephen McIntyre wherein Mann refers McIntyre to Mann’s colleague Scott Rutherford.]; McIntyre, S. (2003, Apr. 11), [E-mail correspondence from Stephen McIntyre to Scott Rutherford]. (McIntyre contacts Rutherford to request full access to the data.); Rutherford, S. (2003, Apr. 11), [E-mail correspondence from Scott Rutherford to Stephen McIntyre]. (McIntyre contacts Rutherford to request full access to the data.); McIntyre, S. (2003, Apr. 11), [E-mail correspondence from Stephen McIntyre to Scott Rutherford]. (McIntyre responds to Rutherford’s question about the data requested.); McIntyre, S. (2003, Apr. 22), [E-mail correspondence from Stephen McIntyre to Scott Rutherford]. (McIntyre follows up with Rutherford and asks, “Any luck with this [earlier request]?”); Rutherford, S. (2003, Apr. 22), [E-mail correspondence from Scott Rutherford to Stephen McIntyre]; Rutherford, S. (2003, Apr. 23), [E-mail correspondence from Scott Rutherford to Stephen McIntyre] (Rutherford sends McIntyre the proxy data.); McIntyre, S. (2003, Sept. 9), [E-mail correspondence from Stephen McIntyre to Michael Mann]; Mann, M. (2003, Sept. 25), [E-mail correspondence from Michael Mann to Stephen McIntyre].
in our articles.”146 M&M interpreted these events as Mann closing access to the raw data and the algorithm that reconstructed the temperatures.

Even with most of the raw data, M&M still could not replicate the study as they understood it, especially without the algorithm. The study replication problems concerned M&M. In an interview with USA Today, McKitrick criticized Mann for not giving him and McIntyre all the raw data and the algorithm to reconstruct the data. He argued that “[i]f a study is going to be the basis for a major policy decision, then the original data must be disseminated and the results have to be reproducible.”147 Accordingly, between 2003 and 2004, M&M sought scientific support to audit MBH’s research.

In M&M’s first challenge of MBH’s hockey stick within the scientific community, they published problems that they allegedly encountered with the MBH98 data, the scientists’ methodological approach, and MBH’s analysis. Their paper appeared in the November 2003 issue of Energy & Environment (“M&M03”).148 The paper’s primary finding was that the “particular ‘hockey stick’ shape derived by MBH98 is primarily an artefact of poor data handling and use of obsolete proxy records.”149 M&M reportedly observed several problems with the data used, including “collation errors, unjustified truncation or extrapolation of source data, obsolete data, incorrect principal components calculations, [and] geographical mislocations.”150 Based on their analysis, the Northern Hemisphere’s temperatures during the 20th century were “unexceptional compared to the preceding centuries.”151 That finding conflicted with MBH’s principal argument. That is, MBH claimed that the 20th century had been warming and the 1990s

146 Mann, M. (2003, Sept. 25), [E-mail correspondence from Michael Mann to Stephen McIntyre]. Retrieved at http://www.climatechange.net/
149 McIntyre & McKitrick, 2003, p. 752.
150 McIntyre & McKitrick, 2003, p. 766.
151 McIntyre & McKitrick, 2003, p. 766.
constituted the warmest decade in 600 years. Nonetheless, M&M contended that “the extent of errors and defects in the MBH98 data means that the indexes computed from it are unreliable and cannot be used for comparisons between the current climate and that of past centuries.”\textsuperscript{152} M&M concluded that MBH’s evidence could not support claims that “temperatures in the latter half of the 20\textsuperscript{th} century were unprecedented,” and “even the warmer intervals in the reconstruction pale in comparison with mid-to-late 20th-century temperatures” or that the 1990s was “likely the warmest decade.”\textsuperscript{153}

Having reported numerous errors with the MBH research in a peer-reviewed journal, M&M moved forward with their second challenge of MBH’s research to members of the scientific community. In an effort to access all the raw data and the accompanying algorithm, M&M filed a “materials complaint” to the MBH98 journal publisher, Nature.\textsuperscript{154} As a condition of publication, Nature requires that its authors make their data available. That editorial policy furthers both goals of scientific communalism and study replication. In the complaint, M&M described how unhelpful they felt Mann was to their efforts of replicating MBH98. M&M asserted that “[w]e have been systematically and deliberately stymied by Professor Mann on the most elementary requests: a proper listing of his data series and the exact computational procedures used.”\textsuperscript{155} Besides allegations that Mann withheld information, M&M raised concerns about data discrepancies. For instance, the number of proxies reported in the article was

\textsuperscript{152} McIntyre & McKitrick, 2003, p. 766.
\textsuperscript{153} McIntyre & McKitrick, 2003, p. 766 (citing excerpts of the IPCC 2001, which relied on MBH98 and MBH99 among its sources).
\textsuperscript{154} McIntyre, S., & McKitrick, R. (2003, Nov. 17), [Letter from Stephen McIntyre & Ross McKitrick, Editor of Nature]. Archived at http://www.uoguelph.ca/~rmckitri/research/fallupdate04/MM_MC.Nov03.pdf (McIntyre and McKitrick request additional information about MBH98 data, so they can replicate the study).
\textsuperscript{155} McIntyre & McKitrick, Nov. 17, 2003, paragraph 14.
different from what Michael Mann’s Web site indicated as the number of proxies used to reconstruct temperatures for the MBH98 article.

After the *Nature* editor sent the MBH98 authors a copy of the materials complaint, MBH reviewed the data and the publication. Through their review, they discovered that certain data had been “either mistakenly included in the Supplementary Information, or mistakenly left out.”\(^{156}\) In July 2004, the authors corrected the MBH98 supplementary information in a corrigendum statement, which, like an errata statement, serves as a correction to the original publication. While the corrected data presented more accurate information so other academic scientists could replicate the MBH98 study, the MBH98 authors also noted that they believed “[n]one of these errors affect our previously published results.”\(^{157}\)

*Scene 2:*

In the opening of Scene 2, M&M continue to inform the scientific community about alleged problems with MBH’s research. They apply various techniques to demonstrate possible statistical flaws and defects with the proxy data, and they publish their works through two scientific venues.

In early January 2005, M&M published a follow-up paper in *Energy & Environment* (‘‘M&M05a’’).\(^{158}\) Then, in mid-January 2005, *Geophysical Research Letters*, the same academic journal that printed MBH99, accepted M&M’s article and placed an early version of that paper in its online forum. Known as M&M05b, the article reported alleged statistical errors in the MBH

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\(^{156}\) Mann, Bradley, & Hughes, 2004, p. 105.

\(^{157}\) Mann, Bradley, & Hughes, 2004, p. 105.
In that article, M&M concluded that the MBH method “when tested on persistent red noise [i.e., random data], nearly always produces a hockey stick” shape in M&M’s reconstruction of the data.

Although M&M reported very similar findings in prior presentations and even in their 2003 and 2005 publication in *Energy & Environment*, the media took notice of M&M’s criticisms after the *Geophysical Research Letters* publication. According to journalist Marcel Crok of the *National Post*, one of Canada’s leading newspapers, M&M’s research had not gained much attention prior to January 2005 because he believed “none of McIntyre and McKitrick’s findings had [previously appeared in] major scientific journals.” This publication placed M&M in a different sphere. Now M&M’s perspectives on the climate change research of MBH was reaching the general public.

In February 2005, the front page of *The Wall Street Journal* reported M&M’s concerns about the hockey stick study. The news article conveyed many of the main findings from M&M’s *Geophysical Research Letters* publication, but did it in lay terms. For instance, the article reported McIntyre’s finding that “Dr. Mann’s mathematical technique in drawing the graph is prone to generating hockey-stick shapes even when applied to random data.” It also reported that M&M had previously found errors with MBH98. When M&M exposed the problems to the journal *Nature*, “Dr. Mann and his two co-authors had to publish a partial

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162 The *Wall Street Journal* article provided a brief discussion about M&M’s encounters with MBH to address the data errors that they found and the problems that they had with accessing the raw data and the algorithm to run the data. In Act 1, Scene 1, above, I describe these interactions between M&M and MBH in greater detail so the reader has more particulars about the situation, including the timing and context of events.
correction.” In addition, *The Wall Street Journal* article indicated that M&M had been trying to access other MBH raw data and the computer code, but Mann had refused to provide that information.

In another illustration of the increasingly public nature of the engagement, various organizations invited M&M to speak about their research findings challenging MBH’s hockey stick. For example, one of M&M’s presentations took place at the George C. Marshall Institute’s Washington Roundtable on Science and Public Policy in May 2005. In their presentation, M&M conveyed three major points:

First, M&M heavily criticized MBH’s statistical procedures, alleging, for instance, that MBH had used the wrong statistical significance measure. M&M also claimed that MBH performed another statistical procedure, known as the principal components analysis, in the wrong manner, and those mistakes caused the conflicting statistical significance errors. Put simply, M&M alleged that MBH’s statistical procedure and their approach to confirming the accuracy of the statistical procedure presented were scientifically flawed.

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The George C. Marshall Institute is a think tank on national science policy matters, and its roundtables often present evidence that casts doubt about human-induced global warming. According to Gerhard Sonnert of the Harvard-Smithsonian Center for Astro-Physics and Gerald Holton of the Harvard physics department, “scientists associated with the [Marshall] Institute have tended to be skeptical about the accuracy and scientific validity of forecasts of human-induced global warming, and critical of what they consider hasty policy reactions” (Sonnert & Holton, p. 158).


165 M&M argued that the $R^2$ statistic, one measure in statistical modeling, would have more accurately reported the MBH model’s insignificance. M&M observed that MBH did not report the insignificant $R^2$ statistic, which would have indicated the model’s extremely low explanatory power to predict the temperature reconstructions. M&M did note, however, that MBH reported the reduction of error statistic (*i.e.*, RE statistic), another measure in statistical modeling, to report the cross validation among the proxies used in the study. MBH reported a high figure.

166 The principal components analysis groups data to simplify data effects. Stated another way, the steps for a principal components analysis transform the data, so a researcher might be able to see data patterns in a more simplified structure.
Second, M&M believed that MBH used bad data. They pointed to MBH’s overemphasis on certain temperature proxies, such as the bristlecone pine data from California. According to M&M, the data had been derived from researchers who even commented that the data did not represent an accurate “temperature signal,” and M&M suspected that the proxy did not “match local temperature records.”

Based on M&M’s review of the literature and other sources, bristlecone pine data were “the least qualified to represent temperature[, yet those] data end up getting most of the weight and drive[ing] the results in the hockey stick graph.”

Third, and possibly the most interesting observation, was M&M’s assertion that “the studies and the [bristlecone pine] proxy data are not independent, as any ordinary person would understand the word independent.” M&M contended that “some of the most often cited” studies using bristlecone pine proxies for temperature reconstructions derived from a small circle of academic scientists. M&M observed that the authors’ names rotate, so “Briffa et al. is not a different set of authors than Jones et al.[,] and Mann et al. is not different than Jones and Mann.”

M&M posited:

So the question then would be, if there is a problem with bristlecone pines and bristlecone pines are in eight of the ten other studies [supporting the conclusion of increased temperatures during the 20th century], is the “active ingredient” of each of these hockey sticks the same thing? Can each of these other papers survive a sensitivity study to bristlecone pines possibly being non-climatic?

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169 McIntyre & McKitrick, 2005, p. 17.
170 McIntyre & McKitrick, 2005, p. 18.
171 McIntyre & McKitrick, 2005, p. 18.
M&M suggested that the small network of people in the climate science field colluded to achieve their desired results. That is, Mann and researchers affiliated with Mann compromised their research objectivity and independence. McKittrick also made a similar assertion about the close-knit climate community in a presentation at the April 2005 Asia Pacific Economic Cooperation conference. In that talk, McKittrick stated, “Group efforts are always at risk of self-selection and groupthink,” and therefore, the climate community might suffer from that closed-minded environment.\(^\text{172}\)

In sum, during this scene, M&M captured a wider audience to whom they could disseminate their messages. They moved beyond addressing members of the scientific community who would read their documents. In this scene, they captured international attention from mass media interviews and invitations to speak at association and government meetings. These venues gave them an opportunity to convey the problems that they allegedly found with MBH’s research, such as the use of flawed statistical procedures and application of bad data. In addition, M&M used these venues to speculate about the possible reasons why the climate community had not been receptive to M&M’s scientific audits: They argued that the close-knit community of climate researchers furthers a groupthink mentality, so, generally speaking, the community has not been receptive to opposing views.

As I will illustrate in the following Acts of this metaphorical play, challengers of MBH used M&M’s arguments (i.e., flawed statistical procedures, bad data, limited data access, and closed networks) as their justifications to question, and at times attempts to discredit, MBH98 and MBH99. The significance of these events in Act 1, then, is that M&M laid the evidentiary

foundation for other actors, including government officials and scientists, to contest MBH’s two federally sponsored research studies.

**Act 2: Congressional Inquiry**

In Act 2, I present the initial government challenges to MBH’s 1998 and 1999 federally sponsored research studies. In this Act, I illustrate several instances in which government officials used M&M’s claims from Act 1 as the basis for a congressional investigation. In addition, I report how certain observers, both scientific associations and federal policymakers, perceived those allegations as unjustified attempts at publicly discrediting MBH’s work.

**Scene 1:**

This scene opens during the summer of 2005. Upon learning about McIntyre and McKitrick’s (“M&M”) criticisms of the hockey stick study and the problems they had with accessing the data, two members of Congress initiated an investigation. Because federal grants from the National Science Foundation (“NSF”) sponsored the hockey stick research and the two studies’ findings linked to a significant public policy issue, Congress had authority over the matter. That is, Congress has authority to act on behalf of the public’s welfare. This authority includes reviewing federally supported research and gathering more information on public policy issues, such as climate change.

Exercising that authority, on June 23, 2005, U.S. Representative Joe Barton (Texas, Republican), chair of the Committee on Energy and Commerce, and Representative Ed Whitfield (Kentucky, Republican), chair of the Subcommittee on Oversight and Investigations, sent letters to the three climate researchers of the hockey stick study – Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes (“MBH”) – the chair of the Intergovernmental Panel on
Climate Change ("IPCC"), and the director of NSF. They noted that a congressional review would take place "because the dispute surrounding these studies [i.e., MBH98 and MBH99] bears directly on important questions about federally funded work upon which climate studies rely and the quality and transparency of analyses used to support the IPCC assessment process."174

Congressmen Barton and Whitfield relied heavily on M&M’s public criticisms of MBH’s research as the basis for this inquiry. For instance, in each of the June 23, 2005, letters, the Congressmen specifically mentioned *The Wall Street Journal* as one of the sources for their actions. They referenced the article that reported M&M’s discovery of MBH’s alleged statistical errors, data reliability problems, and refusal to disclose certain data and codes. Barton and Whitfield wrote: “Questions have been raised, according to a February 14, 2005 article in *The Wall Street Journal*, about the significance of methodological flaws and data errors in studies by Dr. Mann and co-authors of the historical record of temperatures and climate change.”175

Congressmen Barton and Whitfield also referred to M&M’s publications as scientific justifications to question MBH’s research.176 Noting the significance of these competing studies, Barton and Whitfield wrote:

> [R]ecent peer-reviewed articles in *Science, Geophysical Research Letters*, and *Energy & Environment* [question] the results of [MBH’s] work. As these

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174 Barton & Whitfield, 2005, paragraph 5.

175 Barton & Whitfield, 2005, paragraph 1.

176 Barton and Whitfield did not expressly mention M&M’s research as their scientific sources; however, M&M’s were the primary articles that appeared in two of the peer-reviewed journals that Barton and Whitfield identified as raising concerns about MBH’s research. Thus, Barton and Whitfield’s sources from peer-reviewed studies, which appeared in two of these journals, very likely derived from M&M’s published works.
researchers find, based on the available information, the conclusions concerning temperature histories – and hence whether warming in the 20th century is actually unprecedented – cannot be supported by the Mann et al. studies[, which were cited in the 2001 Intergovernmental Panel on Climate Change (IPCC) Third Assessment Report].¹⁷⁷

In addition, Barton and Whitfield conveyed concerns about MBH’s data transparency and access. They specifically noted M&M’s complaints about this matter. The Congressmen wrote:

[R]esearchers have failed to replicate the [MBH] findings…, in part because of problems with the underlying data and the calculations used to reach the conclusions. Questions have also been raised concerning the sharing and dissemination of the data and methods used to perform the studies. For example, according to the January 2005 Energy & Environment, the information necessary to replicate the analyses in the studies has not been made fully available to researchers upon request.¹⁷⁸

Emphasizing the significance of replication in the scientific process, they added to their inquiry: “Given the questions reported about data access surrounding these studies, we also seek to learn whether obligations concerning the sharing of information developed or disseminated with federal support have been appropriately met.”¹⁷⁹

The June 23rd letters also demanded professional and financial records. For instance, Barton and Whitfield asked each of the climate scientists to produce a CV, along with a list of all articles on climate research and sources of funding, a list of all financial support for all research including honoraria, a list of all financial support from federal grants, locations identified for all

¹⁷⁷ Barton & Whitfield, 2005, paragraph 2.
¹⁷⁸ Barton & Whitfield, 2005, paragraph 2.
data archives, including where and when information related to research was discovered or first identified, information regarding source codes, information regarding requests made for data and responses to those requests, and information regarding a response to a particular article in relation to past studies conducted. The chair of the IPCC and director of NSF received similar, very detailed requests. The letters requested that each recipient of the letter submit his materials to the congressional committee by July 11, 2005, which gave each individual less than three weeks to respond to those detailed requests.

Scene 2:

Public reactions to Congressmen Barton and Whitfield’s letters differed. Many critics of human-induced global warming appeared to support Barton and Whitfield’s investigation. Capturing this general sentiment, Steven Milloy, a science policy journalist and adjunct scholar at the Competitive Enterprise Institute, who frequently contests assertions that global warming can be human induced, wrote an editorial for FoxNews.com to argue that Barton and Whitfield’s June 2005 congressional inquiry was nothing more than gathering additional information about a science policy matter. Milloy commented that “a scientist’s refusal to provide colleagues with his data and methodology is suspicious.” Milloy also suggested that the circumstances would have been very different if the congressmen had issued subpoenas. According to Milloy, congressional subpoenas would have clearly signaled an investigation for possible wrongdoing, whereas this inquiry represented requests for information about the science. Myron Ebell, director of global warming and international environmental policy for the Competitive Enterprise Institute...

181 Milloy, 2005, paragraph 15.
Institute, argued that “scientists … have vested interests” in certain activities. He pointed out that “just in terms of federal funding for climate research,” it reached “over $2 billion a year.” In his opinion, that funding generates “self-interested motives that [the public] could be suspicious of.” Given the environment, Ebell speculated that the scientific community has failed to critically evaluate the climate change research. He suggested that many scientists “joined … the global warming bandwagon” without questioning it. He supported the Barton-Whitfield investigation as a formal inquiry about the research. Indeed, Larry Neal, a spokesperson for the House Energy and Commerce Committee, described the congressional inquiry as “[s]eeking scientific truth,” though he did apologize if “our little request for data has given [some scientists] a chill.”

By contrast, numerous supporters of MBH vocally opposed Barton and Whitfield’s inquiry. Some of the MBH supporters suggested that Barton and Whitfield intended to raise questions of scientific misconduct. For instance, Alan Leshner, the CEO of the American Association for the Advancement of Science, wrote: “[R]ather than simply expressing interest [about the research and] asking for a hearing …, [Barton and Whitfield] went and, with a highly

182 The Competitive Enterprise Institute is a nonprofit organization that advances policies of free enterprise and limited government activities. It has been an active player in countering the human-induced global warming campaign.


183 Ebell, 2005, paragraph 57.


184 Ebell, 2005, paragraph 74.

185 Ebell, 2005, paragraph 74.

186 Ebell, 2005, paragraph 57; see also Ebell, 2005, paragraph 79.


accusatory tone … asked some very, very detailed questions with a short turnaround.\textsuperscript{188} In addition, numerous MBH supporters argued that scientific assessments belonged with scientists, not politicians. For instance, speaking on behalf of the National Academy of Sciences (NAS), which includes the National Research Council, NAS President Ralph Cicerone wrote to Representative Barton advising that a “Congressional investigation, based on the authority of the House Commerce Committee is probably not the best way to resolve a scientific issue, and a focus on individual scientists can be intimidating.”\textsuperscript{189} Cicerone offered his organization’s services to conduct a thorough scientific review. Further, “If the House Commerce Committee would like to have additional information regarding the state of scientific knowledge in the area of research being conducted by Drs. Mann, Hughes, and Bradley, the National Academy of Sciences would be willing to create an independent expert panel (according to our standard rigorous study process) to assess the state of scientific knowledge in this area, or perhaps one of the professional scientific societies could take on this task for you.”\textsuperscript{190}

Several federal policymakers also conveyed their disapproval of the Barton and Whitfield investigation.\textsuperscript{191} For instance, on July 1, 2005, Representative Henry Waxman (California, Democrat), a senior member of Congress and a member of the House Committee on Energy and Commerce, accused Barton of acting in a manner that did “not appear to be a serious attempt to

\textsuperscript{188} Leshner, 2005, paragraph 30.

\textsuperscript{189} Cicerone, 2005, paragraph 2.

\textsuperscript{190} Cicerone, 2005, paragraph 2.

Inslee, J., & Schakowsky, J. (2005, Jul. 23), [Letter from U.S. Representatives Jay Inslee (Washington, Democrat) and Jan Schakowsky (Illinois, Democrat) to U.S. Representative Joe Barton]. (“We are very concerned that the tone of your letters indicates that these were not requests for hearing background materials, but rather an attempt to intimidate Dr. Michael Mann, and discredit peer reviewed scientific research”) (Inslee & Schakowsky, 2005, paragraph 1).
understand the science of global warming.”  Instead, he suggested that “[s]ome might interpret
[Barton’s requests] as a transparent effort to bully and harass climate change experts who have
reached conclusions with which you disagree.”  U.S. Representative Sherwood Boehlert (New
York, Republican) strongly suggested that Barton and Whitfield commission the National
Research Council to assess MBH’s research.

These interactions between the government challengers and MBH supporters serve as a
prelude to Act 3, in which each side initiated a separate independent review of MBH’s 1998 and
1999 federally sponsored research. Finally, it should be noted that during Scene 2, while various
parties vociferously objected to Barton and Whitfield’s inquiry, Mann, Bradley, and Hughes as
well as the National Science Foundation submitted the materials that Barton and Whitfield
requested.

Act 3: Two Reviews

In the opening of Act 3, Congressmen Barton and Whitfield commission a team of
statisticians to examine MBH98 and MBH99, and separately, Boehlert commissions another
group of scientists to review the MBH studies. This Act describes interactions leading up to and
the issuance of the two scientific reviews. In addition, in this Act, I present each review team’s
findings, which in many respects, reflect significantly different conclusions.

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194 See, e.g., Bradley, R. S. (2005, Jul. 13), [Letter from Raymond S. Bradley to U.S. Representatives Joe
Representatives Joe Barton and Ed Whitfield]; Mann, M. E. (2005, Jul. 15), [Letter from Michael E. Mann to U.S.
Representatives Joe Barton and Ed Whitfield].
Scene 1:

In September 2005, Joe Barton’s office commissions a team of academic statisticians to review the statistics behind the hockey stick. Edward J. Wegman of George Mason University chaired the committee, which included David W. Scott of Rice University and Yasmin H. Said of Johns Hopkins University (“Wegman team”). The Wegman team had a very narrow focus. The House Committee on Energy and Commerce “staff asked for advice as to the validity of the complaints of McIntyre and McKitrick … and related implications” based on the team’s assessment of M&M’s claims. As Yasmin Said observed, “None of our team had any real expertise in paleoclimate reconstruction.” That said, she believed that the scope of the project fell within their expertise because they “were arguably pretty good statisticians.”

The Wegman team functioned publicly as unbiased referees. For example, to avoid criticism, the Wegman team conducted its study without compensation. Yet, behind the scenes, Peter Spencer, the energy committee aide to Barton, served as the lead source to direct the investigation. Spencer sent them a “daunting amount of material” to review within a nine-month span. According to Said, Spencer also communicated that the Committee on Oversight

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and Investigations wanted to better understand the criticisms about Mann and his associates’
work. Therefore, as part of the review, the committee directed Wegman’s team to examine
reasons why earlier reports claiming statistical concerns about the hockey stick study were not
“taken seriously within the climate change community.”

Between October 2005 and June 2006, the Wegman team conducted the review using the
materials presented and referring to other sources that the team gathered. Although the report
did not undergo a formal peer-review process, the Wegman team did send off drafts of the report
for comments to an undisclosed number of reviewers who were fellow statisticians within
Wegman’s professional networks.

Simultaneous with the Wegman review, another team of scientists also assessed the MBH
research. Those events also started in the fall 2005, when Representative Boehlert learned that
Barton and Whitfield had commissioned the Wegman team rather than working with the
congressionally established scientific assessment group, the National Research Council
(“NRC”). In light of that news, Boehlert decided to contract the NRC. Justifying the need for
the NRC review, David Goldston, the House Science Committee’s Chief of Staff, stated: “It
appeared that the issue was not going to go away by itself. We thought this was an appropriate
way to get an assessment of the science.” In January 2006, the NRC accepted the
congressional request and proceeded with its study. Among its directives,

[T]he committee will describe and assess the state of scientific efforts to
reconstruct surface temperature records for the Earth over approximately the past

\[201\] Said, 2007, p. 4.
\[202\] Questions Surrounding the “Hockey Stick” Temperature Studies: Implications for Climate Change
Assessments: Hearing before the Subcommittee on Oversight and Investigations of the Committee on Energy and
of the Wegman Report).
\[203\] Regalado, A. (2006, Feb. 10). Academy to referee climate-change fight, Scientists’ group agrees to
2,000 years. The committee will summarize current scientific information on the
temperature record for the past two millennia, describe the main areas of
uncertainty and how significant they are, describe the principal methodologies
used and any problems with these approaches, and explain how central the debate
over the paleoclimate temperature record is to the state of scientific knowledge on
global climate change.\textsuperscript{204}

The NRC committee, which was formally titled the Committee on Surface Temperature
Reconstructions for the Last 2,000 Years, consisted of 12 independent reviewers with scientific
expertise on the subject matter. The NRC specifically diversifies committees, so it draws on
multiple perspectives to address the questions posed. The committee included scientists with
specialties both in climatology and statistics. Yasmin Said, an author of the Wegman report,
referred to the two statisticians on the NRC committee as “well respected” academics in the
field.\textsuperscript{205} In addition, the NRC committee members had different perspectives on whether and to
what extent human-induced global warming was taking place.

Between January and June 2006, the committee reviewed volumes of scientific papers
and peer comments. In addition, in March 2006, it interviewed 11 individuals, who had
researched in this area, including Steven McIntyre and Ross McKitrick (i.e., M&M) as well as
Michael Mann and Malcolm Hughes of MBH. Based on these interviews, various research
studies, and other relevant sources, the NRC committee wrote an initial draft report. An external
review panel consisting of experts who had not participated in the initial steps of the NRC
process reviewed the draft report and provided feedback to the committee. The committee then

\textsuperscript{204} NRC, 2006, p. 135.
\textsuperscript{205} Said, 2007, p. 18.
determined if any of the external review panel’s comments warranted changing the document and completed the final version of the report.

Scene 2:

On June 22, 2006, the National Research Council issued its report, which evaluated both the state of paleoclimate science on surface temperature reconstructions and MBH’s research, focusing particular attention on the accuracy of MBH98 and MBH99. At the press conference announcing the NRC findings, the committee chair, Gerald North, Distinguished Professor of Meteorology and Oceanography and holder of the Harold J. Haynes Endowed Chair in Geosciences at Texas A&M University, informed the audience:

There is sufficient evidence from tree rings, boreholes, retreating glaciers, and other “proxies” of past surface temperatures to say with a high level of confidence that the last few decades of the 20th century were warmer than any comparable period in the last 400 years.²⁰⁶

MBH98 and MBH99 argued that, at the time of the study, the last few decades of the 20th century were warmer than any other period within the past 600 years and 1,000 years, respectively. The NRC report could not validate those findings with high degrees of certainty. Instead, the report indicated:

Based on the analyses presented in the original papers by Mann et al. and this newer supporting evidence, the committee finds it plausible that the Northern

Hemisphere was warmer during the last few decades of the 20th century than during any comparable period over the preceding millennium.\textsuperscript{207}

The report proceeded to explain the committee’s hesitation to affirm MBH’s temperature claims for periods prior to A.D. 1600. According to the committee, “substantial uncertainties currently present in the quantitative assessment of large-scale surface temperature changes prior to about A.D. 1600 lower our confidence in [MBH’s] conclusion”\textsuperscript{208} that “the 1990s [were] likely the warmest decade, and 1998 the warmest year, in at least” the 1,000 years prior to the 1990s.\textsuperscript{209} The report’s authors determined that these uncertainties derived from the imprecision of selected proxy data for selected time periods.\textsuperscript{210} Since the NRC committee observed some uncertainty, Gerald North explained that it could not support MBH’s claim about the last millennium using language that would attach some level of predictability. Given the uncertainty, he described the findings prior to the year 1600 as “plausible.” At a congressional hearing, which took place a month after the NRC issued the report, North clarified that “what we mean[t] by plausible [was] there just [didn’t] seem to be any counter information.”\textsuperscript{211} Thus, MBH’s claim about the last millennium was “a reasonable thing” to conclude (i.e., plausible but not certain).\textsuperscript{212}

The NRC committee indicated that its findings did not discount MBH’s claims. In fact, the NRC’s high confidence in the data between the 1600s and 1990s and reduced confidence in the data prior to 1600 generally aligned with MBH’s works. MBH98 and MBH99 included statistical error bars to represent the authors’ level of confidence in their data. These error bars

\begin{enumerate}
\item[207] NRC, 2006, p. 109.
\item[208] NRC, 2006, p. 109.
\item[211] North, Jul. 19, 2006, p. 122.
\item[212] North, Jul. 19, 2006, p. 122.
\end{enumerate}
reported patterns consistent with the NRC findings. That is, MBH displayed error bars that “were relatively small” from 1600 to the 1990s, indicating a high level of confidence; the error bars increased for the period between 1000 and 1600, and again increased for the period prior to A.D. 1000.\footnote{NRC, 2006, p. 105.} Thus, both the NRC report and the two MBH studies acknowledged problems with precision when researchers plotted proxy temperatures for earlier time periods; however, the NRC committee believed that MBH’s “uncertainties of the published reconstructions [were] underestimated.”\footnote{NRC, 2006, p. 107.}

The NRC committee did raise a significant concern about the MBH statistical methodology. The committee observed some statistical weaknesses with that methodology. Most notably, it recognized M&M’s assertions that MBH did not properly recalibrate or normalize each of the variables over the entire time period. MBH calibrated the data to the time period when temperature measures were based on more reliable instruments, not the periods relying heavily on the proxies. However, that approach could have influenced the “principal components [calculations] in unanticipated ways.”\footnote{NRC, 2006, p. 87.} That is, the NRC committee believed that MBH’s procedures could perform at a “suboptimal” level.\footnote{NRC, 2006, p. 88.} Further, while the committee acknowledged MBH’s use of a validation metric in both studies, it referred to that statistical check as a minimum requirement.\footnote{NRC, 2006, p. 88.} The committee questioned “whether any single statistic can provide a definitive indication of the uncertainty inherent in the reconstruction” and recommended the inclusion of other validation metrics.\footnote{For a more in-depth explanation, see NRC, 2006, pp. 80-85, 88-92, & 110.}
Despite these concerns, the NRC committee determined that the approach used by MBH did not lead to any significant problems with the data outputs. In support of the committee’s conclusion, the NRC report noted that other reconstruction studies, which used the preferred statistical methodologies, reported outputs that were “qualitatively similar to the original curves presented by Mann et al.”\(^{219}\) For example, in 2006, Eugene R. Wahl and Caspar M. Ammann constructed their own code to replicate MBH’s statistical methodology.\(^{220}\) Unlike MBH, however, they recalibrated the MBH data for the whole time period, which happens to be consistent with the NRC statistical perspective. Wahl and Ammann concluded that decentering or recalibrating the data did not result in significantly different output. In fact, the NRC committee reported

\[\ldots\] an array of evidence that includes both additional large-scale surface temperature reconstructions and pronounced changes in a variety of local proxy indicators, such as melting on icecaps and the retreat of glaciers around the world, which in many cases appear to be unprecedented during at least the last 2,000 years.\(^{221}\)

Thus, while statisticians would “not recommend” MBH’s methodology, their methodological approach did “not appear to unduly influence reconstructions of hemispheric mean temperature[,]” as subsequent studies appear to validate MBH’s findings.\(^{222}\)

\(^{219}\) NRC, 2006, p. 106.

\(^{220}\) At the time of the NRC assessment review, the Wahl & Ammann paper was “in press” (i.e., to be published), and M&M and other scientists working on this topic had access to the paper.

The current citation of the paper is listed below.


\(^{221}\) NRC, 2006, p. 3; see also NRC, 2006, pp. 29 & 109 for additional support of this statement.

\(^{222}\) NRC, 2006, p. 106.
The NRC committee also acknowledged MBH’s significant contributions to the field. As the NRC committee pointed out, MBH98 represented the “first systematic, statistically based synthesis of multiple climate proxies.” According to the NRC, MBH’s early research represented the first “large-scale surface temperature reconstructions … to include explicit statistical error bars to properly indicate the statistical confidence that they had with their findings.” In addition, the committee observed that “despite [some of the data] limitations, the … efforts to reconstruct temperature histories for broad geographic regions using multiproxy methods [served] an important contribution to climate research and that these large-scale surface temperature reconstructions contain[ed] meaningful climatic signals.”

Generally speaking, the NRC findings led to two opinions. Challengers of the MBH research argued that the NRC report offered another objective source to point out problems with MBH98 and MBH99 and discredit these studies. Supporters of the MBH research put forward a different perspective. They contended that the NRC report substantially affirmed the MBH studies, and they highlighted the various other studies that corroborated MBH’s findings of human-induced global warming.

Scene 3:

Besides the NRC report, the challengers also asserted that the conclusions of another scientific review showed the MBH research to be flawed, and therefore MBH should be discounted. On July 14, 2006, several weeks after the release of the NRC report, the statisticians (i.e., the Wegman team), whom Representatives Barton and Whitfield had commissioned, announced their findings. Unlike the NRC report, the Wegman report, officially known as the

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225 NRC, 2006, p. 3.
Ad Hoc Committee Report on the “Hockey Stick” Global Climate Reconstruction, in no respect indicated that MBH98 or MBH99 had any scientific value. Instead, the Wegman team described these publications as “somewhat obscure and incomplete,” and the “criticisms of MM03/05a/05b to be valid and compelling.” Given that statement, it is not surprising that the Wegman team listed only flaws with MBH’s research and reiterated many of M&M’s challenges of the MBH studies. In this Scene, I briefly describe the Wegman team’s three principal complaints and connect them to instances where M&M reported the same criticisms.

For the first major complaint, the Wegman team identified several problems with MBH’s data and statistical methodology. Just as M&M had faulted MBH in their two 2005 publications, the Wegman team criticized MBH for using proxies that sometimes result in inaccurate temperature replacements. In addition, the team claimed that MBH’s research consisted of data-treatment errors, leading to numerous statistical mistakes. Explaining its concerns, the Wegman team wrote:

The controversy of Mann’s methods lies in that the proxies are centered on the mean of the period 1902-1995, rather than on the whole time period. … The net effect of this decentering using the proxy data in MBH98 and MBH99 is to produce a “hockey stick” shape. Centering the mean is a critical factor in using the principal component methodology properly.

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The report may also be retrieved from http://republicans.energycommerce.house.gov/108/home/07142006_Wegman_Report.pdf

In other words, the team found that the MBH authors did not calibrate the proxy data to the whole time period, when “[n]ormally, one would try to select a calibration dataset that is representative of the entire dataset.” According to the Wegman team, this allegedly incorrect selection of calibrated proxy data “leads to a misuse in principal component analysis,” the statistical methodology used. Furthermore, repeating the steps that M&M performed, the Wegman team observed that the algorithm used in MBH98 “will reproduce any desired shape, depending on what shape exists in the proxies.” Thus, based on the data reconstruction using M&M’s work, the Wegman team concluded that the MBH temperature reconstructions were severely flawed.

For the second major complaint, the Wegman team expressed concerns about data accessibility of the MBH studies, especially in light of the public visibility of the research. The Wegman team indicated that MBH failed to disclose enough information for M&M to reproduce selected portions of their work. The team also encountered limited data. It reported: “[W]e did not find adequate material to reproduce the MBH98 materials,” even after reviewing multiple sources. Further, when MBH had shared data, the Wegman team, like M&M, described those occurrences as “haphazard and often grudgingly done.” The report’s authors emphasized that scientific norms include the practice of communalism, which establishes a professional expectation that scientists share their data for reproduction and further development of the scientific concept to create independent verifications. Nonetheless, the Wegman team indicated

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that the data access problems and arrangements of the data had the result that “independent verification [was] impossible” without significant guessing and reworking of the data.235

The Wegman team’s third major complaint raised a concern about the paleoclimatic community’s reliance on the MBH temperature-reconstruction studies. The team claimed that the paleoclimatic community is so close-knit that these researchers cannot objectively evaluate the studies in this area. The basis for the Wegman team’s claim derived from a statistical analysis known as social network analysis, which involved examining the relationship among the temperature-reconstruction authors. The Wegman team posited that “if there is a tight relationship among the authors and there are not a large number of individual[s] engaged in a particular topic area, then one may suspect that the peer-review process does not fully vet papers before they are published.”236 The presumption is that tight relationships in a small field likely demonstrate “very sympathetic referees,” especially since the odds are high that these “referees may have co-authored other papers with a given author” who is under review.237

When the Wegman team conducted the network analysis using Michael Mann as the network center, the data identified strong ties between Mann and cliques of paleoclimatic scientists who examine temperature reconstructions. These linkages demonstrated that Mann worked or was closely connected with cliques whose members published most of the articles on temperature reconstructions. Given these strong ties, the Wegman team also analyzed the relationships between various temperature proxies and papers published. The analysis demonstrated that paleoclimatic researchers often relied on the same set of proxies that led to temperature reconstructions. Given the dependence on the same proxies, the Wegman team commented that “[i]t is not surprising that the papers [published in the area] would obtain similar

results” about temperature changes over time. According to the Wegman team, the network analysis of the shared proxies suggested that “the ‘independent reconstructions’ are not as independent as one might guess.” Further, the team observed that “the paleoclimatology community has not recognized the validity of the MM05 papers and has tended to dismiss [McIntyre and McKitrick’s] results as being developed by biased amateurs.” Wegman’s team attributed the “closed networks with many redundant ties” as the primary reason for the paleoclimatic community to discount M&M’s research and “rall[y] around the MBH98/99 position.”

This third criticism, postulating that possible biases might have occurred based on social networks, had been raised by M&M in a talk at the Asia Pacific Economic Cooperation conference in April 2005 and at the Marshall Institute seminar in May 2005. In each of those forums, which I discussed in Act 1, Scene 3, M&M suggested that the tight-knit nature of the climate studies community possibly explained (1) acceptance of MBH’s data and methodology without criticisms and (2) the general rejection of M&M’s criticisms of MBH’s research in that field. While neither M&M nor the Wegman team presented concrete evidence of actual biases resulting from these networks, in Act 4, I will highlight instances in which several government challengers of the MBH research revisited this matter to question the trustworthiness of the peer-review process.

In short, the MBH challengers treated the two scientific reviews, the NRC’s and the Wegman team’s, as reports concluding that MBH’s studies were flawed. For example, McIntyre testified at the congressional hearing that he interpreted both reports as concluding “no

239 Wegman, Scott, & Said, 2006, p. 46.
confidence could be placed on reconstructions prior to 1600 and that Mann’s statistical methods were unsatisfactory.”242

Scene 4:

On July 19 and 27, 2006, the government challenges to MBH’s federally sponsored research continued with a formal hearing before the House Energy and Commerce Subcommittee on Oversight and Investigations.243 The witnesses included Michael Mann and nine individuals, who had participated in the review of the MBH studies, such as Edward Wegman, author of the Wegman report, Gerald North, chair of the NRC committee, and Stephen McIntyre, the individual who had been auditing Michael Mann’s work.244 Unlike past challenges, this interaction consisted of arguments from both sides of the debate. For members of Congress, each side generally reflected the party line – Republicans as the government challengers and Democrats as the MBH supporters.245

For the most part, the two-day hearing repeated the challengers’ arguments in Acts 1-3 as well as the responses of MBH and their supporters, which I will address more fully in the next section. That is, the hearing presented three disputes between the challengers and supporters of the MBH study regarding (1) the true purpose of the hearing, (2) the accuracy and effect of the MBH data and statistics, and (3) the potential bias from the peer-review process. Rather than repeat the key actors’ arguments and counterarguments, which appear in other parts of this chapter, I will only highlight the data revealing new insights or clarification about actors’

242 McIntyre, 2006, p. 234.
245 Barton referred to the two conflicting groups within the subcommittee as largely a division by party line (i.e., Republicans and Democrats) (Barton, 2006, p. 248).
interpretation of events. Specifically, I will present the challenges regarding the sufficiency of the peer-review process.

During the hearing, actors on both sides debated the Wegman hypothesis about whether the climate science community favored certain scientists’ work and closed off access for others. In addition, Edward Wegman, Stephen McIntyre, Joe Barton, and Ed Whitfield called into question the objectivity of the peer-review process and raised concerns about the trustworthiness of research thus reviewed when policymakers rely on these studies to make laws. But the events revealed differing expectations of the peer-review process. Before I present the challengers’ claims about the problems with the peer-review process, I briefly explain the approaches of the different types of review processes addressed in this case study in Table 6-1.

Table 6.1: Differences in Peer-Review Processes

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<th>SCIENTIFIC JOURNAL’S PEER-REVIEW PROCESS</th>
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<td>One form of peer review determines the acceptance of an article for publication. Ralph Cicerone, the President of the National Academy of Sciences, described how the traditional peer-review process for scientific journals involves several steps. In a written statement to the House Energy and Commerce Subcommittee on Oversight and Investigations, he explained:</td>
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<td>Each scientific journal has its own policies for peer review, but there are some standard features among them. Generally, submitted manuscripts are assigned to an editor who has expertise on the general topic. These editors conduct a first level review of the manuscript to determine if it meets basic criteria for publication in the journal (e.g., length, appropriateness of subject matter, etc.). Some manuscripts are rejected outright at this stage.</td>
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<tr>
<td>If a manuscript is considered suitable for the journal, it will be sent out for peer review. Usually, two or more reviewers are chosen by the editor. In some cases, the journal may ask the author(s) to suggest potential reviewers who have the appropriate expertise, but the editor or an</td>
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associate editor will make the ultimate decision. Reviewers are typically asked to submit comments on the manuscript and to make a recommendation on its suitability for publication in the journal. Once the reviewers’ comments have been submitted, the editor decides whether to accept the manuscript for publication (perhaps with some minor revisions), ask the author(s) to make significant revisions and submit the paper for re-review (in which case the process is repeated), or reject the paper. In the case that two reviewers disagree about the suitability of a manuscript for publication, the editor may solicit an additional tie-breaking review or ask the author(s) to prepare a rebuttal to the critical reviews. Usually all of the review comments are shared with the author(s), regardless of the decision to publish, but the identity of the reviewers is kept anonymous throughout the process, even after publication, unless a reviewer specifically request[s] for their anonymity to be waived.246

NATIONAL RESEARCH COUNCIL’S PEER-REVIEW PROCESS

The National Research Council assesses the research for a given scientific topic. It selects a panel of typically 12 independent reviewers with scientific expertise on the project topic. The panel constructs its initial draft report based on interviews of other scientific experts on the matter, reviews scientific papers directly related to the topic, and examines other sources deemed relevant to understanding the scientific topic.

The initial draft undergoes a series of reviews before the NRC publicly announces its findings. Ralph Cicerone, the NAS president, described this process specifically in terms of the NRC report that assessed MBH98 and MBH99. He indicated that a “Report Review Committee (RRC), [which consists] of approximately 30 members of the National Academy of Sciences, National Academy of Engineering, and Institute of Medicine,” oversaw the outside review process, which involved selecting independent reviewers to comment on the initial draft.247 Cicerone explained:


247 Cicerone, 2006, p. 780.
Questions Surrounding the “Hockey Stick” Temperature Studies: Implications for Climate Change Assessments: Hearing before the Subcommittee on Oversight and Investigations of the Committee on Energy and
The Board on Atmosphere Sciences and Climate, in consultation with the RRC, appointed a group of 13 independent reviewers with a broad range of expertise and diverse perspectives on the issues addressed in the report. … Reviewers were asked to provide written comments on any and all aspects of the draft report, including the accuracy of the committee’s analysis and the responsiveness of the committee to its charge. [Then, the NRC committee, which authored the report was] required to respond in writing to every review comment, revising the report where appropriate. These responses were evaluated by a review monitor appointed by the RRC and by a review coordinator appointed by the NRC Division on Earth and Life Studies. The report was not released until after the review monitor, the review coordinator, and all members of the authoring committee approved the revised draft. Once the review process was successfully completed, no changes (other than minor editorial corrections) were made to the approved text.248

Thus, the NRC report undergoes several rounds of reviews, including by a group of experts, who review and write the report. Various reviewers, along with staff, coordinate an external review process.

INTERGOVERNMENTAL PANEL ON CLIMATE CHANGE’S PEER-REVIEW PROCESS

The IPCC assessment reports involve multiple phases.249 In the first phase, a body consisting of the IPCC Secretariat, the task force members (i.e., the group that coordinates the entire report), government representatives, and other participating organizations identify international experts in various specialties, such as scientists who study observed climate variability, radiative forcing of climate change, model evaluation, and physical climate processes and feedbacks. A different panel of scientists addresses each major topic, and the IPCC allocates each panel of experts a specific chapter in the report.

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Cicerone (2006) added that “The identity of reviewers is considered to be privileged information during the review process. Anonymous review is intended to encourage individual reviewers to express their views freely and to permit the authoring committee to evaluate each comment on its merits without regard for the position or status of the reviewer. The names and affiliations of the reviewers were made public when the report was released, but their comments remain confidential” (p. 781).


During this initial phase, the Task Force selects lead authors and coordinating lead authors. The IPCC2001 panel that reviewed MBH98 and MBH99, *Chapter 2: Observed Climate Variability and Change*, consisted of ten lead authors, two of whom served as coordinating lead authors, 140 contributing authors (including Raymond Bradley and Malcolm Hughes of MBH), and two review authors/editors. Coordinating lead authors are responsible for the entire chapter while the lead authors take responsibility for a designated section within the chapter. With the lead authors, the contributing authors participate in the assessment and review of the literature.

The second phase is much longer and involves two rounds of reviews. Thomas Karl, Director of the National Oceanic and Atmospheric Administration’s National Climatic Data Center, was one of the coordinating lead authors for the IPCC2001 chapter that cited to MBH’s studies. He described his experience during the second phase.

> [E]ach of the lead authors are asked to assess the published literature up until a certain time after which no more new material can be considered and what lead authors do is take a look at that material and try to write up their consistencies among what has been published, inconsistencies, what is available today compared to what was available during either the previous IPCC report or previous to that.\(^{250}\)

Because the IPCC process involves international experts within a specialized area and the report is an assessment of the best scientific knowledge in that area, the authors of a given chapter are likely to have written some of the studies that the panel assesses. The IPCC process includes many authors who may participate in the assessment. In addition, after a chapter is written, the review editors help identify international expert reviewers, who “comment on the accuracy and completeness” of the chapter.\(^{251}\) These individuals are external to the process and do not serve as an author to that particular IPCC report. As Karl explained, the chapters “are subjected to international review.”\(^{252}\) Then, the chapter’s authors make changes as needed. After the first round of reviews, another round of reviews calls on international experts and government officials from IPCC member nations to comment on the initial draft. Karl noted the exhaustiveness of this second review process, pointing out that

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\(^{250}\) Karl, 2006, p. 249.


\(^{252}\) Karl, Jul. 19, 2006, p. 249.
“[a]nyone and everybody is open to review [the] report and the process takes place over several years.”\textsuperscript{253} After the second round of reviews, the authors prepare the final document.

For the third and final phase, the working group for that assessment establishes a special team to create a Summary for Policymakers (SPM) report. That team typically consists of lead authors from the respective chapters to contribute to the SPM. Before that document is distributed, government officials review that report. Then, upon approval, the working group and the IPCC leadership review all the documents one last time before publication.

Stephen McIntyre of M&M criticized the peer-review processes. During the July 2006 congressional hearing, he conveyed in more explicit terms his position about the level of review that should have taken place for studies used to inform policymakers. He continued to advocate for an \textit{audit} of each study, but this time, he outlined what he meant. He indicated that “peer review as practiced by academic journals is not an audit but something much more limited” and is insufficient to review studies that impact national policy.\textsuperscript{254} He also distinguished the NRC report from the Wegman report. He characterized the NRC report as simply a “literature review” and the NRC committee “did not attempt to replicate or audit these other studies” it reviewed.\textsuperscript{255} By contrast, he recognized the Wegman report as something useful because it engaged in “independent testing … [about] whether Mann’s method was biased towards producing hockey stick-shaped series,” which he considered to be a proper scientific audit.\textsuperscript{256}

Similarly, challengers of the MBH research also raised objectivity concerns with the IPCC assessment process. According to Joe Barton, the IPCC assessment process is “not independent or impartial.”\textsuperscript{257} Barton believed that if scientists can participate in an assessment of their own work, then the process cannot be independent or impartial. The Wegman team

\textsuperscript{253} Karl, Jul. 19, 2006, p. 249.
\textsuperscript{254} McIntyre, 2006, p. 234.
\textsuperscript{255} McIntyre, 2006, p. 234.
\textsuperscript{256} McIntyre, 2006, p. 234.
\textsuperscript{257} Barton, 2006, p. 9.
agreed. They recommended that authors of assessment reports “should not be the same people as those that constructed the academic papers,” which the report cites. They suggested that scientific experts, who contribute new knowledge to the field and then participate in an assessment that includes their research, compromise the objectivity of that assessment report. Thus, they argued that these studies should be discounted from the policy arena, and others that successfully withstand an audit substituted.

As I discuss in the next section, the supporters of the MBH research defended the peer-review process and continued to clarify the different forms of peer review and the processes involved for each review format. They sought to explain how the challengers’ description of the process did not fully appreciate the extent of the review process for journal articles or scientific assessments.

Finally, while the congressional hearing highlighted the differences between the two sides, there was one matter in which the participants reached consensus. Both the MBH research challengers and its supporters did agree that the scientific community and government should establish a uniform system for data sharing. Among the recommendations, scientists and government officials suggested creating national depositories to house the raw data. Since the conflict between the two groups was based in part on allegations that MBH had not shared their data, this recommendation would presumably alleviate future conflicts about this issue.

**Summary of the Challenges**

In this section, I examined the interactions surrounding the challenges to MBH’s research. The challenges began in 2003 with a series of events in which two scientists, who had a mathematics and statistics background, questioned the MBH98 and MBH99 data and methodology that created the hockey stick graph. They identified possible problems with those

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studies and conveyed their messages first to the scientific community, then to the general public. Their concerns eventually captured the attention of Representatives Barton and Whitfield, who initiated an investigation.

The congressmen’s inquiry arguably inferred that MBH had conducted questionable scientific work, possibly to achieve their intended findings that humans significantly contribute to global warming. To uncover the science behind the MBH hockey stick, the MBH challengers commissioned a study specifically to examine concerns about MBH’s data treatment and statistical approach as well as to investigate possible reasons that the climate research community was not very receptive to M&M’s findings. At the same time, supporters of MBH commissioned another group of scientists to evaluate the MBH research. At the end, in a congressional hearing, the two groups debated and argued for each side, largely repeating earlier arguments. Thus, throughout the three Acts, the challengers sought to discredit the MBH research, and MBH and their supporters responded in various ways to fend off those who questioned the science.

In the next section, I elaborate further on ways that MBH and their supporters took action to defend MBH’s federally sponsored research.

Responses to the Challenges

In this section, I detail how various actors who supported Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes (“MBH”) employed three significant strategies, so MBH could exert control over their federally sponsored research. Consistent with the conceptual framework discussed more fully in Chapter 3 of this dissertation, I divide the discussion of these strategies into actions that relied more heavily on (a) internal characteristics, (b) external factors, and (c) both internal and external factors of the academic profession.
Internal Characteristics

One of the strategies that MBH and numerous academic scientists who supported their research employed was the use of established academic norms as the “rules” for scientific reviews. I present, below, two illustrations – one involving requests to follow academic norms for the scientific review process and another demonstrating how academic norms led to content clarifications of non-climate scientists’ interpretations of MBH’s research.

Campaigning for Scientific Processes

When several actors in Washington began to meddle in what had been a fight between scientists, MBH supporters, such as scientific associations and climate scientists, participated in a letter-writing campaign to publicly express their objections to the Barton-Whitfield June 2005 inquiry. Their letters conveyed their concerns with the congressmen’s choice of a political, rather than a scientific, venue to assess the contested research. For instance, the American Association for the Advancement of Science (AAAS) CEO Alan Leshner expressed to the congressmen concerns about using a congressional subcommittee as a forum to evaluate scientific studies. Speaking on behalf of his organization, he indicated: “While we fully understand that the policy-making functions of the Congress require integrating the best available understanding of relevant science with other considerations, we think it would be unfortunate if Congress tried to become a participant in the scientific peer review process itself.”259 As stated earlier in Act 2, Scene 2, the National Academy of Sciences (NAS) president, Ralph Cicerone, conveyed a similar message, and he offered the services of his organization to

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259 Leshner, A. I. (2005, Jul. 13), [Letter from Alan I. Leshner, CEO of the American Association for the Advancement of Science and Executive Publisher of the journal Science, to U.S. Representative Joe Barton].
create an independent expert panel … [review] to assess the state of scientific knowledge” on the contested subject matter.\textsuperscript{260}

Many of the scientists and scientific organizations that supported MBH also characterized the letters that the Barton committee sent Mann and others as unfounded accusations of wrongdoing rather than an attempt to learn about concerns that others had expressed regarding MBH’s research.\textsuperscript{261} Speaking on behalf of the AAAS, Leshner commented that scientists around the world interpreted the tone of the letters as “a hostile inquisition as opposed to a reasonable inquiry into the kinds of findings that might be relevant to the decisions policy-makers have to make.”\textsuperscript{262}

Congressmen Barton and Whitfield received similar messages from the Association of American Geographers,\textsuperscript{263} American Geophysical Union, American Meteorological Society,\textsuperscript{264} and a group of 20 U.S. scientists, who claimed to have “expertise relevant to the understanding

\textsuperscript{260} Cicerone, 2005, ¶2.
\textsuperscript{261} Quirin Schiermeier (2005) of \textit{Nature} reported that “[s]cientists have called the aggressive tone of letters disturbing and dangerous” (p. 7).
\textsuperscript{262} Leshner, 2005, paragraph 30.
\textsuperscript{263} Richardson, D. (2005, Aug. 12), [Letter from Douglas Richardson, Executive Director of the Association of American Geographers, to Representative Joe Barton].
\textsuperscript{264} The American Geophysical Union and American Meteorological Society sent a joint letter from their organizational presidents, “whose more than 50,000 members include most scientists engaged in climate research,” expressing concerns about the scientific process. Orcutt, J., & Lyons, W. (2005, Aug. 8), [Letter from John Orcutt, President of the American Geophysical Union, and Walter Lyons, President of the American Meteorological Society, to Representative Joe Barton].
of Earth’s changing climate.” In addition, 30 scientific and engineering research and education organizations, such as the American Astronomical Society, Association of American Universities, Ecological Society of America, National Council for Science and the Environment, and the University Corporation for Atmospheric Research, also issued a single letter criticizing Barton and Whitfield’s investigation and expressing support of MBH’s research.

In several ways, these letters helped further academic rules as the normative behavior to address scientific debates. First, the letters demonstrated the involvement of professional associations. These official societies and other groups of scientists formed a coalition to protest the government challengers’ actions. Their numbers and collective expertise represented a significant force for observers, such as the media and politicians, to take into account. While the letters did not stop Barton and Whitfield from pursuing the investigation, they placed the congressmen on notice that these professional associations and groups of scientists disapproved of the inquiry and were carefully watching the government challengers’ next moves.

Second, the letters sent a clear message that many academic scientists stood behind the professional ideology that scientific assessments occur without political influence. For instance, the 20 U.S. scientists who claimed to have “expertise relevant to the understanding of Earth’s changing climate,” to Representatives Joe Barton and Ed Whitfield. The letter’s authors included James E. Hansen, the Director of NASA’s Goddard Institute for Space Studies, John P. Holdren, the Teresa and John Heinz Professor of Environmental Policy at Harvard University, and William H. Schlesinger, the James B. Duke Professor of Biogeochemistry and Dean of the Nicholas School of the Environment and Earth Sciences at Duke University.

changing climate” believed that Barton and Whitfield’s political pressure “risk[ed] compromising the independence of scientific opinion that is vital to the preeminence of American science as well as to the flow of objective advice to the government.”\textsuperscript{268} Furthermore, several of the professional association letters, such as the joint letter by the presidents of the American Geophysical Union and American Meteorological Society, interpreted the congressional inquiry as “an attack on particular scientific results” that countered certain government officials’ desired policy outcomes.\textsuperscript{269} These scientific leaders warned that the “prospect for scientists of defending unpopular results in a political arena rather than before their ‘peers’ in the literature has the potential to undermine the scientific process and, if persistent, to produce tainted results.”\textsuperscript{270}

Emphasizing the divisions of labor between policymakers and scientists as a third point to affirm the academic rules, these scientific organizations reminded the congressmen and others who were observing these exchanges that scientists and policymakers have two separate roles in society. Scientists conduct the research and policymakers create policies after weighing the findings of scientific research and other public policy concerns. Accordingly, they asked that “Congress respect this time-tested process of scientific quality control” and not interfere with the scientific process.

\textit{Exercising Subject Matter Expertise}

Another strategy that MBH and their supporters employed was demonstrating the significance of subject matter experts to conduct studies in a highly technical sub-field. Specifically, on several occasions, MBH and their supporters had to clarify misunderstandings that M&M and the Wegman team had about data treatment and terminology. These

\begin{footnotes}
\item[268] Bender, M., et al., 2005, paragraphs 1, 4.
\item[269] Orcutt & Lyons, 2005, paragraph 3.
\item[270] Orcutt & Lyons, 2005, paragraph 3.
\end{footnotes}
clarifications rested largely on matters unique to the climate research community. For instance, Jay Gulledge, an environmental scientist and senior research fellow for the Pew Center for Climate Change, testified at the July 2006 congressional hearing that the Wegman report failed to address some of Mann’s claims that “McIntyre and McKitrick didn’t apply [Mann’s] method correctly.” Gulledge asserted that “[i]f those criticisms are being used to question the work then that has to be examined.” Yet, according to Gulledge, the Wegman team failed to examine the “[c]orroborating evidence.” Gulledge was referring to Eugene Wahl and Caspar Ammann’s 2006 research project, in which they constructed their own code to replicate MBH’s statistical methodology and recalibrated the MBH data. They concluded that the decentering did not significantly change the output. They found that the “overall trajectory and conclusions of MBH are completely unaffected by this result.”

McIntyre admittedly dismissed the so-called corroborating evidence because Wahl and Ammann thought “certain steps [were] fine, we [didn’t].” According to McIntyre’s testimony at the July 2006 congressional hearing, Wahl and Ammann “have in my opinion not carefully considered the implication of bristlecones” as a data proxy. McIntyre used the NRC report as his primary basis to discount the bristlecones, which he believed represented bad temperature

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276 McIntyre, 2006, p. 733.
277 McIntyre, 2006, p. 733.
proxies. According to McIntyre’s testimony, the NRC panel agreed with his position that “strip bark bristlecones should be avoided in temperature reconstructions.”

Given the proxy’s alleged reliability problem, McIntyre removed the bristlecone data. Using MBH’s approach, McIntyre’s output graph no longer had the hockey stick shape. According to McIntyre, “the hockey stick shape is dependent on” the bristlecones.

Nonetheless, at that same congressional hearing, the chair of the NRC committee, Gerald North, refuted McIntyre’s interpretation of the NRC report. Using his professional knowledge and skills in this scientific specialty, he explained that “there is a carbon dioxide fertilization effect in some trees, but not in all the places where the samples used in the Mann et al. studies were taken.”

The NRC report elucidates the bristlecone controversy: Data derived from strip-bark samples are not always useful as temperature proxies because the increase of carbon dioxide weakens their utility to reconstruct temperatures accurately. Since atmospheric carbon dioxide concentrations increased significantly after 1850, “other studies [appropriately] … rely on [bristlecone] strip-bark pine records only … to infer past temperatures prior to 1850.” In other words, the NRC report did not recommend a wholesale removal of the bristlecone proxy, as McIntyre had argued. Furthermore, MBH’s data did not include all the samples subject to the carbon dioxide fertilization effect. Thus, for the MBH research, bristlecone proxies were not necessarily a problem.

These differences demonstrated how professional knowledge and skills differ widely within the general classification of scientists. Given these differences, scientists working on an interdisciplinary project should consider the divisions of labor among the researchers.

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278 McIntyre, 2006, p. 680.
279 McIntyre, 2006, p. 680; see also McIntyre, 2006, p. 685 for an illustration of the statistical graph output.
280 North, 2006, p. 590.
281 North, 2006, p. 590.
Accordingly, while the Wegman team repeatedly argued that statisticians should be included in statistically driven temperature reconstructions as the experts for the statistical methodology and analysis, McIntyre’s misunderstanding illustrated the need for climate researchers to participate in the assessments of climate research involving proxies.

Besides data treatment differences, misunderstandings also arose about scientific language. While the government challengers relied on the criticisms of M&M and Wegman about MBH’s alleged lack of understanding of scientific language as further justification to discredit MBH’s research, MBH and their supporters explained how the misunderstanding actually rested with M&M and Wegman, not MBH. For example, when Wegman stated that he was particularly troubled by some of the words and phrases that MBH used, such as “statistical skill,” Mann explained that the term is used in the American Meteorological Society, which “considers it such an important term in the context of statistical weather forecasting verification.”²⁸² Using the American Meteorological Society definition, Mann read into the Congressional Record that statistical skill is a “statistical evaluation of the accuracy of forecasts or the effectiveness of detection techniques.”²⁸³ Emphasizing the term’s basic application in meteorology, Mann explained that the “skill score is useful for evaluating predictions of temperatures, pressures, et cetera, et cetera, so I was very surprised by that statement.”²⁸⁴ In short, Mann’s response to Wegman’s discounting of specialized language illustrates the value of expertise and the exercise of professional knowledge and skills that may be unique to certain scientific disciplines, such as climate scientists.

²⁸² Mann, 2006, p. 720.
²⁸³ Mann, 2006, p. 720.
²⁸⁴ Mann, 2006, p. 720.
External Factors

Actors outside of the academic community, who had legal authority and public influence, helped MBH exert control over their research. MBH and their supporters came to rely heavily on political allies to reframe the government challengers’ actions, so observers would discount the challenges as political interferences, not public scrutiny of questionable research. In addition, MBH and their supporters publicly disseminated scientific findings that essentially validated MBH’s research and countered the challengers’ claims.

Gaining Support from Political Allies

Several political allies supported MBH.285 One of these allies, U.S. Representative Sherwood Boehlert (New York, Republican), drew the most media attention because of his party affiliation, strong language opposing the Barton-Whitfield inquiry, and recommendation to use a recognized scientific assessment process known as the National Research Council. On July 14, 2005, Boehlert wrote to fellow Republicans Barton and Whitfield to express his “strenuous objections” to what he viewed as their “misguided and illegitimate investigation” into MBH’s research.286 Boehlert argued that Barton’s Committee on Energy and Commerce lacked jurisdiction over this matter. According to Boehlert, “Both the National Science Foundation [“NSF”] and climate change research are under the purview of the House Committee on

Jul. 23, 2005: Inslee, J., & Schakowsky, J. (2005, Jul. 23), [Letter from U.S. Representatives Jay Inslee (Washington, Democrat) and Jan Schakowsky (Illinois, Democrat) to U.S. Representative Joe Barton]. “We are very concerned that the tone of your letters indicates that these were not requests for hearing background materials, but rather an attempt to intimidate Dr. Michael Mann, and discredit peer reviewed scientific research” (Inslee & Schakowsky, 2005, paragraph 1).
286 Boehlert, 2005, paragraph 1.
At the time, Boehlert chaired the House Committee on Science, and he believed that the matter fell within his committee’s jurisdiction. While Boehlert’s jurisdictional claim failed, it demonstrated the efforts of political allies to identify means within their legal authority that could be used to help MBH.

Further, while Boehlert noted that the first issue might not stop the investigation, he wrote that his main concern was Barton and Whitfield’s use of a political forum rather than a scientific one to investigate this matter. Boehlert pointed out that Barton and Whitfield opted not to follow traditional processes to “understand scientific disputes that impinge on public policy.” According to Boehlert, Congress typically holds “hearings with a balanced set of witnesses, briefings with scientists, and requests for reviews by the National Academy of Sciences or other experts.” Boehlert accused Barton of taking a “decidedly different approach – one that breaks precedent and raises the specter of politicians opening investigations against any scientist who reaches a conclusion that makes the political elite uncomfortable.” Boehlert also highlighted how MBH complied with the NSF guidelines on data sharing and participated in debates within the scientific community. Thus, he professed not to understand why Barton had requested the investigation. According to Boehlert, “[t]he investigation is not needed to gain access to data. The investigation is not needed to get balanced information on a scientific debate. The investigation is not needed to prompt scientific discussion of an important issue.” Instead, Boehlert characterized the purpose of Barton’s investigation as actions “to intimidate scientists

287 Boehlert, 2005, paragraph 2.
289 Boehlert, 2005.
290 Boehlert, 2005, paragraph 5.
rather than learn from them, and to substitute Congressional political review for scientific peer review."\textsuperscript{292}

In short, Boehlert’s and other political allies’ statements reframed Barton and Whitfield’s investigation. While Barton’s aides had previously described their inquiry as an “honest effort to learn about climate change,”\textsuperscript{293} the political allies of MBH framed the inquiry as Barton-Whitfield’s bullying and intimidation tactics to silence MBH and scientific work on human-induced global warming.\textsuperscript{294} Thus, the political allies informed the public of what they believed was taking place: unjustified congressional challenges of MBH’s research masked as sincere attempts to investigate the studies.

Similarly, at the Barton-Whitfield hearing in July 2006, Democrats who sat on the subcommittee framed the hearing as a disingenuous attempt to examine the science behind global warming. Several of the subcommittee Democrats characterized this hearing as yet another example of federal actors’ intervening or downplaying harms to the environment. Representative Tammy Baldwin (Wisconsin, Democrat) described several instances in which the Republican presidential administration allegedly altered government documents to minimize what government scientists were terming the effects of global warming.\textsuperscript{295} She described how “[i]n 2003, the EPA was ordered by the White House to delete critical sections relating to

\begin{footnotes}
\footnotetext[292]{Boehlert, 2005, paragraph 3.}
\footnotetext[295]{Baldwin, 2006.}
\end{footnotes}

climate change from its report on the environment.”

She also noted that “[i]n 2005, the White House insisted upon weakening language relating to the impact of global climate change in a document that served as the basis of negotiations during the G8 Summit.”

Baldwin posited that this hearing appeared to reflect a continued pattern of political interference, led by the presidential administration, in which government actors censor “sound science … in order to maintain a political agenda.”

Representative Jay Inslee (Washington, Democrat) believed that Congressmen Barton and Whitfield sought to replicate the tobacco industry’s strategy of sowing doubt about the science rather than actually examining the science. Inslee displayed a slide with the message “Our Product is Doubt!” and a tobacco industry ad stating, “Not one single case of throat irritation due to smoking Camels!”

Inslee reported scientific findings that excessive release of CO₂ in the atmosphere had already contributed to “acidifying the oceans,” which had in turn disrupted the ecosystem’s food chain. Inslee believed that Congress had misdirected its attention. “[I]nstead of really engaging Congressional talent in figuring out how to deal with this problem, we try to poke little pinholes in one particular statistical conclusion of one particular study where the overwhelming evidence is that we have to act to deal with this global challenge.”

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296 Baldwin, 2006, p. 32.
297 Baldwin, 2006, p. 32.
298 Baldwin, 2006, p. 32.
301 Inslee, 2006, p. 11.
302 Inslee, 2006, p. 18.
need to deal with technology rather than statistical recreations of the tobacco industry’s effort to create doubt.”

In sum, MBH political allies such as Baldwin and Inslee reframed the Barton-Whitfield inquiry as a forum for behaviors that resembled past instances of government interference in scientific work. They pointed out that the Barton-Whitfield investigation represented efforts to censor research, create doubt, and dismiss studies that ran counter to the challengers’ political interests – all tactics of a distasteful past. To avoid repeating the past, the MBH political allies sought to inform the public of these alleged political interferences, which were disguised as inquiries into MBH’s 1998 and 1999 research studies.

Disseminating Climate Science to the Public

Throughout the controversy, MBH and several of their supporters actively participated in venues where they might engage the broader audience of environmentalists, policymakers, media, scientists, and other observers. Notably, in December 2004, one group of climate scientists, including Michael Mann and Ray Bradley of MBH, established a blog, www.RealClimate.org. These scientists used the blog to comment on opposing research,

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303 Inslee, 2006, p. 18.
305 MBH and their supporters used various means to disseminate information to the public, especially news media venues, which I discuss, below, in the Summary & Conclusions section. They also participated in various blogs such as Climate Progress, Desmogblog, Prometheus, and the Heat is Online. I mention realclimate.org as the most notable one because the Wegman team criticized it in the Wegman report and at the July 2006 congressional hearing as a strategy that MBH and their supporters used to disseminate information. The Wegman team, however, characterizes the information on that blog as misinformation and an effort to bias the public. Yet, scientists and other members of the public, including reporters, used the blog as a source of news to receive updates on the government challenges to MBH’s federally sponsored research and to read about developments on climate science.
clarify their research, and highlight supporting research. According to Mann, the purpose of the blog was “to provide a rapid response to developing stories and provide the scientific context that’s often missing in the media coverage.” Mann commented, “We’re not circumventing the peer-review process, as some have claimed, we’re simply trying to provide the context of what existing peer-reviewed science has to say about certain issues.”

Mann believed that the blog helped support the three scientists’ work. As evidence, he indicated that because of the blog, “[w]e’ve received good media coverage, and have had over 200,000 visitors [between December 2004 and April 2005].” That led him to conclude that the blog seemed “to be serving the purpose that we intended, which is to provide a resource for people looking for an honest broker for stories like the hockey stick attacks and State of Fear, both of which are riddled with fallacies.”

Now that I have demonstrated how political allies reframed the challenges and MBH and their supporters disseminated information in a light favorable to MBH, in the next subsection, I discuss how climate scientists and the science itself played a role in MBH’s ability to exert control over their research.

Both Internal Characteristics and External Factors

MBH and their supporters relied on the science as a strategy to address the challenges and to enable MBH to exert control over their federally funded research. This reliance on the science drew on both internal characteristics and external factors of the academic profession,

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308 Kancler, 2005, paragraph 65.
309 Kancler, 2005, paragraph 65.
specifically, the profession’s exercise of professional knowledge and skills (an internal characteristic) and its reliance on bodies of knowledge (an independent external factor).

First, in defending the hockey stick research, MBH, along with several climate scientists, used their *professional knowledge and skills* to explain the data and the data treatment to overcome critics’ complaints about their alleged errors. Two studies illustrate this claim. In 2005, Scott Rutherford, then a marine research scientist at the University of Rhode Island’s Graduate School of Oceanography, along with six other authors including MBH, cowrote an article that examined the effects of different methodologies on temperature reconstructions.\(^{310}\) They compared reconstructions, including MBH98, to determine the effects of the various methods. They concluded that the “proxy-based temperature reconstructions are robust with respect to a wide array of alternative statistical approaches,” thus validating MBH’s claims of “exceptional late-twentieth century warmth in the context of the period since A.D. 1400 (in warm, cold, and annual temperatures).”\(^{311}\) In addition, Eugene Wahl and Caspar Ammann’s 2006 research project also replicated MBH’s statistical methodology, but they went further than Rutherford, recalibrating the data over the entire period.\(^{312}\) They found essentially the same results. As they described it, their output data showed “hardly any effect on the ‘hockey stick’ shape of the original MBH reconstruction, at most adding a small ‘knob’ to the ‘stick’ in the early 15\(^{th}\) century.”\(^{313}\) Thus, they argued that there was “no evidence for removing the MBH

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\(^{311}\) Rutherford et al., 2005, p. 2326.


\(^{313}\) Wahl & Ammann, 2007, p. 58.

Northern Hemisphere temperature reconstruction from the list of important climate reconstructions of the past six centuries, on the basis of alleged ‘flaws’ in its use of proxy data or underlying methodology.”  

Relying on the science, which this dissertation’s theoretical framework places under bodies of knowledge, these two studies scientifically verify the legitimacy of MBH’s research. In fact, these studies were cited in subsequent discussions in the National Research Council report and in the Intergovernmental Panel on Climate Change’s 2007 report (“IPCC2007”) as reliable indicators of the current state of climate change. For example, in Chapter 6 of the IPCC2007 report (i.e., the Fourth Assessment Report), the paleoclimate chapter authors even noted, in a scientific manner, the problems with M&M’s reconstruction and the significance of the Wahl and Ammann (2007) article in understanding the temperature reconstruction procedures:

McIntyre and McKitrick (2003) reported that they were unable to replicate the results of Mann et al. (1998). Wahl and Ammann (2007) showed that this was a consequence of differences in the way McIntyre and McKitrick (2003) had implemented the method of Mann et al. (1998) and that the original reconstruction could be closely duplicated using the original proxy data.

Thus, the interaction between applying professional knowledge and skills and the bodies of knowledge that emerge from the uncontrolled outcome of science largely supported MBH’s research.

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316 Jansen, et al., 2007, p. 466.
In addition, the academic profession and other supporters used the NRC report as further scientific evidence to validate the MBH research.\footnote{The NRC panel generally confirmed MBH’s findings. Most significantly, it concluded “with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period during the preceding four centuries” (NRC, 2006, p. 112). The report, however, also revealed “[l]ess confidence” regarding the temperature reconstructions between A.D. 900 to 1600 and “[v]ery little confidence” to reconstructions before A.D. 900 (NRC, 2006, p. 112). The panel explained that the data sources from these earlier periods presented reliability problems, yet they made clear that “[d]espite these limitations, the committee finds that efforts to reconstruct temperature histories for broad geographic regions using multiproxy methods are an important contribution to climate research and that these large-scale surface temperature reconstructions contain meaningful climatic signals” (NRC, 2006, p. 3).} As discussed earlier, the NRC committee concluded “with a high level of confidence that global mean surface temperature was higher during the last few decades of the 20th century than during any comparable period.”\footnote{NRC, 2006, p. 3.} The bodies of knowledge could not fully support MBH’s claim that the 1990s were the warmest decade within the prior 2,000 years; however, the NRC chair, Gerald North, explained that the MBH assertion was plausible, though the MBH evidence and subsequent studies failed to support that claim with a high degree of certainty.

Further, while both sides agreed that MBH employed statistical procedures that would raise concerns of statisticians, the two sides disagreed about the effect of the statistical procedures. Wegman indicated that the statistical errors showed reliance on an incorrect methodology. In his testimony, he expressed that “if something is wrong with this piece of work, it ought to be discarded as a policy tool.”\footnote{Wegman, 2006, p. 91.} The NRC committee chair, Gerald North, cast the statistical procedures as suboptimal in his professional opinion, but he believed that MBH’s claims had some merit. North testified that “the criticisms don’t mean that the MBH...
claims were wrong. They just mean that the MBH claims are not convincing by themselves.”

He explained that the MBH claims should be viewed as a piece of a larger body of knowledge, “[s]o if you pull together other information, then that does change the view a bit[,]” as one recognizes how research builds and continuously adds more knowledge about a subject matter.

He warned the congressional subcommittee and other members of the audience “to be careful here and not throw the baby out with the water,” inasmuch as MBH’s research (i.e., MBH98 and MBH99), though not perfectly executed, had been largely confirmed through subsequent studies.

Thomas Crowley, the Nicholas Professor of Earth Systems Science at Duke University, and Hans von Storch, the Director of the Institute for Coastal Research at the GKSS Research Centre in Geesthacht, Germany, also testified that the effect of MBH’s statistical procedural choices had little effect on the outcome.

Thus, North opined that “while the techniques used in the original Mann et al. papers may have been slightly flawed, the work was the first of its kind and deserves considerable credit for moving the field of paleoclimate research forward[,]” especially since “the main conclusions of the Mann et al. studies have been supported by subsequent research.”

320 North, 2006, 60.


321 North, 2006, 60.

322 North, 2006, 62.

323 The GKSS Research Centre is a nonprofit, independent research organization funded by the German Hermann von Helmholtz Association. Although it is not housed within a university, it operates much like a German university research center.


Summary of the Responses

MBH and their supporters employed three principal strategies. First, drawing on the internal characteristics of the academic profession, they asserted academic norms to guide the process and content of the scientific reviews. Second, they also counted on political allies and information dissemination methods, such as blogs, to frame the situation in a negative manner for the challengers and a positive manner for MBH. Third, MBH and their supporters used the science, which integrated the profession’s exercise of professional knowledge and skills (an internal characteristic) and the bodies of knowledge (an independent external factor) as a strategy to help MBH exert control over their research. By relying on these strategies that drew from the internal and external characteristics of the academic profession, MBH was able to exert control over its research and to maintain its intellectual freedom.

Summary & Conclusions

In this chapter, I explored the challenges to two federally funded research studies and the strategic responses that aided the authors in exerting control over their research. The controversy originated from a 2001 Intergovernmental Panel on Climate Change (“IPCC”) report. The IPCC report concluded that humans were significantly contributing to rising global temperatures. Among its evidence, the report inserted a prominently displayed graph of temperature increases, which resembled the shape of a hockey stick. The graph was connected to two federally sponsored research studies published in 1998 and 1999 by Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes (“MBH”).326 The distinctive look of the graph became the iconic representation of this claim, though many skeptics of global warming questioned its accuracy.

326 The hockey stick was connected with these two studies; however, it appeared only in MBH’s 1999 article.
Reconstructing the interactions among the primary actors in the conflict as a dramatic play, I recounted the circumstances and events surrounding the challenges to the studies. Initially, Stephen McIntyre and Ross McKitrick (“M&M”), two scientists with backgrounds in mathematics and statistics, presented a series of challenges to MBH’s research. They claimed that MBH had problems with data treatment and statistical methodology. They also accused MBH of not providing them access to the data and of possibly participating in a closed network that continued to use unverified data and shun outsiders who questioned their works.

Congressmen Barton and Whitfield then took several steps to challenge MBH’s research, resting their inquiry on the evidentiary foundation of M&M’s scientific criticisms and those two scientists’ contentions of related problems. They initiated a government investigation requesting volumes of data within a short period of time. They coordinated the inquiry rather than turning the matter over to scientists or using other academic mechanisms. They commissioned a small team of statisticians, instead of tasking other full-service scientific entities with a broader range of skills, such as the National Research Council, to inquire into the controversy. Then, finally, they called for a congressional hearing to further contest the MBH studies and other scientists’ arguments that had been made in MBH’s defense.

In response to the challenges, MBH and their supporters employed three significant strategies, which emerged when I applied a modified version of Freidson’s model of the profession. Drawing on the model, I divided the responses of MBH and their supporters into events that relied heavily on (a) internal characteristics, (b) external factors, and (c) both internal characteristics and external factors of the academic profession. That led me to uncover several strategies that MBH and their supporters used.
Under the internal characteristics frame, MBH and their supporters called on academic norms to guide the process and content of the scientific reviews. Professional associations and groups of scientists participated in a letter-writing campaign to object to the political review in lieu of a scientific review. Further, climate scientists, including MBH, highlighted the significance of the academic norms regarding professional expertise and divisions of labor. They demonstrated how scientists who had challenged their studies had problems with interpretation or application of highly technical language used within the climate research community, because they lacked expertise in this area of the profession. Under the external factors frame, MBH and their supporters relied on individuals and groups outside of the academic profession for assistance in communicating their positions. In particular, MBH and their supporters counted on political allies and information dissemination methods such as blogs to frame the challenges in a negative manner and to present MBH and their supporters in a positive manner. Other actions combining internal and external factors of the academic profession and using the science and professional expertise also served as strategies that helped MBH exert control over their research. Under this frame, scientists demonstrate the value that derives when knowledge builds on the research of others, and MBH did just that. They initiated one of the first large-scale multiproxy studies, and their conclusions were largely confirmed from many studies that followed.

Finally, I comment on two other observations and tie them back to my theoretical framework. First, this chapter demonstrated how politicians challenged and supported the MBH research. Second, I also showed how media reports of the controversy had mixed effects. I noted instances in which the media negatively spun the situation for MBH as well as instances where their research or supporting evidence was positively framed. This observation is
consistent with my theoretical framework, which is a modified version of Freidson’s theory of the professions. The framework suggests that external factors, such as individuals with legal authority and sources for public dissemination of information, can moderate, positively and negatively, the extent to which a profession can exert control over its work. Thus, while we recognize the contributions of internal characteristics, external factors, and the blend of both internal characteristics and external factors to help the academic scientists exert control over their research, we acknowledge that the external factors also have the capacity to limit or even eliminate the extent and nature in which the academic scientists can actually exercise their intellectual freedom.

<table>
<thead>
<tr>
<th>Table 6.2: Acronyms for the Climate Change Case</th>
</tr>
</thead>
<tbody>
<tr>
<td>IPCC = Intergovernmental Panel on Climate Change</td>
</tr>
<tr>
<td>M&amp;M = Stephen McIntyre and Ross McKitrick</td>
</tr>
<tr>
<td>MBH = Michael E. Mann, Raymond S. Bradley, and Malcolm K. Hughes</td>
</tr>
<tr>
<td>NAS = National Academy of Sciences</td>
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<tr>
<td>NRC = National Research Council, an arm of the National Academy of Sciences</td>
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<td>NSF = National Science Foundation</td>
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</table>
### Table 6.3: Timeline of Relevant Actions and Events in the Climate Change Case

<table>
<thead>
<tr>
<th>DATE</th>
<th>ACTION/EVENT</th>
</tr>
</thead>
<tbody>
<tr>
<td>1995-2006</td>
<td>Prior to the hockey stick hearing, the House Energy &amp; Commerce Committee holds only one other hearing about climate change.</td>
</tr>
<tr>
<td>2001</td>
<td>The United Nations Intergovernmental Panel on Climate Change (“IPCC”) issues its Third Assessment Report (“IPCC2001”). Among its citations supporting human-induced global warming, it includes MBH98 and MBH99 as well as prominently displaying the hockey stick graph.</td>
</tr>
<tr>
<td>Apr.-Sept. 2003</td>
<td>M&amp;M request multiple data; Mann and associates reply.</td>
</tr>
<tr>
<td>Sept. 25, 2003</td>
<td>After numerous e-mail exchanges, Mann informs McIntyre that he is unable to respond to future requests.</td>
</tr>
<tr>
<td>Jul. 1, 2004</td>
<td>MBH correct MBH98 with a corrigendum statement.</td>
</tr>
<tr>
<td>Dec. 2004</td>
<td>Several climate scientists, including Michael Mann and Ray Bradley (i.e., M and B of MBH), launch Realclimate blog.</td>
</tr>
<tr>
<td>Jan. 31, 2005</td>
<td>M&amp;M along with others launch Climateaudit blog.</td>
</tr>
<tr>
<td>Feb. 14, 2005</td>
<td>A <em>Wall Street Journal</em> article highlights M&amp;M’s criticisms about MBH’s research and problems gaining access to the data.</td>
</tr>
<tr>
<td>Mar.-May 2005</td>
<td>M&amp;M continue to present their criticisms of MBH’s research. They also raise questions about the independence of peer reviewers and the closed-network environment of the climate science community.</td>
</tr>
<tr>
<td>Jun. 23, 2005</td>
<td>U.S. Representatives Joe Barton and Ed Whitfield send letters to MBH, the IPCC chair, and the NSF director. The letters request various documents regarding the challenged research as well as other past studies and materials.</td>
</tr>
<tr>
<td>Jul. 1-23, 2005</td>
<td>Several U.S. Representatives serving on the House Energy &amp; Commerce Committee write to Barton and Whitfield to condemn their actions as political meddling in matters requiring scientific expertise.</td>
</tr>
<tr>
<td>Jul. 13-Aug. 25, 2005</td>
<td>Several professional associations representing climate scientists, such as the American Association for the Advancement of Science, American Meteorological Society, American Geophysical Union, and numerous climate scientists write letters to Barton and Whitfield to express concerns about the MBH investigation.</td>
</tr>
<tr>
<td>Jul. 14, 2005</td>
<td>U.S. Representative Sherwood L. Boehlert writes to his fellow Republicans, Barton and Whitfield, to express his concerns about the investigation. He recommends the National Research Council (“NRC”) as appropriate experts to evaluate the science, rather than members of Congress.</td>
</tr>
<tr>
<td>Sept. 2005</td>
<td>Barton and Whitfield commission the Wegman team to evaluate MBH’s research.</td>
</tr>
<tr>
<td>Nov. 2005</td>
<td>Boehlert commissions the NRC to evaluate MBH’s research.</td>
</tr>
<tr>
<td>Jun. 22, 2006</td>
<td>NRC releases its report. The committee concludes that an “array of evidence” supports MBH’s claims.</td>
</tr>
<tr>
<td>Jul. 14, 2006</td>
<td>Wegman team issues its report. The Wegman team focuses on the multiple problems with the MBH research.</td>
</tr>
<tr>
<td>Jul. 19, 2006</td>
<td>House Energy and Commerce’s Subcommittee on Oversight and Investigations conducts the first day of the <em>Questions Surrounding the “Hockey Stick” Temperature Studies: Implications for Climate Change Assessments</em> hearing.</td>
</tr>
<tr>
<td>Jul. 26, 2006</td>
<td>The Subcommittee on Oversight and Investigations holds its second (and last) day of the <em>Questions Surrounding the “Hockey Stick” Temperature Studies: Implications for Climate Change Assessments</em> hearing.</td>
</tr>
<tr>
<td>Date</td>
<td>Event</td>
</tr>
<tr>
<td>---------------</td>
<td>----------------------------------------------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>Sept. 4, 2006</td>
<td>Democrats on the House Energy and Commerce Committee use Republican quotes to request a hearing on climate change.</td>
</tr>
<tr>
<td>Oct. 30, 2006</td>
<td>Economist Nicholas Stern issues the Stern Review shifting the debate toward economic issues of climate change.</td>
</tr>
<tr>
<td>Feb. 2, 2007</td>
<td>The first of several reports of the IPCC Fourth Assessment (IPCC2007) becomes available and largely supports MBH’s research, but it relies on other, new research.</td>
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CHAPTER VII: THE SALVAGE LOGGING CASE

Introduction

This chapter presents the findings of the salvage logging case, the third and final case of this project. First, I provide a brief overview of the science and the public policy debate about salvage logging. I also introduce the academic scientists’ federally sponsored study, which several groups challenged. Second, I describe the challenges that several groups raised about the study at issue and against its researchers. Highlighting the circumstances and events surrounding the challenge, I capture the interactions among individuals and groups involved to explain the actors’ reported perceptions and interests. Third, I present my data on the strategies that academic scientists who supported the challenged study employed. I explain, below, how these strategies helped the challenged researchers exert control over their federally sponsored work. Specifically, drawing on my theoretical framework (Chapter 3), I examine the internal characteristics of and external factors influencing the academic profession to help us understand how certain responses from members of the academic profession and their allies aided the researchers of the contested study. Fourth, I summarize the key findings.

The Science, the Public Policy Debate, and the Study

The Controversial Science

Post-fire salvage logging is the practice of cutting down (i.e., “felling”) trees from areas severely burnt by wildfires, then replanting trees and other vegetation species that had been present prior to the fire. It is a controversial post-fire forest-recovery approach. Many academic scientists, environmental advocacy groups, foresters, and citizens in the Northwest region of the United States have debated whether salvage logging helps or does more harm to the environment after a wildfire.
Opponents of salvage logging, especially environmental groups such as Save America’s Forests, Sequoia Forest Keeper, and the Sierra Club, have argued that this forest-recovery approach oftentimes harms the fire-damaged forest even more. They claim that salvage logging disrupts the area and places ecological stresses on the land. For instance, when the timber companies enter the land, they typically pave a path in previously roadless areas so their large equipment can enter. Their presence also harms the ground seedlings so they cannot grow naturally. In addition, as Robert Beschta, a professor emeritus in forest ecosystems at Oregon State University, and his coauthors have noted, several academic scientists have observed that “the removal of standing and downed large wood may eliminate important structural components for the recovery of terrestrial and aquatic systems.” In particular, the elimination of the trees alters the wildlife habitat. Further, salvage logging opponents have cited examples in which timber companies logged an area, but either the federal government or the contracted party failed to reforest the area and clean up the logging slash.

Supporters of salvage logging present a very different view of this post-fire forest-recovery approach. They suggest that salvage logging restores the land more efficiently and addresses harms that arise from the so-called natural alternatives, which advocate for little to no human intervention. Sarah Gilman of the High Country News, an eco-friendly newspaper based in the Northwest region of the United States, captured the often-cited distinctions between salvage logging and the natural approaches to forest recovery. Gilman quoted John Fertig, a forester for the land impacted by the 2002 Biscuit Fire in southern Oregon and northern California, explaining that “natural regeneration is unpredictable and could take up to 200 years.

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Active salvage, replanting and thinning can speed up the process to 150 years.”

In addition, Gilman and others have reported that many foresters and timber industry representatives, along with some academic scientists have claimed that natural forest recovery efforts actually contribute more harm than salvage logging does when it comes to wildlife and plant regeneration goals. According to this perspective, the natural approaches often lead to the entry of new insects seeking the charred areas. The insects also stunt and at times prevent tree and vegetation growth. Also, the natural approaches often leave behind shrubs and other fire-fueling materials, which have contributed to subsequent forest fires.

These debates tend to generate strong feelings for individuals on each side of the issue. Describing these emotionally driven debates, Melissa Block of National Public Radio reported that “[i]n some parts of the American West, you can start a bar fight by mentioning [the] forestry practice called salvage logging.”

The Science Policy

In July 2002, one of the worst forest fires in U.S. history damaged nearly half of the Rogue River-Siskiyou National Forest, or approximately 500,000 acres of federal forestlands in southern Oregon and the northern portions of California. Known as the Biscuit Fire, this forest fire took approximately five months to contain, with suppression costs calculated as totaling around $153 million.

During the fire containment period in late 2002, the U.S. Forest Service recommended immediate recovery efforts to regenerate the forest. Its most controversial

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329 Gilman, 2006, p. 3.
331 In comparison to the 2007 Witch Creek fire in San Diego, one of the largest fires in Southern California in recent history, the Biscuit Fire covered 2.5 times greater land area and took over six more months to contain, in part because the forests fueled the fire. The largest wildfire in U.S. forest history, a 1910 fire referred to as the Great Fire and Big Burn, scorched sections of ten national forests and other land – totaling approximately 3 million acres within Washington, Idaho, and Montana. News reports that highlighted the 100-year anniversary also included references to the Biscuit Fire as one of the most severe wildfires in the United States within the last decade.
recommendation was proposing post-fire salvage logging, an approach that the Forest Service argued was the most practical and scientifically sound.

Initially, in the early part of 2003, the Forest Service planned on sales of 372 million board feet, but it revisited that idea after the release of a July 2003 report. Commissioned to examine the salvage logging capacity over time, John Sessions, a senior academic scientist at Oregon State University’s College of Forestry, and his team of researchers concluded that as much as “2.0 billion board feet … [was] economically salvageable.” Thus, the report (i.e., the Sessions Report) calculated logging potential to be more than 537% of the initial Forest Service estimates. The Sessions Report also noted that delays in logging would only reduce logging production potential, because delays would increase the number of trees deemed unsalvageable because of rotting. In turn, reductions in logging would decrease revenue predictions and possibly harm the reinvestment potential of the burnt forest. The Sessions Report advocated for immediate and aggressive actions, including helicopter logging, to avoid the time and energy needed to pave roads and overcome other logistical hurdles.

The Sessions Report sparked even more debate and angered opponents of salvage logging. Eventually, the opponents escalated their protest activities through sit-ins that barricaded the logging equipment pathway, along with hunger strikes to express their willingness to take extreme measures. In addition, opponents sought several injunctions to stop the

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333 Sessions, Buckman, Newton, & Hamann, 2003, p. 53

334 By avoiding the need to pave roads, helicopter logging activities place less stress on the land and potentially overcome a Department of Interior regulation that prohibits logging activities under a Roadless Area Conservation Rule.

While the court cases generally permitted the logging to continue, the legal decisions rested on the sufficiency and compliance of the environmental assessments, the environmental impact reports, and the approved Biscuit Fire Recovery Project statements.

Several members of Congress observed that existing federal laws and regulations made the reporting and approval processes for salvage logging much too cumbersome. Consequently, during the 2005-2006 congressional session, several members of the U.S. House of Representatives proposed a new bill to streamline this process. Congressman Greg Walden (Oregon, Republican) principally authored this new House bill, The Forest Emergency Recovery and Research Act (“FERRA”). The bill would relax procedural approvals for forest recovery efforts, including salvage logging.

Conflict Between the Federally Sponsored Study & the Science Policy

In 2003, the Bureau of Land Management (“BLM”) awarded a three-year grant totaling $307,000 to Oregon State University (“OSU”). The Co-Principal Investigators (“Co-PIs”), W. Douglas Robinson, J. Boone Kauffman, and Beverly Law, were academic scientists employed at OSU’s College of Forestry (“CoF”). To assist in the research, the Co-PIs added another CoF academic scientist, John Campbell, and two CoF graduate students, Dan Donato and Joe Fontaine. The team started the research soon after the grant award. The research project was to

338 Officially, the Joint Fire Science Program, a federal interagency unit that helps administer funding for research projects, awarded the grant on behalf of the BLM.
examine the effects of grass seeding and salvage logging within a section of the Siskiyou National Forest in southwestern Oregon. This project location represented a portion of the land struck by the Biscuit Fire.

On January 5, 2006, Science, one of the leading journals for academic scientists, published the team’s initial study findings on its online forum, Science Express. The study, which identified Dan Donato as its lead author, reported that post-fire salvage logging actually harmed forest regeneration. Using data collected from the area burned by the Biscuit Fire, the researchers compared land conditions from two locations – where salvage logging had and had not taken place. Based on this data, the non-invaded areas displayed greater regenerative signs. Given that difference between the main effect and control group, the Donato team concluded: “Our data show that postfire logging, by removing naturally seeded conifers and increasing surface fuel loads, can be counterproductive to goals of forest regeneration and fuel reduction.”

The Donato team findings became the center of this case controversy. First, the findings debunked the “widely held and commonly cited … view that postfire (salvage) logging diminishes fire risk via fuel reduction, and that forests will not adequately regenerate without intervention that includes logging and planting.” Second, many interpreted the findings to be in direct conflict with Congressman Walden’s proposed legislation. In fact, given the study’s findings, a Science staff member posted a reference to Walden’s FERRA bill beside the online article. When the Donato team discovered that editorial addition referencing the bill, the team

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Scienceexpress is also known as Science Express.


340 Donato et al., 2006a, p. 1.
asked for its removal. The Science staff agreed to remove any mention of the FERRA bill in the print version of the article, which would appear later that month. Unfortunately for the Donato team, the political damage had already been done. Numerous supporters of FERRA either saw or heard about the study’s placement beside information referencing the bill, and that placement gave the appearance that the study findings directly contradicted the public policy rationale justifying FERRA. Not surprisingly, that led to an uproar within the communities representing federal policy, academic science, the timber industry, and forestry along with several challenges against the Donato team and its study. These critics challenged the Donato study and attempted to challenge the authors for their alleged scientific errors, omission of specific environmental factors for the area examined, and perceived political motives to block the FERRA bill from passing.

**Interactions Surrounding the Challenges**

To describe these challenges and the circumstances and events surrounding them, I present the interactions among the key actors. Indeed, many policymakers, foresters, environmental groups, and timber industry representatives in some way challenged the Donato team and its study. However, rather than detailing each interaction and listing all the actors, in this section, I focus on what I consider to be the significant challenges to the Donato study. That is, I draw attention only to the challenges that capture the primary actors and interactions involved in this case study. Like a play, the challenges appear in Acts. In this metaphorical play, there are two Acts. In Act 1, I describe how several academic scientists initiated the challenges against the authors of the contested federally sponsored research. This Act also reveals that these academic challengers worked behind the scenes with other influential actors. In Act 2, I discuss how governmental actors then assumed responsibility for challenging the
authors of the contested federally sponsored research. Together, these two Acts dramatize the multistage challenges against the Donato team and its federally sponsored research.

**Act 1: Academic Scientists Lay the Groundwork**

In Act 1, I draw attention to several academic scientists, who were the primary initiators of the Donato study challenges. To present the challenges, I divide the events into two major scenes. In Act 1, Scene 1, I introduce the CoF dean, Hal Salwasser, as the primary challenger. Salwasser attempted to minimize the study’s role in policymaking. As I discuss in Scene 1, he described the Donato study as a small research project that was insufficient to fully address the different policy options. He had hoped that framing the study in this manner would not harm Congressman Walden’s FERRA bill. In Act 1, Scene 2, I spotlight other CoF academic scientists, along with several U.S. Forest Service staff, who took a larger role in discrediting the Donato study. Scene 2 describes how this group made scientific and practice-based claims to challenge the Donato study. Throughout the explanation of both scenes, I also identify various nonacademic actors, who actively opposed and challenged the Donato study. As I will explain, however, these actors appeared as supporting cast to the academic challengers.

Now that I have provided a basic foundation for Act 1, I begin my data review with the first significant attempts to challenge the Donato study.

**Scene 1:**

Scene 1 captures irregularities in behavior that portend much more significant circumstances and events that would occur as challenges to the Donato study.341 To begin, the

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341 In April 2006, a state open-records request uncovered Salwasser’s January and February 2006 e-mails in which he engaged logging industry representatives and other individuals in forestry to assist him in downplaying the Donato study’s importance. Oregon Hearing regarding the Oregon State University College of Forestry’s involvement with the timber industry (Apr. 7, 2006) (Testimony of Hal Salwasser; Statements of Oregon State Senators Charlie Ringo - Beaverton, Democrat, and Frank Shields - Portland, Democrat).
Donato study received little praise from several senior members of the CoF.\textsuperscript{342} As noted earlier, an academic research team’s publication in Science represents one of the most distinguished scholarly achievements.\textsuperscript{343} In most cases, an academic unit would recognize the publication in Science by one of its research teams, since that is a highly visible and respectable journal. In this case, however, the CoF dean issued a memo to the college faculty and staff on January 11, 2006, within a week of the Science Express publication, whose message some recipients interpreted as an effort to downplay the Donato study.\textsuperscript{344}

According to the dean, his memo attempted to “contextualize the [Donato] study” so the CoF community would understand how the study fit into the larger body of literature.\textsuperscript{345} His memo contained three primary messages. First, he cautioned how the public might misinterpret the study’s significance. He wrote:

> Occasionally preliminary results or research results from a single study relevant to a controversial, highly visible issue become general public knowledge before the


\textsuperscript{343} Salwasser later recognized the accomplishment. He wrote: “Few faculty, let alone graduate students, get their work published in this prestigious journal” (Jan. 26, 2006, paragraph 3).

\textsuperscript{344} As I elaborate later in this section and later in this chapter, Oregon state legislators uncovered e-mails about Salwasser’s deliberate actions to downplay the Donato study. Reporting on the Donato study controversy, Kera Abraham (2006) described the dean’s action as follows: “Rather than congratulating his student on publication in the nation’s most rigorous science journal, Salwasser turned his attention to ‘damage control’” because the article did not support the timber industry’s interests (Abraham , 2006, p. 15). Abraham, K. (2006, Apr. 13). The battle of Biscuit. \textit{Eugene Weekly}, 25(5), 14-16.

\textsuperscript{345} Salwasser, Jan. 11, 2006, paragraph 2.

Salwasser, H. (2006, Jan. 11), [E-mail memo to CoF faculty and staff].
complete scientific process is through. This can intensify public discussion and positioning before adequate scientific debate and further research can confirm, modify, disprove or place the results in the appropriate context.346

Second, he suggested that the Donato study might not adequately address this politically charged topic, because the findings were too premature to make any policy conclusions. He referred to the study as a “single-study, short-term research,” and he advised that academic scientists needed to explore this issue further so the scientific debate could take place with the “full body of pertinent knowledge.”347 Third, given the need for additional research, Salwasser directed readers’ attention to two pending bills that could financially support these future research efforts. He wrote: “Only long-term, well designed, replicated field studies, such as … the post-fire research called for in Rep. Greg Walden’s Forest Ecosystem Recovery and Research Act (FERRA) and Sen. Gordon Smith’s Forests for Future Generations Act, will ultimately resolve the scientific issues.”348

While the dean characterized his intentions as a message to contextualize the Donato study, not everyone viewed the memo as simply an exercise in providing context. Some academic scientists, including faculty and graduate students at OSU, and numerous media outlets, such as Environment & Energy Daily, Eugene Weekly, The Oregonian, and Science, criticized the dean’s action of issuing the memo as an attempt to undermine the contributions of the study. According to a faculty review committee that investigated this incident and the others related to this controversy, a group of graduate students responded to the context memo by writing the dean to express their concerns about the CoF’s possible conflicts of interest. The graduate students’ letter referred to “public perceptions of bias toward the timber industry and

346 Salwasser, Jan. 11, 2006, paragraph 2.
347 Salwasser, Jan. 11, 2006, paragraph 3.
federal agencies, lack of support for graduate students conducting research on controversial or policy-relevant topics, abuse of power, and the politicization of science. \textsuperscript{349} Furthermore, the review committee observed that junior faculty feared what would happen to them if their research presented views contrary to the timber industry’s interests. Other Donato supporters also questioned whether Salwasser’s memo was intended to serve as a political endorsement of the pending legislation. For example, Susan Ash of the Audubon Society of Portland contested the issuance of the memo, suggesting it was Salwasser’s attempt at lobbying for the two pending bills. \textsuperscript{350}

These Donato team supporters pointed out that the dean’s message focused on the study’s limits, with references to the Donato study as a “single-study, short-term research,” while an informed scientific debate – per the dean’s memo – requires the “full body of pertinent knowledge.” \textsuperscript{351} Thomas Hinckley, professor of forest resources and former interim dean for the School of Forest Resources at the University of Washington, conveyed his opposition to Salwasser’s letter. He e-mailed Salwasser stating, “I am ashamed and embarrassed by the content and wording of your memo.” \textsuperscript{352} Further, several senior CoF faculty noted that the dean had never contextualized other CoF studies – particularly in a manner that speaks to a study’s limitations. \textsuperscript{353} Additionally, many who criticized the dean for his memo observed his failure to recognize the research team’s accomplishment of publishing in one of the highly regarded

\textsuperscript{350} Ash, S. (2006, Jan. 11), [E-mail correspondence from Susan Ash to Hal Salwasser].
\textsuperscript{351} Johnson et al., 2006.
\textsuperscript{352} Hinckley, T. (2006, Jan. 13), [E-mail correspondence from Thomas Hinckley to Hal Salwasser].
\textsuperscript{353} Johnson et al., 2006.
scientific journals. According to these critics, that academic achievement would have typically yielded praise rather than criticism.\textsuperscript{354}

Salwasser acknowledged that his January 11, 2006 memo was not well received within the CoF community. He stated: “I wrote a context piece that was interpreted by some as not being supportive of the scientists who published the article.”\textsuperscript{355} However, he indicated: “I support the freedom of all our faculty, students and staff to pursue their scholarly or scientific work.”\textsuperscript{356}

Despite Salwasser’s statements, numerous individuals who disapproved of the dean’s memo speculated on the reasons behind the dean’s attempts to minimize the policy effect of the Donato study. Some of them believed that the CoF constituents played a significant role in pressuring the dean to issue the memo. For example, Eleanor Vandergrift, an alumna and academic scientist who supported the Donato team, expressed her outrage at her alma mater for acting on behalf of the politicians and timber industry.\textsuperscript{357} She wrote, “It is appalling that political advocacy” plays a role in the CoF actions.”\textsuperscript{358} She criticized the CoF for supporting “studies that only show[ed] … increased logging is the best outcome after wildfire … when the research data does not agree with the sale of board feet and income to the college.”\textsuperscript{359} Michael Milstein, a journalist for \textit{The Oregonian}, asked Salwasser about the CoF’s industry ties. Speculating about the connections, after having interviewed several individuals about the dean’s memo, Milstein wrote:

\textsuperscript{354} Johnson et al., 2006.
\textsuperscript{355} Salwasser, Jan. 26, 2006, paragraph 2.
\textsuperscript{356} Salwasser, Jan. 26, 2006, paragraph 2.
\textsuperscript{357} Jan. 22, 2006: Vandergrift, E. V. H. (2006, Jan. 22), [E-mail correspondence from Eleanor Vandergrift to Hal Salwasser].
\textsuperscript{358} Vandergrift, Jan. 22, 2006.
\textsuperscript{359} Vandergrift, Jan. 22, 2006.
The question came up [in my interviews] about the legacy of the College’s relationship to the timber industry. Clearly you must be responsive to private forestry, since it contributes to funding. I sense that was what you were trying to do in your memo – addressing the flak. Has that relationship influenced what happened here?360

Salwasser responded, “We were very much concerned with reaction of [the] forest scientific community as industry and agencies.”361 However, he never indicated that they influenced his action to write the memo. Nonetheless, between January and March 2006, the dean’s critics speculated that the dean likely felt pressured to downplay the effect of the Donato study because these constituents had some interest in the salvage logging process (e.g., timber industry). As initial support of the critics’ hypothesis, Salwasser even recalled in a National Public Radio interview that “as soon as the press picked up the [Donato] article and it started showing up in the papers I had alums, people in forest agencies and people in the forest industry, and even some other scientists sending me e-mails saying now what’s going on here and why didn’t you guys stop the publication and all this.”362

However, in April 2006, the dean’s critics had more concrete evidence of their beliefs when Oregon State Senator Charlie Ringo obtained Salwasser’s e-mails through a public records request.363 Among Salwasser’s disclosed e-mail messages, the records showed that he consulted advocates of FERRA, which included certain senior faculty at OSU’s CoF, timber industry

363 Like most states, Oregon has a statutory requirement that individuals and organizations may obtain copies of public records for a nominal fee. These public records include state employee e-mails discussing matters in pursuit of a policy decision, such as Salwasser’s e-mails dealing with the Donato study.
representatives, and forest service staff in order to strategize on how to “do damage control.”

These e-mails represented Salwasser’s efforts to minimize the influence of the Donato study on forest policy and manage the rising discontent of CoF constituents who believed that the Donato study harmed their interests. Reporters for national outlets and various critics of the dean’s memo also suggested that the CoF had a financial interest in downplaying the Donato study and supporting salvage logging efforts. These reporters and critics noted that Oregon had a timber tax, which at the time generated about 10% of the CoF’s annual revenues. Their contention was that if FERRA passed, an aggressive logging process would likely occur, and that would generate significantly more revenue for OSU’s CoF. In short, the dean’s memo represented an initial challenge against the Donato team. Notably, the challenge highlighted conflicts within the academic scientific community. It also spurred involvement from supporting actors, who were

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365 For example, Hal Salwasser sought assistance from Max Merlich of Columbia Helicopters and his colleagues in drafting his January 11, 2006 “context” memo. Columbia Helicopters has a significant financial interest in salvage logging, as one of the government contractors hired to remove cut trees in areas with insufficient roadways. Merlich agreed to help Salwasser craft the context memo. Merlich wrote: “I am going to try to do some damage control … How OSU handles this [situation] from this point forward on could play an important part on our issues.” Salwasser, H. (2006, Jan. 10), [E-mail correspondence from Hal Salwasser to Max Merlich] and Merlich, M. (2006, Jan. 10), [E-mail correspondence from Max Merlich to Hal Salwasser].

In the “Responses to the Challenges” section, I note other actors involved to assist with “damage control,” including individuals who challenged the Donato team after the issuance of the dean’s context memo.


367 Jan. 10, 2006: Newton, M. (2006, Jan. 10), [E-mail correspondence from Michael Newton, professor emeritus of forest science, to Hal Salwasser] in which Newton, noting that the “institution may lose funding” because of the Donato article, suggests damage control.
not academic scientists. The dean coordinated with these supporting cast members – which included forest professionals, representatives of the timber industry, and others who had an interest in passing FERRA – to help manage the damage-control efforts necessitated by the Donato study’s publication.

**Scene 2:**

While some critics of the dean’s memo believed that he acted inappropriately and overstepped his boundaries, another group of academic scientists, foresters, and others felt that Dean Salwasser did not act forcefully enough indiscounting the Donato study. Dissatisfied with the dean’s handling of the study, many of these objectors to the Donato study sought other means to challenge it. Most notably, a group of dissenters, referred to as the Gang of Nine ("GoN"), challenged the study and its researchers in a more visible and controversial manner. The GoN explicitly discredited the study. In addition, since the print version of *Science* is the more widely disseminated forum, the GoN attempted to delay that publication until the dissenting group could find more concrete evidence to support its position that the Donato study was flawed and unworthy of publication.

This challenge became apparent on January 17, 2006, when the group consisting of nine scientists, including senior faculty from the OSU CoF and staff from the Forest Service, formally expressed their concerns about the Donato study. On that date, the GoN issued a letter to

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The members of the Gang of Nine were Paul Adams, professor of soil and watershed management at OSU; Tom Atzet, retired Southwest Oregon regional ecologist for the USDA Forest Service; Steve Fitzgerald, extension specialist at OSU; Mike Newton, professor emeritus of silviculture and reforestation at OSU; Robert Powers, program manager and senior scientist for one of the USDA Forest Service research stations; Robin Rose, professor of forest regeneration and director of a cooperative research program at OSU; John Sessions, University Distinguished Professor of forest planning and engineering at OSU; Carl Skinner, geographer and science team leader for one of the USDA Forest Service research stations; and Steve Tesch, professor of silviculture and department chair of forest engineering at OSU.
Donald Kennedy, the editor of *Science*. The GoN outlined what they viewed as clear problems with the study, and questioned how the article ever passed peer review. In their introductory paragraph, the GoN highlighted four overarching problems with the Donato study. They stated that the Donato article did “not adequately define the environmental setting,” drew “sweeping inferences the statistical design (a case study) and data do not support,” contributed “no new science,” and presented forest “management actions out of context.”

Drawing on their professional expertise, they expressed the opinion that these deficiencies required immediate action. Their solution: “We respectfully request that you delay publication until the environmental setting of the study is better defined and the inferences can be supported by data. Alternatively, we ask that you include our stated concerns in a letter accompanying publication.”

Absent evidence of scientific misconduct, the traditional protocols for determining publication of an academic article involve peer review. Typically, two experts review the paper and judge whether it is worthy of publication. According to Donald Kennedy, the editor, *Science* followed that protocol with the Donato study, and the two reviewers found no notable problems with the paper. In fact, they recommended the article for publication, and *Science* accepts less than 8% of originally submitted manuscripts. The GoN asserted that an academic journal, especially one of the caliber of *Science*, should not have printed what the GoN alleged to be a flawed study. In their minds, this article’s recommendation for publication demonstrated that “the peer review process failed as a quality control measure.”

Addressing that viewpoint, Kennedy expressed his trust in the process and stated that he would not allow the GoN to “pre-

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empt” or “censor” the print publication of the Donato study. Others agreed with that decision. For instance, Kathleen Dean Moore, an OSU distinguished professor of philosophy who teaches environmental ethics, expressed dismay about attempts to censor research. In an interview with *The Oregonian*, she stated: “It’s about hearing all the voices so we can learn from them.”

Robert Buckman, OSU professor emeritus of forestry, also agreed that the publication should continue through the established peer-review process, yet he did not believe that the paper was that impressive. In an interview published in *The Scientist*, he stated, “If *Science* had known the context issues adequately, that paper would have been a lot less attractive,” but he disagreed with any decision to “delay publication of the paper.” Further, other academic scientists in forestry research agreed with the *Science* editor’s decision to publish the article. David Epstein reported in *Inside Higher Education*, an online newspaper geared toward higher-education faculty and staff, that Grame Berlyn, editor of the *Journal of Sustainable Forestry* and professor of anatomy and physiology of trees at Yale University, if he had had Kennedy’s choice to make would have likely kept the publication as scheduled under the circumstances presented. Epstein quoted Berlyn stating, “if there’s nothing erroneous, I wouldn’t keep it out.”

Many academic scientists believed that the GoN’s action reflected an unusual request. Beverly Law, a professor of forest science at OSU and one of the authors of the Donato study, was surprised by the action of her colleagues. In an interview with National Public Radio, she

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stated, “In my career I’ve never seen anyone try to delay a publication in *Science*.” Other members of the academic community, including academic scientists within the forestry field, felt that the GoN took inappropriate steps in expressing their concerns about the Donato study. For example, James Karr, a professor of fisheries and biology at the University of Washington, told a newspaper reporter that he was “appalled at the way” the GoN addressed the scientific dispute. Admittedly, Karr opposed salvage logging, so his position was not aligned with the GoN. Putting aside that difference, Karr contended that the letter harmed the scientific community. His belief was that the letter had “a chilling effect on other OSU researchers” who might have expressed an opposing view to positions held by powerful interests, but now feared doing so. Karr’s statement resonated with other observers, such as the OSU provost, Sabah Randhawa, who also believed that the GoN’s letter to Kennedy, whether intentionally or not, conveyed to graduate students and junior faculty that senior faculty members may exercise some authority, including attempts to quash research that contradicts their position. Pinpointing an underlying problem within the CoF as well as at many other universities, the provost stated at the February 2006 Faculty Senate meeting that he had concerns over the imbalance of power that the GoN’s actions displayed. He explained, “It’s important that those in positions of power don’t misuse their rank, intimidating faculty or students with less power.”

Donald Kennedy, along with many academic scientists, viewed the differences between the GoN and the Donato team as part of an academic debate. Kennedy and other Donato supporters recommended that academic scientists use scholarly journals as venues to express

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379 Milstein, Jan. 20, 2006, paragraph 11.
381 Johnson et al., 2006; Kennedy, 2006; Milstein, Jan. 20, 2006; Ringo, Apr. 7, 2006; Ringo, Aug. 2, 2006.
differing research perspectives and approaches, so others could be informed about the debate as well as participate. Kennedy even agreed to print the various perspectives of this debate in an upcoming issue of *Science*.

While the GoN unsuccessfully attempted to delay the Donato study publication, its efforts drew attention to the controversy: that senior professors at OSU’s CoF and forest service staff had concerns about the scientific integrity of the Donato study. Many individuals and groups opposing the Donato study asserted the GoN’s specific claims of alleged scientific errors as their scientific basis to discredit the study. For instance, when corresponding to Dean Salwasser, Mike Cloughesy, the Director of Forestry for the Oregon Forest Industry Council (OFCI), affirmed the GoN’s reasoning and noted his strategy for dealing with the situation. He wrote, “I want to stress that to OFRI Speakers Bureau audiences the issue has been what is the proper forestry response to wildfire, not the academic freedom issue.”  Similarly, other publications and public venues such as blogs also reiterated the GoN criticisms about the data sources and approaches. Equally important, they cited the GoN’s letter requesting the publication delay to demonstrate that these senior professors and forest service staff (i.e., the GoN) thought the Donato team’s scientific errors were severe, and thus unacceptable, absent further scientific commentary.

Combining the effects of the dean’s context memo and the GoN’s letter, we witness most significantly that it was the academic scientific community who were the critical actors to

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initially challenge the Donato study. These challengers laid the scientific foundation to scrutinize the Donato study. In Act 2, we observe how several federal actors build on the efforts of the dean, GoN, and their supporting cast and also challenge the Donato team.

**Act 2: Government Challenges**

Act 2 opens in February 2006 during a period in which the Republican Party leads both the executive and legislative branches of the U.S. government and vocally supports salvage logging policies. Furthering those interests, two separate government entities orchestrate challenges against the Donato team, and their actions raise questions of whether the governmental actors are properly asserting government protocols or politics. Joining the timber industry and its trade organization representatives, the academic scientists who opposed the Donato study now take a backstage role regarding the challenges.

**Scene 1:**

In Act 2, Scene 1, a group of challengers to the Donato study made another academically unorthodox move: They convinced the Bureau of Land Management (BLM) project inspector to place a stop-work order on the federal grant that funded the Donato study. That led to the BLM’s issuing a grant suspension for the final year of funding, essentially refusing payment of approximately $93,000.

In the letter to OSU explaining reasons for the grant suspension, the BLM identified three actions that violated terms of the grant provisions. First, the BLM claimed that one of OSU’s acts violated two agreements associated with the grant funding. The BLM indicated that OSU placed references to a pending bill, *The Forest Emergency Recovery and Research Act*, next to the Donato study in an attempt to influence readers. Describing the action, the BLM stated:

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Original abstract published on *Science Express* website posted on or about January 5, 2006, states “Legislation currently pending in U.S. Congress, HR 4200, would expedite postfire logging projects, citing reforestation and fuel reduction among its goals. To help inform the dialogue…”

According to the BLM, reference to the bill violated two grant provisions, which explicitly prohibited using federal funds for lobbying or related activities. Second, the BLM contended that OSU violated the grant provision that required consultation with the agency prior to publication. The letter indicated:

The Bureau Assistance Officer is unaware of the efforts made by OSU to consult with Bureau representatives (including the Project Inspector or the Assistance Representative) prior to publishing results relating to the subject task order.

Third, the BLM observed that the unauthorized publication materials failed to include a mandated disclaimer per one of the grant provisions, which stated: “[A]ll information submitted for publication or other public releases of information regarding this project shall carry a disclaimer.”

According to the BLM, these grant violations justified the bureau’s action of suspending OSU’s funding. Even though the BLM had suspended the funding, it gave OSU an opportunity to respond, in case the federal agency had inadvertently misinterpreted any of the events.

A few days after the suspension and before OSU responded, the media publicized the BLM’s action. According to the news reports, various individuals perceived the BLM’s grant suspension as a retaliatory action for presenting research that conflicted with the federal government’s policies. On February 7, 2006, Jeff Barnard of the Associated Press reported that

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Erik Berg, the director of the Joint Fire Science Program, the federal interagency unit that helps administer funding for research projects such as the Donato study, considered the suspension highly unusual. Further, Berg indicated that he had “never heard of this kind of inquiry before into research funded by [that particular] federal program.”

While it is possible that the BLM had never faced a question of a grant recipient’s allegedly using its federal funds for lobbying purposes, many critics of the BLM action speculated that the agency might not have taken this action if the Donato study findings had supported the pending FERRA bill. According to an Associated Press news clip, Andy Stahl, the director of the Forest Service Employees for Environmental Ethics, an environmental group based in Eugene, Oregon, believed that the BLM intentionally suspended the grant because of the Donato team’s findings. He added: “If BLM can point to a single other instance where it has suspended scientific research because of alleged illegal lobbying, I’ll eat my hat.” However, based on the available data, neither the BLM nor anyone else ever identified a prior incident in which it suspended a grant award for a similar situation.

Besides academic scientists’ and environmentalists’ raising questions about the BLM’s grant suspension, several government actors were also unconvinced that the BLM suspended the grant solely on the basis of the grant contract terms. On several occasions, U.S. Representative David Wu (Oregon, Democrat) characterized the BLM suspension as having a “chilling effect” on federally sponsored academic research that conflicted with a government policy.

388 Barnard, Feb. 9, 2006b, ¶12.
Representative Jay Inslee (Washington, Democrat) feared the same, and he called for an investigation. On February 7, 2006, he issued a letter to Earl Devaney, the Department of Interior’s inspector general, to investigate whether the BLM retaliated against the Donato team because its federally sponsored research conflicted with the Republican presidential administration’s policies.390

Immediately following the Inslee statement, Congressman Walden had concerns that the BLM’s grant suspension might be viewed by some as retaliation, because the research came down on the wrong side of his pending bill, so he took two steps to distance himself from what critics claimed to be federal interference with academic freedom and acts of political censorship. First, he disavowed any connection. He stated in a subcommittee meeting that he did not support the BLM’s decision to suspend the funds. Second, on February 8, 2006, he, along with Congressman Brian Baird (Washington, Democrat), another cosponsor of the FERRA bill, wrote to Kathleen Clarke, the BLM Director, asking for reinstatement of the grant funds.391 The congressmen stated: “We understand the position in which the BLM has been placed by recent events. However, it is important that post-catastrophic research continue to be carried out in an open, non-politicized manner.”392 They opined that the suspension of funds might lead observers to think “that the agency is only politicizing [the issue even] further,” which would leave “the impression of scientific censorship by the BLM.”393 Given the false impression they felt was being created regarding the reasons for the withdrawal of grant funding to OSU, they recommended that Clarke’s agency reinstate the grant funds to the university.

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393 Baird & Walden, Feb. 6, 2006, paragraph 2.
On that same date, Peggy Lowry, the director and institutional authorizing official within OSU’s Office of Sponsored Programs and Research Compliance, replied to the BLM suspension letter.\textsuperscript{394} Lowery presented explanations of how OSU generally complied with the grant provisions. For instance, to address the allegations of lobbying, Lowry recounted the exchange between the Donato team and the staff at \textit{Science}. Lowry admitted that the “language was included in the manuscript initially submitted to \textit{Science} for review for the sole purpose of highlighting the relevance and timeliness of the research, not to influence legislation.”\textsuperscript{395} She disclaimed any intent by the researchers to lobby: “The authors never intended to promote public support or opposition to pending legislation or to influence a member of Congress or governmental official to support or oppose legislation, but rather only to note the timeliness of the research.”\textsuperscript{396} Further, the tracing of events revealed that \textit{Science} posted the links even after the parties agreed to remove references to the bill. Lowry described these events as well as other sources of miscommunication and suggested that is where the BLM’s belief of contractual violations originated. With the exception of neglecting to place a disclaimer on the article, Lowry asserted that OSU had been in full compliance with the grant provisions.

Immediately following receipt of the OSU letter, the BLM reinstated the grant funding. Michael Milstein, a journalist for \textit{The Oregonian}, reported that the BLM faced “mounting pressure and criticism” to return the funds, so it did “within a few hours” of OSU’s response.\textsuperscript{397} The BLM, however, characterized the events as complying with its interpretation of the grant terms.\textsuperscript{398} According to a BLM press release, “OSU … adequately addressed BLM concerns

\textsuperscript{395} Lowry, 2006, p. 2.
\textsuperscript{396} Lowry, 2006, p. 2.
\textsuperscript{397} Milstein, Feb. 9, 2006, A1.
\textsuperscript{398} BLM, Feb. 8, 2006a; Coleman, 2006.
regarding administrative aspects of the agreement and task order.” Despite the agency’s statement, many observers of the Donato study controversy believed that the letter from Congressmen Walden and Baird likely played a larger role and ultimately convinced BLM staff to restore the grant funds to OSU. Dave Skinner, a writer for the industry-friendly magazine *Evergreen,* also described the sequence of events. Based on data he gathered, he too concluded that the money was quickly restored “after high-level communications between lead HR-4200 sponsor Congressman Greg Walden and BLM Director Kathleen Clarke.”

In sum, despite the reinstatement of the grant funding, according to many supporters of the Donato study, the BLM’s suspension represented an irregular governmental action arising out of a government challenge of the Donato study. Further, this scene revealed a conflict between two federal entities striving to achieve their goals. Although both the BLM (via the presidential administration) and Congressmen Walden and Baird had salvage logging as a political agenda item, Walden preferred to separate himself from the impression of any politically motivated retaliation – which the BLM’s grant suspension appeared to be – so he sought scientific arguments to further his policy interest. In Scene 2, we learn that Congressman Walden reasserted the interests of FERRA through a special hearing that appeared to correct the effect of the BLM’s challenge. In addition, the data reveals a seemingly orchestrated effort by Walden and Baird to discredit the Donato study through a congressional hearing in which actors tried to assert grant procedures and scientific justifications rather than political motivations.

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399 BLM (2006b, Feb. 8). *OSU complies with forest fire science study agreement: BLM lifts suspension* [Press release, no. 06-12], p. 1. Portland, OR: Bureau of Land Management.


401 Skinner, 2006, p. 11.
Scene 2:

In Act 2, Scene 2, I present an account showing how Congressman Walden’s next set of actions establish another venue for governmental actors to challenge the Donato study. But this time, the events occur in a seemingly well-coordinated manner in terms of (a) the actions that explained the justification for the congressional hearing and (b) the roles that Walden and Baird each had at the congressional hearing.

On February 8, 2006, the same date as the BLM reinstatement of funds, Walden announced that he and the House Natural Resources Committee Chair, Richard Pombo (California, Republican) had agreed to hold a House Forestry Panel hearing about the Donato study. He indicated that Representative Tom Udall (New Mexico, Democrat) had initiated the request, and he was carrying out that request. According to the House press release, Congressman Walden felt that Congress needed to gather more information about forest treatment after fires.

Several observers of the Donato controversy, including members of the media and various bloggers, noted that Walden’s scheduling of this special hearing represented a turn of events. Explaining the need for the hearing, Dan Berman, a reporter for Environment and Energy Daily, quoted Walden explaining: “As a result of our prior hearings, it became obvious that there is a need for additional research on how best to manage our forests after catastrophic events.”402 That quote is derived from a February 8, 2006 statement. Yet, as Berman wrote: “The decision was a reversal of Walden’s stance two weeks ago” when Walden asserted that the Donato study added nothing to science knowledge about forest recovery following major fires.403

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402 Berman, Feb. 9, 2006a, paragraph 3.
403 Berman, Feb. 9, 2006a, paragraph 4.
In late January 2006, when Udall first requested the hearing, Walden stated, “There wasn’t anything new” in the Donato study. He criticized the study and added: “My opinion is you need to look at a broader timeframe than a couple of years … and what happened if you get in right away as our bill envisions.” Walden also expressed less patience with the scientific process. He thought that “[s]cientists are going to debate and disagree” but as a policymaker, he would still “need to make a decision.” Despite Walden’s public position in late January 2006, on February 8, 2006, he decided to hold a hearing, so the House Committee held a field hearing in Medford, Oregon, on February 24, 2006.

As the events unfolded, critics of Walden, particularly those opposing FERRA, began to assert that Walden likely intended to use this February 24\textsuperscript{th} hearing to position his policy in a positive light and challenge the Donato study as a poor scientific project. Numerous media reports, including pieces in the \textit{Washington Post}, \textit{Oregonian}, \textit{Seattle Post-Intelligencer}, and \textit{Chronicle of Higher Education}, portrayed this field hearing as a coordinated attempt to discredit the Donato study. While Walden and Baird conveyed a uniform message that they believed the Donato team was more concerned about policy advocacy than science, the media outlets reported that each of the congressmen also appeared to have a specific role in challenging the Donato team and its study.

\begin{footnotesize}
\begin{itemize}
  \item 404 Berman, Jan. 26, 2006, paragraph 11.
  \item 406 Berman, Jan. 26, 2006, paragraph 12.
  \item 408 These news outlets referred to Congressmen Walden and Baird as a two-member team in which each raised different concerns about the study when questioning Donato. Walden inquired about the allegations that the Donato study violated the BLM contract provisions while Baird posed questions about the science. According to Joel Connelly of the \textit{Seattle Post-Intelligencer}, Baird was the “put-down man” in the metaphorical boxing match (i.e.,...)
\end{itemize}
\end{footnotesize}
At the February 24, 2006 hearing, Donato and four witnesses, including academic scientists and forestry professionals, testified at the first half of the hearing. It became clear that Walden’s role was to inquire about the Donato team’s actions, which appeared to have violated the federal grant protocols, including provisions within the federal research grant contract. During the questioning period, Congressman Walden focused on putting science and policymaking questions to the panelists, except for Donato. At this stage of the hearing, Walden asked the other witnesses one question: “[H]ow important is timing in post-fire treatments?” After each of them responded, he directed his attention to Donato, stating, “I have a couple of questions obviously for you.”

Walden did not ask Donato science-based questions; he posed questions and comments only about whether the Donato team had followed correct grant-funding protocols. For example, Walden asked: “In terms of the protocols [pause] In terms of the protocols you were required to follow, were you supposed to – did you have a [BLM] project investigator that you were supposed to report to prior to submission to any publication of your work?” Donato replied that the communication standards were not clear, but he did, in December 2005, a month prior to the publication, communicate with Project Investigator Tom Sensenig.

Following that comment, the exchange between Walden and Donato might be viewed as Walden’s cross-examination of Donato – with the intent to question the veracity of Donato’s comments.

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Mr. WALDEN. So and was that the December meeting that Mr. Sensenig asked you to attend?

Mr. DONATO. Yes.

Mr. WALDEN. And in that meeting you told him you’d submitted the science.

Mr. DONATO. That we were submitting a paper, yes.

Mr. WALDEN. You did? Because he has an e-mail that we have in our record that doesn’t indicate that at all. It’s a much different version. Have you seen that e-mail?

Mr. DONATO. No, I’m afraid I haven’t.

Mr. WALDEN. All right. There’s an e-mail from Tom Sensenig, the principal investigator and project inspector to the contracting officer, Mr. Shapiro, which … indicates that he called the meeting with you in early December to prepare for a conference. Scheduled a meeting for December 15th in Corvallis. And he says, and I quote here: “Despite having already prepared and submitted their paper to Science, Dan did not offer any information regarding the other authors’ involvement or the fact that they had submitted a paper for publication.”

Mr. DONATO. This really harkens to just a miscommunication as to the level of consultation required. This is an issue that has been resolved between the university and the agency as a miscommunication. It really was.412

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412 Scientific research and the knowledge-base concerning forest management following wildfires and other major disturbances. Oversight Field Hearing before the Subcommittee on Forests and Forest Health of the Committee on Resources, U.S. House of Representatives, 109th Cong. at 48-49 (2nd Sess.) (Testimony of Daniel Donato).
Many observers in the media and bloggers supporting the Donato team viewed Walden’s role in the hearing as the one who established that the Donato team had circumvented the BLM channels. They also believed that Baird’s contrasting role was to challenge Donato on the science and to raise concerns about the study’s scientific integrity.

Baird, a former college psychology professor, has a Ph.D. in clinical psychology and previously taught statistics. He used his educational training and experiences to guide him through the Donato questioning. Like Walden, he presented straightforward inquiries to all the other witnesses. For instance, he asked all the witnesses about the value of the bill’s inclusion of setting up work plans for logging activities. In addition, he asked one witness whether “preeminent scientific journals” would require demonstrating the data with certain statistical steps. For two other witnesses, he solicited opinions about the sufficiency of the research on salvage logging. Collectively, Baird’s questions and comments amounted to approximately 67 transcript lines for those four witnesses. By contrast, Baird’s questions and comments amounted to 180 transcript lines for Donato, but more significantly, Baird’s tone changed when he went from questioning experts to questioning an academic scientist with allegations of research misconduct.

Baird forewarned Donato: “So if I ask you tough questions, I’m not picking on you. This is how science works. It’s worked for thousands of years this way.”413 He did, however, start his questioning of Donato as a rather professional exchange. He raised concerns about the study, in a seeming attempt to distinguish it from the proposed legislation. “I never saw you say that had the logging commenced prior to the two-year time allowed under the Biscuit Fire, the

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413 Baird, Feb. 24, 2006, p. 56.

Feb. 24, 2006: Scientific research and the knowledge-base concerning forest management following wildfires and other major disturbances. Oversight Field Hearing before the Subcommittee on Forests and Forest Health of the Committee on Resources, U.S. House of Representatives, 109th Cong. at 56 (2nd Sess.) (Statements of Brian Baird).
mortality of seedlings would have been substantially different.”414 According to Baird, that represented a significant omission. He argued that: “I think you needed to say it because the entire purpose of our legislation is to allow folks to go in while the existing wood has more value and before you got seedlings coming up. … Did I miss something or did you address that.”415 Donato explained: “Our goal in the paper was to present the numbers and present the dates and not make management recommendations. We just wanted to present the data.”416

Baird’s perceptions about the Donato team’s intent became clear when he expressed, on multiple occasions, that he was unconvinced that Donato responded honestly. In three instances, Baird referred to Donato’s statements as “disingenuous,” and he asserted that the Donato study contained “value-laden statements” – likely to imply that the Donato team sought to advance a policy direction without sufficient science to support that position.

Further, he criticized the statistical analysis as inaccurate. Addressing his point, he said to Donato: “You have chosen a methodology for analysis and data report that is subject to significant misinterpretation. And this is not, my friend, a subtle academic issue; this is a matter of important policy decisions.”417 Then, a back-and-forth over the statistical approach ensued:

Mr. DONATO. Can I also point out the statistical test that I used, which is the Wilcoxon signed rank test, which is used on before/after data for each plot, … completely takes care of this problem, 100 percent.

Mr. BAIRD. No, it doesn’t.

Mr. DONATO. Yes, it does.

Mr. BAIRD. It does not. It does not deal with the magnitude. I’m sorry. With respect, it doesn’t. It does not deal with the absolute magnitude of the difference. It rank orders the variables or the plots on which ones are different. It rank orders the magnitude, but it doesn’t tell the absolute magnitude. It just doesn’t.

Mr. DONATO. The median is a measure of central tendency of all nine plots beforehand and all nine plots after.

Mr. BAIRD. No, it’s not.

Mr. DONATO. Yes, it is.

Mr. BAIRD. It is not.

Mr. DONATO. Yes, it is. I disagree.418

This part of the questioning involved Donato trying to explain the team’s reasoning behind certain data analyses and Baird relying on his educational training and experience to argue that the Donato team erred in its analyses.

The media and some observers of the hearing, especially those sympathetic to the Donato team and those against the FERRA bill, characterized Donato’s experience as unfair interrogation in which the Congressmen “grilled” him about the controversial study.419 For instance, Blaine Harden, a journalist with The Washington Post, referred to Donato’s role as the “principal punching bag” in an effort to discredit the study.420 Les AuCoin, a retired Democratic Congressman representing Oregon, perceived the hearing as an event that “bordered on a star

chamber attack” to question Donato’s research integrity.\textsuperscript{421} According to AuCoin, an important lesson he gained from observing the hearing is that “in today’s [political] climate, if a scientist follows his findings to wherever they lead, he risks sticking his neck into a congressional noose.”\textsuperscript{422}

Many proponents of the FERRA bill, who also tended to represent the group supporting challenges to the Donato study, offered a different view of the hearing. They believed that Congressmen Baird and Walden questioned Donato to uncover the scientific problems surrounding the study. For example, David Reinhard, a conservative commentator for several papers in the Northwest region, interpreted the Walden and Baird investigation as a necessary forum to learn about the poor research of the Donato study.\textsuperscript{423} Many proponents of the FERRA bill also noted that at the time of the hearing, the Donato team still had not released the raw data to Baird, as he had requested. Inferentially, they conveyed their underlying suspicion that the Donato team withheld the data so no one could replicate the study, which might have revealed errors or uncovered evidence of scientific misconduct. Thus, based on this camp’s perspective, any grilling or punching that allegedly occurred took place to examine the science.

Finally, the field hearing disclosed another noteworthy theme that did not seem to appear in other publicly accessible documents. On several occasions during the hearing, Congressmen Walden and Baird, along with Rich Drehobl, a retired BLM field manager who testified at the field hearing, blamed the Donato team for the framing of media messages to the public and held the researchers accountable for how the public used that information. For example, Baird stated:

\begin{itemize}
\item \textsuperscript{421} AuCoin, 2006, B7.
\item \textsuperscript{422} AuCoin, 2006, B7.
\end{itemize}
Rightly or wrongly, intentionally or unintentionally, largely based solely on the title of your article and then what the press made of it subsequently, this study is becoming as if it were the total body of literature about post-fire logging. And people show up at townhalls with how could you dare put forward this bill when science has proven this.  

Similarly, Walden noted, “it really raises questions about this issue that is so much in the press.” He believed that “every one of us here has weighed in to defend academic freedom, but we also have an obligation to make sure that the contractual obligations that you and your colleagues are involved in are met.” Drehobl expressed his frustration with the media’s coverage of the Donato study challenges: “And I’ve been following the news media blitz with all this controversy that’s going on and BLM ... But the notion in the media that the BLM is stifling academic freedom is absolutely false.” He explained, “Academic freedom does not apply to intentionally misleading or publishing disingenuous or politically motivated science that’s funded by taxpayers.”

In sum, the field hearing reflected the second significant government challenge against the Donato team and its research. Opponents of the Donato study interpreted the events as governmental actors investigating a poorly constructed scientific study. By contrast, supporters of the Donato study viewed the actions as government’s attack on Donato and the team’s study. The circumstances and events surrounding the field hearing reveal a well-orchestrated, multilayer challenge. First, Walden accused the Donato team of violating the grant provisions,
particularly the terms concerning the BLM clearance. Walden insinuated that the researchers avoided the BLM project investigator to evade scientific scrutiny from a seasoned forestry researcher-practitioner. Second, Baird asserted that the Donato team used incorrect scientific methods and analyses. In making those assertions he raised concerns about the scientific integrity of the research. Third, Walden, Baird, and Drehobl blamed the Donato team for the media’s purportedly incorrect messages. They suggested that the Donato team might have strategically used the media to advance the researchers’ allegedly flawed study. Walden, Baird, and Drehobl each represented a layer of the challenge, and collectively, they attempted to orchestrate a common message: The Donato study reflected a poor scientific project, in which the researchers, rather than setting out to conduct sound science, likely intended to use their published work to advocate for a certain policy position. Put simply, through multiple layers of questioning the scientific soundness of this study, the House Forestry Panel hearing intended to discredit the Donato study.

Summary of Challenges

To present the challenges, I described a series of interactions involving the primary actors. In Act 1, I highlighted the CoF dean’s actions to minimize the Donato study. In addition, I reported that the GoN took stronger steps to discredit the Donato study; it tried to delay the Donato study’s print publication and used this group’s influence as seasoned researchers and practitioners in the field to discredit it. In Act 1, I also noted actors who were involved in activities behind the stage. These individuals included policymakers supporting FERRA and industry representatives. In Act 2, I introduced governmental actors who also challenged the Donato study and its researchers. In the first scene of Act 2, I traced events surrounding an important, yet unsuccessful, challenge against the Donato team. In that scene, in a highly
irregular move, the BLM suspended grant funding, based on allegations that the Donato team violated terms of the grant contracts. Most significantly, the BLM claimed that the Donato team lobbied against a proposed House bill. Concerned that observers might perceive the grant suspension as academic censorship and/or retaliation, the author of that bill, Congressman Greg Walden, along with Congressman Brian Baird, requested the reinstatement of the funds. The BLM complied. Then, Walden created another opportunity to challenge the Donato study. He decided to hold a congressional field hearing. According to some observers, that hearing amounted to a well-orchestrated event to publicly challenge the study. A graduate student, Dan Donato, one of the contested study’s authors, was at the center of their target. Collectively, these events, which I depicted as a two-act play, dramatized the multistage challenges against the Donato team and its federally sponsored research.

Responses to the Challenges

In this section, I detail how academic scientists who supported the Donato team employed three significant strategies, so the Donato team could exert control over its research. Consistent with the conceptual framework discussed more fully in Chapter 3 of this dissertation, I divide the strategies into actions that relied more heavily on (a) internal characteristics, (b) external factors, and (c) both internal characteristics of and external factors influencing the academic profession.

Internal Characteristics

One of the strategies that academic scientists supporting the Donato team employed involved using traditions of the academic profession to establish “rules” of the professional interaction. These academic rules drew on internal characteristics of the academic profession to structure how the Donato team could control its federally funded research. Illustrating this
strategy of academic rules, I describe, below, two responses of academic scientists who supported the Donato team.

*Academic Censure*

On several occasions, supporters of the Donato study publicly admonished the GoN for their attempts to attack the Donato study. For example, on February 8, 2006, University Provost Sabah Randhawa and Faculty Senate President Bill Boggess, two OSU campus leaders, issued a joint letter to the university community rebuking the CoF professors who were signatories to the GoN letter.429 According to the provost and faculty senate president, when the GoN “disagreed with the study’s conclusions,” their actions represented an “inappropriate … request to delay publication.”430 Affirming the protocols within academic science, the provost and faculty senate president stated that “[d]ifferences of perspectives drive the scientific inquiry process. These should not only be encouraged, but fostered in our academic community.”431 Further, the provost and faculty senate president made clear that academic organizations such as OSU operate with a “culture of open query and expression, where diversity of opinions is valued and individuals are free to express themselves without the fear of censorship.”432

Similarly, on February 9, 2006, the OSU Faculty Senate unanimously passed a resolution that excerpted sections of the faculty handbook’s statements on academic freedom as a professional code of conduct.433 In addition, the resolution expressed public disapproval of the GoN’s efforts to delay the publication of the Donato study, with the last paragraph of the resolution stating:

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429 Feb. 8, 2006: Randhawa, S., & Boggess, B. (2006, Feb. 8), [Letter from Provost Sabah Randhawa and Faculty Senate President Bill Boggess to Oregon State University (OSU) Campus Community].
430 Randhawa & Boggess, 2006, paragraph 3.
431 Randhawa & Boggess, 2006, paragraph 3.
432 Randhawa & Boggess, 2006, paragraph 3.
The Faculty Senate of Oregon State University stands resolved that the actions taken by faculty members of this university that attempted to interfere with the publication of … Donato, et al.’s research in Science violated both the spirit and the letter of the “Statement of Faculty Responsibilities.”

These public statements sent a uniform message, which the final paragraph of the joint letter from the provost and the faculty senate president captures quite clearly. These vocal supporters of the Donato research team intended to “reaffirm [their] commitment to academic freedom, professional responsibility, and the scientific method” – which included voicing in a public manner their opposition to the GoN’s letter as being not acceptable then or in the future.

These forms of academic censure for the opposing group conveyed a professional ideology of research’s independence from undue influence. The statements repeatedly reaffirmed the academic values of open inquiry and scientific review. The academic censuring also identified roles of various individuals when academic scientists challenge their colleagues’ federally sponsored research. Examining these roles in terms of divisions of labor, these senior leaders (i.e., the provost and faculty senate president) and the faculty representative body within OSU served as academic enforcers who guarded against inappropriate use of power within their system (or in this case, their university). That the senior leaders took on this role would allow the Donato team to focus on its research, not enforcing violations of professional conduct. Furthermore, under the labor markets and careers factor, which in Freidson’s model reflects a profession’s ability to dictate who can enter and work in the profession, the academic censure sent a message that academic groups cannot inappropriately exercise their power within the profession nor can they change the rules of the academic profession on how to debate an

434 OSU Faculty Senate, 2006.
435 Randhawa & Boggess, 2006, paragraph 5.
academic matter. Finally, the statements of academic censure might at first appear to be blanket condemnations of the OSU faculty members within the GoN; however, that is not the case. Although the censuring shone attention on these OSU faculty members for using their power inappropriately when they attempted to stop the dissemination of the Donato study, the academic censure statements allowed that the OSU faculty members within the GoN could use their professional knowledge and skills to contest the research in a more open environment (i.e., an environment that follows the traditions of the academic profession).

While academic censuring represents one dimension of the academic rules, in the next subsection, I describe another dimension – the committee review process.

Committee Review & Policy Development

Supporters of the Donato study also pushed for an internal review of the professional work environment at the OSU CoF. While the institution reaffirmed its concern for principles of free inquiry through the academic censure, not everyone supporting the Donato team had confidence that the CoF organizational climate fostered the same ideals. Illustrating this viewpoint, which many of the Donato team supporters held, the provost suggested that the GoN’s actions might have reflected power imbalances within the CoF. He stated, “It’s important that those in positions of power don’t misuse their rank, intimidating faculty or students with less power.” Based on an interview with the provost, Mary Ann Albright, a writer for the Corvallis Gazette-Times, the local paper for the OSU community, wrote that the provost believed

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437 OSU Faculty Senate, 2006.
there would be “a need to look at the hierarchy inherent in the university structure” in order to propose solutions to avoid future misuse of professional authority.438

In an effort to find a resolution to the internal conflicts, several OSU administrators and faculty encouraged Dean Salwasser to establish a committee to investigate the working climate and make recommendations to improve the environment. He acted on those suggestions, and on February 10, 2006, he officially announced the establishment of the CoF internal review team, the Committee on Academic Freedom and Responsibility (CAFR).439 According to minutes of the CoF Executive Committee, the new committee’s charge was to recommend a plan of action addressing “how to lead the College through a conversation about what academic freedom and core values of the College mean” to members of the CoF community.440

The establishment of CAFR represented an important strategy for supporters of the Donato study. It engaged nearly the entire CoF community (i.e., faculty, staff, and graduate students) to voice their perspectives about the organizational climate. Initially, CAFR held open meetings for members of the CoF community to express their opinions on how to advance the principles of academic freedom and the CoF’s core values. While CAFR and the CoF Executive Committee minutes reported that the attendance met their expectations, some members of the CoF community, particularly junior faculty and graduate students, told CAFR members that they

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439 Forestry Executive Committee (2006, Feb. 10). Minutes of the forestry executive committee meeting.
Corvallis, OR: College of Forestry, Oregon State University.

In February 2006, the Committee consisted of eight senior faculty (seven men and one woman). Salwasser referred to the committee as a group of “distinguished and endowed professors” (Feb. 10, 2006, p. 2). Approximately two months later, the dean diversified the committee and added five other individuals, four women and one man. All but one was a faculty member (at the junior or midcareer stage); the one non-faculty member was a graduate student. According to the April 4, 2006, faculty meeting minutes, the committee chair, Norman Johnson, stated that those additional committee members were intended “to gain a greater perspective of the issue” (paragraph 1).
“felt reluctant to speak” openly.\textsuperscript{441} Rather than exclude the very individuals who might have sensed the power differential, CAFR took active steps to gather feedback beyond the open forums, such as collecting data from e-mail and verbal communications. Furthermore, CAFR commissioned another unit within the university to administer an anonymous survey to obtain the CoF community’s perceptions and attitudes about the college’s culture and climate in terms of its leadership, protections for research independence, and professional behavior. In addition, in May and June 2006, it presented preliminary drafts of its reports to obtain community feedback about the findings and recommendations. Through this complex data-collection process, CAFR intended to propose new or revised policies and practices that welcomed multiple perspectives and maintained the value of free inquiry.

Indeed, CAFR’s work led to several noticeable changes in the CoF. First, to advance the professional ideology of capturing a diverse, learned perspective, the CoF adopted several new policies.\textsuperscript{442} For the Forest Research Laboratory (FRL) Advisory Committee, the CoF added “two fully vested members-at-large” with the express intent of broadening the “perspectives represented on the College’s primary decision making body.”\textsuperscript{443} In the fall of 2006, the dean initiated attempts to diversify the perspectives of the FRL. The FRL Advisory Committee is a statutorily defined committee that requires representation from the CoF, state and federal agencies, and alumni. It has served as the external advisory board for the CoF, but according to CoF meeting minutes and a statement from the dean, it has had significant representation from individuals who adhere to the human-intervention perspective in forestry, particularly individuals

\textsuperscript{441} Johnson et al., 2006, p. 8.
from the timber industry.\textsuperscript{444} As an initial step to diversify the external advisory committee’s perspective, Dean Salwasser appointed an individual who embraced a naturalistic approach to forestry with an ecosystem lens, and the dean promised to consider other appointments of individuals with a naturalistic approach when an opening, which is not pre-designated by statutory mandate, became available.\textsuperscript{445} Further, the dean established the College Advisory Council, consisting of representatives from various groups within the CoF, to give input to the college leadership.\textsuperscript{446}

Second, CAFR’s work led to changes in the training and development of new and future members of the academic profession. Within research methods courses, the CoF integrated lessons on research independence from inappropriate influences, especially from industry, government, and even other academic scientists.\textsuperscript{447} The dean also exposed a group of graduate students to policymaking to advance an open dialogue. The dean asked the Graduate Student Council (GSC) to develop CoF’s policies and practices that dealt with the CoF responses to conflicts arising from presentations of controversial scientific issues.\textsuperscript{448} That led to the GSC and the dean’s office crafting policies and sponsoring forums on advancing a collegial debate.

Besides professional development for the graduate students, the dean appointed the associate

\textsuperscript{444} Forestry Executive Committee (2006, Apr. 25). Minutes of the forestry executive committee meeting. Corvallis, OR: College of Forestry, Oregon State University.

\textsuperscript{445} Salwasser, Jul. 26, 2006.

\textsuperscript{446} FRL Advisory Committee (2006, Nov. 22). Minutes of the Oregon forest research laboratory advisory committee meeting. Corvallis, OR: College of Forestry, Oregon State University.


\textsuperscript{448} Building Community Committee, 2008.
dean for academic affairs with the express responsibility of faculty mentoring and advancing the culture of research independence.

In sum, the internal review and its adopted policies established several similar messages as a form of academic censure to help the Donato team exert control over its federally sponsored research. It espoused the professional ideology of research independence as a priority at OSU. Also, it reiterated that both the academic challengers and the challenged academic scientists had opportunities to exercise their professional knowledge and skills. However, unlike the academic censure, the internal review and its adopted policies presented several distinct contributions to aid the Donato team. Under the internal review, the division of labor relied more on peers, not those who held a special formal authority, such as the title of provost or faculty senate president. CAFR also focused on the involvement of the entire community to give feedback, not just representative bodies. The internal review also created forums for graduate students. As a training program for future professionals, the involvement of graduate students reflected a significant factor to support the Donato team and future graduate students who engage in research about controversial issues. For instance, since two of the authors, including the lead author of the Donato study, were graduate students, the internal review process and subsequent policies conveyed a message that the college intended to protect graduate students as well as faculty. In addition, based on CAFR’s recommendations, the CoF adopted a training program to inform graduate students about academic freedom and related professional responsibilities. In sum, the internal review and its adopted policies moved beyond just admonishing academic violators for undue research interference, it collected data to evaluate the college’s work environment, isolated several identifiable problems, and implemented new and revised policies and practices for the CoF. These actions helped address some of the challenges to the Donato
team as well as addressing future problems that might arise when academic scientists at OSU do research on a controversial issue.

External Factors

In this subsection, I examine how the supporters of the Donato study relied on external factors, such as actors outside of the academic community who held legal authority and public influence. In particular, I describe how supporters of the Donato study relied heavily on political allies to overcome some of the challenges facing the Donato team. In brief, the supporters would first inform the political allies about the contested matter. Then, the political allies would use government venues and channels to reveal what the supporters believed were unfair challenges to the Donato study and its authors. Each of these instances of political support contributed in some way so that the Donato team could exert control over its federally sponsored research. I address below, more specifically, how the involvement of the political allies led to (a) the reinstatement of the BLM grant, (b) a congressional forum to reaffirm the Donato study findings and conclusions, and (c) disclosure of the challengers’ interests.

Questioning Intentions

Political allies of the Donato team relied on the media and general public to question the intentions of the BLM. When Congressmen Jay Inslee and David Wu learned about the BLM grant suspension, they quickly drew attention to this government action on the House floor and captured the media’s attention. The congressmen framed the BLM grant suspension as another example of an allegedly larger pattern of federal censorship over federally funded scientific research.⁴⁴⁹ Escalating the matter, Congressman Inslee also contacted Earl Denavey, the Department of Interior’s inspector general to investigate the BLM’s decision as a possible

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⁴⁴⁹ The allegations referred to questions surrounding several research projects carried out by federal scientists employed at a federal agency and academic scientists with federal grants, such as those who researched the hockey stick study that I discussed in Chapter 6.
financial retaliation. Put simply, the political allies painted the picture of a political agenda driving the actions.

Supporters of the Donato team suggested that the political showcasing of this matter, which led to public awareness, played a large role in the BLM’s decision to return the funds. As Andy Stahl, the director of Forest Service Employees for Environmental Ethics, characterized the events to one reporter, “The key to effective censorship is to make sure no one’s looking, and this time everyone was watching.”\footnote{Barnard, Feb. 9, 2006b, paragraph 13.} Stahl and other Donato supporters believed that too many public observers were following the Donato study controversy, and that foiled the BLM’s act of financial retaliation. Further, as mentioned earlier, Congressmen Walden and Baird feared that Inslee’s public display of the problem might have connected the suspension to FERRA. Concerned about those negative impressions, Walden and Baird, who objected to the science behind the Donato study, nevertheless benevolently requested that the BLM reinstate the funds. In short, the attention drawn to this issue from political allies of the Donato study led to BLM’s reinstatement of the grant funding.

**Balancing the Perspectives**

Political allies of the Donato team balanced the research perspectives by giving academic scientists who sided with the Donato study a voice on the congressional floor. In March 2006, Congressman Tom Udall (New Mexico, Democrat) held another special hearing on the Donato study to gather more information.\footnote{Barnard, J. (2006b, Feb. 9). BLM restores funding for controversial logging study. Associated Press State & Local Wire, paragraphs 1-17.}

\textit{Balancing the Perspectives}

Udall felt that Congressman Walden’s special hearing in February 2006 presented a heavily weighted perspective in favor of salvage logging. Udall’s
panel consisted of five academic scientists, whom reporters characterized as leading scholars in forest science, who did not have a salvage logging agenda. Each of these academic scientists had previously taken an ecosystem perspective, which tended to rely more on natural approaches. In their testimony, they suggested that salvage logging might present a viable approach for forest recovery under certain conditions. But, drawing on their expertise, they felt that salvage logging would create significant land disturbances to ecologically sensitive areas such as the one the Donato team studied. They also reiterated that past scientific research had supported their conclusions and the Donato team’s research too. Many media outlets (e.g., The Chronicle of Higher Education, The Oregonian, Seattle Post-Intelligencer, The Washington Post) along with supporters of the Donato study used the testimony in further support of arguments stating that Congressman Walden’s hearing presented a favorable picture of salvage logging, ignored the larger body of research, and unfairly discounted the Donato study.

Possible Conflicts of Interest

According to numerous blogs, news coverage quotes, and editorials, many Oregon citizens had long suspected that the CoF responded favorably to timber industry needs. Previously, these expressions relied heavily on conjecture or mere speculation. However, the controversy surrounding the Donato study gave political allies of the Donato team an opportunity to investigate the possible ties between the CoF and the timber industry.

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452 These scientists were Dave Perry, professor emeritus of forest ecosystems at Oregon State University; Tania Schoennagel, postdoctoral fellow, Dept. of Geography, University of Colorado-Boulder; Jon Evans, professor of biology at the University of the South; James R. Karr, professor of fisheries and zoology at the University of Washington; and Richard Hutto, director of the avian science center and professor of biology at Montana State University.

The investigation started after the CoF dean issued his memo (which had the effect of minimizing the Donato study) and the Gang of Nine tried to discredit and delay the study’s publication. During the spring of 2006, Oregon State Senator Charlie Ringo, a supporter of the Donato study, initiated a public records request of the CoF dean’s e-mails involving communications about the Donato team’s *Science* article. The retrieved e-mails revealed that various members of the CoF acted in ways to advance the timber industry’s interests. For example, prior to the dean’s issuance of his January 11, 2006 memo to “contextualize” the Donato study, he circulated drafts of his memo, requesting feedback on how to frame his statements. Rather than seeking feedback from both sides of the issue, the dean contacted only individuals who opposed the study. His e-mail distribution included two members of the Gang of Nine, John Sessions and Mike Newton, as well as Chris West of the American Forest Resource Council (AFRC), an advocacy group for the timber industry, and Rita Neznek, governmental affairs manager for the Society of American Foresters, a group that strongly advocated for salvage logging as an approach to forest recovery after wildfires. Several e-mail exchanges among members of the GoN, the dean, representatives of industry, and other proponents in the salvage logging debate also referred to the e-mailed individuals’ actions to discredit the Donato study and to participate in “damage control.”

As Senator Ringo stated in an April 2006 Oregon legislative session and expressly testified before the U.S. Senate in August 2006, the e-mails demonstrated a “very close coordination with industry representatives and government officials to put the right spin on this

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454 Like most states, Oregon has a statutory requirement that individuals and organizations may obtain copies of public records for a nominal fee. These public records include state employee e-mails discussing matters in pursuit of a policy decision, such as Salwasser’s e-mails dealing with the Donato study.

455 The AFRC’s mission states that its purpose is to “create a favorable operating environment for the forest products industry.”

[study] so that it would not have an inconvenient impact for this legislation. It is very possible that the dean, Gang of Nine, timber industry, and proponents of the FERRA legislation relied on sound science to determine their position. Nonetheless, their actions appear to present a coordinated effort to challenge a study by closing the debate rather than fully exploring the science behind the study. Observers, particularly supporters of the Donato study, emphasized this perception and noted that some of the Donato challengers, particularly the dean and Gang of Nine, might not have spoken in a disinterested manner. The supporters, instead, characterized these challengers as acting in a way that unfairly objected to the Donato study because the challengers wanted to advance the goals of salvage logging.

Put simply, the investigation by the Donato study’s political allies weakened the credibility of the challengers and concomitantly strengthened the credibility of the Donato team.

Collective Impact

In sum, each of the three events (i.e., reinstatement of the BLM grant funding; the congressional forum reaffirming the Donato study findings and conclusions; disclosure of the challengers’ interests) raised questions about the interests and credibility of the lay governmental actors who challenged the Donato study. In addition, collectively, these events demonstrated the challengers’ policy preference for salvage logging even if that support meant employing irregular measures or using one-sided perspectives to discredit a federally funded academic research study despite insufficient evidence of sloppy scientific work or other sorts of wrongdoing.

Both Internal Characteristics & External Factors

Supporters of the Donato study combined both the internal characteristics of and external factors influencing the academic profession as a strategy to address the challenges and to enable the Donato team to exert control over federally funded research. Under this strategy, supporters

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of the Donato study relied heavily on the science – both the profession’s exercise of professional knowledge and skills (an internal characteristic) and the bodies of knowledge (an independent external factor).

First, in various news reports and other publicly accessible sources – including Donato’s testimony at the February 24, 2006 field hearing, the Donato team stood behind its research as being scientifically sound. The team emphasized its exercise of *professional knowledge and skills*, such as its rationale behind the statistical approach and the data sites used. The team also explained what it believed was the study’s contribution within the scope of the larger body of literature on post-fire forest-recovery approaches to overcome assertions from critics that the study was an outlier with little to no scientific support in the academic and forest industry communities. Second, other academic scientists reviewed the literature on post-fire forest-recovery approaches and concluded that the Donato study findings reflected scientific consistency. Drawing on the external factor of *bodies of knowledge*, these other academic scientists publicly supported the Donato team’s findings. For example, a group of 169 academic scientists, who researched forest recovery-related issues from a variety of environmental specialties, such as biology, forest science, fire ecology, and geography, also concluded that salvage logging “actually slow[ed] the natural recovery of forests and of streams and creatures within them,” presented increased forest fire risks, and failed to offer economic benefits.\(^{458}\) They wrote a letter to Congress to express their opposition to salvage logging and their support for the Donato study.\(^ {459}\) Third, other forms of this particular external factor, *bodies of knowledge*,


\(^ {459}\) In August 2006, before the U.S. Senate voted on the FERRA bill, 546 scientists wrote to members of the Senate to convey the opinion that salvage logging actually contributes to fueling future forest fires and causes significant land disturbance. Although the letter supported the Donato study, the authors centered much more on the science of salvage logging than defending the research of one study.
bolstered the Donato study’s credibility after the Donato controversy subsided. These subsequent studies, which were published in selective academic journals, also presented similar findings about the effects of natural seedling growth after a wildfire. Thus, these studies provided some confirmatory evidence that the Donato study represented sound research. Indeed, this strategy of relying on the science convinced many academic scientists, forestry professionals, and policymakers that the Donato research team had conducted a defensible study (though not necessarily a study without flaws and/or contestable research perspectives).

Summary of Responses

The academic scientists who supported the Donato team and its study employed three principal strategies. First, drawing on the internal characteristics of the academic profession, these academic scientists used the “gameplay” of the academic profession, turning academic rules into a strategy. Second, these academic scientists relied heavily on political allies as an external factor to overcome some of the challenges facing the Donato team. Third, these academic scientists used the study’s science, which integrated the profession’s exercise of professional knowledge and skills (an internal characteristic) and the bodies of knowledge (an independent external factor) as a strategy for the Donato team. These three overarching strategies demonstrate how structuring into workable actions the internal characteristics of and external factors influencing the academic profession helped the Donato team exert control over its federally funded research.

Summary & Conclusion

In this chapter, I presented the challenges to a federally funded research study and the strategic responses that helped the academic scientists who wrote the contested study to exert control over it. The contested study, a federally sponsored research project referred to as the Donato study, questioned the effectiveness of salvage logging as a forest-recovery approach to certain landscapes harmed by the Biscuit Fire. According to critics of salvage logging, the Donato study contradicted the policy rationale of a proposed bill, the Forest Ecosystem Recovery and Research Act (FERRA), which provided for an expedited review process for salvage logging projects.

Reconstructing the interactions among the primary actors of the conflict like a dramatic play, I recounted the circumstances and events surrounding the release of the study and the multistage set of challenges that followed. At first, members of the academic community and a few forest service staff initiated the challenges. Events revealed that these challengers had some connection to the timber industry and policymakers, but they acted primarily as backstage crew and at times in a supporting-cast role. Later, several governmental actors entered the stage, and they challenged the Donato team and its research through a grant suspension and a field hearing. From the perspective of the Donato team supporters, the government actions reflected a targeted attack on one of the study’s authors, Dan Donato, who was a graduate student and one of the most junior academic scientists on the Donato team.

In response to those challenges, academic scientists who supported the Donato team employed three significant strategies, which emerged when applying a modified version of Freidson’s model of the profession. Following that model as my conceptual framework, I divided the Donato team supporters’ responses into events that relied heavily on (a) internal
characteristics of the academic profession, (b) external factors influencing the academic profession, and (c) both internal and external factors of the academic profession. That led me to uncover several strategies that the Donato team supporters used.

Under the internal characteristics frame, the academic scientists supporting the Donato team asserted traditional academic “rules” for professional interaction. Initially, they used academic censure as a way to admonish the Gang of Nine and set the tone that they would stand behind the Donato team. Next, they pushed for the creation of an academic committee to investigate the conflicts and develop new internal policies and practices. The new policies and practices started to alter the organizational culture of Oregon State University’ College of Forestry. It made efforts to balance the power between senior and junior faculty, gave the graduate students a voice, emphasized being open to different perspectives – especially drawing in the more natural ecology perspective – and vocalized a conscious effort to advance the goals of free inquiry in research (i.e., avoid actions that might be considered to show undue research interference).

Under the external factors frame, the supporters of the Donato study relied on actors outside of the academic community who held legal authority and public influence to help the Donato team exert control over its federally sponsored research. Typically, the events involved supporters of the Donato team connecting with political allies and informing them of the challenges. Following an event of that type, the political allies used public venues to call public attention to the situation and protest the challengers’ actions. Public outcry would ensue and demand corrective action. Based on this series of actions, the Donato political allies managed to push for reinstatement of the BLM grant, publicly reaffirm the Donato study findings and
conclusions, and weaken the challengers’ public standing by disclosing their intentions, which appeared to conflict with the public’s interests.

When combining both the internal and external factors of the academic profession, actions using the science and professional expertise of the Donato research supporters and the Donato team itself emerged as a significant strategy to help the Donato team exert control over its federally sponsored research. Drawing from their science and professional expertise, many academic scientists, forestry professionals, and policymakers became convinced that the Donato research team had conducted a nearly defensible study – at least one credible enough to stand for the general proposition that natural regrowth of forests presents a viable approach to forest recovery.

One final observation: In this chapter, I captured the mixed effects of federal actors and members of the media. I demonstrated how governmental actors were both challengers and supporters. Similarly, I illustrated how the challengers used the media against the Donato team and helped publicize irregular challenges to garner support for the Donato team. These data remind us that neither federal policymakers nor members of the media reflect monolithic groups. Further, these data draw us back to my theoretical framework, which is a modified version of Freidson’s theory of the professions. The framework indicates that external factors, such as individuals with legal authority (i.e., federal policymakers) and sources for public dissemination of information (i.e., media) can moderate, positively and negatively, the extent to which a profession can exert control over its work. Put simply, the Responses to the Challenges section showed how the internal characteristics, external factors, and the blend of both internal characteristics and external factors revealed ways that academic scientists can exert control over
their research. Further, we recognize that the external factors also have the capacity to limit or even eliminate that ability of academic scientists to control their research.

In the next chapter, I present my cross-case analysis of the sewage sludge, climate change, and salvage logging cases.
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<th>Acronym</th>
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<td>CAFR</td>
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<td>Co-PIs</td>
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<td>FERRA</td>
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<td>Sessions Report advocates for immediate and aggressive actions to avoid the time and energy needed to pave roads and overcome other logistical hurdles.</td>
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<td>Sept. 2003</td>
<td>Bureau of Land Management (BLM) awards a three-year grant totaling $307,000 to Oregon State University (OSU).</td>
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<td>2003-2005</td>
<td>Environmental groups initiate lawsuits for temporary restraining orders to stop salvage logging.</td>
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<td>2005-2006</td>
<td>Congressman Greg Walden (Oregon, Republican) is principal author of House bill, the <em>Forest Emergency Recovery and Research Act</em> (FERRA). The bill would relax procedural approvals for forest-recovery efforts including salvage logging.</td>
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<td>Jan. 11, 2006</td>
<td>Hal Salwasser, Dean, OSU College of Forestry, distributes memo to “contextualize” the Donato study.</td>
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<tr>
<td>Feb. 1, 2006</td>
<td>BLM suspends Donato research grant.</td>
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<tr>
<td>Feb. 5, 2006</td>
<td>BLM issues letter to OSU asking about the grant activity.</td>
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<tr>
<td>Feb. 7, 2006</td>
<td>U.S. Representative Jay Inslee (Washington, Democrat) sends letter to Department of Interior Inspector General Earl Devaney requesting he investigate grant suspension.</td>
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<tr>
<td>Feb. 8, 2006</td>
<td>Congressmen Greg Walden and Brian Baird ask BLM Director Kathleen Clarke to reinstate Donato research funding.</td>
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<tr>
<td>Feb. 8, 2006</td>
<td>Oregon State University responds to BLM inquiry.</td>
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<tr>
<td>Feb. 8, 2006</td>
<td>BLM reinstates grant funds.</td>
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<tr>
<td>Feb. 8, 2006</td>
<td>U.S. Representative Walden agrees to conduct field hearing based on a January 2006 request from U.S. Representative Tom Udall (New Mexico, Democrat).</td>
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<tr>
<td>Feb. 8, 2006</td>
<td>University Provost Sabah Randhawa and Faculty Senate President Bill Boggess issue joint letter to the university community rebuking the College of Forestry professors who participated in the Gang of Nine letter.</td>
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<tr>
<td>Feb. 9, 2006</td>
<td>Faculty Senate unanimously passes a resolution affirming academic freedom for Donato team and admonishing the College of Forestry professors who participated in the Gang of Nine letter.</td>
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<tr>
<td>Feb. 10, 2006</td>
<td>Hal Salwasser announces the establishment of the College of Forestry’s Committee on Academic Freedom and Responsibility to investigate the working climate and make recommendations to improve that environment.</td>
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<tr>
<td>Feb. 24, 2006</td>
<td>House Forestry Panel conducts a field hearing about the Donato study.</td>
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<tr>
<td>Mar. 14, 2006</td>
<td>169 academic scientists, who research forest recovery-related issues, write to members of Congress in support of the Donato study findings.</td>
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<tr>
<td>Apr. 2006</td>
<td>Oregon State Senator Charlie Ringo releases Dean Hal Salwasser’s e-mails involving communications about the Donato team’s Science article. E-mails demonstrate the college’s connection with industry and trade groups advocating for the FERRA bill.</td>
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<tr>
<td>Jul. 25, 2006</td>
<td>Committee on Academic Freedom and Responsibility issues its report findings, including commitments that the dean will make to further the goals of academic freedom.</td>
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<tr>
<td>Aug. 2006</td>
<td>Oregon State Senator Charlie Ringo testifies before the U.S. Senate about the College of Forestry’s spin on the Donato study.</td>
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<tr>
<td>2006-2007</td>
<td>From the spring 2006 term through the fall 2007 term, the College of Forestry takes steps to gain more diverse perspectives and establish policies and practices that further academic freedom.</td>
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CHAPTER VIII: GOVERNMENT CHALLENGES & THE ACADEMIC PROFESSION’S RESPONSES: PRESENTATION OF THE FINDINGS

Introduction

At the beginning of this dissertation, I stated that the purpose of my study was to investigate what it means (a) for government actors to challenge academic scientists’ federally sponsored research and (b) for academic scientists to exert control over that research. I reviewed the existing literature on these subjects in chapters 2 and 3, respectively, and identified gaps in that literature.

For example, the literature I review in Chapter 2 suggests that government efforts to alter, discredit, or suppress the findings of federally funded research are often driven by the policy preferences and political agendas of those in power. It also appears that such efforts rest on evidence and assumptions that are generated through governmental fact-finding methods (e.g., public hearings) and political interactions rather than through the peer-review processes on which scholars usually rely.

Similarly, the literature I review in Chapter 3 suggests that the academic profession’s efforts to protect itself and to maintain control over the findings of federally funded research it conducts draw mostly on the academic profession’s internal characteristics, in which, for example, credibility rests on expert knowledge, specialized training, and recognized qualifications for practice within a field. These, collectively, represent institutionalized arrangements that have given professors autonomy over their work.

However, after reviewing the extant literature in both of those chapters, I argue that these initial characterizations – of government efforts to control research findings and the academic
profession’s efforts to protect its research findings and its professional autonomy – overlook several key factors. First, as I mentioned in both chapters 2 and 3, the literature pays little attention to other, potentially important, actors in this process. Prior studies tend to focus almost exclusively on the relationship between the government challengers and the academic researchers who have conducted the challenged studies, but other people and entities also play important roles. Second, as I discuss in Chapter 3, the literature on the academic profession’s responses to government challenges tends to omit an examination of additional internal characteristics of the academic profession that highlight its exercise of expertise. It also tends to overlook ways in which the profession generates external support for its research and its methods, as well as considerations regarding the boundaries of work for academic scientists and policymakers when addressing science policy questions through federally funded research. In addition, prior research often does not consider how the academic profession’s control over the training and development of future researchers potentially plays out in the academic profession’s responses to government challenges. My study seeks to address some of these gaps, drawing in part on the work of Freidson.

To explore the concepts of government challenges and academic scientists’ efforts to exert control, I set out to uncover the pressures and tactics that governmental and nongovernmental actors have employed to challenge academic scientists’ federally sponsored research and the strategies and tactics the academic profession has used to defend that research and protect its autonomy. As part of that investigation, in chapters 5, 6, and 7, I reported the data for each of my three cases using a chronological narrative to recount the circumstances and events surrounding the government challenges and the profession’s responses. I also rely on
Freidson’s theory of the professions, which I introduced in Chapter 3, to organize the data representing the academic profession’s responses.

In this chapter, I continue to frame my findings in terms of the government challenges and the academic profession’s responses. In Part I, I present two themes, derived from the data, that identify the kinds of governmental and nongovernmental actors who participated in posing challenges to academic scientists’ research on sewage sludge, global warming, and salvage logging and, especially, the methods (i.e., pressures, tactics, and other means) those actors employed to carry out those challenges. In Part II, I report three cross-case themes that emerged from the data to identify the strategies and tactics as well as the range of strategies, tactics, and/or other means that academic scientists (here as representatives of the academic profession, per Freidson) employed in responding to the government challenges. As I explain each cross-case theme I present supporting evidence for it, and I conclude the discussion of each theme with a summary claim that responds to the research question at issue. In all, the chapter offers five claims, which I present as the conclusions of my study.

Part 1: Government Challenges

In Part 1, I present two data patterns that respond to the research question: Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research, (a) what kinds of governmental actors have participated or are now participating in activities that challenged academic scientists’ research on sewage sludge, global warming, and salvage logging, (b) what kinds of nongovernmental actors have participated or are now participating in activities that challenged academic scientists’ research, (c) what methods did governmental actors employ to challenge academic scientists’ research, and (d) what methods
(i.e., pressures, tactics, and other means) did nongovernmental actors employ to challenge academic scientists’ research?

**Raising Job Performance Concerns**

In the three cases, government challengers, as investors in the federally funded research projects, pressured certain governmental and nongovernmental actors to conform to their requests or face potential penalties. To carry out this action, the government challengers raised job performance concerns, alleging poor oversight, evaluation, and execution of the federally sponsored research project at issue. The government challengers communicated their dissatisfaction with the handling of the sponsored research not only to the researchers themselves but also to three other sets of actors who had some role in its progress or dissemination. While these actors often had behind-the-scenes roles, they had the capacity to advance or inhibit the progress of that federally funded research or possibly other federally funded research projects. Given these actors’ potential importance, the pressures that government challengers placed on them presented real threats to the academic scientists in all three cases.

Specifically, the government challengers in my three cases criticized the job performance of (a) governmental and nongovernmental actors, who held no public policymaking authority but had oversight authority for the federally sponsored project, (b) the peer reviewers who approved the study’s publication, and (c) other academic scientists who either already had or might seek federal sponsorship for their research. The government challengers sent messages strongly suggesting that these three sets of actors take corrective actions, such as downplaying or characterizing the research as untrustworthy. The suggestion was that they might otherwise face penalties, such as reductions in federal sponsorship of academic research. Below, I briefly describe manifestations of this tactic.
Alerting Actors with Oversight Responsibilities

In each case, the government challengers alerted actors with management or research project oversight authority that they had concerns about the federally sponsored research, which ultimately fell within their control. They conveyed the need for these individuals to take corrective action in each circumstance. For instance, in the sewage sludge case, one government challenger informed two Cornell administrators, in writing, about alleged scientific errors with the Cornell Waste Management Institute (CWMI) research, which was at the center of that controversy. Although the Cornell administrators did not act on the matter, various observers interpreted the notification as an attempt to influence the administrators to take action such as downplaying the study or reconsidering its dissemination. In the salvage logging case, the Oregon State University (OSU) administrator who aided the government challengers did act on their behalf after several governmental actors and others who supported the salvage logging approach placed pressure on him to discredit the federally sponsored project at issue (i.e., the Donato study). However, rather than discrediting the Donato study, the OSU administrator chose to downplay the research and endorse the pending bill that two of the government challengers were co-sponsoring. In the climate change case, the government challengers raised concerns with National Science Foundation (NSF) officers after the project had been disseminated. The government challengers wrote to the NSF director questioning how the organization had failed to catch the errors in Michael Mann, Raymond Bradley, and Malcolm Hughes’s (MBH) research (i.e., the hockey study), and why posting the algorithm and raw data was not required in the funding agreement. Further, as observers including academic scientists and other government officials reported, the government challengers’ letter and public statements also suggested that the NSF, as a federal funding agency of academic research, needed to revise
its policies and procedures about data quality and transparency if it wanted continued congressional support.

Besides pointing out job performance concerns, observers in each case reported that the government messages implicitly suggested that future funding could be in jeopardy if the actors with oversight responsibilities (i.e., Cornell administrators, NSF project officers, and OSU’s CoF administrators) chose to do nothing about the issues in question. Although the government challengers did not always respond to these pressures, the point here is that the government actors applied the pressures and hoped they would bring about the desired outcome. In other words, these authorities with oversight perceived the government messages as not-so-subtle threats.

Criticizing the Peer Reviewers

The government challengers in each of my cases also criticized the job performance of peer reviewers who considered the underlying research to be worthy of publication. As investors in academic research, the government challengers indicated their interest in the studies and the review process that is in place to vet and correct research papers. The government challengers reported, however, that the peer reviewers had failed to exercise due diligence in reviewing papers produced with federal funding. Citing other scientists, policymakers, and members of the media, some of whom in each case had reported flaws in the studies, the government challengers raised several concerns, including their sense that the peer reviewers conducted poor evaluations of the papers, which misled the government investors to believe that their funds (i.e., the government resources) had been well spent.

Of the three cases, the government challengers in the climate change case were the most critical of the peer reviewers. They argued that the peer reviewers were likely biased. Based on
information from their consultants, the government challengers expressed concerns about the research cliques of the hockey stick study’s lead author, Michael Mann. Individuals in this alleged clique were other paleoclimate scientists, who relied on the same proxy data and co-authored with Mann. Given the tight-knit community of paleoclimate scientists, the government challengers contended that these scientists were less likely to criticize each other’s works when they served as peer reviewers, which in turn would explain why the peer reviewers had not caught alleged problems with Michael Mann, Raymond Bradley, and Malcolm Hughes’s data proxies or with the statistical procedures that led to the hockey stick figure. Expressing concerns that the reviewers had some job performance problems, Congressman Ed Whitfield stated: “[I]t is clear that peer review[ers] somehow failed to pick up the flaws in the hockey stick studies.”

In the other two cases, the government challengers also criticized the peer reviewers’ job performance, but they did not explicitly allege that the peer reviewers were likely biased. In the sewage sludge case, the government challengers contended that the peer reviewers should have noted several significant errors, such as the Cornell Waste Management Institute researchers’ incorrect assertions about the sludge effects and improper risk-assessment measures. In the salvage logging case, the government challengers claimed that appropriate experts in the field would have caught the special context about the studied land site and the limits in the data’s usefulness. According to the government challengers and their supporters, that information would have led to identifying problems with the Donato study and likely to its rejection by a journal of note.

In short, the government challengers reported that the peer reviewers in the three cases conducted poor-quality reviews that severely misjudged the value of the government’s investments. They suggested that if the peer-review process was not altered, government officials would need to entertain another vetting process for federally funded academic research. In the climate study case, the government challengers and their supporters even suggested a full research audit, which would include replicating the study under government supervision before it could be considered in shaping science policy.

Forewarning Other Academic Scientists

In the three cases, some academic scientists reported that the government challengers’ actions communicated a subtle message to other academic scientists not directly involved in the controversy surrounding the challenged study. According to these observers, the government challengers, by their actions, conveyed to the larger audience of academic scientists an unstated rule. The rule was that scientists who seek or receive federal funding for their research could face penalties if their work were to run counter to policies that government officials had publicly supported through actions such as authoring or advocating for an agency regulation or federal statute.

In the sewage sludge case, several academic scientists interpreted the U.S. Environmental Protection Agency’s (EPA) letter to the Associate Dean at Cornell’s College of Agriculture as a message that if other Cornell scientists were to disseminate research like the CWM’s Case for Caution paper, the university might face problems when it sought future federal grant funding. Similarly, in the salvage logging case, academic scientists believed that the government challengers were also suggesting that research in conflict with a policy could adversely affect future funding prospects. They cited the Bureau of Land Management’s temporary grant
suspension as evidence of the government challengers’ message: that grant recipients should not disseminate findings that conflict with a pending bill. Finally, in the climate change case, academic scientists also felt threatened; they believed that the government challengers would penalize them financially for research inconsistent with the officials’ preferred federal policies. In this case, the academic scientists believed also that the government challengers could impose other penalties, including highly burdensome information requests, such as the need to: produce volumes of materials, including past research records that were not part of the federally funded project. That is what happened to the three “hockey stick” researchers at the center of the climate change case.

Collectively, these examples illustrate that other academic scientists, even those who were not actively involved in the funded research, were also affected by the government challengers’ actions. They interpreted the government challengers’ actions as pressures that they, too, would encounter if they used federal funding to produce findings inconsistent with policies that officials had publicly supported (e.g., by authoring a regulation or sponsoring pending legislation).

Claim: Influencing Indirect Participants in the Federally Funded Research at Issue

As the preceding examples suggest, government actors are not necessarily silent investors who leave scientific decisions to the scientists. At times, government actors translate their role as investors in research into opportunities to influence actual research findings. For example, my cases illustrate how government actors as funders of academic research can comment on a researcher’s job performance, or the quality of the research, in an attempt to alter the direction of a sponsored project.
But it does not always stop there. In the three cases, the government challengers also made evaluative comments about the job performance of other actors who were indirect participants in the federally funded research. As indirect participants, these actors had no role in scientific decisions about the challenged study, such as crafting the research design, collecting and analyzing the data, or articulating the study findings. Nonetheless, as the government challengers were well aware, these indirect participants, with seemingly minor roles in the controversy, had the capacity to advance or inhibit the progress of the federally funded research study at hand or the progress of federally funded research projects by other scholars observing the events surrounding these cases. In other words, the data suggest that the government challengers may try – sometimes successfully – to exercise their influence over indirect participants of the federally funded research in an attempt to control the dissemination of the federally sponsored research findings. As the data demonstrate, they may do so in a variety of ways – for example, by questioning the quality of job performance of actors who are indirect participants in the federally funded research. In particular, this may involve actions such as alerting actors who have oversight authority over the federally sponsored project, criticizing the quality of the peer reviewers’ reports about the funded research, and issuing messages that, though focused on the case at issue, also serve to forewarn the larger community of academic scientists (including those wishing to apply for research support) of potential penalties if the study they produce contradicts an environmental policy that the government officials have publicly supported.
Table 8.1: Summary of Finding #1

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<thead>
<tr>
<th>Data Pattern</th>
<th>Evidence</th>
<th>Claim</th>
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<tr>
<td>Raising Job Performance Concerns</td>
<td>Alerting Actors with Oversight Responsibilities</td>
<td>The government challengers may try – sometimes successfully – to exercise their influence over indirect participants of the federally funded research in an attempt to control the dissemination of federally sponsored research findings.</td>
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<td>Criticizing the Peer Reviewers</td>
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<td>Forewarning Other Academic Scientists</td>
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Defining What Qualifies as Legitimate Science

The academic peer-review process rests heavily on the assumption that independent experts, who are peers in the field, are most qualified to judge the quality of a research paper. Accordingly, members of the academic profession and practicing scientists typically serve as peer reviewers. My data, however, exhibited a pattern that deviated from the traditional academic peer-review process. Specifically, *in my three cases, the government challengers positioned themselves as definers of scientific legitimacy by attempting to alter or even replace the peer-review process for scientific research*. In this section, I identify three forms this took in my cases, namely through (a) the review process, (b) the selective use of sources, and (c) government challengers’ evaluative expressions about the science.
Finding Governmental Substitutes for Peer Review

As mentioned above, the government challengers in each of my cases claimed that the peer reviewers had failed to exercise due diligence when examining the contested studies. Rather than relying on the scientific review process, in each case, the government challengers created a different system with which to review the federally funded study. For instance, in the sewage sludge case, the government challengers participated in an internal review of the study at issue through a closed review process. Specifically, the EPA and U.S. Department of Agriculture (USDA) evaluated the Cornell Waste Management Institute’s (CWMI) assertions by exchanging internal government memos to counter the CWMI scientists’ criticisms of the 503 sludge rules. As I discuss later in this section, this closed review is what the EPA relied on in discounting the CWMI research.

In the climate change and salvage logging cases, the government challengers conducted both closed reviews and public forums, that were only open to invited guests, to evaluate the research studies. In the climate change case, the government challengers first commissioned their own team of academic statisticians to evaluate the MBH studies. Then, following the issuance of that report, the government challengers held a congressional oversight hearing, in which the invited guests with a position that disagreed with the academic research were over-represented as speakers. They also presented allegations of scientific flaws with the study at issue and attacked the academic peer-review process. Similarly, drawing on feedback from various sympathetic academic scientists and forest management practitioners, the government challengers in the salvage logging case evaluated the study. Using the information in the evaluation report, they held a congressional oversight hearing to challenge the Donato team’s
research, which was at the center of this controversy. In sum, the government challengers used their own review process, guided by political partisans, to evaluate these scientific studies.

*Selecting Expert Sources to Contest the Validity of the Federally Funded Research*

In each case, the government challengers relied heavily on carefully selected reports and sources as their basis for arguing that the federally sponsored research studies at issue had used flawed data and faulty methodological approaches. Based on these allegations of imperfect data and methodologies, the government challengers publicly criticized the findings of these federally sponsored studies and characterized them as invalid works (i.e., not worthy of scientific recognition). These sources consistently presented perspectives that aligned with their policy preferences. For example, in the sewage sludge case, the EPA referred to several internal memos and USDA correspondence as the basis for challenging the Cornell Waste Management Institute (CWMI) study. While referring to these selected papers, they omitted many others. Most notably, the EPA omitted studies supporting the CWMI paper, including research by Dr. David Lewis, then an EPA scientist (see Chapter 5). His and other studies strongly suggested that the CWMI researchers’ findings and recommendations presented viable scientific claims, and found that the sewage sludge rules did not sufficiently consider potential harms from certain chemical and metal exposure.

In the climate change case, the government challengers heavily cited critics of the hockey stick studies, chiefly Stephen McIntyre and Ross McKitrick, to question the reliability of the federally funded research by Michael Mann, Raymond Bradley, and Malcolm Hughes (MBH), yet the challengers did not fully explore opposing research. Illustrating this point, the letter with which Congressmen Barton and Whitfield initiated the investigation extensively quoted McIntyre and McKitrick’s (M&M) allegations of scientific flaws in the hockey stick study and
MBH’s alleged failure to disclose data as the basis for the congressional inquiry. Yet, neither McIntyre nor McKitrick had any training in paleoclimate science. And when the government challengers quoted the National Research Council (NRC) study, which largely supported the MBH research, they focused only on negative comments from the report and did not acknowledge the NRC’s recognition of MBH’s scientific contributions. They also did not cite other research that generally supported MBH’s findings.

Similarly, in the salvage logging case, the government challengers referred to literature and called on experts with a pro-salvage-logging perspective to attack the Donato study, the key federally sponsored academic research at issue in that case. For instance, when Congressman Walden held the February 2006 oversight field hearing, a majority of the experts asserted reasons why the Donato study was insufficient as a policy tool, and they generally expressed support for Walden’s proposed salvage logging bill.

In sum, these events reflected the government challengers’ practice of relying on carefully selected sources, such as policy reports, research studies, and experts that tended to favor the government challengers’ policy position regarding the environmental issue in question and that supported the government challengers’ efforts to discredit the data and methodologies of the federally sponsored research.

**Drawing Conclusions About the Science**

The government challengers in each of my cases drew their own conclusions as they sought to invalidate the studies at issue. Although they often based their evaluative comments on scientific reviews that they selected, these governmental actors extended their comments beyond matters of science policy and into the specific content of environmental science. For example, in the sewage sludge case, the government challengers discounted the CWMI study. On several
occasions, they described the study as fraught with scientific inaccuracies, and they repeatedly conveyed the opinion that the science did not justify the CWMI recommendations. In addition, several government challengers even went so far as to suggest that the CWMI researchers had examined the wrong scientific question. They contended that instead of attending to sewage sludge, the funded researchers should have explored possible harm associated with agricultural applications of animal manure.

The other two cases parallel the storyline of the sewage sludge case. The government challengers in the global warming case indicated that the data proxies, statistical analyses, and findings were scientifically flawed. While Congressmen Barton and Whitfield often cited Stephen McIntyre and Ross McKitrick (M&M) and the Wegman team as sources for their assertions, it was Barton and Whitfield who ultimately determined that the MBH studies could not inform policymakers. They concluded that the studies were not scientifically sound. These government actors along with others also argued against claims that members of the scientific community were in agreement about anthropogenic causes of global warming. Similarly, in the salvage logging case, the government challengers contested the scientific significance of the Donato study. They alleged that the Donato study presented scientifically misleading evidence, and one government actor, who had a Ph.D. in psychology, stated that the statistical approach presented confusing data that were subject to significant misinterpretation.

In short, although the government challengers usually referred to scientific sources for expert assistance in formulating their comments about the science at issue, ultimately they drew conclusions that extended beyond policy and assessed the scientific content itself.
Claim: Determining What Counts as Legitimate Scientific Research

The data drew attention to government challengers’ actions of evaluating federally funded research projects about environmental science with results that conflicted with federal policies that the government officials had publicly supported. Specifically, in these three cases, the government challengers established a review process. They used selective policy tools to help them understand the science. Then, they drew scientific conclusions or asserted their judgments about the research. In other words, based on the data, we see that government actors, though not scientists themselves, relied heavily on their own judgment to declare publicly the kinds of activities that can count as legitimate scientific research, rather than relying on the traditional scientific peer-review process. Their tactics might include substituting governmental processes for peer review and using the process as a forum to judge the scientific merits of federally sponsored research; selecting expert sources to use as reliable scientific evidence to contest the validity of the federally funded studies’ data, methodology, and findings; and relying on their own judgments – but as nonscientists – to draw conclusions about scientific matters.
Table 8.2: Summary of Finding #2

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<td>Defining What Qualifies and Does Not Qualify as Legitimate Science</td>
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<tr>
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<td>and Findings</td>
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Part 2: The Academic Profession’s Responses

In Part 2, I present three data patterns that respond to the research question: Drawing on circumstances and events surrounding several lay challenges to academic scientists’ federally funded research, (a) how did academic scientists, universities, professional associations, and other professional bodies in the Academy respond to the lay challenges to academic scientists’ research on sewage sludge, global warming, and salvage logging, (b) what range of strategies, tactics, and/or other means did academic scientists, universities, professional associations, and other professional bodies employ to preserve or defend the profession’s intellectual freedom?

Engaging the Public as Arbitrators of the Dispute About Who Should Evaluate the Science

The academic scientists did not respond to the government challenges by themselves. Among their strategies, they sought assistance from others, in particular, members of the public.
In my study, these actors came from multiple groups, such as other scientists who did not conduct the challenged research, residents of the affected areas that the challenged research addressed, members of the media, and environmental groups. In the three cases, the academic scientists engaged the public as arbitrators of the dispute regarding whether academic scientists or policymakers should address the scientific matters at hand. They did this by presenting to the public: (a) arguments to rebut the government challengers’ statements; (b) reasons that likely explained the policymakers’ interests in the outcome; and (c) past instances of government mischaracterizations of scientific research. Then, the public would take action that defended the academic scientists’ research. Below, I provide evidence for this claim.

Providing the Public with Arguments to Rebut the Government Challengers’ Assertions

In all three cases, the academic scientists fed the public with arguments rebutting the government challengers’ statements. In the sewage sludge case, the Cornell Waste Management Institute (CWMI) researchers disseminated information about multiple instances in which they claimed that the EPA made false assertions about their research. For example, when the EPA alleged that the CWMI researchers had misled readers of the study by using incorrect assessment levels and applying metal salts to the sludge samples to produce results that demonstrated harmful effects, the CWMI researchers offered evidence that contradicted the government’s allegations. Members of the public, including protestors who opposed sewage sludge application, cited these examples of research mischaracterizations in newsletters, media reports, and public discussions, as reasons to discount the government challengers’ statements. In the climate change case, the academic scientists created a blog, Realclimate.org, to clarify or counter many of the government challengers’ claims. One much-discussed posting presented evidence that a group of climate scientists had, in fact, replicated the Michael Mann, Raymond Bradley,
and Malcolm Hughes’s (MBH) hockey stick, despite assertions that replication was not possible, from Stephen McIntyre and Ross McKitrick (M&M), the non-paleoclimate scientists whose work the government relied upon to contest the validity of the hockey stick research, and from the Wegman team, the statisticians the government had commissioned to evaluate the hockey stick researchers’ work and whose work relied heavily on M&M’s data and algorithm.

In the salvage logging case, the academic scientists and their supporters provided members of the public scientific evidence to support the Donato paper, the research study that the government had challenged. Citing several peer-reviewed scientific studies, the academic scientists and their supporters countered the government challengers’ and environmental opponents’ assertions that forest recovery after severe fires required human intervention and land disturbances – namely, salvage logging. Confirming the Donato paper, these studies presented findings that forest-recovery efforts using a natural, eco-friendly, approach, had been successful in the past in regrowing forests after severe fires. Using this information, environmental groups, scientists, and citizens – the last group including members of the public who lived near the burnt areas that the Donato paper examined – presented these arguments in several venues, such as newspapers and blogs, as justifications to support the Donato team’s findings.

As these events illustrate, after the academic scientists rebutted the government challengers’ statements, some members of the public responded by way of an active campaign in support of the academic scientists’ research.

**Identifying Policymakers’ Policy Agenda and Preferences**

Academic scientists identified for the public what the policymakers’ interests were in supporting their policy preferences. Generally speaking, the academic scientists and their supporters presented the policymakers’ interests in terms of the policymakers’ personal
motivations to advance the policy and the potential benefits of aligning the policy with the presidential administration’s agenda. In the sewage sludge case, the academic scientists exposed the governmental actors’ interest in preserving their reputations. Spokespersons for the academic scientists reported that many of the government officials who enforced and defended the policy had historical ties to the policy, including several who had crafted the 503 regulations when they were initially adopted and viewed the policy as a well-crafted solution to the ocean dumping problem. In addition, the academic profession noted another way that these policymakers sought to advance their personal reputations as environmentally friendly problem solvers. Several of the government officials, who helped challenge the CWMI research, also supported the EPA decision to hire a public relations group to spin the positive benefits of sewage sludge. The policymakers’ interests in the climate change and salvage logging cases differed from those in the sewage sludge case. In the climate change and salvage logging cases, the academic scientists and their supporters identified financial contributions from industry groups and lobbyists as the guiding source for policymakers’ interest. The academic scientists in all three cases also indicated that besides personal gains, the policymakers had an interest in supporting a particular environmental policy because it aligned with the presidential administration’s agenda. The presumption was that the policymakers’ support would lead to political favors, but the data never explicitly revealed these actors’ reasons.

What is clear in the data is that after the academic scientists and their supporters revealed the policymakers’ financial and political interests, many observers – including scientists who had played no part in conducting the challenged research, residents of the affected areas, and environmental groups – believed that those interests had probably motivated the policymakers’ actions, particularly their attempts to discredit the academic scientists’ research at the center of
each dispute. Through newspaper editorials, blogs, newsletters, and other means of public dissemination, these observers conveyed to other members of the public the message that the academic scientists should make the scientific judgments and that government should not interfere in that process.

Referring to Past Instances of Scientific Mischaracterization

Academic scientists showed how the government might be mischaracterizing scientific implications in ways that paralleled government behavior in similar recent situations. In the climate change and salvage logging cases, the academic scientists connected the government challenges to evidence of an overarching pattern of Republican Party interference with scientific studies that conflicted with federal policies the party endorsed. Scientific groups such as the Union of Concerned Scientists reported that government challengers had done likewise on environmental issues in the decade that began in 2000, including government efforts to cover up the harmful effects of tobacco use. By that time, an overwhelming majority of scientific experts had concluded that tobacco use created significant health hazards for exposed individuals. Nonetheless, some governmental actors sowed doubts about the science on tobacco use and exposure. Similarly, the academic scientists connected the sewage sludge case to past complaints of alleged EPA cover-ups. During the late 1990s and early 2000s, for example, environmental groups and academic scientists had accused the EPA of longstanding efforts to silence environmental research showing adverse health effects from toxic substances such as those found in pollution. The academic scientists tied the sewage sludge case to these past incidents to demonstrate a continuing problem in the EPA. That is, the EPA had continued to close off discussions or to address only partly the health implications of sewage sludge – as it had with other environmental issues in the past.
To summarize, the academic scientists drew attention to the government challengers’ past practices of sowing doubt about scientific research. They did this to point out that government officials could be using the same tactic in these new cases.

Claim: Moving a Significant Player into the Dispute

During these three disputes, academic scientists sometimes brought the public into the debate over whether government officials or academic scientists should evaluate the science that informs federal policymakers. The “public” here included scientists who had not conducted the challenged research, residents of the affected areas, and environmental groups that represented members of the public. The academic scientists and their supporters helped engage the public as arbitrators by providing arguments to rebut the government challengers’ statements, identifying policymakers’ agendas and preferences, and referring to past instances of scientific mischaracterization by government actors. In these ways, members of the public became significant participants in such disputes.

Table 8.3: Summary of Finding #3

<table>
<thead>
<tr>
<th>Data Pattern</th>
<th>Evidence</th>
<th>Claim</th>
</tr>
</thead>
<tbody>
<tr>
<td>Engaging the Public as Arbitrators of Who Should Evaluate the Science</td>
<td>Providing Arguments to Rebut the Government Challengers’ Statements</td>
<td>Academic scientists may involve members of the public in the dispute. When that happens, the public may help decide whether government officials or academic scientists are better equipped to address the scientific matters associated with the federal policy.</td>
</tr>
<tr>
<td></td>
<td>Identifying Policymakers’ Agendas and Preferences</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Connecting to Past Instances of Scientific Mischaracterization</td>
<td></td>
</tr>
</tbody>
</table>
Drawing on the Assistance of Political Allies

Political actors function within systems that have features – such as congressional hearings or commissioned studies – that outsiders cannot always access. However, at times, academic scientists and other nongovernmental actors strive to gain entry to the political system, and at times they succeed. Indeed, this happened in my three cases. Specifically, in each case, academic scientists drew on the support of political allies to initiate action that would serve as a countervailing force to the government challengers’ political actions – namely, of questioning the scientific research. As I discuss below, the political allies, including other public officials, countered the government challengers’ attacks by (a) publicly objecting and criticizing their peers and (b) supporting the academic scientists within political arenas.

Publicly Objecting and Criticizing Peers

Academic scientists worked with political allies to address concerns they had about politicians’ meddling in scientific matters in which they had little or no expertise. Aiding the academic scientists, the political allies publicly objected and criticized their political peers for challenging the academic scientists about the science. For instance, in the sewage sludge case, academic scientists worked with U.S. Representatives James Sensenbrenner, Jr. (Wisconsin, Republican) and Sherwood Boehlert (New York, Republican), who chastised the EPA and the Democrat-led presidential administration for cutting off the academic scientists’ discussion about the possible adverse effects of sludge. In the climate change case, Congressman Boehlert criticized fellow Republican Congressmen Joe Barton (Texas) and Ed Whitfield (Kentucky) for their alleged bullying and harassment of climate scientists. In the salvage logging case, Democratic Congressman Jay Inslee of Washington contested the congressional grilling by Greg
Walden (Oregon, Republican) and fellow Democrat Brian Baird (Washington) of Dan Donato, the lead author of the challenged research on salvage logging. At the time, Walden and Baird had co-sponsored a pending bill that would have afforded the logging industry an expedited process to enter a burnt forest. However, the federally sponsored Donato study concluded that forests can regrow naturally after severe fires, suggesting that the logging industry does not need to remove trees from large sections of the forest to aid regrowth.

The media captured each instance of the political peers’ formal objections and criticisms. Accounts of these public conflicts, including some between members of the same political party, added support to the academic scientists’ claims that the challenges they faced were political, not scientific, in nature.

Facilitating Academic Scientists’ Friendly Encounters

Having consulted with the academic scientists, political allies set the stage for academic scientists to enter friendly venues where they could defend their research. In the sewage sludge and salvage logging cases, political allies scheduled hearings specifically to address the academic scientists’ perspectives, which the government challengers had closed off through earlier actions. For instance, in the sewage sludge case, Congressman Sensenbrenner conducted a hearing that gave the lead author of the challenged study, Ellen Harrison of the Cornell Waste Management Institute (CWMI), an opportunity to describe the ways in which the EPA had closed off the discussion and misconstrued the CWMI research.

In the climate change case, Congressman Sherwood Boehlert commissioned a National Research Council (NRC) study to investigate the allegations that Congressmen Barton and Whitfield had leveled about errors in the federally sponsored hockey stick study. As noted earlier, Barton and Whitfield had chosen to appoint their own three-person investigative team,
making it subject to political control. Boehlert, by contrast, sought the appointment of an NRC panel that would employ typical scientific methods and review processes to conduct a scientific assessment. The NRC study provided a comprehensive scientific review by scientists who understood the relevant science and could evaluate MBH’s research methods and findings. In the salvage logging case, Congressman Tom Udall (New Mexico, Democrat) convened a hearing in which academic scientists, who did not have a salvage logging agenda but had expertise on the scientific issues, presented evidence supporting the Donato team’s study finding that forests can naturally regenerate without having to undergo large-scale salvage logging and forest replanting.

In addition, in all three cases, political allies also posed questions that allowed the academic scientists to present their research in a more favorable light. Even in the climate change hearings, which had been characterized as a political venue to bully the hockey stick study’s lead author, Michael Mann, and the salvage logging field hearing, which the media and other commentators described as a session aimed at attacking Dan Donato, political allies managed to ask questions that allowed these academic scientists to defend their research methods and findings.

In short, the political allies afforded the academic scientists opportunities to participate in political venues in which they could speak openly in defense of their research. In addition, the political allies posed questions that gave the academic scientists opportunities to explain and defend their research.

Claim: Balancing Perspectives Using the Policymakers’ Setting

The preceding discussion suggests that political allies can support academic scientists’ efforts to defend their research within the policymakers’ setting. All told, political allies, sometimes including public officials, represent another set of actors who can participate in the
academic scientists’ strategies of defending their federally sponsored research when government actors challenge their methods and findings. Examples of actions that political allies can take include publicly objecting to and criticizing their political peers for challenging the academic scientists’ federally sponsored research and facilitating academic scientists’ friendly encounters in government venues (e.g., congressional hearings) so the academic scientists can explain and defend their research.

Table 8.4: Summary of Finding #4

<table>
<thead>
<tr>
<th>Data Pattern</th>
<th>Evidence</th>
</tr>
</thead>
<tbody>
<tr>
<td>Drawing on the Assistance of Political Allies</td>
<td>Political Allies Publicly Objecting to and Criticizing Their Political Peers for Challenging the Academic Scientists’ Federally Sponsored Research</td>
</tr>
<tr>
<td></td>
<td>Political Allies Facilitating Academic Scientists’ Friendly Encounters in Government Venues (e.g., congressional hearings) so the Scientists Can Defend their Research</td>
</tr>
</tbody>
</table>

**Claim**

Academic scientists’ political allies can support academic scientists’ efforts to defend their research within the policymakers’ setting.

Reaffirming the Value of Using Academic Conventions to Address Scientific Questions

In these cases, the government challengers generally objected to academic conventions, casting them as inadequate practices that, they said, had led to the release of flawed studies. Nevertheless, in the United States, academic conventions, such as peer review and establishing open environments that foster a marketplace of ideas, have existed for several hundred years as the standard practices for conducting and assessing scientific research. Consistent with these practices, the academic scientists in my three cases advocated and actually used traditional
academic conventions to defend their research. Specifically, in the three cases, academic scientists reaffirmed the value of using academic conventions (e.g., peer review) as the appropriate standards and procedures with which to address scientific questions. The academic scientists did this by forcing a comparison of academic conventions that the scientists used with the political conventions (e.g., congressional hearings) that the government challengers used. These comparisons highlight how academic scientists and the government challengers (a) maintained fundamentally different assumptions, which led to different conclusions, (b) functioned in different environments, which led to differences in transparency, and (c) relied on academic conventions, particularly peer review, to evaluate science, which by the government challengers’ own actions weakened their criticisms on assessing academic research effectively.

Differing Assumptions About the Development of Scientific Knowledge

In these three cases, academic scientists reported what they perceived to be the fundamental differences between the government challengers’ and academic scientists’ view of science: Both groups maintained fundamentally different assumptions about the process by which pieces of scientific information contribute to the state of scientific knowledge of a given topic. Academic scientists think of science as a continuous process. Rather than examining a single study to inform science policy, academic scientists referred to the overall body of knowledge about the given topic and how the challenged study was situated in that larger body of research. This way of thinking appeared to conflict with the government challengers’ policy-driven perspectives on science. According to the academic scientists, the government challengers, eager to make quick decisions consistent with their own policy preferences, were content to rely on one study or on a small number of studies, rather than to review and rely on a larger body of research conducted over a longer period of time.
For instance, in the sewage sludge case, the academic scientists observed that the
government challengers based their criticisms of the Cornell Waste Management Institute
(CWMI) papers on a 1996 National Research Council (NRC) study. Although the NRC report
had been issued only a few years earlier, the academic scientists argued that the report was no
longer relevant. They contended that the report had been written when land-applied sludge use
was a new practice, so scientists had limited available data. In addition, the science had evolved
significantly since the release of that report. As the academic scientists noted, a subsequent NRC
study in 2002 confirmed many of their assertions. Similarly, in the climate change case, the
academic scientists emphasized that scientific inquiry is an evolving process. With dramatic
changes taking place in science, they repeatedly raised concerns about the government
challengers’ interest in focusing on studies that had taken place eight years earlier. Although
they acknowledged the hockey stick authors’ (i.e., Michael Mann, Raymond Bradley, and
Malcolm Hughes) research contribution (it was the first project to use multiple proxies to
reconstruct temperatures), they also identified the practical limits of the hockey stick study. The
academic scientists pointed out that their knowledge about climate change had evolved
substantially in the eight years between publication of the MBH papers and the government
actors’ challenge. Finally, the academic scientists in the salvage logging case argued that the
government actors had relied on older research and ones based on different types of forest lands
as the basis to challenge the Donato study, the academic research project at the center of that
dispute. Although the reforestation studies that government challengers relied upon were old,
they still represented useful science, the academic scientists explained, and they did not
necessarily contradict what the Donato team had found more recently. That is, these studies did
not disprove the possibility that forests could regrow naturally after a severe fire, suggesting that
salvage logging does not present the only policy alternative, as the government challengers stated.

In short, the academic scientists pointed out how the government challengers and the academic scientists worked from different assumptions, leading them, generally, to different conclusions. By comparing the government challengers’ approach, which looked to scientific research for a definitive answer, with the academic scientists’ approach of viewing science as an evolving process that gains perspective over time, the academic scientists convinced many observers – including other scientists, members of the media, environmental groups, and policymakers – that the academic conventions appeared more compelling as an accurate depiction of the science.

*Differing Expectations, Between Academic Scientists and Government Challengers, About Openness and Dialogue*

Academic scientists also argued that they and the government challengers had different expectations about the process involved to understand and evaluate scientific research. For instance, in the sewage sludge case, on numerous occasions, Ellen Harrison of CWMI conveyed her interest in addressing differences between the CWMI and EPA perspectives. Similarly, in the climate change case, academic scientists, including professional associations such as the American Association for the Advancement of Science and National Academy of Sciences, recommended an open forum using scientific channels rather than political venues. The government challengers in both of these cases chose review processes that deviated from academic traditions. They relied on hearings that solicited information from selected individuals and made use of only limited data. As discussed in the last section, the academic scientists’ political allies pushed for National Research Council (NRC) studies. As comprehensive
scientific assessments, NRC studies require an independent panel to extensively review the relevant literature on the scientific topic, interview experts in the field, especially those with different perspectives, subject the report to peer review, and gather feedback from the public. In other words, the academic scientists experienced an open forum involving a multistep review and comment process while the government challengers reviewed the work under a more closed process. Finally, in the salvage logging case, the government challengers also reviewed the academic scientists’ research at issue (i.e., the Donato study) without having an open dialogue – just as had occurred in the other two cases. However, in this case, the editor of *Science* reserved a special section of an issue to address the differing professional opinions about salvage logging versus natural forest regrowth, allowing for an academic discourse in print. In addition, the Oregon State University College of Forestry, where the Donato team worked, also held forums to present the competing sides of the issue.

Collectively, these approaches convey the sense that the government challengers’ approaches were confining and not open to dialogue, whereas the academic conventions offered the possibility of open discussions to critique and/or affirm the academic scientists’ research. Others were involved as well: other scientists, members of the media, environmental groups, and policymakers.

*Using Academic Conventions as Standard Practice*

Academic scientists cited instances where government challengers and their supporters preferred peer review to evaluate scientific research. This point is ironic, because in each case, the government challengers had earlier criticized the peer-review process as biased. Nonetheless, in each case, government challengers and their supporters found that they, too, needed to rely on peer review as a means of showing that their research had been vetted and
represented scientifically credible research. For instance, in the climate change case, which contained the most vocal criticisms about the peer-review process, the government challengers emphasized how their commissioned study, the Wegman Report, had been peer-reviewed. Although the academic scientists and their political allies questioned whether the Wegman Report had actually been peer-reviewed, as the academic scientists claimed, the government challengers still referred to the process as the standard practice for judging scientific research. In the salvage logging case, the government challengers also argued that papers supporting their perspective, such as the government-commissioned Sessions Report, had been peer-reviewed. In the sewage sludge case as well, the government challengers and the industry consultants, CPF Associates, questioned the Cornell Waste Management Institute research on the grounds that it had not been properly vetted through peer review.

In short, while the government challengers found fault with the peer-review process that led to publications’ containing viewpoints and conclusions that they opposed, these government actors still accepted the practice as the preferred method to judge whether a paper was scientifically credible research.

**Claim: Asserting Academic Conventions as Standard Practice**

Earlier in this chapter, we learned that government actors strived to define what qualifies as legitimate science. The evidence presented in the preceding section suggests that **academic scientists may assert academic conventions, such as principles of research openness and peer review, as the standard (or possibly the preferred) practice in evaluating science, even when government officials challenge the academic conventions.** Academic scientists may employ this strategy, which reaffirms the value of using academic conventions to address scientific questions, by pointing out (a) the differing assumptions, between academic scientists and government
challengers, about science development, (b) the differing expectations, between academic scientists and government challengers, about openness and dialogue, and (c) that despite criticisms of the academic conventions, both academic scientists and government challengers use them as standard practice to evaluate scientific research reports. Thus, even when challenged, academic conventions are sufficiently recognized processes for evaluating scientific research that they are often used to lend authority in a political forum.

Table 8.5: Summary of Finding #5

<table>
<thead>
<tr>
<th>Data Pattern</th>
<th>Evidence</th>
<th>Claim</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reaffirming the Value of Using Academic Conventions to Address Scientific Questions</td>
<td>Differing Assumptions, Between Academic Scientists and Government Challengers, About Science Development</td>
<td>Academic scientists may assert academic conventions (e.g., peer review) as the standard (or possibly as the preferred) practice through which to evaluate science, even when government challengers question the validity of those conventions.</td>
</tr>
<tr>
<td></td>
<td>Differing Expectations, Between Academic Scientists and Government Challengers, About Openness and Dialogue</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Using Academic Conventions as Standard Practice for Both Academic Scientists and Government Challengers</td>
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</tbody>
</table>

Part 3: Synthesizing the Emergent Themes about the Challenges and the Responses

In the preceding sections, I identified five data patterns and constructed corresponding claims that emerged from my data. In this section, I explore further what I learned after having
inspected the data. I asked myself: What do the five data patterns tell me? I respond to this question as follows:

First, the data reveal that the academic scientists and the government challengers drew on other actors, who were not initially part of the controversies, to assist them in the conflict. The government challengers involved actors with oversight responsibilities, peer reviewers, and other academic scientists, who had or might seek federal sponsorship of research. The academic scientists brought in members of the public, political allies, and other members of the scientific community in order to convince observers, such as the media, policymakers, scientists, and interested citizens, that scientific matters should remain within the domain of the scientific experts to decide – not in the hands of policymakers.

Second, the data illuminate how the academic scientists and the government challengers exercised some degree of influence over these additional actors in order to achieve their desired outcomes. The government challengers employed threats of penalties to influence the additional actors to conduct their work consistent with the challengers’ policy interests. The academic scientists engaged some members of the public and the profession in the conflict by portraying the government challengers as having self-interested motives and raising questions as to whether they had a real interest in uncovering the state of the environmental science at issue. They also drew on support from political allies, so they could gain entry into political settings and counter the government challengers’ allegations.

Third, the data demonstrate how the academic scientists and the government challengers often relied on traditional practices within their respective professional settings to address the scientific questions. The government challengers relied heavily on congressional forums and science-policy review processes, such using policy tools to inform them about the science issue.
The academic scientists relied heavily on academic conventions, such as academic forums for open discourse and peer-review processes to subject the work to expert critiques and proper vetting.

Fourth, the data uncovered inconsistencies between rhetoric and reality in the way that the academic scientists and the government challengers behaved. While the academic scientists and the government challengers typically drew on their respective traditional practices, they also adopted practices of the other party’s professional setting. That is, government challengers selectively applied academic conventions (e.g., using peer review), and academic scientists selectively applied policymakers’ processes (e.g., pushing for the opportunity to participate in congressional hearings). This point is ironic, given that the practices that each side employed also were the very things that each party criticized the other for having done.

In short, the data indicated that the academic scientists exerted control over their federally funded research in much the same way as the governmental actors challenged them.

Closing Remarks

In sum, in the preceding sections, I addressed my two research sub-questions by presenting the major themes across my three cases to articulate what I learned about the government challenges and the academic profession’s responses. The findings suggest that multiple actors have participated in the process of judging federally sponsored research on sewage sludge, global warming, and salvage logging; to do so, they wielded multiple approaches. To develop these lessons more fully, in the next chapter, I present my interpretation of the findings, which ties in the prior literature on government challenges to the academic profession’s research and the academic profession’s responses to those challenges. In addition, it
takes a closer look at the usefulness of Freidson’s theory to conceptualize what it means for academic scientists to exert control over their federally sponsored research.
CHAPTER IX: ANALYSIS, IMPLICATIONS, AND CONCLUSION

Introduction

In Chapter 1, I introduced the federal government’s 1945 policy strategy of spurring scientific innovation by having the federal government financially support useful research. Basically, the understanding between the federal government and academic scientists was that “[g]overnment promises to fund the basic science that [the scientific panel of] peer reviewers find worthy of support, and [academic] scientists promise that the research will be performed well and honestly and will provide a steady stream of discoveries that can be translated into new products, medicines, or weapons” (Guston & Keniston, 1994, p. 2). As part of that understanding, policymakers agreed to allow the “free play of free intellects.” The federal government would support uninhibited inquiry into basic research, and refrain from meddling into scientific matters, which were generally considered to be outside of its expertise.

As I described in Chapter 1 and several other chapters within this study, despite that promise, government officials have challenged academic scientists on numerous occasions with regard to scientific matters involving their federally sponsored research. Kidd (1963), a well-respected science policy researcher at the time, predicted that reliance on federal dollars would produce political pressures on scientists to alter significantly the direction of their academic inquiries. Kidd was right. As I traced in earlier chapters, instances between 1945 to the present demonstrate that government officials have limited the concept of “free play of free intellects” with respect to professors’ federally sponsored research.

To understand these limitations and the relationship between academic scientists and federal government officials, I noted in Chapter 1 that the purpose of my study was to investigate what it means for government actors to challenge academic scientists’ federally sponsored
research and for academic scientists to exert control over that research. As part of that investigation, in chapters 5, 6, and 7, I reported data for each of my three cases, using a chronological narrative to recount the circumstances and events surrounding the government challenges and the profession’s responses. I also relied on Freidson’s theory of the professions, which I introduced in Chapter 3, to organize the data representing the academic profession’s responses.

In this chapter, I explain my findings further. In Part I, I analyze my findings’ contributions to the literature in terms of what my study added, affirmed, or left unanswered. In Part II, I revisit my conceptual framework. Grounded in (a) what happened in my cases, (b) what emerged from the interactions among actors within my cases, and (c) what relational concepts might explain my findings, I present a revised conceptual framework. In Part III, I raise practice implications for academic scientists who have received or seek federal sponsorship of their research and for policymakers. Finally, in Part IV, I suggest further research on this topic of government challenges to academic scientists’ federally sponsored research and academic scientists’ responses in defense of their work.

What the Study Findings Contribute

Government Challenges

Claim #1: Influencing Indirect Participants in the Federally Funded Research at Issue

One tactic of the government challengers was to raise job performance concerns regarding the work of three sets of actors who had not conducted the challenged research. As discussed in Chapter 8, the government officials criticized the performance of (1) governmental and nongovernmental actors, who held no public policymaking authority but had oversight authority for the federally sponsored project, (2) the peer reviewers who approved the study’s
publication, and (3) other academic scientists who either already had or might seek federal sponsorship for their research. I referred to these actors as indirect participants, who had potentially significant roles in the controversy because they had the capacity to advance or inhibit the progress of the federally funded research at issue or potentially another federally funded research project. In Chapter 8, I indicated that my data pattern and evidence led to my claim that the government challengers might try – sometimes successfully – to exercise their influence over indirect participants of the federally funded research in an attempt to control the progress or dissemination of those research findings. I observed that the government officials did so by (a) alerting actors with oversight responsibilities about problems with the study, (b) criticizing the peer reviewers for failing to reject the problematic study, and (c) forewarning other academic scientists that they must take actions conforming to the government challengers’ policy position or else face penalties, such as loss of federal research funding.

In many respects, this finding is fairly consistent with findings presented in prior publications that examined government officials’ challenges to professors’ research (see, e.g., Beneke, 1998; Gutfeld, 1970; Lewis, 1988; Schrecker, 1980, 1986). As Chapter 2 notes, those studies reported government actors trying to draw in other actors to assist the government in minimizing the effect or discrediting the researchers or research projects at issue. Many previous studies reported that when academic research contradicted the government actors’ policy positions, these government officials pressured actors who did not directly participate in the research but had roles that made it possible for them to interfere with the progress or dissemination of the academic scientists’ sponsored research. As in my study, prior literature indicated that government challengers could influence actors serving the role of indirect participants in the research, such as individuals who had managerial responsibilities over the
academic researchers (e.g., college administrators) (Lewis, 1988; Schrecker, 1980, 1986), other professors (Beneke, 1998; Lewis, 1988; Schrecker, 1980, 1986), and reviewers involved in the publication process – including editorial board members and peer reviewers (Garrison & Kobor, 2002; Lilienfeld, 2002) – to take steps that would silence or minimize the effect of the researchers’ work when government-sponsored research conflicted with a policymaker’s interest.

While my study generally reflects similar patterns in terms of the government challengers’ attempts to influence indirect actors, I draw attention to one significant difference between my data (see Chapter 8) and the discussion of past studies (see Chapter 2). The difference pertains to the reasoning behind the government officials’ criticisms of the peer reviewers. In earlier studies, the government actors focused on the peer reviewers’ failure to reject research that the government actors characterized as being on socially unacceptable topics that others might consider politically inconvenient topics. For instance, as described more fully in Chapter 2, Garrison and Kobor (2002) and Lilienfeld (2002) found that government officials pressured both the American Psychological Association (APA) as the publisher of a highly controversial article and the editorial staff of the APA’s *Psychological Bulletin*. The government actors publicly condemned the article and criticized the APA for publishing the article’s findings. To recap briefly, the study at issue – also known as the Rind study – concluded that child sexual abuse does not always result in severe psychological harm to the victims. Socially deviant groups, such as the North American Man/Boy Love Association (NAMBLA), and critics of the Rind study interpreted the findings as the academic researchers’ active support for adult-child sexual relations. These events ignited state and federal legislators to publicly condemn the article and chastise the APA for its journal staff’s decision to print it.462 The government

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462 It is possible that the government’s attack on the journal was directed at the academic professional organization, the APA, rather than simply being an effort to contest the peer review standard. While that may be
challengers characterized the APA’s publication of the Rind study as an endorsement of pedophilia, in hopes that the negative publicity would pressure the APA to retract the article or renounce the article’s scientific value. Accordingly, the basis for the challenge rested on reframing the Rind study as advocating pedophilia, then asserting that such a position violates societal values. Thus, the government officials criticized the peer reviewers, not for the reviewers’ scientific analysis of the article, but in light of the researchers’ choice of topic, for allowing publication of the article, especially given the findings.

In my study, the government officials criticized the peer reviewers for allegedly failing to vet scientifically flawed studies properly, based primarily on the scientific analyses. That is different from prior studies that reveal criticisms of the peer reviewers in the course of lay challenges to academic scientists’ federally funded research.

**Claim #2: Determining What Counts as Legitimate Scientific Research**

My second research finding builds on the last one. As I indicated in the preceding subsection, government officials criticized peer reviewers for failing to vet research they found flawed, and they threatened possible penalties if peer reviewers continued to perform in a manner that the government officials found unacceptable. Given the government officials’ concerns about the quality of the academic peer reviews in each of my cases, it is not surprising that government officials employed a tactic that disregarded the academic peer reviews. In fact, I observed that the government officials created their own review process in an effort to discount or discredit the research at issue. More specifically, as I stated in Chapter 8, the government challengers in my cases positioned themselves as definers of scientific legitimacy by attempting to orchestrate the peer-review process for scientific research. They did so by using government

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true, Garrison & Kobor (2002) and Lilienfeld (2002) inform us that the government challengers did focus on peer review as the basis for the attacks/criticisms against the APA. Thus, we cannot discount this effect.
substitutes for peer review to judge the scientific merits of federally sponsored research; selecting expert sources to rely upon as credible scientific evidence to contest the validity of the federally funded studies’ data, methodology, and findings; and relying on government challengers’ own understandings of the science to draw conclusions about scientific matters. The data pattern and evidence that emerged led to my claim that the government challengers, though not scientists themselves, relied heavily on their own judgment – rather than relying on the traditional scientific peer-review process – to declare publicly the kinds of activities that can and cannot count as legitimate scientific research. This finding contributes to the extant literature on government challenges of academic scientists’ federally sponsored research in two ways.

First, it further illustrates empirical findings and other reports in the literature that government challengers have previously made attempts to dictate the scientific merits of federally sponsored research (see, e.g., Garvin, 2001; Jasanoff, 1985, 1987, 1992; Shapiro & Guston, 2007). In Chapter 2, I reported actions that government officials took to carry out their efforts. Many of these actions resemble ones found in my study. For instance, as I discussed in Chapter 2, prior literature revealed that government challengers created investigative settings, such as congressional and regulatory hearings, in order to gather criticisms about the federally sponsored study from various sources – including lobbyists, industry representatives, scientists, and other government officials (Cole, 1983; Hornstein, 2006; Jasanoff, 1985, 1987; McGarity & Wagner, 2008; Wagner & Michaels, 2004). As I observed here and others have indicated previously, these governmental forums appeared to the public as transparent processes to discuss the data, methods, and scientific analyses. Nonetheless, these investigative forums permitted
government actors an opportunity to vet the research rather than relying on the evaluations from the academic peer-review process.

Similarly, government officials have previously attempted to dictate the scientific value of federally sponsored research through their selective use of experts and resources. In my study and previous ones examining government challenges to academic scientists’ federally sponsored research, the government officials (a) commissioned experts they selected to write a report supporting their policy preferences, (b) stacked review committees with individuals who sided with a particular perspective, and (c) cherry-picked reports and studies based on their support for the policymakers’ policy preferences (Ashford, 1983; Jasanoff, 1985, 1987; Shapiro & Guston, 2007). Each of my cases illustrated these actions, which led to the attempted discrediting or discounting of the academic research at issue.

Second and more significantly, this finding demonstrates that government officials have greater opportunity to determine scientific legitimacy of governmentally sponsored research than nongovernmentally sponsored research. That distinction is significant. It contributes to the literature by illustrating what researchers have identified as a potential problem but have rarely offered evidence to support (Couzin & Unger, 2006; Hornstein, 2006; Kaiser, 2002; Wagner, 2003, 2005; Wagner & Michaels, 2004).

As I reported in Chapter 2, government officials have increased their authority to inspect and audit federally sponsored research. To explain this authority, I traced several major frames that government officials applied to justify their jurisdiction over and limiting of “free play of free intellects” for scientists working on federally sponsored research. Under the frame of accountability, I mentioned how several federal laws (e.g., Data Access Amendment and Data Quality Act) and federally sponsored research agreements grant government officials and, under
certain conditions, members of the public, access to federally sponsored research data (Couzin & Unger, 2006; Wagner, 2005). As I noted in Chapter 2, this data access typically occurs when questions arise about the sponsored researchers’ data, methods, or analyses. Some observers might interpret the policy or terms of the sponsorship agreement as efforts to increase transparency and scientific communalism of federally funded research. Nonetheless, the extant literature aptly points out that the policy or agreement creates an unfair divide in terms of the treatment between governmentally sponsored research and nongovernmentally sponsored research (Couzin & Unger, 2006; Hornstein, 2006; Kaiser, 2002; Wagner, 2003, 2005; Wagner & Michaels, 2004). Because policymakers and even some nongovernmental actors potentially have greater data access to federally sponsored research relative to other research, these actors can audit the research, allege errors, and question aspects of the study. These events took place in each of the three cases my study explores.

My study illustrates the potential unequal treatment between governmentally sponsored and nongovernmentally sponsored research. As I reported in Chapter 8, the government officials and other actors (e.g., interest groups) scrutinized the federally sponsored research projects in each of my examples and built a case to discredit and dismiss the study. Based on the public hearings (e.g., congressional testimony and the interactions of the various actors), at no time did we witness the same audits for the non-federally sponsored research studies, which may have been less reliable, since those reports did not undergo the same vetting process as the sponsored research and they were written by individuals who were commissioned by either policymakers or industry representatives with a direct interest in the policy outcome. Nonetheless, government

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463 Each of the cases highlights how non-governmentally sponsored studies and reports served as policy tools to assist the government officials in their challenge of the academic scientists’ federally funded research studies and how these non-governmentally sponsored studies did not experience the same level of scrutiny. For instance, Chapter 5 mentions the government officials’ reliance on an industry-sponsored report without discussion of any
officials argued that the non-federally sponsored research should inform their policy decisions and be used as evidence to discredit the federally sponsored research studies at issue in each of my cases. Further, the academic scientists, who encountered the government challenges, and their supporters could not audit the non-federally sponsored research that the governmental challengers relied upon.

**Academic Profession’s Responses**

*Claim #3: Having the Public Determine Whether Government Officials or Academic Scientists Are Better Equipped to Address Scientific Matters*

In one of my findings, I indicated that the academic profession exerted control over its research by engaging the public as arbitrators of the dispute. Based on my data, academic scientists engaged members of the public in the dispute by offering counter-arguments to the government challengers’ statements, identifying policymakers’ agendas and preferences, and referring to past instances of scientific mischaracterization. Those observations led to my claim that by bringing members of the public into the dispute, the public may become a significant player in the debate about whether the government officials or the academic scientists are best suited to address the scientific matters associated with the federal policy.

This finding suggests a different purpose for engaging the public in these disputes than those asserted in earlier studies. As I discussed in Chapter 3, researchers of the academic profession’s responses to governmental challenges previously identified two ways in which the public helped defend professors’ federally sponsored research. One line of research reported that
the professors encountering the challenges and their supporters educated the public about the technical aspects of the research at issue (i.e., educating citizens through public literacy involving the contested works) so members of the public could understand and articulate the research, rally behind it, and publicly defend it (see, e.g., Bailey, 2002; Garrison & Kobar, 2002; Keller, 1996; Lilienfeld, 2002; Saltmarsh, 1991). Thus, some members of the public became allies in defending the challenged research. Another line of research identified professors using the public to help explain some of the scientific research findings and policy choices (Guston & Sarewitz, 2006; Hunt & Sharkley, 1999; Jasanoff, 2005; Nowotny et al., 2003). These studies argued that public participation generated knowledge regarding the cultural context of the challenged study (particularly in terms of how members of the public felt the challenged study affected them). Drawing from that participation, the public participants could form ideas about the policy choices, usually to defend the academic researcher’s study as support for their policy preference. Thus, these studies viewed the public as informants, who could contribute to the defenses of the challenged study by using their cultural context to generate additional arguments of the study’s value.

While my finding suggests that the scientific arguments are important to the defenses of the challenged study, my finding more specifically demonstrates a strategy largely involving the nontechnical aspects of the study: educating the public on reasons why they should not trust the government challengers. As I described in Chapter 8, the academic profession’s methods of convincing the public that government officials provided untrustworthy science involved

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464 For instance, as Chapter 3 notes, Saltmarsh (1991) examined the government challenges to Scott Nearing’s teaching and research of a controversial economic theory. Saltmarsh observed that Nearing’s strategy of educating the public helped him gain support from various groups, especially labor unions. He did so by pitching his writings and lectures at the appropriate educational level of his audience and disseminating this information through town squares and written materials. Members of the public responded favorably, and they began disseminating his works and challenging government officials who criticized Nearing’s economic position.
providing the public counter-arguments to the government challengers’ assertions about the scientific flaws; highlighting the government challengers’ interests (e.g., financial support from industry groups and lobbyists) as possible explanations for the misinformation; and directing attention to a pattern of behavior, specifically how the policymakers’ tactics in the case resembled government challengers’ past instances of mischaracterizing valid scientific findings. These actions led members of the public to question the government officials’ motives and actions. In addition, the public took up the argument that academic scientists with the appropriate expertise, not government officials, should evaluate scientific research. Accordingly, I observed that the public assumed the role as arbitrators of the dispute between government officials and the academic scientists regarding who should determine the scientific matters in these challenges.

In short, my finding suggests that to engage public literacy involves much more than just educating the public about the technical aspects of a professor’s expertise. According to my data, public literacy includes educating the public about the nonscientific matters of the situation, specifically highlighting the interests and agendas of the actors challenging the academic scientists. I observed that informing the public about the interests and agendas of the actors challenging the academic scientists presented a useful strategy to gain public support for the view that academic scientists, not government officials, are in the best position to judge federally sponsored academic research.

Claim #4: Balancing Perspectives Using the Policymakers’ Setting

My findings also identified another strategic supporter, political allies. In Chapter 8, I asserted that academic scientists’ political allies can support the academic scientists’ efforts to defend their research within the policymakers’ setting. I illustrated two prominent ways in
which the academic scientists sought support from political allies. Specifically, based on my data, I learned that political allies’ defenses of the academic scientists’ federally sponsored research included publicly criticizing their political peers for challenging the academic scientists’ research and facilitating academic scientists’ friendly encounters in government venues such as congressional hearings. As I described in Chapter 8, these actions made it possible for the academic scientists to defend their research.

This finding is significant, because it demonstrates a useful strategy for the academic profession when government officials attack academic scientists’ government-sponsored research. As I indicated in Chapter 3, the academic profession’s use of legal authority to help defend professors’ government-sponsored research has been largely limited to constitutional arguments (see, e.g., Eisenberg, 1988; O’Neil, 1997; Rabban, 1990; Slaughter, 1988). Specifically, the literature describes several instances in which professors successfully argued that they experienced undue government interference. The literature indicates that courts have recognized that the government violated the professors’ academic freedom and cited to legal justifications under the First and Fourteenth Amendments. Eisenberg suggests that these legal justifications likely apply to government officials’ challenges of academic scientists’ federally sponsored research too. Nonetheless, the academic scientists in my cases had not relied on legal authority through the judicial branch or by asserting Constitutional rights to contest the government officials’ challenges.

My study reports that the academic profession called on legislators and regulatory agency personnel as legal authority. As I indicated in Chapter 8, one of my findings shows that the academic profession draws on the support of policymakers as political allies, who have legal authority as legislators, and regulatory agency staff. While earlier literature (see Chapter 3) has
not identified this strategy as one previously used to defend the academic profession’s
government-sponsored research from challenges made by government officials, the academic
profession has used political allies to combat lay challenges from nongovernmental actors. In
these studies, the lay challenges originated from industry representatives, lobbyists, and interest
groups – not government officials. Often, these studies reported that political allies called into
question nonacademic research (typically from interest groups) and argued for the use of
academic scientists’ federally sponsored studies, which tended to offer an opposing view to the
nonacademic research. This line of past research also reported that political allies provided
government venues (e.g., congressional hearings) for academic scientists to defend their research
findings from nongovernmental lay challengers. In light of the use of political allies in other lay
challenges to academic research, my finding might suggest that the academic profession drew on
a previous strategy that worked with nongovernmental lay challengers, and introduced it in this
setting, which now involves government officials as the lay challengers.

In sum, my study reveals that legal authority is not restricted to Constitutional rights and
judicial decisions based on those rights. There exists another type of legal authority, which the
academic profession may use as a strategy to exert control over its federally sponsored research.
Legislators and regulatory agency staff represent another source of legal authority that may assist
the academic profession in defending its federally sponsored research. The academic profession
has previously applied this strategy (i.e., drawing on the assistance of political allies) when
nongovernmental actors have challenged the academic profession’s funded research, yet this
study appears to be the first report of the academic profession’s employing this strategy to
defend its federally sponsored research from challenges by government officials.
Claim #5: Asserting Academic Conventions as Standard Practice to Address Scientific Questions

The academic profession has rich traditions and normalized practices. Those characteristics of the profession become more obvious in my fifth and final research finding. As I reported in Chapter 8, the academic profession employed a strategy of taking steps to reaffirm the value of using academic conventions to address scientific questions. Based on my data, I indicated that academic scientists may assert academic conventions (e.g., peer review) as the standard (or possibly as the preferred) practice through which to evaluate science, even when government challengers question the validity of those conventions. They did this by exercising internal characteristics of the academic profession, such as professional expertise, or more specifically, they asserted arguments about how academic scientists and government actors had different assumptions, which led to different conclusions, and adopted different practices to examine the scientific questions each side had, which led to differences about the degree of transparency. In addition, academic scientists pointed out that even though the government challengers criticized the academic peer-review process, these government actors and their fellow critics also used that process to legitimize their work. On a number of occasions, government actors argued that their research and reports had been properly vetted and evaluated as scientifically credible research, because they had been peer-reviewed using the traditional academic process. Thus, despite the government’s attacks on academic standards and processes, those same standards and processes represented legitimizing activities for nonacademic scientists commenting on scientific matters.

This finding further confirms what many researchers of the academic profession have long stated: that academic processes and traditions have become institutionalized practices and
standards for how the academic profession and others should evaluate scientific research (see, e.g., Anderson et al., 2010; Slaughter & Leslie, 1997). Indeed, as I discussed in Chapter 3, Freidson’s theory of professional dominance makes clear that scientific experts maintain a set of internal characteristics that have been institutionalized in society and grant them deference in conducting their work, absent external factors such as illegality and significant changes to knowledge of the field, which might, for instance, alter a profession’s existence (Freidson, 2001). Thus, while Chapter 2 identifies instances of government challenges to academic scientists’ research, Chapter 3 along with my explanation of this finding support the notion that academic scientists are not helpless and have asserted in my three cases several academic standards and processes that society views as the accepted practice for scientific experts to carry out their work.

Most evidently, this finding highlights the role of two internal characteristics of the academic profession as institutionalized understandings that further academic scientists’ ability to exert control over their research. First, academic scientists and policymakers have different roles in the science policy process. The divisions of labor illustrate how each actor has different goals, which translate to different ways in which each actor looks at a scientific question. As discussed in chapters 2 and 3 of this dissertation, academic scientists carry out research in a systematic manner so they can discover and create new knowledge (Slaughter & Leslie, 1997). To do so, they build off existing knowledge and note their specific contribution. Nonetheless, their contribution does not always provide clearly defined policy recommendations. Even if the scientific finding could propose a scientific policy, academic scientists would assess policy choices and determine risk assessments, but they do not typically suggest risk management policies such as how much risk the nation can accept. By contrast, science policymakers, such as federal legislators, establish the science policies. They are also responsible for evaluating the
policy choices and determining the government’s plans to manage any risks that may arise from the policy implementation (Garvin, 2001; Smith, 1992). Put simply, the extant literature demonstrates that academic scientists and policymakers have different roles, agreed on in the late 1940s, when it comes to the science policy process. This division of labor may account for their differing assumptions and processes in addressing scientific questions.

Second, this finding highlights the value of peer review as the academic profession’s mechanism for independent judgment and self-regulation. As Ruhl and Salzman (2006) write, “Scientific peer review is generally described as a rigorous review and critique of a study’s methods, results, and findings that is conducted by others in the relevant field who have the requisite training and expertise, who have no pecuniary or other disqualifying bias with respect to the topic, and who are independent of the persons who performed the study” (pp. 4-5). While a line of research as well as writings based on non-empirical data introduced in Chapter 2 argue that academic conventions such as peer review are imperfect processes to judge science, I note in chapters 2 and 3 that scholars of the academic profession and science policy have indicated that there is no effective alternative to peer review. In fact, the extant literature reports both academic scientists and policymakers often describing peer review as the gold standard to evaluate academic research.

In short, members of the academic profession as well as government officials have argued for academic processes (e.g., peer review) as a signal that their works are legitimate. Yet, the effect of these assertions appears to have only reaffirmed the value of the academic conventions of peer review and open dialogue and justified the rationale behind using experts (i.e., divisions of labor) to address specific parts of a problem. Indeed, these institutionalized practices and standards have set professional expectations for evaluating scientific research.
Thus, when what purports to be valid scientific research fails to meet the institutionalized practices and standards (i.e., the academic processes and traditions in place), such as when it fails to undergo the full scrutiny of the academic process, the study tends to receive limited validation, and this in turn reaffirms the value of using academic conventions to address scientific questions.

**Synthesis of the Challenges and Responses**

In Chapter 8, I synthesized my five emergent themes reporting that the academic scientists in my three cases exerted control over their federally funded research by using tactics similar to those used by the government actors who challenged them. Specifically, I noted that (1) the data reveal that the academic scientists and the government challengers drew on other actors, who were not initially part of the controversies, to assist them in the conflict; (2) the data illuminate how the academic scientists and the government challengers exercised some degree of influence over these additional actors in order to achieve their desired outcomes; (3) the data demonstrate how the academic scientists and the government challengers often relied on traditional practices within their respective professional settings to address the scientific questions; and (4) the data uncovered inconsistencies between rhetoric and reality in the way that the academic scientists and the government challengers behaved. Given this synthesis of the emergent data, I asked myself: What additional insights might I interpret from my findings that will help me better understand my overarching research question and my conceptual framework? That question led me to three additional observations.

First, my findings about the government’s challenges of scientific research reflect a larger issue about government officials’ trying to redefine the boundaries of professional expertise for both academic scientists and policymakers. As I discussed above, the policymakers attempted to influence actors in an effort to control the dissemination of the federally sponsored research.
They also made multiple attempts to use their own judgments to determine scientific legitimacy, rather than employing the traditional scientific peer-review process. Collectively, these actions suggest that policymakers tried to take actions that exceeded their professional competence. More specifically, these government officials became *boundary breakers* by trying to infiltrate the jurisdictional responsibilities of the academic scientists.465

Second, despite the government officials’ attempts to engage in professional boundary-crossing activities, my findings regarding the academic profession’s responses to the lay challenges are also noteworthy. As the preceding section points out, the academic profession employed several strategies to exert control over its federally sponsored research. The professors asserted institutionalized practices and standards of the profession (e.g., peer review and open dialogue) and drew on the assistance of two types of external actors (i.e., members of the public and political allies). These responses represented the academic profession’s *countervailing forces* to defend its research.

Third, the data indicate that the academic profession does not operate off a static model of institutionalized arrangements that grant authority and autonomy. Nor do the government officials’ challenges and academic profession’s responses reflect a linear relationship. Instead, the interactions involving the government challenges and the academic profession’s responses to defend its research represent a much more fluid exchange among various actors, who participated in the conflict. Specifically, the data suggest that the academic profession’s ability to exert control over its research relies heavily on social legitimacy. In other words, intellectual

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465 If federal officials had investigated these studies based on allegations of scientific/research misconduct, their actions would not be perceived as violating norms or as boundary-crossing activities, because legal authority, such as federal regulations, would grant an investigation.

For purposes of this discussion, I adopt the definition established by the National Science Foundation to describe scientific/research misconduct. That definition states: “Research misconduct means fabrication, falsification, or plagiarism in proposing or performing research funded by [a governmental entity], reviewing research proposals submitted to [a governmental entity], or in reporting research results funded by [a governmental entity]” (see 45 C.F.R. § 689.1 (2008)).
freedom is not simply a professional liberty defined by a professional organization. Intellectual freedom represents *societal legitimation of a professional liberty*, guaranteeing there will be no unjustified intrusions on professors’ research.

This synthesis of the challenges and responses is quite informative; it helped me better understand the relationships among my concepts within the study’s framework, which I discuss below.

**Revising the Conceptual Framework**

In Chapter 4, I described my methodology as a circumscribed grounded theory. I used, quite substantially, Freidson’s theoretical framework to guide large sections of this study. Although I drew on prior conceptual understandings to make sense of my data, I still relied heavily on the data themselves to structure my thoughts and develop more explanatory concepts that described the interactions among the actors in my three cases. Specifically, my findings demonstrate that the government officials acted to break the boundaries of professional expertise by trying to influence certain actors connected with the academic scientists’ federally sponsored research (i.e., indirect participants, who had the capacity to advance or inhibit the progress of that federally funded research or potentially another federally funded research project) and publicly declaring what counted as scientifically legitimate research. In addition, my findings illustrate that the academic profession has the capacity to counter the government’s challenges by drawing in external actors (e.g., members of the public and political allies) and asserting the value of its internal characteristics based on academic conventions.

Although my initial conceptual framework guided my investigation and illuminated my findings, my study revealed more about the relationships among several concepts. Reflecting on what I learned, I now revisit my conceptual framework. Using the principles of grounded theory
as the basis for my conceptual inquiry, I have connected three pieces of information – what happened in my cases, what emerged from these interactions among actors within my cases, and what relational concepts might explain my findings. That analysis led me to compare the framework generated by my data with the framework that I initially used in my study to analyze my data. As I described my conceptual framework in chapters 2 and 3, I present the framework’s relevance and recommended changes in two parts. First, I review the conceptual relationships that account for the interactions surrounding the government challenges; then, I review the conceptual relationships that account for the academic profession’s responses to those challenges and related events. At the end of these reviews, I present my grounded conceptual framework that depicts government challenges of academic scientists’ government-sponsored research and the academic profession’s responses to those challenges.

Government Challenges

The first part of my conceptual framework guided my understanding of the circumstances and events surrounding government officials’ challenging academic scientists’ federally sponsored research (see Figure 9.1). Earlier literature suggested an investigation emphasizing the roles and actions of the two primary actors in the dispute – the government challengers and the academic scientists. While some of the literature also referred to other actors (such as federal scientists, university administrators, and industry leaders) as having a significant part in the challenges to the academic scientists’ sponsored research, these authors largely limited their discussion of these actors to passing comments and structured their analysis on the two central actors (i.e., the governmental challengers and the academic scientists). Given this limitation, I expanded the scope of actors in my inquiry at the start of this study. I included actors who had some participation in the dispute, paying special attention to actors who had some interest in the
academic research topic, government funding, or related science policies. Thus, rather than
limiting my inquiry to the circumstances and events that describe the government’s challenge, I
expanded my scope from the outset to include the circumstances and events surrounding the
government’s challenges. This inquiry included exchanges leading up to and after the initial
attacks on the academic scientists’ federally sponsored research.

Also, my initial inquiry into the government’s challenges explored the significance of
language and symbols that actors used. I tried to factor in the human capacity for thought and
action, especially since my cases involved publicly visible events. Some people communicate
with an audience in mind, so I had to consider the timing and meaning making that take place
with an actor’s use of language and symbols, and the ongoing process of interpretations and
interactions that occur. Broadly speaking, this portion of my framework continued to help me to
(1) identify the actors involved, (2) recognize the varied interests of each actor, and (3) offer
insights about each actor’s understandings of how events unfolded. More specifically, it guided
me in my construction of each case study’s chronological narrative, describing the historical
context of the science policy controversy, the government officials’ triggers leading up to the
challenge, the government officials’ actions to challenge the academic scientists’ government-
sponsored research, the immediate reactions from the significant actors in the case, and the
impact the challenges had on the research at the heart of the case. In short, my data
demonstrated the value of this portion of my conceptual framework, which examined the
government’s challenges. I find it a useful tool.
Academic Profession’s Responses

Overview

To conceptualize the academic profession’s defenses to the government challenges, I used Freidson’s theory of professional dominance. To recap, Freidson’s theory, like the extant literature on the academic profession’s responses, considers both the internal characteristics of and external factors influencing the profession (Freidson, 2001). In Freidson’s theory, the internal characteristics, which he refers to as a set of defining components that make it possible for the profession to control its work, include five static, interrelated parts. These components are: (1) professional knowledge and skills, (2) divisions of labor, (3) labor markets and careers, (4) training programs, and (5) ideological commitment to the profession itself (Freidson, 2001).

Professional knowledge and skills are the formal knowledge of the profession that rely heavily on mental judgments over technical details, such as the academic profession’s application of specific research approaches to gather data for a study. The divisions of labor component considers the jurisdictional boundaries related to occupations working on different aspect of a project, such as the academic profession’s assessing the scientific implications of a policy while policymakers draft the legislation. The labor markets and careers component reflects the
profession’s authority in society to determine who qualifies for practice and how one qualifies; one example is the academic profession’s default standard that the Ph.D. is a prerequisite for many college faculty positions. *Training programs*, usually graduate schools, are the formal educational settings that prepare future professionals and create new knowledge for future professional practice (e.g., a new research methodology). *Ideology* reflects the values embedded in the profession’s actions, such as the academic profession’s work to advance the interest of the public good through education.

In addition to the internal characteristics, Freidson (2001) also identifies several external conditions that moderate the extent and nature of a profession’s control over its work. He focuses on two factors, bodies of knowledge and the state. Freidson describes how *bodies of knowledge* serve as a potential source to generate more resources. He explains that bodies of knowledge represent society’s capacity to recognize the value of a profession’s formal knowledge and grant resources based on that understanding. Similarly, the *state* serves as a potential source to provide legal authority or protections, so the profession can formally control its work. He cites government-controlled licensing boards as an illustration of this external factor.

Figure 9.2, which is displayed below and also in Chapter 3, lists the internal characteristics and external factors that define Freidson’s theory of professional dominance. The relationship is explained as a linear one, where the external factors moderate the extent to which a profession may exercise controls deriving from its internal characteristics.
Based on my study, Freidson’s theory of professional dominance (2001) offers a fairly useful conceptual framework to understand how the academic profession can defend itself when government officials challenge academic scientists’ federally sponsored research. The framework makes clear that the internal characteristics of the profession give it the power in society to control its work. As I noted in the preceding section, the framework helped me recognize that these internal characteristics of the academic profession represented the organizing functions that legitimized academic processes and traditions, such as academic freedom, open dialogue, and respect for sub-field expertise, as institutionalized practices worthy of defense.
However, my findings indicate that Freidson’s model does not properly account for three significant, conceptual relationships when accounting for the academic profession’s responses in defense of its government-sponsored research. I explain them below.

*Identifying Professional Associations as a Collective Body Representing the Internal Characteristics of the Profession*

First, my study confirms that with respect to the academic profession, professional associations represent an institutionalized internal force, not an external factor of the profession. From the outset, I made this modification. In Chapter 3, I described how Freidson (2001) posits that professional associations have a limited role, and that the role is typically to convince the state to grant additional legal authority. He also observes that professional associations do not necessarily represent the larger corporate body of the profession. While Freidson’s observations might be true in certain instances, in Chapter 3, I proposed identifying professional associations as an internal characteristic of the academic profession.

My data supports that conceptual modification. My data repeatedly referred to professional associations’ actions to collectively defend the academic scientists’ federally sponsored research. The professional organizations participated in reviews to help defend a project; they also protested government endorsement of research that conflicted with the science at issue when these other studies failed to comply with the academic review processes and/or appeared biased (for example, when the research was paid for by a party that had a financial stake in the outcome). Given their organizational value in supporting the academic profession, professional associations fit better as a component within the internal characteristics than as an external factor.
Engaging the Public Around the Interests and Agendas of Actors Involved in the Dispute

Second, my study also illuminated the scope and manner in which external entities play a role. Specifically, in one of my findings, the public had a significant part in defending the academic scientists as the party in the best position to evaluate the science, not government officials. Freidson (2001) identifies the public’s role typically in terms of supporting or inhibiting the profession, based on its contribution to society through its “bodies of knowledge.” The extant literature on the academic profession’s responses characterizes these actions as taking steps to encourage public literacy about the academic profession’s position or research so the public can properly defend it. In this study, the public literacy regarding the academic profession’s technical work is not as important. My data indicates that the public’s knowledge of the government’s mischaracterization or awareness of policymakers’ agendas and preferences are more important, because recognizing deceit, misinformation, and motivating factors educates the public to see that the policymakers might not be unbiased, objective actors in the dispute. This finding illustrates that the academic profession seeks to gain public support not only in terms of technical aspects of the professors’ research (i.e., “bodies of knowledge”), but also about the nonscientific matters. Accordingly, my finding suggests that the academic profession might engage the public by examining interests and agendas of the actors involved in the dispute, because that activity reflects a more accurate depiction of this countervailing force, rather than bodies of knowledge.

Expanding the Concept of Legal Authority

Third, another of my findings informs me that political allies can support academic scientists’ efforts to defend their research within the policymakers’ setting. Freidson (2001) referred to laws and other powers granted by the state, such as state laws on professional entry.
Certainly, Freidson implicitly acknowledges the value of political allies to adopt laws that protect a profession; however, under Freidson’s conception of the state and the role of political allies, he referred to more formalized actions, such as the passage of state licensure requirements, as manifestations of legal authority. He did not conceive of the idea of a profession’s political allies combating political opponents as a strategy. Yet, my data demonstrate that a team of politicians, in my case political allies, defending the academic profession from another team of politicians (i.e., political challengers) reflects a viable strategy or a countervailing force. Since these actors still represent a conflict between two teams with legal authority, I do not propose an explicit change to the illustrated model; however, I note here that my data make clear that the conception of legal authority is broader than in Freidson’s (2001) original description. It includes both the laws and policies adopted to advance or inhibit a profession as well as the actors with authority to adopt these laws and policies.

*Revised Conceptual Framework*

As I explained above, my data demonstrates a reassignment of professional associations as an internal characteristic and a re-envisioning of the public and political allies’ roles in defending the academic profession. These differences reflect a more fundamental revision of this conceptual framework. In its current form, Freidson’s theory represents a fairly linear relationship between the internal characteristics and external factors. Below, in Figure 9.3, I depict a grounded conceptual framework of the relationships between the internal characteristics and external factors as an exchange or a force/counterforce model, as my findings reported. That is, my study indicates that the government officials employed pressures and tactics as professional boundary breakers. At the same time, my study finds that the academic profession
employed strategies and tactics to create a countervailing force. Thus, this grounded conceptual framework depicts that exchange.
Figure 9.3: Revised Conceptual Framework

Government Officials Acting as Boundary Breakers

Academic Profession

Other Actors in the Controversy

Knowledge & Skills

Divisions of Labor

Labor Markets & Careers

Training Programs

Ideologies

Professional Associations

Legal Authority

Bodies of Knowledge

Interests & Agendas

Autonomy & Authority Over Academic Profession’s Research
Implications

Implications for Academic Scientists Who Have Received or Seek Federal Sponsorship

The findings of this study present at least four implications for academic scientists who have received or seek federal sponsorship. First, this study points out that academic scientists have a broader audience to address when revealing scientific findings. Academic scientists should not view their research products as communications relevant only to the scientific experts within the field. As these findings suggest, academic scientists should continuously engage the public and policymakers about their research, and should do so in lay terms. Additionally, they should expose scientists in other sub-fields to the research they are doing. Thus, academic scientists must engage others through accessible language.

Second, academic scientists must contextualize their research. As the disputes in my three cases make clear, the communication format about the research should also contain the sponsored research’s contributions within that line of scientific inquiry. The context helps identify the limits of the study as well as its significance. That information helps those with an interest in disseminating the study avoid misstatements or other mischaracterizing effects of the research.

Third, academic scientists should be prepared for significant scrutiny. As reported in my three cases, policymakers may be interested in the findings of a study when it impacts proposed science policy, and particularly when it conflicts with a science policy. In addition, academic scientists should anticipate the possibility that government sponsors may inquire about how their funds are spent.

Fourth, academic scientists, who encounter challenges to their research by government officials, are not helpless. While these lay challenges bring new meaning to the phrase “indirect
costs” associated with a federal grant, this study demonstrates that the academic profession maintains a countervailing force that allows academic scientists and their supporters to defend the research at issue from government officials who attack it. They should consider finding support from political allies and members of the public. In addition, they should continue to assert academic conventions as accepted practices to conduct and evaluate scientific research.

Implications for Policymakers

This study also raises multiple implications for policymakers. In this section, I highlight three significant ones. First, policymakers should be aware of their degree of influence and control over other actors connected to the primary ones in a dispute between academic scientists and themselves. As my data indicate, policymakers may influence indirect participants of the federally sponsored research. While those actions might not amount to legal violations of the academic scientists’ intellectual freedom, the government officials who do use their influence to try to discredit academic scientists’ research, even when the study is government funded, tread on the thin line between a profession’s self-governing functions and government intrusions on scientific research.

Second, my findings illustrate potentially unfair or disproportionate inspections and attacks on academic scientists’ federally sponsored research relative to non-federally sponsored research. As I discussed earlier in this chapter and more fully in Chapter 2, sponsored research agreements and several federal laws (e.g., Data Access Amendment and Data Quality Act) create legally justifiable means for actors other than government officials to place serious delays on federally sponsored research or subject them to ad hominem attacks. These barriers, however, do not apply equally to non-publicly funded research. An unintended consequence may be that policymakers rely more on privately funded research, which is not subject to the same levels of
scrutiny as publicly funded research, as a primary resource in creating science policy, and this would open another door to private interests’ influence in policymaking. Accordingly, policymakers will need to adopt mechanisms that combat the unfair or disproportionate treatment between non-federally sponsored research and academic scientists’ federally sponsored research.

Third, while policymakers are not in the business of judging science, they have the role of managing science policy, which requires some interpretation of scientific findings and knowledge of the state of science. Accordingly, policymakers must entertain adopting systems that would yield unbiased reviews, or seek an expert team of scientists to evaluate scientific research that contributes to science policy. One solution may be to reinstate the Congressional Office of Technology Assessment (OTA), which stopped its operations in 1995 because of non-renewed funding. As the Union of Concerned Scientists (2010) argues, no other federal agency has the structure and capacity to inform Congress, work with the policy stakeholders, and recommend forward-thinking recommendations as the OTA did before it closed. Whether the solution is the OTA and/or other scientific bodies, my findings draw attention to the need of a dedicated science and technology expert body to guide our policymakers, but who do not work for or report directly to our policymakers.

Recommendations for Future Research

Academic scientists will likely continue to rely on government-sponsored research, and government entities will likely continue to award academic scientists funding to conduct research with science policy implications. Yet, researchers of the academic profession need to learn more about this relationship. First, a scientific system that allows government actors to decide what qualifies as legitimate scientific research potentially draws concern about the state of science. If the government has as much say in federally sponsored research as industry currently has with its
funded research, society will have fewer options from where it can draw on independent scientific research that serves the public good. Thus, researchers must investigate more deeply the understandings and expectations of science policymakers and academic scientists, as these parties engage in dual relationships of grantor/grantee and science policymaker/scientific discoverer. This inquiry is especially important in light of Vannevar Bush’s 1945 report, which outlined the social contract between government and the academic profession as allowing the “free play of free intellects.”

Second, researchers must have a better grasp of the role and boundaries of the public, too. The public’s role raises an interesting conundrum. If, as my findings state, academic scientists can engage the public to arbitrate the dispute between the academic profession and policymakers, then it is possible that government officials can also engage the public to support their position. Further, does that mean it is possible that interest groups can also influence the public in the same way that academic scientists can? Thus, here is the conundrum: Should the public be the party with so much power to arbitrate this matter when other actors might be able to use them too?

Third, my grounded conceptual framework indicates that government officials may assert an action or “force,” such as breaking the professional boundaries between policymakers and the academic profession. It also demonstrates that the academic profession has a countervailing force that can reinforce traditional boundaries between government policymakers and academic scientists. How does this model hold in disputes between government officials and academic scientists involving non-governmentally sponsored research? Further, does it have versatility to capture disputes between nongovernmental sponsors who challenge academic scientists’ research, such as disputes between industry and academic scientists? Finally, how useful is the model in
explaining the interactions when academic scientists initiate a challenge to policymakers’ actions and policymakers retaliate? Put simply, further research is needed to test this revised conceptual framework of Freidson’s model of professional dominance.
REFERENCES


Cases
