THE SCIENTIST'S DILEMMA: THE ETHICS OF ADVOCACY

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ABSTRACT OF THE DISSERTATION

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Scientific information can play a key role in how people manage risk. While much sociological research has explored how non-scientists struggle to acquire policy-relevant information, relatively little research has explored how scientists struggle to provide it. Drawing on the "institutional logics" perspective, I describe scientists' efforts to positively impact society. My three empirical chapters explore scientists' discourse regarding morality and risk communication at the levels of institutions, individuals, and organizations.

With respect to institutions, I found that prominent handbooks on the nature of science offer no indication that scientists ought to communicate with the public or any useful advice for overcoming perceptions that scientists who engage in advocacy are biased. Moreover, consensus reports on risk tend not to discuss ethical issues regarding public communication. These findings suggest that scientists lack norms for why and how they should communicate risk to the public.

With respect to individuals, I spoke with academic scientists about appropriate and inappropriate ways of communicating risk. Although respondents spoke of risk communication as a moral responsibility, several factors made it a moral dilemma,

particularly for scientists who lacked tenure. Compared to immediate threats like looming hurricanes, scientists were more reluctant to reach out to the media about intangible threats like climate change. Scientists also tended to view more direct forms of public engagement as personally risky. Additionally, respondents discussed structural and material constraints to conducting policy-relevant science within the academy, resulting in a paucity of information to communicate.

With respect to organizations, I examined how science organizations from around the world responded to a manslaughter trial against seven scientists and engineers. The prosecution argued that the defendants had offered poor scientific advice in the days before a deadly earthquake. I argue that the lack of norms for communicating about immediate seismic threats prompted science organizations to rally behind the defendants. Some organizations argued for the scientists' innocence, while others advocated what I call "scientific immunity," that is, the notion that scientists cannot be held legally accountable for what they communicate to the public, even when what they communicate is scientifically unfounded.

An overarching finding is that academic scientists experience moral dilemmas regarding the public communication of risk, particularly about hazards that the public does not believe pose an immediate threat. Because these dilemmas are partially rooted in the lack of norms for public communication, academic scientists might benefit from more training in how to communicate with the public. Normalizing risk communication might lead to better risk management strategies that would ultimately improve public health.

# Acknowledgments


*Aversion to artfulness is not limited to write-ups of analyses, literature reviews, and methods. It invades even the most personalized components of our writing: the acknowledgment sections and prefaces of our books.*
—Sauder (2019:1)

*Academic life is a mad hazard. … I have found that only a few persons could endure this situation without coming to grief.*
—Weber (1946:134)

*I have fought the good fight, I have finished the race, I have kept the faith.*
—Paul (2 Timothy 4:7)

I am immensely thankful for Trader Joe’s Pound Plus 72% Cacao Dark Chocolate (TJDC), which was a key driver of this dissertation. My most productive writing sessions began with popping a square of TJDC into my mouth. Writing sessions that began with some other snack were typically aborted within two minutes. TJDC squares are big enough that, before they finish melting, I’m able to get past the mundane and not-so-mundane anxieties that arise at the start of writing sessions. And TJDC’s darkness is my Goldilocks—sweet enough to always be appealing, but not so sweet as to compel me to leap from my chair to get a second square before a writing session is over.

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I'd also like to acknowledge the moral support and friendship I received from many others during my studies, most notably the Stroffolino family, and also

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TABLE OF CONTENTS

# List of Tables



# List of Figures

# Commonly-Used Abbreviations

CGR: National Commission for the Prediction and Prevention of Great Risks

DCP: Department of Civil Protection

ILP: institutional logics perspective

NAS: National Academy of Sciences

USACE: US Army Corps of Engineers

# Chapter 1

# Introduction: Morality in the Social Studies of Science?

*The utter lovelessness of social theory has increasingly disturbed me. There is something spooky, even perverse, about a field in which a theorist like Michel Foucault, to whom my understanding of institutional logics is indebted, can put the details of torture on display, but cannot conjure a single caress, even when he talks about Greek erotics!!*
—Roger Friedland (2013:29), co-founder of the "institutional logics" perspective

## Overview of the dissertation

A bronze statue of Albert Einstein lies in the southwest corner of the grounds of the National Academy of Sciences (NAS; see Figure 1). The monument bears the following inscription:

> The right to search for truth implies also a duty; one must not conceal any part of what one has recognized to be true.

Figure 1. The Einstein memorial
*Source: Wikipedia*

*Responsible Science*—a NAS publication that has this quote as its epigraph—was written to foster discussions about "ethical issues that arise in the contemporary research environment"; in particular, the book is about "scientific misconduct."[1] Einstein's comment, however, was about more than scientific misconduct. He was talking about

[1] *Responsible Science* defines scientific misconduct as "fabrication, falsification, or plagiarism, in proposing, performing, or reporting research" (National Academy of Sciences, National Academy of Engineering, Institute of Medicine 1992:5).

the "duty" of intellectuals to speak out about what they believe to be true, no matter how controversial those beliefs might be (Einstein, Nathan, and Norden 1960:541–74). In this dissertation, I focus on academic scientists who study environmental hazards. In particular, I examine the moral dilemmas they encounter while communicating risk to the public.

Some scholars in science communication have painted a rather rosy picture of public engagement, especially in the context of risk communication. Such scholars argue that once the science is "settled," scientists simply need to "step up" and communicate what they know. For example, Maibach, Myers, and Leiserowitz (2014:297) implore climate scientists to "inform the public about the empirically determined conclusions of their field"—that virtually all climate scientists agree that global environmental change is happening and that humans are largely responsible for it. The main focus of Maibach et al. is on how the public would benefit from learning about the extent of the scientific consensus on climate change (p. 296). They neglect to discuss any reasons why climate scientists might want to "conceal" this message. I argue that communicating risk may affect not only the public, but also scientists themselves—that the decision to "step up" and communicate risk may present scientists with a moral dilemma.

In the remainder of this chapter, I argue (a) that the "institutional logics" perspective should be extended to include theories about how values affect action, (b) that the mainstream literature in the social studies of science has neglected the importance of values, and (c) that academic scientists who adhere to the logic of "public science" may experience moral dilemmas. This dissertation is a meditation on these arguments, which I elaborate below. I then conclude this chapter by outlining the empirical chapters that follow.

## Institutional logics and morality

Since the 1950s, sociologists have become increasingly less likely to discuss "morality." Indeed, as Figure 2 reveals, the incidence of "moral(s)" and "morality" as a proportion of all words in two of the top sociology journals—*American Journal of Sociology* and *American Sociological Review*—has dropped by more than half since the middle of the twentieth century. This general trend seems to have played out in the same way in various subfields within sociology, including organizational sociology, where one of the most prominent theories is the institutional logics perspective (ILP).

According to Friedland and Alford (1991:248), the founders of the ILP, each institution provides "a central logic—a set of material practices and symbolic constructions—which constitutes its organizing principles and which is available to organizations and individuals to elaborate." In line with Friedland's (2013) explanation of the Weberian roots of the ILP, I argue that each institutional logic reflects a combination of instrumentally-rational and value-rational action, the latter being more closely associated with what most people think of as morality. In Weber's (1978:24–26) terms, value-rational action is undertaken "for its own sake, … independently of its prospects of success." It is "required by duty, honor, the pursuit of beauty, a religious call, personal loyalty, or the importance of some 'cause.'" Instrumentally-rational action, in contrast, is about "rationally pursued and calculated ends."[2]

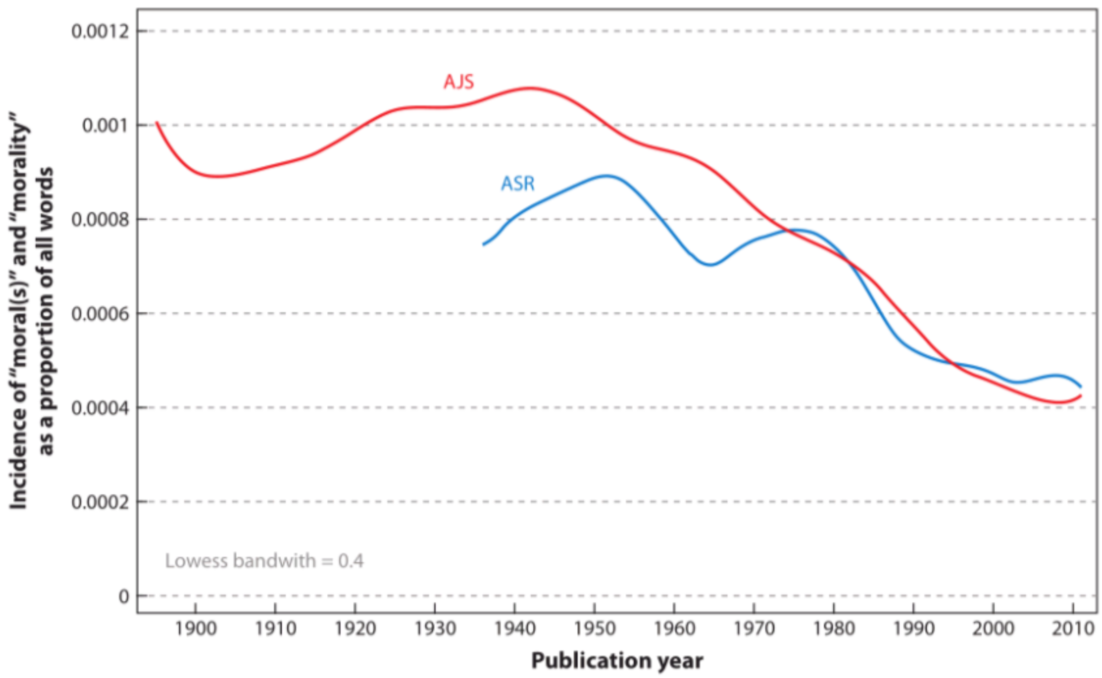


Figure 2. Incidence of morality terms in the *American Journal of Sociology* (AJS) and the *American Sociological Review* (ASR), 1895-2011
*Source: Hitlin and Vaisey (2013:52)*

But as Friedland (2012:585) pointed out, social scientists have elaborated the ILP as a mostly instrumentally-rational account of action. In his review of Thornton, Ocasio, and Lounsbury's (2012) synthesis of the ILP literature, Friedland highlighted their "most

[2] As Rutgers and Schreurs (2006:409–10) pointed out, the motivations beneath instrumentally- and value-rational action map neatly onto Kant's hypothetical and categorical imperatives (see also Friedland 2013:30–31). Similar to instrumental rationality, hypothetical imperatives are essentially conditional motivations—you may not care much about doing X, but you do it because it's necessary to obtain Y, a desired end. Similar to rationality based on values, the categorical imperative entails doing X for its own sake, without consideration of any other ends.

critical omission—value." The following depiction of the ILP by Thornton et al. (2012:44) is telling:

> In Friedland and Alford's (1991:232–5) words, "each order is an institutionally specific cultural system for generating and measuring value." This means that within each institutional order, individuals are confronted with different types of instrumental choices.

Interestingly, what Thornton et al. portrayed as "Friedland and Alford's words" are not to be found in the passage they quoted, which was about the logic of Capitalism. "A market," Friedland and Alford (1991:234–35) actually wrote, "is not simply an allocative mechanism but also an institutionally specific cultural system for generating and measuring value." While the misquotation itself is trivial since it does convey the core of the ILP, the explanation of what the misquotation means—that the ILP is primarily geared toward explaining instrumentally-rational action—is misleading. Friedland and Alford's purpose in reviewing the logic of Capitalism was to argue that portrayals of "society as marketplace" pale in comparison to their image of society as "a potentially contradictory interinstitutional system" (p. 240). "Other institutional realms—families, states, religions—are more likely to generate values, and hence utilities, as absolutes which cannot be traded off against alternatives" (p. 235). The difference between monetary evaluations and other types of evaluations (e.g., whether something is "good" or "bad" for the family) "is not between rational and irrational, but between different transrational orders" that go beyond or surpass rationality.

Thornton et al. (2015:9–10) stated that the institutional logics perspective "is well suited to theorize and measure the heterogeneity of cultural context that informs different variants of moral thought,” but they conceded that the topic of morality has been “underdeveloped.” Moreover, in their effort to argue that the literature on morality has been expanding its scope, Thornton et al. discussed Friedland and Alford’s (1991) foundational emphasis on Christianity in the same sentence as more recent attempts to explore “the emerging economies of India and China,” without explaining how these phenomena are related or how they might relate to morality. Given their market-based reading of Friedland and Alford’s work, it is not surprising that they slipped economic issues into their discussion of morality. Indeed, it is perhaps *predictable* that the institutional “logics” perspective, with its emphasis on different types of “rationality” has come to be characterized more in terms of instrumental rationality than value rationality. “Logic” and “rationality”—in contemporary colloquial usage at least—are intimately tied to cognition and instrumental rationality.[3]

The fundamental critique that the ILP has neglected the importance of values in explaining social action is by no means limited to the dialog between Thornton et al. and Friedland. Munir (2015:91), for example, wrote that issues of morality have been “expunged” from institutional theory, and Klein (2015:335) argued that institutional theorists have “obliterated” the role of values in explaining social action. The result, Moore and Grandy (2017:146) argued, is a portrayal of individuals and organizations as “amoral entities.”

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[3] Personally, my first impression of the term “value-rationality” was that it seemed like an oxymoron.

In a bow to Friedland and Alford (1991), who sought to "bring society back in" to institutional theory, Moore and Grandy (2017) sought to "bring morality back in." The distinction between their study and most others that discuss institutional logics is its focus on values that go beyond "satisficing." That is, whereas most of the literature has focused on the bare minimum of action and lip service that is needed to be a successful member of an organization, Moore and Grandy sought out people who might have an "awareness or concern for a higher purpose for their actions" (p. 158)—in short, people who might exhibit value-rational action. By sampling congregants of various Christian denominations in northeast England, Moore and Grandy found that many respondents articulated a "teleological/theological" identity that "led directly into practical action," for example, evangelism and acts of service (p. 154). The point here is that Moore and Grandy used the ILP to describe a logic in which moral issues are in the foreground, with people readily explaining their action in terms of underlying values.

In terms of the generalizability of their findings to other institutional settings, Moore and Grandy (2017:158) remarked that "while we would not necessarily expect respondents in a business setting to supply a justification for their actions by way of a summary of (their belief in) capitalism or of the necessity of property rights, for example, we might properly expect them to be able to speak of their organization's purpose and how its goods or services contribute to the common good." In this dissertation, I explore the role of values in another institutional setting—one that is, perhaps, quite different from religious logics.

## Values in the sociology of science

In 2012, the journal *Social Studies of Science* commemorated the contributions of Bruno Latour to the field of science studies, occasioned by the 25th anniversary of his *Science in Action: How to Follow Scientists and Engineers through Society*. Though Latour certainly has his detractors,[4] *Science in Action* has, by any measure, been incredibly influential. As of this writing, Google Scholar lists it as Latour's most cited work, having been cited nearly 28,000 times, with an annual peak of 1,736 citations in 2016. The book is important to this dissertation because it outlines a vision of the relationship between science and society that has become deeply engrained within science studies.

If *Science in Action* could be summarized in one word, it would be "technoscience"—a term that describes all the multifarious things that are tied to what we often think of as "science." Wielding the concept of technoscience, Latour wrote that any perceived separation between "science" and "non-science" is "a figment of our imagination" (p. 174). The following passage exemplifies his argument:

> If you get inside a laboratory, you see no public relations, no politics, no ethical problems, no class struggle, no lawyers; you see science isolated from society. But this isolation exists only so far as other scientists are constantly busy recruiting investors, interesting and convincing people. (Latour 1987:156)[5]

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[4] Several of the contributors to the special edition glossed over Latour's contributions to science studies, instead focusing on the contributions of Thomas Kuhn, whose *The Structure of Scientific Revolutions* had turned 50 and was also being commemorated. Harry Collins (2012), for example, bluntly remarked that "Latour's book had no impact on me" and titled his contribution "Comment on Kuhn." And Sheila Jasanoff (2012) wielded citation counts to point out that *The Structure of Scientific Revolutions* had been cited four times as often as *Science in Action*.

[5] Consider that by replacing just a few words, Latour's observations about science could apply to any of the bureaucratic organizations that characterize modern life:

In this passage, and in the book as a whole, Latour's argument has two parts. On the one hand, scientific work might seem objective and impersonal, almost bureaucratic.[6] On the other hand, what goes on inside of laboratories relies heavily on what goes on outside—that "the bigger, the harder, the purer science is inside, *the further outside other scientists have to go*" (Latour 1987:156 see also p. 159). In short, the internal purity (real or imagined) of science depends on a considerable amount of what might seem like "extra-scientific" work.

In a related vein, Bucchi (2008:63) has noted that sorties into the public sphere might not be about funds, but about using the public sphere as an arena for settling scientific disputes; as he puts it, "scientific discourse at the public level may … not really [be] addressed to the public, but instead … intended to reach a large number of colleagues rapidly" (p. 63). When communicating with the public, scientists are freed from the constraints of specialist communication, both in terms of the limited reach of specialist research and the conservative nature of scientific discourse (e.g., stating caveats and uncertainties). As a result, scientific research may gain credibility within the academic sphere through its wide exposure in the public sphere.

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> If you get inside a *bureaucracy*, you see no public relations, no politics, no ethical problems, no class struggle, no lawyers; you see *organizational routines* isolated from society. But this isolation exists only so far as other *bureaucrats* are constantly busy recruiting investors, interesting and convincing people.

Latour's observations about science are thus similar to those of neoinstitutional theorists about modern organizations. Meyer and Rowan (1977), for example, described how organizations produce "rational myths" as a means to gain legitimacy, aligning themselves with broader institutional norms about performing efficiently, which may or may not align with actual efficiency.

[6] In fact, in *Laboratory Life*, Latour and Woolgar (1986:245–46) suggested that scientists—given their "obsession" with order and documentation—are even more bureaucratic than bureaucrats!

One of the inadequacies of these models of science is that they make no room for the tension that scientists might experience at the science–society interface. Both reasons for public engagement—obtaining funds and settling scientific debates—are ultimately a means to academic ends.[7] But numerous studies have noted the tension that scientists may encounter as they navigate the science–society divide. For example, consider the November–December 2010 issue of *Academe*, titled “The Conflicted University.”[8] The overarching theme of the issue is aptly summarized by its cover image (reproduced in Figure 3), which depicts a scientist dressed in a suit emblazoned with the logos of his sponsors, smiling broadly as he examines the contents of a test tube. The implication is that these sponsors have in some way invaded the

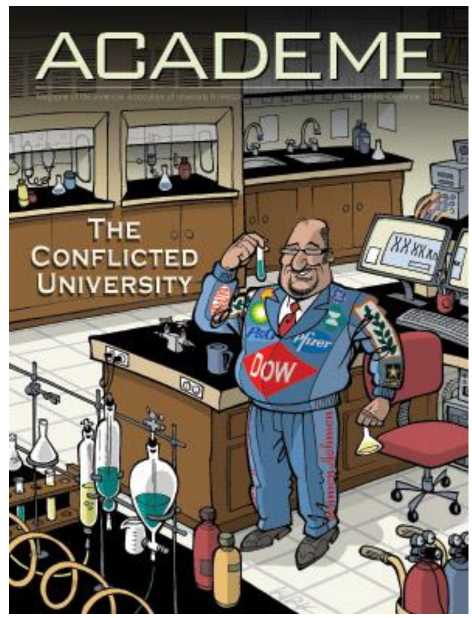

Figure 3. Cover of the November–December 2010 issue of *Academe*

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[7] This cynicism is not restricted to the sociology of science. For example, literature in the sociology of professions initially sought to define professions by “listing the characteristics of an ideal-typical profession against which actual examples of occupational groups could then be assessed a more or less professional.” Each ideal type characterized professions as essentially altruistic. Beginning in the 1960s, however, interactionists began to erode the notion that professionalization instills altruistic traits. As Macdonald (1995:2–4) put it:

> Trainee physicians were portrayed as developing cynicism rather than altruism (Becker et al., 1961), doctors appeared as wielders of power, not servants of the social good (Freidson, 1970a) and most of the professional “traits” were shown to have an ideological tinge (Daniels, 1973) or even to be characterized as “mythology” (McKinlay, 1973b: 62).

I argue that such reactionary analyses, in rightly distinguishing between substance and appearance, often impute negative characteristics onto professions. Research in the sociology of science is thus part of a broader trend that seeks to avoid explaining the behavior of individuals and groups in terms of altruistic motivations, while subtly imputing egoistic motivations.

[8] Academe is the magazine of the American Association of University Professors (AAUP).

scientist's lab and therefore his objectivity. The implicit message is that academic scientists may have conflicts of interest that are not so obvious.

Accounts of science that invoke the ILP shift attention back toward people and the tension they may encounter while pursuing diverse and sometimes conflicting goals. Murray (2010), for example, described four possibilities for when institutional logics overlap: (a) the "blending" of logics, with each one losing some of its characteristics, (b) the "domination" of one logic over the other, (c) the easy coexistence of logics, where neither one impacts the other, and (d) a "productive tension," with the distinction between logics being sharpened. In her examination of how publications (the normative product of academic research) intersect with patents (the normative product of commercial research), Murray found that some academic scientists filed patents not as a means to stake an exclusive proprietary claim on their discoveries, but to prevent *others* (e.g., for-profit companies) from staking such proprietary claims (see also Fini and Lacetera 2010). Their goal was to ensure that certain research topics remained open, and not restricted to just commercial laboratories. In this case, Murray described the intersection of academic and commercial science as one of "productive tension," with the result being a sharpening of the distinction between "academic" and "commercial" science.

These two logics—academic and commercial—capture the essence, respectively, of what Latour says happens "inside" and "outside" of laboratories. The guiding value of academic science is "advancing the boundaries of knowledge," preferably through publications in peer-reviewed journals (Merton 1979:274; Murray 2010; Friedland 2013:30; see also Jasanoff 1994:80 on "research science"). The guiding value of commercial science is Capitalism, that is, the maximization of material wealth (Cooper 2009; Frickel and Moore 2006:15–16; Kleinman and Vallas 2001; Lam 2010; Murray 2010; Vallas and Kleinman 2008). The lack of tension in Latour's depiction of science suggests the "blending" of academic and commercial logics.[9] That is, the reason why scientists go on extra-scientific sorties—"interesting and convincing people"—is to obtain the resources they need to conduct more research. As several commentators have pointed out (Blok and Jensen 2011:16; Brown 2009:23; Collins and Evans 2003:445; see also Hess 1997:111; McNeil 2013:602; Sismondo 2012:417; Spinuzzi 2004; Waelbers and Dorstewitz 2014), this is a rather Machiavellian account of science.[10]

One reason why some sociologists have portrayed scientists as amoral (if not immoral) may be rooted in a key mission of sociological research—to question those in positions of authority. "Our program," Whooley (2018:253) remarked about the field of science studies, "has been to take science down a peg by showing how it fails to live up

[9] The lack of tension in Latour's work may also be rooted in his emphasis on nonhumans in explaining social action. As Collins (2012:411) has noted, Latour's sociology is not really about actors, but actants—about webs of associations where all nodes (e.g., people, animals, rocks, etc.) are the same.

[10] Interestingly, Harman (2009:19) titled his book on Latour *The Prince of Networks.* Seemingly countering the Machiavellian connotation of the title, Harman responded that "Latour is no Machiavellian reducing truth to human power games." Though a scientist may amass "a formidable army of allies" in the construction of a "fact," Harman's point is that "not all of these allies are human": a scientist's allies may include "mighty politicians who grant him funding, pieces of glassy or metallic equipment, and even bacilli themselves. Actors become more real by making larger portions of the cosmos vibrate in harmony with their goals, or by taking detours in their goals to capitalize on the force of nearby actants." To me, by arguing that Latour portrays scientists as enlisting both human and non-human actors, Harman portrayed Latour as even more Machiavellian than Machiavelli himself! Consider that Latour (1988) has been quite explicit about the influence of Machiavelli on his work. As an exception, Hache and Latour (2010) portrayed some environmental scientists as genuinely interested in the non-academic uses of science.

to its own ideals. But we have done less to articulate what those ideals should be, or what kind of expertise might warrant our support." As an exception, Whooley pointed to the work of Collins and Evans (2002:249-250), who argued that those with "special technical expertise" should have a "special part in the decision-making process." The underlying premise of their argument is that people will make better decisions if they have access to such expertise. Recent political events have furthered this notion. Donald Trump's efforts to discredit and defund climate scientists prompted some sociologists to propose a moratorium on critique of climate scientists (MacKendrick 2017). This ignited a debate about the role of sociologists in, as Shostak (2018) put it, "standing up for science."

My goal in this dissertation is not to "stand up for science," but to argue that the public communication of science sometimes takes on moral dimensions. Scientists may perceive public engagement as a moral responsibility. Moreover, when scientific findings are controversial, enacting that responsibility may present a moral dilemma. As Latour has argued, scientists sometimes act in self-interested ways at the science–society interface, but I argue that scientists sometimes resolve moral dilemmas through altruistic acts.

## The logic of public science

I define the logic of "public science" in value-rational terms, with scientists having the underlying goal of positively impacting society. Several different streams of research tap into this idea.[11] Here I make three contributions to this literature:

(1) I systematize the literature on science and society in a new way, taking into account the different times at which academic and public science occur in relation to research.

(2) I argue that the literature on hybrid logics of academic–public science has mainly been concerned with what goes on during study design or implementation, not with what happens after research has been completed.

(3) I draw attention to the moral aspects of science communication, including both moral responsibilities and moral dilemmas.

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[11] As detailed by Hessels and van Lente (2008; with Smits 2009), this research has been called "mode 2" (Nowotny, Scott, and Gibbons 2001), "post-normal" (Funtowicz and Ravetz 1993, 2003; Kastenhofer 2010; Ravetz, Funtowicz, and International Society for Ecological Economics 2013), and "post-academic" (Ziman 2002). And Jasanoff (1994:80) discussed "regulatory science," a specific form of public science that is conducted by government agencies.

I also note in passing that the term "public science" has been used other ways. For example, it has been used to refer to *published* science, with the intended audience being other academic researchers (Larsen 2011; Owen-Smith 2003; Whitley 2003). Thus, this use of public science is quite similar to what the ILP literature has described as "academic science" and does not aim to describe the "outside" of science. Another use of "public science" is closely related to Latour's analysis of science; as Turner (1980:589) put it, "public science," is the "rhetoric, argument, and polemic" that scientists employ to "justify their activities to the political powers … upon whose good will, patronage, and cooperation they depend." This second connotation has the critical edge, bordering on cynicism, that characterizes much of the research in the sociology of science. Scientists, the argument goes, must have some hidden agenda for interacting with the public. Indeed, as I argued in the previous section, mainstream sociological researchers tend to assume that scientists are pragmatic actors who engage the public only to the extent that it bolsters their credibility (Bauer, Allum, and Miller 2007:88).

Table 1. Institutional logics of academic, commercial, and public science

| | Academic Science | Commercial Science | Public Science |
|---|---|---|---|
| Goal | Advance knowledge, produce reliable data, establish facts | Accrue profit and status for self and organization | Positively impact society |
| Cognition | Consider how research affects other research | Strategize how best to accrue profit and status | Consider the social benefits of research |
| Practices – time in relation to data collection | | | |
| ***Before*** | Choose research topic that advances understanding of world | Choose research topic that promises to increase profit and status for self and organization / maintain job security | Choose research topic with positive social impact |
| ***During*** | Follow research ethics (technical) | Satisfy funders and organization of adherence to research ethics | Follow research ethics (moral) |
| ***After*** | Publish findings in academic journals, give presentations at professional conferences | Widely disseminate findings that align with funders' or organization's policy stance; place other findings in "file drawer" | Publicize: produce technology, give advice to decision-makers, provide expert testimony, speak with reporters |

Table 1 summarizes what is novel about my notion of public science by elaborating it alongside the logics of academic and commercial science. The top half of the table acknowledges the importance among academics of publishing journal articles, but it also acknowledges that scientists may have non-academic goals, for example, making money or positively impacting society. The bottom half of the table shows how the typical practices oriented toward each of these values vary by the time period in relation to data collection.

Most of the empirical research on scientists' social responsibilities focuses on the time period prior to data collection (Corley, Kim, and Scheufele 2016; Ladd et al. 2009; Small 2011). This literature flows from two related streams. One stream of research looks at the funding structures that prompt scientists to pursue some research topics over others (Angell 2000; Lesser et al. 2007). For example, the dairy industry may fund research that highlights the "positive aspects" of milk, such as calcium and protein content, but not fund any research on its "negative aspects," such as fat content (Folker, Holm, and Sandøe 2009:229). And Woodhouse (2006:155–56) has noted the impact of a "brown chemistry paradigm" among chemists:

> Although the synthetic organic chemical industry is now in its second century, with many tens of thousands of chemicals in commerce and considerable opportunity to learn from the bad experiences, some of the world's brightest and most highly trained experts continue to poison their fellow humans and the ecosystem without fundamental reconsideration of whether there is a better way to do things.

Woodhouse also noted that most people are plagued by the same "cognitive, institutional, and economic momentum" that affects chemists: "almost everyone assumes that there is no realistic alternative" to brown chemistry—that the only choice is between "better living" with brown chemistry and "back-to-the-cave living" without it. The point here is not that the scientists who carry out studies about the benefits of milk, brown chemistry, etc. are psychologically biased, but that financial incentives favor some research projects over others. To add transparency into possible relationships between funding and outcomes, journal editors routinely request that authors provide "conflicts of interests" statements.

The other stream of literature on public science at the time of choosing a research topic relates to self-censorship and is thus more rooted in social psychology. This literature most commonly examines research with clear social impacts, such as weapons research (Moore 2008) or human genetic manipulation (Nicholas 1999). Kempner, for example, asked scientists about the types of research they thought were "forbidden" (Frickel et al. 2010; Kempner, Merz, and Bosk 2011; Kempner, Perlis, and Merz 2005). She found that the potential political implications of research prompted scientists to consider it off limits. Certain findings, they feared, might be used to exacerbate already existing inequalities or produce new ones. For instance, consider the controversial hypothesis of Harvard president Larry Summers that the higher proportion of men in tenured STEM positions might be attributable to sex-based differences in aptitudes.[12] This hypothesis was controversial because of the wide array of consequences that might ensue if it were true: job-seekers might self-categorize themselves as being genetically unsuited for certain careers, and employers might, in violation of federal law, use genetic differences as guidance in hiring one category of people over another (e.g., men over women). In short, scientists' perceptions of whether a research topic might have any such negative consequences might make them reluctant to conduct it in the first place.

Moving into the arena of data collection, both public and academic science involve "research ethics," which I discuss in greater detail in Chapter 2. Within the logic of academic science, with its goal of producing knowledge, research ethics entails things

[12] Summers's specific hypothesis was in relation to sex-based differences in *variation of aptitudes*, with more men than women being at *both* ends of the STEM aptitude spectrum. The full speech is available here: https://web.archive.org/web/20081212070850/http://www.president.harvard.edu/speeches/2005/nber.html.

like peer-review to reduce innocent mistakes and the intentional fabrication of results.[13] Within the logic of public science, the concern is to make sure that people experience minimal harm during research. For example, Fisher (2015) described how Phase I clinical trials are *designed* to produce adverse effects in healthy study participants. Ethical issues here concern (a) balancing health risks to study participants with monetary benefits and (b) balancing those risks with the potential for the pharmaceuticals being studied to ultimately benefit those inflicted with some disease (i.e., the "greater good"). These sorts of calculations happen all the time. A less banal example of harm to research subjects was the Tuskegee syphilis experiment. Those studying the progression of syphilis did not inform the rural African-American men in the study that they had the disease or treat them for it after the discovery of its cure. And Reverby (2011) recently discovered that some of the same researchers had actively infected hundreds of Guatemalan prisoners, soldiers, and mental health patients with syphilis by granting them access to syphilis-infected prostitutes and through direct exposure to the bacteria that cause syphilis. In both cases, study participants were harmed and received no benefits.

The focus of this dissertation is on the public communication of science. The literature on this topic comes from two main camps (Boholm 2009; McNeil 2013). Researchers in one camp are typically housed in departments of Communication and focus on the "public understanding of science" (PUS). To increase the public's

[13] For a recent review and interesting extension of the literature on research ethics, see Johnson and Ecklund (2016).

understanding of science, they stress the importance of transferring information *from* the scientific community *to* the general public. Researchers in the other camp—who come from a variety of departments, including Anthropology, Geography, History, Philosophy, Political Science, and Sociology—are part of the broader field of science studies. Since these researchers focus on the bidirectional relationship between science and society, they take a critical stance toward communication that only flows *from* scientists and *to* the public. They argue that power relations are intrinsic to communication (Hilgartner 1990).

The PUS account of science communication is often referred to as a "deficit model" because it assumes that scientists and government officials have special access to the "truth," and that the public would be better off if they were made to understand that truth. At its best, the deficit model may help the public to understand information that will improve their lives (see Demuth et al. 2012 for an example on hurricane risk communication). At its worst, it may frame all public resistance to the aims or methods of science as being rooted in a lack of understanding. One conceptual problem with the deficit model is that it fails to separate science from values. As has often been pointed out, science can only give answers about "what is"; it cannot determine "what ought to be" (Lackey 2007). Yet those who are in power may use the deficit model to portray moral and political issues in purely technical terms. For example, they might argue that Joe and Jane Public would make the "right" decision about issues like stem cell research and genetically modified foods if only they could be taught to understand the "facts."

In response to the deficit model, those in science studies have highlighted other, more democratic, models of science communication. Trench (2008), for example, describes two models of science–society relations, one characterized by "dialogue" and the other by "participation." In recent decades, many PUS researchers have moved toward the dialogue model of communication, which presents scientists and non-scientists in two-way communication with one another. This might entail surveying a sample from some target population about their understanding of an issue, such as climate change; then, if the population's understanding differs from what experts know, communication efforts would focus on what that population does not understand (McComas 2006:81). But Trench (2008:128) cautions that such efforts at "engagement" may entail "refinement rather than replacement" of the deficit model.[14] In other words, the underlying goal of engagement might not be to learn from the public, but to find better ways of making the public see what those in power want them to see.

The "participation" model is the most democratic of the three models of public communication. It is two-directional and even allows for a plurality of publics and values. It acknowledges that just because someone holds a public office or has a credential (e.g., a PhD in Chemistry), it does not follow that they have more or better insight into a particular problem. In fact, some uncredentialed members of the public might actually have more expertise than the people who call themselves "experts." For example, Epstein (1995) has documented how some AIDS activists—which he calls "lay

[14] Similarly, Wynne (2006:212) points out that the deficit model is often "buried with great self-congratulatory ceremony, then almost in the same breath reincarnated in some new form."

experts"—were able to gain credibility within expert circles and shape the management of clinical trials. And Brown (1987, 1992, 1997) highlighted cases in which non-experts who suspected that their community had been exposed to environmental hazards engaged in "popular epidemiology"—gathering their own data and marshalling experts to help them better assess and mitigate risk. Such studies demonstrate that public participation need not be restricted to *responding* to science; rather, the public may initiate scientific research and shape its progress.

But I approach knowledge inequality from a somewhat different perspective. I argue that *both* parties in the expert–non-expert relationship may encounter difficulty in reducing knowledge inequality. While terms like "lay experts" and "popular epidemiology" describe the struggles of citizens to be informed about hazards and to perform the research that scientists are unable or unwilling to do, relatively little emphasis has been given to the difficulties that scientists may encounter when providing information to the public. This brings us back to Einstein's quote—that scientists have a "duty" to communicate what they believe to be true and that when scientists' beliefs are controversial, communication may present itself as a dilemma.

As demonstrated above, numerous studies have tapped into ethical issues that arise during study design and implementation, but relatively few have explored ethical issues in the context of communication. As an exception, Rosenthal (1979) wrote of the "file drawer problem," whereby research is conducted, but not reported, because it fails to achieve a certain threshold of statistical significance. In some cases, this may happen due to norms in the scientific community to only share statistically significant results (thus favoring publication of studies that yield false positives as opposed to false negatives). In other cases, researchers may engage in scientific misconduct, suppressing findings strategically. For example, pharmaceutical researchers may suppress findings that the harms of a drug outweigh its benefits by omitting from their sample respondents who did not benefit from the drug or who experienced adverse side-effects.[15] While such misconduct may be in line with the aims of commercial science, it is contrary to the aims of both academic and public science.

Rier (2004) provided an empirical case of something quite similar to the file drawer problem. On the one hand, he found that some toxic-exposure epidemiologists used high-visibility journals—an ostensibly "academic" outlet—to "bullhorn" important findings to both scientific and non-scientific audiences. On the other hand, many of his respondents also admitted to "burying" in low-visibility journals those findings they felt might be harmful—for example, studies that identified smoking as having some health benefits. These scientists thus engaged in an "unobtrusive" form of activism, sacrificing professional recognition for their work out of a "sense of social responsibility" to conceal information that the public might misuse (2004:1544). In short, ethical concerns shaped how some scientists communicated their findings.

[15] To decrease the prevalence of such misconduct within medical research, Brown (2008:207) suggests that journals should require, as a condition of publishing, advanced registration of all clinical trials and full disclosure of any financial conflicts of interest.

## Chapter outline

In the next three chapters, I examine discourse about public science, that is, discourse about scientists' responsibility to positively impact society. In Chapter 2, I seek to identify the "moral background" of science, that is, the conditions that "facilitate, support, or enable" scientists' moral judgments and beliefs (Abend 2016:16–17). What are the structural determinants of which issues scientists think of as "moral" and their discourse surrounding what is appropriate and inappropriate? To address this question, I examine what the National Academy of Sciences (NAS)—arguably the most prestigious science organization in the United States and the self-declared "advisor to the nation"—says about science. In particular, I focus on what NAS "consensus reports" say about how academic scientists ought to communicate with the public about environmental hazards. I view each report as both *reflecting* typical discourse about risk communication at the time it was written and *structuring* future discourse.

In Chapter 3, I move into the moral foreground, getting at the heart of Einstein's quote about public intellectuals. I explore situations in which academic scientists have incentives to conceal from the public information that they believe would mitigate risk. In particular, I spoke with environmental scientists who work in Louisiana about norms of communication about various hazards, such as Hurricane Katrina, which struck the US Gulf Coast in 2005. Why wasn't information about the risk posed by a possible Katrina-like event translated into actions to reduce the vulnerability of people living in low-lying areas?

In Chapter 4, I explore what happens when the public questions taken-for-granted relations between science and society. In particular, I examine how science organizations from around the world responded to a manslaughter trial against seven scientists and engineers. The prosecution argued the defendants had offered poor scientific advice in the days before an earthquake struck L'Aquila, Italy in 2009. According to the prosecution, if the experts had said something different or had said nothing at all, then dozens of people would not have died. In the course of my analysis, I offer translations from Italian of key texts, including the risk communication in question, laws relevant to the trial, and the judge's explanation of the verdict.

While much has been written about the case, relatively little has been written about how the scientific community responded. Interestingly, many science organizations from around the world defended the experts' poorly-worded statements. To explain this phenomenon, I introduce the concept of "scientific immunity," which may be enacted in three ways: (a) misconstruing information pertinent to scientists' legal accountability (e.g., the scientific consensus regarding predictions, the nature of the charges, etc.), (b) sidestepping individual scientists' legal accountability by portraying the case as being about "science" writ large, or (c) distinguishing responsibilities that are scientific from those that are political. In this chapter, I contribute to the overarching theme of the dissertation by arguing that the lack of norms for crisis communication about earthquakes may have resulted in pleas for immunity.

# Chapter 2

# The Moral Background of Science: The role of the National Academies

*Many of us were trained to think of ourselves as working in the "ivory tower" mode—seekers of truth uncontaminated by the outside world.*
—Beckwith and Huang (2005:1479)

## Introduction

What is "science," and what drives scientists to do what they do? Perhaps no scientist has stated his or her position as succinctly as Marc Edwards, Professor of Civil and Environmental Engineering at Virginia Tech, who blew the whistle on two crises regarding lead in people's drinking water: "Science should be about pursuing the truth and helping people."[16] These goals, however, are not always aligned. Scientists typically cannot "pursue the truth"—the hallmark of "academic science"—unless they receive grants from government agencies. So when the government agencies that fund research are actually responsible for hurting people—or failing to help them—Edwards notes that most scientists have "perverse incentives" to keep silent. "If an environmental injustice is occurring," he remarked, "someone in a government agency is not doing their job." But pointing this out can be disastrous for a scientist's career. "You are your funding network," he said. "When was the last time you heard anyone in academia publicly

[16] The quotes in this paragraph are drawn from a February 2016 interview of Edwards by *The Chronicle of Higher Education* (Kolowich 2016).

criticize a funding agency, no matter how outrageous their behavior?" By blowing the whistle, Edwards lost friends, destroyed his funding network, and spent thousands of dollars out of his own pocket.

Other scientists surely have different ideas about what science is and what it ought to be. In this chapter, I show that such notions about science do not develop in isolation, but are structured by science organizations and funding agencies. In the previous chapter, I showed that sociologists have tended to offer morally sterile portrayals of science and scientists. I then outlined the institutional logic of "public science," which has explicit moral elements. In this chapter, I show that organizations form the "moral background" of science, simultaneously structuring and reflecting norms of public communication about risk.

## The moral background of science

Science organizations and funding agencies are elements of what Abend (2016:16–17) calls the "moral background"—that is, the conditions that "facilitate, support, or enable" people's moral judgments and beliefs. To clarify the distinction between the moral foreground (the judgments and beliefs themselves) and the moral background, Abend uses the analogy of a restaurant menu: "What you order is up to you, but what is listed on the menu is not" (p. 38). The relationship between the moral background and the moral foreground thus reflects the distinction, applied to morality, between structure and agency. Organizations shape the extent to which scientists think about and perform various "moral" duties as they carry out their work. And, of course, throughout the scientific process, there is the moral agenda of funders, whose sometimes visible and sometimes invisible hand shapes the research that gets done and the ways in which it is communicated.

One way to probe the moral background, writes Abend (2016:21), is to examine “public moral normativity”—that is, “normative standards” that are published in “socially prominent and prestigious loci.” As an example of public moral normativity, some funding agencies stress the importance of ethical issues by requiring that scientists state in grant applications the extra-scientific impact of their proposed research. The National Science Foundation (NSF), for example, has always taken into account issues of social responsibility in its criteria for awarding grants, and in 1997, it formally required that PIs give equal emphasis to the “intellectual merit” and “broader impacts” of their proposed research (Rothenberg 2010). The moral background here is layered: the NSF publicly states the aspects of scientific research that are important, which presumably shapes (a) research proposals, (b) the proposals that reviewers accept, and, ultimately, (c) the science that gets done. As another example of how funding agencies structure scientific activity, Smith-Doerr (2006) traced the development of ethics training curriculum among health researchers to a 2000 National Institutes of Health (NIH) requirement that all grantees receive ethics training. She found that, in the years following the requirement, the percentage of graduate programs with formal ethics training requirements nearly doubled (p. 419). The NIH funding requirements thus helped to shape the moral socialization of health researchers in the United States.

But in both cases, whether the moral background affects the moral foreground is an empirical question. That is, does the public moral normativity of funding agencies actually shape the research that gets done or the issues that scientists think of as being morally relevant? Watts et al. (2015) found that, despite multiple efforts by the NSF to promote the importance of broader impacts, both principal investigators and reviewers continue to pay more attention to intellectual merit. There is, thus, a gap between what is publicly discussed as important—both broader impacts and intellectual merit—and what principal investigators and reviewers tend to view as important—only intellectual merit. And, in a bow to Meyer and Rowan (1977), Smith-Doerr (2006) found that the ethics training programs that emerged in the wake of the NIH requirement were typically implemented as "rational myths," that is, organizational posturing without any serious changes in curriculum. Most graduate programs allocated no or low credit to courses in research ethics (p. 422), and some scientists ridiculed web-based training materials as "silly," laughable," and "a joke" (p. 416). Presumably, the gap between myth and reality will narrow in academia only to the extent that graduate programs take ethical issues seriously, treating them as more than just "an administrative hoop to jump through" to satisfy requirements for funding (Reardon 2013:179).

The literature on "science" and "ethics" has two limitations that I seek to address in this chapter. First, it has not yet explored the public communication of science. As detailed in the introduction to this dissertation, relatively few studies on the public communication of science have explored ethical issues. Folker et al. (2009:230), for example, portrayed the root "dilemma" of public communication as tension between "scientists' individual and collective interests." The scientists in their study described media interviews as "branding" that would help them to attract corporate sponsors for their research. Yet the scientists openly acknowledged that corporate sponsorship biases scientific research, both in terms of the topics that are researched and the findings that get published. In contrast to such dilemmas, which are rooted in self-interest, the dilemmas that I explore in this dissertation are those that involve considerations of the public. To what extent are scientists motivated by the logic of public science, that is, by helping people? And what cultural incentives and disincentives are there for using their work to redress injustices?

A second limitation of the literature is that there has been little discussion of how ethical issues that arise in the context of public communication have been institutionalized. As Ezzamel and Willmott (2014) have pointed out, the literature in organizational sociology has tended not to focus on the institutionalization of routines that fall under the umbrellas of "ethics" or "morality." As an exception, Kinchy and Kleinman (2003) found that the Ecological Society of America institutionalized its engagement with society through a fellowship program that trains ecologists in how and why to communicate with non-scientists. The program's current mission statement, which includes helping fellows to produce "a strong, positive impact on decision-making," perfectly exemplifies what I've been calling "public science."[17] The program thus functions as an element in the moral background of science, facilitating cognition about the broader impacts of research and public communication as a means to effect change.

In this chapter, I describe the broader moral background of the public communication of science—that is, normative standards that go beyond a particular funding agency or science organization. To that end, I turn to "consensus reports" authored by the National Academy of Sciences (NAS), which is arguably the most prestigious science organization in the United States. Compared to other means of tapping into the moral background of science, analyzing consensus reports is relatively straightforward. For example, another approach to probing the moral background might have entailed analyzing what individual scientists say about public communication and what they don't, for example, by analyzing books like *Real Science: What it is and What it Means* by John Ziman (2002) or *The Everyday Practice of Science: Where Intuition and Passion Meet Objectivity and Logic* by Frederick Grinnell (2011). But because these books are not the product of a collective of scientists, it would be difficult to assess whether they reflect underlying norms regarding public communication or just the views of the individual authors. Using interviews to probe the moral background would pose the same difficulty.

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[17] See https://leopoldleadership.stanford.edu/about/mission.

The NAS, in contrast, is the self-declared "advisor to the nation." Its consensus reports are written by a committee of experts, are subjected to independent peer-review, and reflect the official position of the NAS on wide-ranging public policies. For these reasons, I view the reports as illuminating the broader moral background of science.[18] What follows is my analysis of two groups of NAS reports. First, I examined guidebooks, aimed at scientists in general, that describe what science is and ought to be. Second, because these guidebooks said relatively little about the ethics of science communication, I sought out reports that would be more likely than the typical report to discuss ethical issues. Since research on hazards has obvious ethical stakes, I performed a targeted search for reports about risk. My focus in this chapter is on the advice that NAS reports do and do not offer scientists who communicate with the public. How do the reports address "moral" and "ethical" issues related to communication? What normative standards do they proclaim most forcefully? And what do they leave unsaid?

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[18] A similar analysis of the moral background of science could be performed by analyzing texts published by other science organizations, for example, the American Association for the Advancement of Science (AAAS), which has the motto "Advancing Science, Serving Society." In contrast to the NAS, however, the AAAS has not articulated a broad vision of what science is or how it should serve society. Moreover, while the AAAS publishes numerous "policy statements," these are targeted at specific policy areas (e.g., a press release titled "AAAS Opposes Oklahoma's Controversial Science Education Bill") rather than the proper role of scientists in society. Similarly, while the AAAS also offers a quarterly *Professional Ethics Report* (PER), the papers therein are mainly journalistic in nature. The scholarly publications in the PER are similar to those published in journals like *Science and Engineering Ethics*, written by individuals about some topic with ethical import. Nothing in the PER is prescribed by the AAAS itself.

## National Academy of Sciences reports on the nature of science

### Background on reports on the nature of science

In 1989, in the wake of several scandals of scientific misconduct, the National Academy of Sciences (NAS) published *On Being a Scientist* (OBAS). The 22-page booklet sought to describe for students "the methods and values of scientific research" (p. 9056), focusing on the role of peer review in ferreting out honest mistakes and outright fraud. Three years later, the NAS published *Responsible Science: Ensuring the Integrity of the Research Process* (RS), a more extended (224-page) meditation on scientific misconduct. In 1995, the NAS published a second edition of OBAS, which incorporated material from RS. A notable addition was the inclusion of hypothetical instances of scientific misconduct intended to foster discussion among researchers about appropriate and inappropriate actions. A third edition of OBAS was published in 2009.

Table 2 lists each report, along with its year of publication, its sponsor, and its intended audience. One observation is that the most recent edition of OBAS had only a single sponsor, the NSF, whereas each of the previous editions received only private sources of funding. RS had both private and public funders. Another observation is the growing intended audience across the editions of OBAS, from university students to all researchers in any setting (academic, industrial, and governmental). A related point pertinent to the moral background is the large distribution of the reports. The second edition boasted that over 200,000 copies of the first edition had been distributed to graduate and undergraduate science students. And Figure 4 shows that the PDF of the third edition has been steadily downloaded about 11,000 times per year, having exceeded 100,000 total downloads in 2017. Moreover, an Internet search of "On Being a Scientist" and "syllabus" reveals that the report has been discussed in hundreds, if not thousands, of courses. The report has thus helped to structure the moral education of hundreds of thousands of scientists, delineating what falls inside and outside the scope of their responsibilities as scientists.

Table 2. NAS publications on the nature of science

| Title of Publication (abbreviation) | Year | Sponsor | Intended Audience |
|---|---|---|---|
| *On Being a Scientist* (OBAS) | 1989 | multiple private foundations and organizations | "students who are beginning to do scientific research" |
| *Responsible Science: Ensuring the Integrity of the Research Process* (RS) | 1992 | multiple government agencies and multiple private foundations | "*all* members of the scientific community, regardless of their institutional affiliation" (emphasis in original) |
| *On Being a Scientist: Responsible Conduct in Research* (OBAS) | 1995 | multiple private foundations and organizations | "graduate students and beginning researchers" but "its lessons apply to all scientists at all stages of their scientific careers" |
| *On Being a Scientist: A Guide to Responsible Conduct in Research* (OBAS) | 2009 | National Science Foundation | "graduate students, postdocs, and junior faculty in an academic setting" but also "useful for scientists at all stages in their education and careers, including those working for industry and government" |

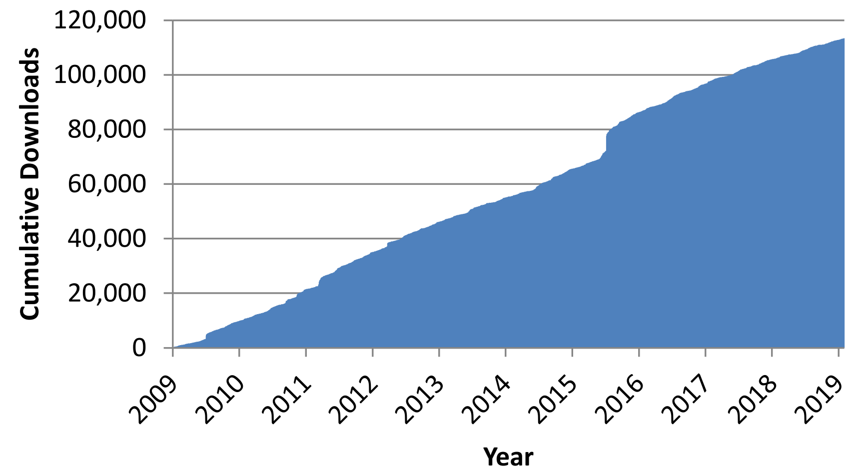


Figure 4. PDF downloads of *On Being a Scientist*, 3rd edition
*Source: csv download in May 2019 of download statistics from https://www.nap.edu/catalog/12192/on-being-a-scientist-a-guide-to-responsible-conduct-in*

## The shifting moral background of the public communication of science

Five observations stood out in my analysis of the guidebooks on science. First, the reports give relatively little emphasis to the public communication of science, as compared to issues of study design and implementation. As my focus in this dissertation is mainly on communication, I've placed my analysis of non-communicative moral issues into an appendix (see Appendix A: Moral issues in science cordoned to "research ethics").

Second, the editions of OBAS gave progressively less emphasis to public science over time. The number of words in the section on public science (titled "The Scientist in Society" in the first two editions and "The Researcher in Society" in the third edition) went from 593 in the first edition to 507 in the second edition to 280 in the third edition—a 53% drop in words from the first edition to the third. Changes in the structure of OBAS also reflect the growing marginalization of this section. While this section has always been among the shortest, the first edition organized this section as a top-level heading, along with just two other sections. In the third edition, this section exists at the same level as 11 other sections.

Third, another change across editions is the weakening prescription for public communication, from a "responsibility"—a social and moral imperative—to a "right"—an option that scientists may or may not elect to pursue. The first edition states that interacting with nonscientists is "a fundamental responsibility for the scientific community." Similarly, the second edition states that when scientists "find that their discoveries have implications for some important aspect of public affairs, they have a responsibility to call attention to the public issues involved." The third edition, in contrast, states that researchers have "the right to express their convictions and work for social change." It exemplifies these rights descriptively: scientists "often" give expert advice, "can" contribute to risk assessments, and "frequently" educate non-scientists about various policy issues. The third edition thus lacks the prescriptions for public communication that are present in the first two editions.[19]

Fourth, along with the weakening of prescriptions for communication comes a more forceful discussion of inhibitions. Both the first and second editions state that, for some scientists, their responsibility to engage the public may bump up against other responsibilities and seem like a "distraction" from their research. At face value, this is at

[19] Interestingly, *Emerging and Readily Available Technologies and National Security* (ERAT), which I discuss in the next section, discusses scientists' responsibilities quotes the third edition of OBAS (p. 22). But the authors of ERAT apparently appreciated the stronger language of the second edition OBAS because they then quoted the corresponding passage in a footnote.

worst an indirect inhibition that is no different from other things that take up time, such as exercise, hobbies, and relationships. (In the next chapter, I discuss why scientists' comments about lacking the time for public communication should not necessarily be taken at face value.) In contrast, the third edition identifies a direct inhibition to public communication, warning that when scientists become "advocates" on an issue, their colleagues and members of the public may perceive them as "biased." Curiously, the only advice that the third edition offers to counter the stigma of bias—to always discuss one's research "as objectively and as accurately as possible"—falls short of the advice it offers in other chapters. Almost every other chapter contains a case study in which hypothetical researchers must grapple with some moral dilemma.[20] Then, for each case study, the NAS applies "professional standards" to help elucidate "better and worse ways" of resolving the dilemma (pp. xv–xvi). The lack of a sustained discussion about "professional standards" for public communication suggests to readers that there are no such standards and that whether there are "better and worse ways" of engaging the public is unknown.

Fifth, the strongest communicative prescription in the guidebooks is about "blowing the whistle" on instances of misconduct. Using stark language, the first edition of OBAS declares that witnessing misconduct by a colleague is "perhaps the most disturbing situation that a researcher can encounter" and that "researchers have a professional and ethical obligation to do something" (p. 9072). The second (p. 18) and

[20] Of the 12 chapters in the third edition, the one on "Laboratory Safety in Research" is the only other one that does not contain a hypothetical case study.

third (p. 19) editions of OBAS state that witnesses have “an unmistakable obligation to act.” Similarly, RS states that blowing the whistle on misconduct is a scientist’s “responsibility” and “part of their professional obligations” (p. 154). The irony here is that most definitions of “whistleblowing” are about witnessing something that is *morally* wrong and then communicating that information to the public. But the NAS uses “whistleblowing” to refer to situations that are almost exclusively academic—witnessing something that is *epistemologically* wrong, and then reporting that information to academic authorities (e.g., to a PI). This cooptation of whistleblowing thus constitutes its removal from the lexicon of “public science” and its corresponding insertion into the lexicon of “academic science.” In line with the previous observation that scientists have no standards for engaging the public, the commandeering of “whistleblowing” means that, compared to other professionals, scientists have one less word at this disposal that describes communicating with the public about injustices.

In sum, the changes in OBAS across editions reveal that the moral background of science has changed over time. In 1989, OBAS portrayed public communication as morally unambiguous; to enact this “fundamental responsibility,” scientists simply need to allocate enough time. By 2009, however, the guidebook portrayed public communication as a moral dilemma. It seamlessly segued between public communication and advocacy, stating that scientists who engage in advocacy risk sacrificing their credibility and coming off as “biased.” One constant across editions is the lack of a substantive discussion of how scientists should engage the public.

## National Academy reports on policies or communication about risk

### Background on reports on risk

Given the paucity of advice for public communication in the handbooks on science, I sought out NAS reports related to risk, which I suspected would be more likely to discuss moral issues than the typical NAS report. On February 8, 2016, I searched the website of the National Academies Press (NAP; http://www.nap.edu/) for *science risk communication*, which yielded 52 results, and *science risk policy*, which yielded 75 results.[21] Eight documents came back in both searches, for a total of 119 unique documents. By reading through the abstracts of all 119 documents, I was able to conduct a preliminary assessment of their relevance to moral issues surrounding the public communication of science. For abstracts whose relevance was unclear, I consulted the executive summary or the body of the report. Reasons for excluding a document included it being (a) "proceedings" (e.g., a workshop summary), which are a compilation of individual opinions and therefore not an official statement from the NAS;[22] (b) an analysis/assessment of some specific problem (e.g., "Long-Term Health Consequences of Exposure to Burn Pits in Iraq and Afghanistan") and therefore not being relevant for scientists more generally; or (c) ostensibly irrelevant to moral issues (e.g., "Research Needs in Subsurface Science"). In other words, I included reports that

[21] The searches were in that word order, without quotes. Changing the word order prompts the NAP search engine to return different documents.

[22] In contrast to consensus reports, "proceedings" contain statements and opinions of those who participated in workshops, symposia, or other events. The NAP website states that the content of proceedings is "not endorsed by other participants, the planning committee, or the National Academies."

reflected consensus among scientists, were written for the typical scientist, and held promise for probing the moral background of science communication.[23] Ultimately, I determined that 6 of the 119 documents returned by my searches were sufficiently relevant to read.

The following year, the NAP published another report that addressed the topic of science communication head on: *Communicating Science Effectively: A Research Agenda*. I also read this document, for a total of seven reports. All seven reports are listed in Table 3 with their year of publication, sponsor, and intended audience.

*Improving Risk Communication* (IRC) offers an in-depth discussion of why risk communication is difficult and how it can be improved, and *Communicating Science Effectively: A Research Agenda* (CSE) aims to help science communicators tailor their messages to specific audiences and contexts. An observation is that these two reports—the ones that address risk communication most directly—were sponsored exclusively by private organizations; the other five reports were sponsored exclusively by government agencies or a mix of private and public funders.

The four reports sponsored fully or in part by the Environmental Protection Agency (EPA) analyze the risk assessment and risk communication practices of the EPA and offer suggestions for improvement; however, much of their insight applies to risk

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[23] Though none of the reports were targeted solely at individual scientists, I favored reports that mentioned individual "scientists" or "researchers" as part of their intended audience. Although *Improving Risk Communication* is directed at "decision-makers," and not "scientists," I opted to read it because it is a classic in the risk communication literature that provides a detailed discussion of the difficulties of risk communication.

assessment and risk communication more generally. *Science and Judgment in Risk Assessment* (SJRA) focuses on EPA's implementation of the 1990 Clean Air Act Amendments. *Understanding Risk: Informing Decisions in a Democratic Society* (UR) argues for more public engagement throughout the risk assessment process, directing it toward informing people's choices and solving problems. Similarly, *Science and Decisions: Advancing Risk Assessment* (SD) focuses on giving people different options to mitigate the risk of various hazards, framing the entire risk assessment process as a tool to help people choose the option that is best for them. And *Human Biomonitoring for Environmental Chemicals* (HBEC) explores how to improve (a) research on biomarkers (biological measures of people's exposure to pollutants), (b) the interpretation of biomonitoring data, and (c) the communication of findings to the public.

Finally, *Emerging and Readily Available Technologies and National Security: A Framework for Addressing Ethical, Legal, and Societal Issues* (ERAT) differs from the other reports in that it does not discuss research on hazards. Rather, it explores ethical issues surrounding the development of technologies with military relevance.

As a final note, most of these reports were not authored by the National Academy of Sciences itself, but by its parallel organizations, which include the National Academy of Engineering and the National Academy of Medicine. Yet for simplicity, I refer to the authoring organization as the "National Academy of Sciences" or "NAS." The NAS is the foremost of the National Academies, having been established by Congress in 1863, whereas the other National Academies were established more than a century later via the NAS charter. As a more practical concern, I use "NAS" because "NA" has the distracting connotation of "not applicable."

Table 3. NAS publications on policies or communication about risk

| Title of Publication (abbreviation) | Year | Sponsor | Intended Audience |
|---|---|---|---|
| *Improving Risk Communication* (IRC) | 1989 | National Research Council | "governments, private and nonprofit sector organizations, and concerned citizens," but "aimed especially at top decision-makers in government and industry" |
| *Science and Judgment in Risk Assessment* (SJRA) | 1994 | Environmental Protection Agency | "EPA and other agencies, risk managers, environmental advocates, scientists, faculty, students, and concerned individuals" |
| *Understanding Risk: Informing Decisions in a Democratic Society* (UR) | 1996 | multiple government agencies (including the EPA) and NGOs | "anyone involved in risk issues: federal, state, and local policymakers and regulators; risk managers; scientists; industrialists; researchers; and concerned individuals" |
| *Human Biomonitoring for Environmental Chemicals* (HBEC) | 2006 | Environmental Protection Agency | EPA and, more generally, researchers who work with biomonitoring data |
| *Science and Decisions: Advancing Risk Assessment* (SD) | 2009 | Environmental Protection Agency | EPA and "those working in the regulatory and public health fields" |
| *Emerging and Readily Available Technologies and National Security: A Framework for Addressing Ethical, Legal, and Societal Issues* (ERAT) | 2014 | Defense Advanced Research Projects Agency | DARPA, but also "policy makers, institutions, and individual researchers" who work on "technologies of military relevance" |
| *Communicating Science Effectively: A Research Agenda* (CSE) | 2017 | multiple private foundations | science communicators seeking to apply research in science communication and researchers who want to fill in knowledge gaps in this literature |

### The uneven moral background of public communication about risk

Like the guidebooks on science, the reports on risk note that public communication can pose various difficulties for scientists. But a general thrust of these reports is that many of these difficulties could have been resolved earlier in the risk characterization process. IRC, for example, states that risk characterization is often viewed as too “sequential” of a process: “(1) the organization’s technical experts assess a risk and explore options, (2) a risk management decision is made, (3) a message is internally prepared, and (4) the message is sent to outsiders” (p. 148; see also HBEC, p. 104; CSE, pp. 57–58). This sequence closely mirrors the so-called “linear model” of science communication, which emphasizes the unidirectional flow of information from knowledgeable experts to an ignorant public.

The NAS conveys the same critiques of the linear model that are present within science studies. For example, Wynne (2006:214) states that the linear model reflects a “falsely narrowed … moral imagination,” promoting “the idea that support for the policy stance is determined by scientific fact, and that no alternative is left.” IRC advocates that risk communication, rather than focusing on “one-way messages,” be “a two-way street” characterized by “dialogue with potentially affected outsiders” (p. 151). Even more in line with contemporary science studies (e.g., Trench 2008:131–32 on “one-way, two-way and three-way models” of communication), HBEC states that risk communication is “at least a two-way, if not a multiple-voice all-talking-at-once, conversation in which scientists are not the sole generators of … data, let alone the only ones able to interpret and use the data" (p. 203). The reports stress that if scientists or government officials only begin to think about communication *after* a risk assessment or policy decision has been produced, the public may feel, perhaps rightly so, that their information needs were never taken into account and that they were not given the opportunity to help shape policy decisions. From their perspective, the entire risk characterization process may have been pointed in the wrong direction, focusing on issues that they find irrelevant. It's not that such risk characterizations are amoral; it's that they're morally disconnected from what people want to know.

To ensure that the public gets the information it needs, the reports prescribe that researchers take the public's needs into account throughout the entire risk characterization process, from problem formulation to risk communication (HBEC, p. 110). To that end, the reports suggest that organizations institutionalize various points of contact with society. For example, the reports highlight the role that funding agencies may play in fostering public communication. HBEC recommends that funding agencies require researchers to explicitly identify communication goals in study proposals, along with how they will evaluate whether the communication goals were successful (p. 105; see also IRC, pp.150–151). "If achievement of the goals cannot be validly or reliably measured," HBEC continues, then "goals need to be reframed or revised, because this problem implies that goals have not been fully thought out" (p. 107). To encourage researchers to evaluate whether they have met their goals, HBEC recommends that the task of evaluating communication be funded with its own line item.

Yet because there is only so much that can be planned for, the reports’ recommendations do not readily apply to emergent threats—that is, to threats that were unforeseen at the time a study was designed. In the two reports that offer the most sustained discussion of ethics—HBEC and ERAT—the omission of such ad hoc communication needs is systematic. Consider HBEC’s depiction of the stages of research projects, which I have reproduced as Figure 5. Note that boxes labeled “Ethics” and “Communication” flow into study design and study conduct. These boxes are about (a) ensuring that research subjects are informed of any potential dangers of a study prior to their agreeing to participate in the study and (b) informing research subjects of what the study aims to measure. As an example of such ethical issues, a study may reveal a condition for which there is no known treatment or cure. Under such circumstances, people may have different views about whether they want to participate in the study, such as “What good does it do to know if nothing can be done?” vs. “I want to know so I can be prepared” (p. 126). But HBEC says relatively little about the communication of population-level findings, that is, about results that extend beyond the particular sample being studied. Indeed, as Figure 5 makes plain, HBEC does not address ethical issues regarding the broader public communication of science. While it might be expected that a figure in a chapter on “Considerations in the Design of Biomonitoring Studies” might zoom in on the themes that are central to that chapter, there are no corresponding figures in later chapters that zoom in on how ethical issues relate to public communication.

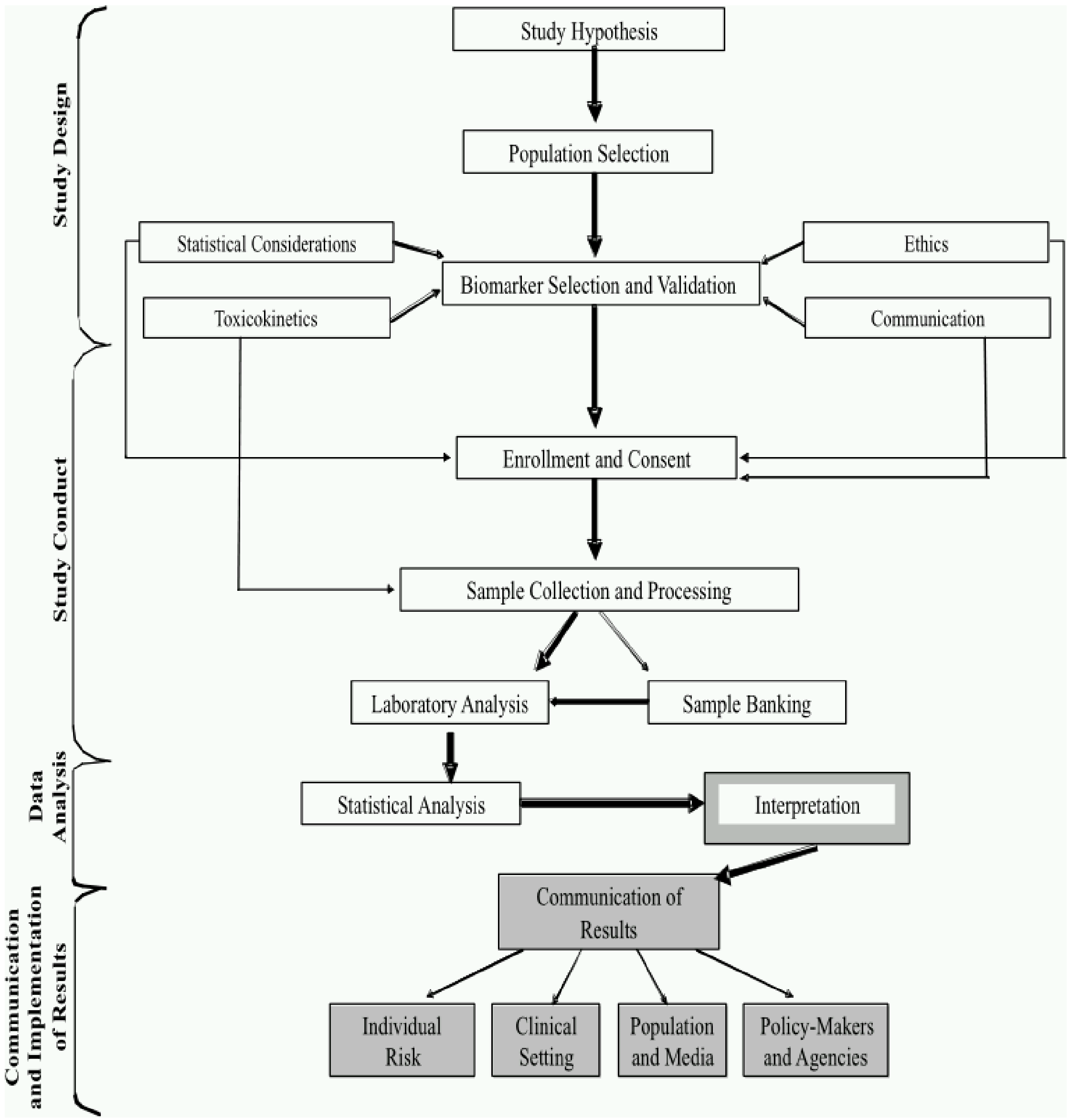


Figure 5: Stages of a biomonitoring study
*Source: reproduced from HBEC, p. 86*

ERAT makes a similar omission in its discussion of "ethical, legal, and societal issues" (ELSI). Although ERAT recommends that funding agencies monitor research projects for any ethical issues that might arise while research is ongoing (see Appendix

B: Getting the "right" science), the report does not discuss ethical issues that might arise after research has been completed.

Interestingly, HBEC and ERAT also offer the most explicit acknowledgment that scientists sometimes take the public's needs into account to attain academic ends. For example, at several points, HBEC seems to indicate that ethical issues are less about promoting public health than they are about promoting the science of biomonitoring:

> ethical issues … constrain the advancement of biomonitoring. (p. 239, see also pp. 8, 252)

> challenges include understanding ethical and communication issues that are essential to the continued advancement of this field. (p. xi, see also p. 21)

> Ethical issues can stop specific studies, and the field in general, dead in their tracks. Therefore, it is incumbent on investigators, policy-makers, and others to consider these issues carefully. (p. 99)

The general thrust of these phrases is that researchers should attend to ethical issues in order to prevent their work from being censured. ERAT is even more overtly attuned to impression management. Funding agencies, the report states, have a "self-interest" in exploring the ELSI of the research projects that they sponsor:

> Ethical and social issues are much more than public relations problems, but they also definitely are public relations problems. Even if an agency were concerned solely with its own future, and not with the broader consequences of its actions, it would still have to worry about the ethical and social implications of its work. (p. 252)

These passages from HBEC and ERAT thus acknowledge that the relationship between advancing knowledge and ethical considerations may be complex. In line with my observations regarding the reports on the nature of science (see Appendix A: Moral issues in science cordoned to "research ethics"), the reports on risk state that scientists must engage in discourse about "ethics" in order to get science done. Whether this discourse is "just" discourse is a separate issue.

A final observation is that the reports on risk say surprisingly little about the role of individuals in facilitating effective risk communication. For example, although the NAP website bills UR as important to a variety of individuals (e.g., "scientists"), the report itself states that its advice is directed exclusively at organizations (p. 13). The two reports that were not sponsored by government agencies—IRC and CSE—are exceptions. They are the only reports that discuss moral dilemmas that individual researchers might encounter when communicating risk. Yet these reports exhibit the same ambivalence about public communication as the third edition of OBAS. CSE, for example, explicitly brackets out "the important question of what knowledge from science is ready to communicate and worth communicating outside of the scientific community," stating that this question "may not be addressable through a research agenda" (p. 16). Yet, just two paragraphs later, the report asserts that "the scientific community has an obligation to communicate the results of its work to the rest of society" (p. 16) and, a few pages after that, that individual scientists have "a duty to speak about their work" (p. 19). CSE thus asserts that there is no objective way of determining when scientists have an obligation to communicate their work to the public, yet they nonetheless have such an obligation.

Perhaps one reason why CSE is vague about whether scientists have a "duty" to communicate with the public is rooted in the report writers' skepticism about whether communication actually has any effect on the public’s understanding of any given issue (p. 16, 81). That is, it would have been difficult for the report writers to argue that scientists have a duty to communicate with the public if the effects of such communication have not been consistently demonstrated. Another reason for the report’s ambivalence is that controversy surrounding an issue can make public communication difficult, regardless of how well it is planned out. To help resolve controversy, IRC stresses the need for a “public forum at which relevant voices have had their say” (p. 169). The idea behind this is that if attempts to influence people’s behavior are questioned, the communicator can point to some public process that had legitimated his or her advocacy (and, in so doing, re-legitimate it). But IRC warns that for some controversies, little can be done to immunize oneself from political backlash—that communicators “should expect their audiences to suspect attempts to influence even when the intent is simply to inform” and that “credibility is a casualty” (pp. 168–169). For this reason, IRC states that advocating a particular course of action can pose a “thorny ethical problem” (p. 72). Moreover, CSE notes that the mere attempt to debunk misinformation may actually reinforce false beliefs and that challenging pre-existing beliefs may prompt people to dismiss the credibility of the messenger (p. 64). The difficulty is that a scientist may want to convey a sense of urgency about some hazard, yet realize that doing so may ultimately have negative personal or social consequences—diminishing his or her own credibility or increasing people’s vulnerability to hazards.

## Discussion

A key finding in this chapter is that prominent consensus reports published by the National Academy of Sciences (NAS) tend not to discuss ethical issues regarding the public communication of science. This is in line with prior research showing that discussions of "ethics" and "science" tend not to address issues regarding the "broader impacts" of science, instead focusing on issues that are internal to the scientific community (Pimple 2002; Schienke et al. 2009). In this chapter, I have extended this literature by focusing on how scientists discuss ethical issues that relate specifically to public communication.

The development of *On Being a Scientist* (OBAS) across editions went hand-in-hand with a shift in how the NAS discusses the public communication of science. In 1989, the first edition had stated that public communication was a "responsibility" whose fulfillment, at worst, might pose an inconvenience. Twenty years later, it was a "right" whose invocation might engender the twin stigmas of "advocacy" and "bias." These changes suggest that the moral background of science has changed, with public communication becoming less prescribed and more stigmatized over time. Moreover, the overall structural development of OBAS across editions—namely, the inclusion of hypothetical scenarios intended to tease out professional standards—makes it plain that there are no professional standards for public communication. As a result, the hundreds of thousands of scientists who encountered the handbook on science found no indication that they ought to engage the public or any useful advice for overcoming perceptions that those who engage in advocacy are biased.

The political and economic climate at the time the third edition of OBAS was published may help to explain this trend. The Great Recession, which began in December 2007, granted fiscal conservatives license to attack the flow of university and government resources toward research they perceived to be driven by “liberal” values and therefore “superfluous.” For example, in *The National Science Foundation: Under the Microscope*, US Senator Tom Coburn (2011) described what he viewed as “tens of millions of dollars spent on questionable studies.” Such political attacks may have prompted scientists to withdraw from the scrutiny of the public sphere. By the time the most recent edition of OBAS was published, scientists may have already redirected their work toward less controversial topics. One way for future researchers to identify why OBAS developed as it did would be to ask the authors of the third edition of OBAS about their reasons for downplaying scientists’ responsibilities to engage the public.

Regarding the reports on risk, one reason why the report writers may have neglected ethical issues related to public communication is that they lacked the vocabulary for doing so. As Schienke et al. (2009) pointed out, “ethics” education in science tends to focus on normative issues that are internal to the scientific community, such as the proper allocation of credit among authors and the threat that data fabrication poses to the advancement of knowledge. For example, in a study of biomedical researchers, McCormick et al. (2009:6) had to use the phrase “societal and ethical issues" (p. 6) to tap into broader ethical issues. When they asked about "ethical issues," respondents only discussed things like plagiarism or issues pertinent to the conduct of research, such as the welfare of animal subjects. Given that funding agencies like NIH defined training in "research ethics" as driven by primarily by such issues, it's not surprising that scientists have adopted the same understanding. Whether ethics education actually affects the practices of scientists is a separate issue—one that Smith-Doerr and Vardi (2015) and Watts et al. (2015) have explored, with mixed results. The point here is that the report writers may also have been constrained by the notion that "ethics" mainly concerns issues internal to the scientific community.

In line with these traditional framings of "ethics," I found that the reports on risk tended to push ethical issues, including issues related to communication, upstream to the time of study design and implementation (see Appendix B: Getting the "right" science). Key recommendations included having funders require that grant proposals address ethical issues related to communication and having research-performing organizations institutionalize procedures that encourage researchers to think about the possible social consequences of their work. In some ways, these recommendations are similar to the NSF's broader impacts criterion for evaluating research proposals. But the reports on risk offered advice that is more focused on public communication, suggesting that researchers explicitly state in their grant proposals their public communication goals and how they will evaluate whether they obtained those goals. The reports also recommended that funding agencies specifically earmark some funds for the task of public communication. Of course, the presence of such recommendations suggests that norms for public communication have not yet been institutionalized.

But even if these recommendations are implemented, scientists are still left with the problem of what to do when they unearth findings that are surprising or promise to be politically toxic, like Marc Edwards' observations regarding lead in people's drinking water. This suggests the need for a typology of ethical issues related to the public communication of science. One category involves ethical issues that arise when performing research on human subjects. For example, *Human Biomonitoring for Environmental Chemicals* recommended that plans be in place for informing research subjects when dangerously high levels of pollutants are found in their bodies. Also relevant for this category of communicating are ethical issues regarding "incidental findings" (Pickersgill 2012), such as detecting tumors in human subjects while conducting brains scans for some unrelated topic. While Pickersgill noted that researchers experience "normative uncertainty" about how to handle such issues, the recommendations in HBEC seek to reduce such uncertainty.

Another category of ethical issues related to the public communication of science, the one of most interest to me in this dissertation, relates to information that might affect people not directly involved in any particular research study. For issues that arise in this vein, the advice that the reports on risk offer is mainly about bureaucratized hazards. For example, most of these reports were sponsored by the EPA, which has clear legal jurisdiction over airborne and waterborne contaminants. Yet, for hazards that fall under the government's jurisdiction, public communication might entail pointing out that, as Marc Edwards puts it, "someone in a government agency is not doing their job." Compared to ethical issues regarding human subjects, these sorts of situations may be more difficult for reports sponsored by government agencies to address.

It's also understandably difficult for government agencies to discuss hazards that do not fall neatly into any particular scientist's or science organization's jurisdiction, particularly in contexts characterized by controversy. The two reports on risk that received private funding explicitly acknowledged the difficulty of communicating controversial information. For example, they noted that when scientists enter the public sphere (as opposed to remaining within academic circles), members of the public and other scientists may view them as having crossed a line. Under such circumstances, risk communication may pose a moral dilemma for which there is no easy resolution.

A related point is that, compared to other professionals, scientists have one less word in their "repertoire of moral concepts" (Abend 2016:38) regarding this second category of public communication—namely, "whistleblowing." According to Alford's (2002:18) definition, whistleblowers are those who, "in the name of the public good," expose some wrongdoing in their organization. Yet a constant across the reports on the nature of science was the NAS's use of "whistleblowing" to refer to something quite different—raising the alarm about things like data fabrication. To the extent that any particular scientist might use "whistleblowing" in the context of revealing some injustice, he or she would have to grapple with the term's ambiguity.

To summarize this chapter, I found that the moral background of the public communication of science is uneven. "Ethical" and "moral" issues mainly arose in the context of study design and study implementation, not the communication of findings. Moreover, the advice that the NAS reports offered regarding communication was mainly addressed toward government agencies. The reports emphasized that in order to produce findings that are policy-relevant, government agencies should take the public's needs into account when defining research questions and plan for how findings will be communicated to the public. The reports did not discuss any professional standards that are in place for how scientists more generally should communicate with the public or suggest any standards that should be in place.

In the next chapter, I explore how academic scientists who study environmental hazards communicate risk in the absence of any such standards. For many of them, engaging the public posed a moral dilemma.

## Appendix A: Moral issues in science cordoned to "research ethics"

The reports on the nature of science addressed moral issues mainly in the context of "research ethics" or "the responsible conduct of research." In line with Pimple's (2002:203) observations regarding the broader scientific community, I found no meaningful difference in the way that the NAS used these terms. According to RS, the goal of training in research ethics is to avoid instances of "scientific misconduct," which it defines as follows:

> Misconduct in science is defined as fabrication, falsification, or plagiarism, in proposing, performing, or reporting research. Misconduct in science does not include errors of judgment; errors in the recording, selection, or analysis of data; differences in opinions involving the interpretation of data; or misconduct unrelated to the research process. (p. 27)

Such concerns fall under the umbrella of what Schienke et al. (2009) have called "procedural ethics," which relates to the inner workings of science. Scientists who falsify results waste the time and resources of other scientists attempting to build on or replicate those results.

Yet procedural ethics are also connected to the broader society through impression management in the public sphere. Indeed, each of the guidebooks laments that when the public learns of instances of misconduct, this may cast doubt on the procedural ethics of scientists more generally, ultimately reducing "public support" for science (RS, p. 32; see also OBAS 3rd ed., pp. ix, 12, 18, 19). As the preface to RS makes plain, this phrase is not so much about citizens' warm feelings toward science as about "the continuing support of science with public funds" (p. iv; see also pp. 43, 80). Thus, not only does scientific misconduct threaten the methodology of scientific research, but also its public image and concomitant funding streams. Scientists follow procedural ethics mainly to advance academic ends.

As exceptions, the guidebooks discuss two issues rooted in procedural ethics that do not strictly serve academic ends. The first is research on human and animal subjects. Though arguments that risks be minimized and benefits be maximized (see, e.g., OBAS 3rd ed., p. 24) surely relate to impression management, these arguments have no direct bearing on the advancement of knowledge. To clarify the distinction, fabricating data directly sets science back, requiring additional work by other scientists to establish an accurate consensus around some issue. But the treatment of research subjects has no such direct negative impact on science. For example, after discovering that US researchers had infected hundreds of Guatemalan prisoners, soldiers, and mental health patients with syphilis to test human reactions to "fresh infective material," Susan Reverby commented that the researchers "just really wanted their results; they fell in love with their data" (Minogue and Marshall 2010). In this case, the researchers conducted an ethically questionable study as a means to advance scientific knowledge about the progression of disease.

The other ethical issue that is not directly related to the advancement of science is the fear that scientific misconduct will produce output that is harmful. The perennial example is medical research: each document notes that people may be harmed if fraudulent results become the basis of a medical treatment (OBAS 1st ed., p. 9068; OBAS 2nd ed., p. 16; OBAS 3rd ed., p. 8; RS, p. 32).

To prevent misconduct, the NAS highlights some procedures that have already been institutionalized. Peer review and IRB (Institutional Review Board) approval help to mitigate, respectively, academic and public concerns associated with the research process. These institutionalized procedures encourage scientists to think through their research methods to ensure that they have adequate scientific rigor and take the wellbeing of human participants into account. If any procedures are not technically or morally sound, gate-keepers have the option to close the gate: journal editors may deny publication or IRBs may require scientists to adjust their research to lessen harm to human subjects.[24]

## Appendix B: Getting the "right" science

As described in the previous chapter, social issues may make contact with science at three points: study design, study implementation, and the communication of results. While the main body of this chapter focuses on moral issues regarding the communication of results, here I discuss the moral background of study design and implementation as well as data analysis, which helps determine which information is worthy of communication.

### The moral background of study design and implementation

The NAS emphasizes the importance of organizations in encouraging scientists to think about the social impact of their research during study design, offering advice to funding agencies as well as to research-performing institutions. The NAS also notes the presence of legal mandates in shaping which hazards government agencies analyze and the advice that the NAS itself is able to offer.

With respect to funding agencies, ERAT recommends that they "educate and sensitize program managers to ethical, legal, and societal issues" (ELSI), conduct a

[24] In addition to their role as the morality police, Hedgecoe (2016:496) discusses the ways in which IRBs are sometimes used to protect institutions moreso than research subjects, functioning as "mechanisms of reputation management."

preliminary screening of research proposals for ELSI, and require that researchers include in their proposals their own attempts to foresee any such concerns (p. 12).[25] To achieve these goals, ERAT stresses the importance of organizational structure. Leaders "set the tone" and can help to foster "an institutional culture that is sensitive to ELSI":

> High-level support from senior agency leadership is required if an agency is to seriously address ethical, legal, and societal issues associated with the research it funds. Such support must be visible and sustained over time: in its absence, little will happen. (pp. 8, 253)

ERAT recommends that leaders periodically make public statements about the importance of ELSI to ensure that program managers and the research community are aware that the funding agency takes ELSI seriously.

To ensure that funding agencies involve themselves beyond the level of discourse—and thus provide "more than lip service" about ELSI (p. 253)—ERAT recommends that they designate a point person to ensure that ELSI are taken into account. The assumption is that more thorough assessments of a project's ELSI would take place because this person would ultimately be accountable for any negative outcomes that arise from the project (pp. 8, 253–255). And because ELSI may become more apparent as research progresses, ERAT recommends that all projects be periodically reexamined for ELSI.

> Projects that seemed to raise substantial ethical, legal, and societal issues may turn out to raise none; projects that seemed to have no ethical or societal implications may turn out to have hugely important consequences. (p. 258)

[25] Acknowledging that program managers may not have sufficient expertise in every project to adequately identify ELSI, they also recommend that funding agencies cultivate "external expertise," though they do not offer insight into how these experts might be identified.

As a result of this reexamination, a research project may need to be stopped or modified.[26] ERAT thus identifies circumstances when moral issues should trump the advancement of knowledge, even after a project has already begun.

With respect to research-performing institutions, ERAT and HBEC note that universities already have in place an ethics consultation service—the IRB. These reports recommend that other types of research organizations establish an in-house IRB or equivalent (ERAT, pp. 235–236; HBEC, pp. 99–100). IRBs help to sensitize researchers to ethical concerns, particularly those related to research subjects. They also help ensure that potential study participants are informed of how they might benefit or be harmed as a result of their participation in the study. On this point, HBEC goes one step beyond what IRBs usually demand. Whereas an IRB might require, for instance, that potential participants in clinical trials be informed that the treatment under study may have negative side effects, HBEC prescribes that they be warned of the potential policy implications of a study—for example, by telling them that if people who are living near a polluting plant are found to be at risk, then policy options might include closing down the plant or relocating those people (HBEC, p. 100).[27] Ethics consultation services within

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[26] Whenever the ELSI of a given project seem unsatisfactory, ERAT notes that agencies have a number of options at their disposal and are not limited to a decision of *funding* vs. *not funding* the research. These include: "pursuing it more slowly, pursuing it in a modified form that mitigates the identified ethical or societal concerns, pursuing the original effort but also pursuing research to better understand the ethical or societal impacts, and so on" (pp. 9, 257).

[27] Such a policy would not result in direct physical or even reputational harms, which are typically within the purview of IRBs. Instead, the research and ensuing policies might produce potential threats to a person's livelihood. While a subject's participation in such a study might mean that they could ultimately have a lower risk of exposure to some hazard, this might come as a result of having to find a new place to work or live.

research-performing institutions might thus foster public science by encouraging researchers to think about the wide-ranging impact that their research might have on people.

Another way to ensure that research topics will have a positive social impact, the NAS notes, is to systematically ask people about their information needs. In the context of risk assessment, IRC prescribes "early and sustained interaction" with interested and affected parties, so as to "illuminate issues of particular salience" that should be researched (p. 154; see also SD, p. 235).[28] SD recommends that the EPA establish "a formal process for stakeholder involvement … with incentives to allow for balanced participation of stakeholders, including impacted communities and less advantaged stakeholders" (pp. 13, 270). UR notes that for some people to "participate meaningfully," they may need travel money or expert assistance in understanding a problem (pp. 79, 160–161; see also, SD, p. 235). Despite concerns that it may be time-consuming or costly to involve the public in the risk characterization process, a general theme across the reports is that such efforts may save time and money in the long term. UR notes that "it is often wiser to err on the side of too-broad rather than too-narrow participation," especially "at the stage of problem formulation, when it may be possible to determine whether a simple procedure for the rest of the process will meet the

[28] A relevant issue, however, is how to determine who the "interested and affected parties" are. Furthermore, how can research organizations ensure that all interested and affected have an equal opportunity to participate? UR (p. 90) enumerates several strategies for public participation, including self-selection, which takes place at public hearings where "anyone who wishes may participate." UR also mentions that organizations might themselves attempt to identify interested and affected parties and seek out representatives. It also mentions various sampling techniques.

needs of the parties" (pp. 4, 161; see also SD, p. 77). Public engagement helps to ensure that researchers "get the right science" the first time, preventing the need to redo a study that found an answer, but had asked the wrong question.[29] Public engagement also helps to determine whether people might find a certain research project inappropriate or unethical.

The NAS also notes the power of legal mandates in shaping the research that gets done. In many cases, the public—as represented by Congress—determines the particular hazards that government agencies will assess and, to some extent, the way in which those hazards will be examined. For example, the Clean Water Act and the Clean Air Act are laws that simultaneously empower and constrain the EPA. One of the reports, SJRA, was written with the explicit purpose of examining the EPA's implementation of the then-recently-amended Clean Air Act. Congress's decision to enumerate 189 chemicals in the Clean Air Act simultaneously entails drawing attention away from other potentially hazardous chemicals. Moreover, the Clean Air Act instructs the EPA to focus "primarily on outdoor stationary sources of hazardous air pollutants and does not consider indoor or mobile sources of those pollutants" (p. 268). A mandate affecting a wider array of agencies and hazards was Presidential Executive Order 12898. UR and SD note that by explicitly mandating "environmental justice," the Order not only acknowledges that government agencies may impact different publics in different ways,

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[29] Though some funding agencies may be unwilling to allocate funds toward identifying ELSI, ERAT notes that such efforts "may be cost-effective if they help policy makers to avoid expenses that might be incurred in the future when programmatic changes are harder and more costly to make" (p. 232).

but also mandates that they mitigate the exposure of minorities and low-income people to environmental hazards (SD, pp. 53–54; UR, pp. 40–41). Legal mandates thus shape both academic and public science by directing government resources toward analyzing certain hazards in certain ways, simultaneously directing resources away from other hazards or methods.

In a meta-commentary on legal mandates, the NAS discusses how its own advice is structured. For example, SJRA notes that advising the EPA about which chemicals it should evaluate or the regulatory decisions that it should make was outside the scope of its charge (pp. 18–20). Similarly, because the Clean Air Act is mainly concerned with the carcinogenic effects of chemical, the authors of SJRA state that "time constraints" reduced their ability to focus on non-carcinogenic effects (p. 18). Yet SJRA does toe the line between giving advice that *fulfils* the EPA's mandate and advice that *expands* it. Rather than suggesting that the EPA examine indoor and mobile sources of air pollutants, it instructs the EPA to inform Congress that such hazards "might well be higher than those related to stationary sources" (p. 268). Thus, even though the scope of its own prescriptions is bound by current mandates, the report notes that those mandates may not necessarily be the best ones for promoting public health.

### The moral ambivalence of data analysis

In contrast to the guidebooks on science, which emphasize more general norms for how scientists should behave (e.g., don't fabricate results), the reports on risk describe specific techniques that will allow scientists to gain a better understanding of hazards. The most sociologically interesting aspect of the NAS's discussion of research methods is not the particular techniques that it recommends, but its description of how organizations have institutionalized and should institutionalize those techniques.

HBEC offers an interesting reflection on how institutional norms impact risk assessment and, ultimately, public health (pp. 211–213). In particular, it discusses scientists' tendency to "err on the side of accepting false negatives," with the default position being to accept the null hypothesis of "no risk." The norm here, HBEC states, is to choose an α beyond which it is highly unlikely that a measureable risk is attributable to chance (e.g., $\alpha = 0.001$). Then, even when "significant" findings emerge, scientists typically do not "accept" the research hypothesis. HBEC notes that scientists tend to perceive "a greater risk of treating as true a proposition that might turn out to be false" because false positives are "more dangerous to science's advance and credibility" than false negatives.[30] To the extent that this is true in the context of risk analysis, scientists may greet evidence that a hazard exists with more suspicion than evidence to the contrary, and they may be prone to interpret ambiguous hazards as safe. This would mean that unless there is at least a high (e.g., 99.9%) probability that a hazard exists, scientists do not have grounds for issuing a warning. In contrast, some social scientists (Hoffman 2013; Marshall and Picou 2008) prescribe a "precautionary principle,"

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[30] In my experience as an introductory statistics instructor, I have not seen a social science textbook that explains how to calculate β (the chance of making a Type II error). Textbooks typically emphasize how "conservative" it is to choose a low α (the chance of making a Type I error). Because α and β are inversely proportional, this means that choosing a higher β is ostensibly "conservative" as well. But context is important in determining whether saying that there is "no danger" when there is danger is "more conservative" than saying that there is danger when there is none.

whereby risk analysts and policymakers assume that chemicals should be assumed dangerous (and thus regulated) unless proven otherwise (see also Freudenburg, Gramling, and Davidson 2008).

The reports describe two strategies to improve risk analysis: reducing uncertainty and standardizing how uncertainty is handled.[31] The main means of reducing uncertainty (thereby reducing the chances of making both a false positive and a false negative) is to conduct a more detailed analysis of hazards. However, the reports point out several reasons why this is not always possible. First, science has its limitations and can only do so much to reduce uncertainty. For example, in the context of biomonitoring, HBEC notes that scientists are limited by their ability to detect low levels of chemicals and to detect health outcomes from exposure to certain chemicals. The techniques used to measure a risk may thus yield false negatives, simply because scientists lack the tools to measure it. Another reason why more analysis may not reduce uncertainty is that there may not be enough time to obtain better information. The NAS states that researchers must "avoid interminable analysis" and eventually "reach closure" (SJRA, p. 252; UR, pp. 6, 119). And, as SD notes, "the goal of timeliness is as important as (sometimes more important than) the goal of a precise risk estimate" (p. 20). The public may demand closure because they want immediate answers to questions about hazards. This is particularly true in the context of emergency situations

[31] Hammond (1996:44–45) makes a similar suggestion for improving decision-making. However, he frames *reducing error* as a scientific exercise and *managing any remaining error* as a political exercise. Only the former, he writes, can actually reduce injustice; the latter merely redistributes injustice among various publics.

(IRC, p. 133). Or there may be deadlines, perhaps legal, for when decisions that rely on analysis must be made. Moreover, more analysis has its costs in terms of the continued allocation of money and personnel. In short, while more analysis can help to reduce uncertainty, there are both technical and political limitations to obtaining more reassuring answers.

In addition to reducing uncertainty, the reports point out that risk analysis can be improved by creating standards for how risks should be assessed. A problem, HBEC states, is that even though various measurements of a hazard may be accurate, it is difficult to interpret findings when measurements are unstandardized (p. 127). The reports also note the social outcomes of various epistemological assumptions. For example, SJRA (chapter 6) notes that for low doses of a substance, EPA explicitly assumes that risk is directly proportional to exposure—that, for instance, reducing exposure by a factor of 10 will also reduce risk by a factor of 10. One downside of this approach is that for substances whose actual dose–response curve is less steep than the standard, reducing exposure by a factor of 10 will not reduce risk by a factor of 10. EPA might thus deem a certain level of such a substance as "safe" even though it poses a threat to public health.

The reports recommend that scientists explicitly state (a) when they have made a default assumption, (b) their reasons for adopting the default, especially when other credible models are relevant, and (c) their reasons for adopting any model other than the default one. While defaults may not reduce the chance of making Type I and Type II errors—as SD notes, "any set of defaults will impose value judgments on balancing potential errors of overestimation and underestimation of risk" (p. 196)—defaults do increase the reliability of the risk characterization process. In the absence of the above steps, the reports note that EPA's risk characterization might be conducted in an ad hoc manner that would undercut the agency's credibility. Moreover, the use of defaults mitigates the need to open up debate about each element of the risk characterization process, saving the EPA time and money.

HBEC also raises issues related to research ethics. The report pushes back against what it calls "overzealous" IRBs that require that researchers to completely anonymize data. The practice of rendering data untraceable back to particular individuals, the report states, is questionable, especially "if they concern a treatable or preventable health impairment" (p. 100). The unstated implication is that a researcher would then be able to communicate that information to the individuals, thereby improving public health. This issue, however, is related more to risk communication than to risk assessment, and HBEC says rather little about risk communication.

# Chapter 3

# Risky Business: Narratives of Public Science in the Academy

*As I see the world deteriorate through a scientist's eyes, I want to do something about it.*
—Henry, bird ecologist

*So I lost my job at LSU, but it didn't stop the truth from getting out.*
—Ivor van Heerden, hurricane scientist

*Crisis: a vitally important or decisive stage in the progress of anything; a turning-point; also, a state of affairs in which a decisive change for better or worse is imminent; now applied esp. to times of difficulty, insecurity, and suspense in politics or commerce.*
—Oxford English Dictionary

## Introduction

Hurricane Katrina struck the Gulf coast on August 29, 2005, flooding 80% of New Orleans, inflicting over $100 billion in damage, and claiming nearly 2,000 lives. In the context of this dissertation, the Katrina case is interesting because technical reports and peer-reviewed literature had discussed critical flaws in the design and construction of floodwalls, yet these findings remained academic. As Newberry (2009:545) puts it, "there was a breakdown in translating crucial information from testing and research into practice." In this chapter, I use in-depth interviews with environmental scientists to examine norms of risk communication in the aftermath of the Katrina disaster. Though the chapter's empirical backdrop centers on the Katrina case, the scientific, moral, and political dynamics in southeast Louisiana apply to risk communication more generally.

Consider the plight of hurricane scientist Ivor van Heerden, a non-tenured faculty member at Louisiana State University (LSU). Shortly after the Katrina disaster, van Heerden told the media that, based on his investigations, several floodwalls had collapsed due to poor design and a "catastrophic structural failure." But the US Army Corps of Engineers (USACE), which had constructed the floodwalls, was maintaining that storm surge had "overtopped" them. Apparently fearing a threat to revenue from the USACE, a Vice Provost at LSU organized a meeting of several administrators. "One of the issues will be Ivor," he wrote in an email. "We must get him on the team and have him change his story" (American Association of University Professors [AAUP] 2011:25). Though the administration made it painfully clear that it was displeased with van Heerden's findings, he refused to change his story and was ultimately dismissed from the university.[32]

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[32] The *New York Times* described the intervening four years as LSU's "slow motion" firing of van Heerden (Schwartz 2009). The AAUP noted that the USACE, for its part, also made life difficult for van Heerden:

> Off campus, a concerted media campaign arose defending the Corps of Engineers and attacking its critics, notably Professor van Heerden, in the New Orleans press. The online affiliate of the *Times-Picayune*, NOLA.com, was hit with thousands of such posts purporting to be from ordinary citizens of New Orleans (more than seven hundred were traced in a single six-week period in late 2008 and early 2009) that were in fact sent from government computers inside the Corps offices in New Orleans. The campaign continued even after summer 2009, when an internal review, prompted by the affidavit of a former NOLA.com employee, led the Corps commander to announce that the problem had been addressed. (p. 31)

While the AAUP found no evidence that these activities were jointly coordinated with LSU, it noted that the university was at this time "aggressively pursuing its partnership with the Corps" (p. 31).

The van Heerden saga raises issues that the field of science communication has only begun to address. The literature on outreach and activism has tended to focus on government scientists or elite academic scientists. Government scientists, wrote Frickel (2018:236), are "softer targets" than academic scientists, being "more vulnerable to dismissals, gag orders, and censorship." Indeed, politicians have the power to pull the plug on government research if its implications come in conflict with their policy agenda. For example, Schoonover (2019) described how the White House blocked his work on climate change and national security. And Clement (2017) detailed the Trump administration's retaliation against government scientists working on climate change.

In contrast, the "1940 Statement of Principles on Academic Freedom and Tenure" declares that that "when [academics] speak or write as citizens, they should be free from institutional censorship or discipline" (AAUP 2014:14).[33] Of course, the existence of such policy statements does not mean that academic freedom is guaranteed. On the contrary, the AAUP's statement exists precisely because academic freedom is an ideal that does not always exist in practice. With respect to Professor van Heerden, the AAUP (2011:39) ruled that LSU had violated his academic freedom, having dismissed him "in retaliation for his continuing dissent from the prevailing LSU position on post-Katrina flooding."

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[33] The AAUP statement contains the caveat that when academics communicate with the public, "they should at all times be accurate … and should make every effort to indicate that they are not speaking for the institution."

In this chapter, I argue that the literature in science communication has overlooked low-status academic scientists who are confronted with a crisis. By "crisis," I mean both of the connotations in the Oxford English Dictionary: a troubling state of affairs and a decision-point. Academic scientists who uncover information about impending disasters may find it difficult to communicate risk. Indeed, all academics who communicate with the public (e.g., about risk) or engage in policy advocacy (e.g., about risk management strategies) have to contend with the narrative that these activities are somehow un-academic. They may also have to contend with powerful actors—such as university administrators, government officials, or industry executives—who seek to restrict the flow of politically inconvenient information. But because non-tenured academics lack job security, they are more vulnerable to threats than their tenured colleagues. As such, low-status academic scientists may be more likely to experience moral dilemmas like the following: *Choice A:* keep the public in the dark about their vulnerability to hazards, thereby avoiding any reputational or career-related risks or *Choice B:* communicate risk to the public, thereby immolating themselves. In short, for low-status academics, communication about risk may be risky.

In this chapter, I make several contributions to the institutional logics perspective (ILP) and the sociology of science communication. First, I elaborate the logic of public science, in which scientists' discourse is explicitly moral. In so doing, I extend the ILP beyond the morally sterile notion of "value" orientations, and I question the sociological literature's tendency to portray scientists as self-interested. Second, I show that academic scientists sometimes encounter moral dilemmas at the intersection of public science and other logics. Third, I show that scientists engage in a spectrum of public engagement activities, with more direct forms of engagement posing more personal risks.

### The moral foreground of science

In contrast to the previous chapter, in which I examined the "moral background" that structures scientists' discourse about public science, this chapter probes the "moral foreground"—that is, the discourse itself. "Public science," which involves cognition about the social applications of scientific research, offers a framework and an empirical case for analyzing such discourse.

While there is ample sociological literature that has explored moral issues that arise when scientists choose a research topic (Corley et al. 2016; Frickel et al. 2010; Kempner et al. 2011, 2005; Ladd et al. 2009; Moore 2008; Nicholas 1999; Small 2011), considerably fewer studies have addressed moral issues in the context of science communication. This is likely because the sociological research on communication is largely limited to scientists who occupy powerful positions in the academic hierarchy, for whom communicating with the public does not pose much of a risk. For example, Kinchy and Kleinman (2003:872) sampled "prominent" ecologists who were organizational "insiders," and Frickel (2004:68, 81) reported that, among the genetic toxicologists he spoke with, all of the academics had tenure. And, while Burchell et al. (2009:20) did not comment on the status of the scientists in their sample, it was restricted to scientists who already had experience with public engagement. These studies are not generalizable to lower-status scientists, who may be less likely to engage the public precisely because of their subaltern position.

One reason for the tendency to focus on elites may be that there are remarkably few non-elites who interact with the public. Indeed, a recent review of the literature on science communication has shown that "public engagement activities" (as measured by several variables including media contacts) are positively associated with a scientist's status (Peters 2013:4). Scholars have typically explained this trend in terms of rewards and cumulative advantage, arguing that, on the one hand, senior scientists have the "privilege" of representing their research groups to the media and that, on the other hand, the media and PR departments tend to seek out the most successful scientists (Kreimer, Levin, and Jensen 2011:45; Peters 2013:3–4).[34] Peters discounts the relevance of scientists' "moral duty" to engage the public because it is a weaker predictor compared to scientists' status (see also Besley, Oh, and Nisbet 2013).

There are two problems with this interpretation that are relevant to this chapter. First, discounting the relevance of a scientist's "moral duty" to engage the public ignores the possibility that lower-status scientists may be more likely to experience moral dilemmas at the science–society interface than higher-status scientists. That is, lower-status scientists who experience a moral pull to communicate their expertise to the

[34] The ability of high-status scientists to successfully engage the public no doubt widens the credibility gap between them and low-status scientists, perpetuating the academic hierarchy through cumulative advantage (Merton 1968, 1988, 1995).

public may remain silent precisely because they feel more vulnerable to reprisals than higher-status scientists. A second limitation is that, in operationalizing scientists' "moral duty" to engage the public, most studies have not tapped into "morality" per se. For example, Tsfati et al. (2011:144) wrote that "scientists receiving public resources for their research have a duty to communicate their research to the public" (see also Kreimer et al. 2011:42; Peters 2013:4, who discussed morality in the same way).[35] An assumption here is that scientists who do *not* receive public resources do *not* have a duty to communicate their research to the public. As opposed to a "moral duty" to engage the public, it would thus be more accurate to say that these studies have examined tit-for-tat business relationships.

To more deeply probe the moral foreground of science, I look to communication about risk. Some scientists may have technical expertise that, if communicated to the right audiences (and in the right way), could reduce people's vulnerability to hazards. To the extent that scientists realize this, they possess a "moral imagination," which Johnson (1994:202) defines as "an ability to imaginatively discern various possibilities for acting within a given situation and to envision the potential help and harm that are likely to result from a given action" (see also Werhane 2015).

Tilly (2005 ch. 4) provides a useful framework for translating the moral imagination into action: *egoism* is when others are harmed for one's own benefit, and *altruism* is when there is harm to self for the sake of others. Table 4 depicts these

[35] Donner (2014) calls this reciprocity the basis for a "Hippocratic Oath for scientists."

scenarios along with *cooperative egoism* and *destruction*. Those in science studies have tended to assume that scientists are more interested in the advancement of science and/or their career than in the betterment of the public. For example, Bauer, Allum, and Miller (2007:88) noted that scientists may engage the public "to sustain public goodwill that may translate into higher citation counts and into funding for future research" (see also Bucchi 2008). This framing suggests that scientists' efforts to communicate with the public are, in essence, strategic maneuvers; scientists will tend to exhibit egoism or, at best, cooperative egoism. It's unlikely that any scientist would act against their own self-interests.

Table 4. Moral categories of action, by ensuing harm to self and/or other

| | **benefit to other** | **harm to other** |
|---|---|---|
| **benefit to self** | cooperative egoism | egoism |
| **harm to self** | altruism | destruction |

Source: table adapted from Tilly (2005 ch. 4)

But consider a scientist who feels a moral responsibility to communicate risk to the public (thereby benefiting others) yet suspects that enacting that responsibility would entail career risk (harm to self). The scientist has clearly imagined the moral situation. The question now is what to do about it. Like Abend (2016:22), I acknowledge the difficulty of revealing the "true" motivations for why people do the things they do and that discourse about morality may in reality be a "ploy." But I apply the same skepticism to the narrative within science studies that scientists will engage the public *only* to the extent that it advances their self-interests.

### Institutional contradictions and moral dilemmas

In the previous section, I argued that the moral foreground of present-day science communication exists, but has not been adequately explored. Here I add that the concept of public science provides a sociological frame for analyzing discourse about morality in relation to other logics.

The idea that institutional contradictions may produce moral dilemmas is a novel contribution to the ILP literature. Although some of the ILP literature has discussed logics that are morally relevant, such as the logic of "social responsibility" within corporations (Christiansen and Lounsbury 2013; Höllerer et al. 2013), these studies have framed morality as a means to some other, typically financial, end. For example, Christiansen and Lounsbury described how a beer company's vision of "responsible drinking" was actually situated within a "market logic." That is, the company's efforts to promote responsible drinking were a "renewed branding effort" that ultimately "prioritized profits over social welfare" (p. 217). Obviously, organizations may engage in moral discourse strategically when it helps their bottom line, and this discourse lies squarely in the moral foreground. But, to my knowledge, none of the ILP literature has examined morality as an end it itself. By introducing the logic of public science, I make room for dilemmas that are less about strategy, reputation, and self-interest and more about moral imagination.

The ILP literature on science, in line with the broader ILP literature, has assumed that scientists' overarching goal at the intersection of logics is pragmatic self-interest—

how to maximize profits and minimize personal costs. Indeed, in the context of science, the most commonly investigated institutional contradiction is between "academic" science and "commercial" (or "industrial") science (Cooper 2009; Folker et al. 2009; Frickel and Moore 2006:15–16; Kleinman et al. 2018; Kleinman and Vallas 2001; Lam 2010; Murray 2010; Vallas and Kleinman 2008).[36] As exemplary, many of the entrepreneurial academics that Lam (2010) interviewed were frank about the purpose of forging industry ties. "Money," one biologist remarked. "Money, money, money. It is just money" (p. 329). But entrepreneurial academics also worried that their industry ties would prompt other scientists to question their objectivity. As a result, those in the process of working their way up the career ladder tended to view the entrepreneurial path as "risky" and lived in "constant fear of being de-coupled from the core academic system." Even apparently successful entrepreneurial academics expressed anxiety about maintaining their credibility and productivity in both spheres (Lam 2010:331).

In this chapter, I argue that academic scientists who do public science may face similar risks. What's novel about the logic of public science, as opposed to other logics described to date, is that it has moral imagination at its fore. In the context of risk communication, the difference is that people (here academic scientists) may not simply seek to advance their *own* interests but also those of others. Stemming from their moral imagination about how others might be harmed by some hazard, scientists may feel a moral responsibility to do what they can to mitigate that hazard. But, through a

[36] There is also much non-ILP literature that examines the intersection of academic and commercial science (e.g., Feldman and Desrochers 2004; Greenberg 2008).

narrative that public engagement is inherently unscientific, other scientists or the general public may stigmatize risk communication (Nelson and Vucetich 2009).

According to the logic of academic science, scientists accrue status and credibility by publishing their research findings in peer-reviewed journals. The notion of a "linear model" of science has been used both descriptively, to explain the relationship between science and society, and prescriptively, to enforce norms of public engagement (Pielke, Jr. 2007:80). In the linear model, the expert knowledge found in journal articles may ultimately reach the public, but Figure 6 illustrates the precarious series of events needed for this to happen: a university may run a press release on the paper, a reporter may write an article about the paper, an editor may choose to print the story, and the story may get picked up by other media outlets, granting it a wide public audience (see also Kastenhofer 2010:320–21). Note that in this process, the agency of individual scientists is almost non-existent. Their only act is to publish journal articles.

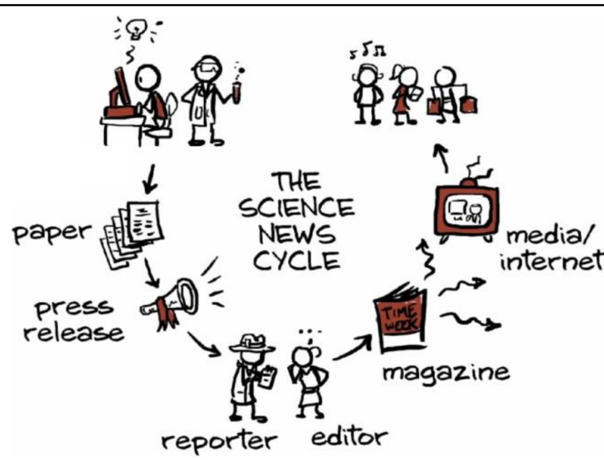


Figure 6. The science news cycle
*Source: Cham (2012)*

Scientists who engage the public more directly than what's prescribed by the linear model may be accused of violating academic norms, particularly when they offer insight on politically sensitive issues. Generally speaking, public engagement exists along a continuum, with more direct forms of engagement more closely resembling "advocacy" (Donner 2014). Crawford et al. (2016) have shown that, at the lower end, scientists include under the umbrella of advocacy things like presenting findings at public events and publishing their findings in non-academic outlets; at the higher end are things like writing letters to Congress about environmental policies. Crawford et al. also reported that the acceptability of outreach varies according to its public reach; for example, it may be more acceptable to discuss policy recommendations in peer-reviewed publications than in media geared toward the public (e.g., editorials). In the context of risk communication, the link to advocacy may be particularly strong. Even if a scientist does not advocate a specific risk-management strategy, telling people that they are at risk is nonetheless an implicit form of advocacy because it implies that something ought to be done to mitigate that risk (Thompson 2012).

Kotcher et al. (2017) have argued that scientists can engage in many forms of public engagement without harming their credibility among the general public, but they neglect two key points. First, for scientists who are deciding whether "going public" might be appropriate, what the general public thinks may be less relevant than the opinions of other scientists or university administrators. Johnson et al. (2014:94) have argued that scientists who conduct research with more direct social applications are more likely to fear that their colleagues will stigmatize their public engagement as "advocacy," which is antithetical to science. "If you're seen as an advocate rather than a scientist," one biologist remarked, "you lose a lot of scientific credibility." The difficulty for academic scientists is that, precisely when their research has broader implications, colleagues or administrators may be more likely to view communicating those implications to the public as advocacy.

Second, in scientists' interactions with the public, "credibility" may not be foremost in their minds. Particularly for highly politicized issues, other scientists or members of the public may personally attack scientists with whom they disagree. Within the academy, conflict may be baldly capitalistic, with scientists being censured for saying something that threatens the funding streams of others scientists or their university more generally. Or conflict may be more subtle, with administrators invoking selectively—when it suits their political needs—the narrative that non-linear modes of communication are "unscientific." Within the public sphere, the classic example is that of scientists who are convinced that climate change is dangerous and those on the fringe who assert that it's not (Conway and Oreskes 2011; Schneider 2009). Marc Morano, for example, routinely lambasts climate researchers on his blog, Climate Depot, posting their email addresses prominently. "I am not worried one bit about someone getting nasty emails, as it is part of the process," he told *Scientific American* (Ogburn 2014). In response to threatening emails, some climate scientists have felt the need to file police reports.

In such contexts, tension at the science–society interface is not only rational, but moral. Scientists can choose egoism, protecting themselves by keeping silent, but they can also choose altruism, endangering themselves by communicating risk. In this chapter, I use such moral dilemmas to elaborate the moral foreground of science communication. My focus is on how scientists navigate the potentially contradictory institutional logics of academic science and public science. In the analysis, the power of organizations looms large. Different organizational contexts (i.e., different universities) help to shape whether scientists perceive a moral obligation to engage the public, as well as whether that moral obligation presents itself as a moral dilemma.

## Methods

To tap into moral dilemmas at the intersection of academic science and public science, I discuss the results of interviews with academic scientists who study environmental hazards. The interviews were conducted by a team of four sociologists: Lee Clarke (co-PI), Allison McKim, Harvey Molotch (co-PI), and myself. Scientists, we hypothesized, might be part of a "disaster warning system." Yet we also presumed that for various reasons, they might be reluctant to sound the alarm.

Each interview was loosely structured by an interview schedule, which is provided below as an appendix. In short, we asked experts to discuss any public engagement activities they had undertaken before, during, and/or after Katrina, as well as their general thoughts regarding risk communication and policy advocacy. We skipped questions that seemed irrelevant to particular scientists, and we allowed new themes to emerge *in situ* by allowing conversations to flow beyond the scope of our initial questions.

Our initial focus was on people who had expertise that was in some way related to the Katrina disaster. With the help of Shirley Laska, a sociologist at the University of New Orleans, we developed a list of experts, scientists and non-scientists alike, on various topics related to the disaster. Of particular interest were those who had warned, in the years leading up to Katrina, that a catastrophe was imminent. For example, we interviewed Ivor van Heerden, whose responsibilities as the deputy director of the LSU Hurricane Center entailed producing technical models of storm surge and developing disaster response plans to assist vulnerable populations. van Heerden had issued several warnings in the years leading up to Katrina, such as his statements in an October 2004 interview with *Nova* (a PBS series on science), that "as the wetlands fall apart, the potential of … hurricanes to do major destruction through storm surges rises and rises and rises" and that "there is the potential for extremely high casualties—people not only killed by flying debris, drowning in the soup, but also just imagine, how do we rescue the survivors?"[37] After the storm, van Heerden also helped to identify the role of the USACE in having constructed a sub-par hurricane protection system. We spoke to another scientist who felt that some floodwalls collapsed because his warnings about

[37] van Heerden's moral imagination was particularly graphic. "The water stays," he continued. "And it stays for months and months and months. How do you rescue all of these people? If there's 200,000 survivors, you get 20,000 out a day, that's 10 days. So how are they going to hang on? You know, this is one of the big nightmares: how do you rescue those survivors? What are they going to need?" (Lewis 2005)

termite infestations had not been taken seriously. "Presently," Gregg Henderson wrote in *American Entomologist* (2008:161), "little attention is being paid to my call for alarm, and I believe that the termites pose a continuing danger that requires immediate attention." We also interviewed Laska herself, who had published an article in the *Natural Hazards Observer*, where she warned that the New Orleans flood protection system was "unable to keep up with the increasing flood threats from a rapidly eroding coastline and thus unable to protect the ever-subsiding landscape" (Laska 2004).

In addition to this core sample of Katrina experts, we searched faculty profiles and news stories, interviewing experts whose work was related to environmental hazards more generally. By thus extending our sample, we were able to interview several recently-minted PhDs on their thoughts regarding risk communication and advocacy. While some of these respondents had expertise that was in some way related to the Katrina disaster, some did not. But because all of the experts we spoke with lived and worked in southeast Louisiana, the Katrina disaster provided a readymade backdrop, allowing us to ask them about how their work might be related to a "Katrina-like event."

Beginning with our initial round of interviews in April of 2008, we used snowball sampling to identify additional respondents. All interviews were completed by June of 2009.

In this chapter, I restrict my analysis to the "academic scientists" we interviewed, defined as those who (a) had earned a PhD in science or engineering and (b) were employed by universities. This left a sub-sample of 34 scientists, all of whom were located in Louisiana, except for one who had recently moved to New Jersey. I included one respondent who was technically a government employee because he was in a co-op position—with university office space, a teaching load, and graduate student mentees. I also included three respondents who had recently left the academy. One had been pressured into leaving LSU. Another had retired several years before we interviewed him, but maintained close ties with LSU and ended up rejoining the university after we spoke with him. And we interviewed Ivor van Heerden shortly after his contract was not renewed. (We also interviewed him the previous year, when he was still employed by LSU.)

At the time of their interview, the average amount of time that respondents had held their PhD was 23 years (median: 22 years), ranging from 2 to 41 years.[38] The "academic age" (i.e., years since PhD) distribution in Figure 7 shows that most respondents were in the middle to late stages of their career and were of "high status." Academic age, however, was not always aligned with status. For example, when we first spoke with van Heerden, it had been 25 years since he had earned his PhD, yet he did not have tenure.

These interviews, which averaged about 70 minutes in length, were transcribed, imported into Atlas.ti, and coded for various themes. For example, when scientists discussed the media, we coded the corresponding block of text as "media." Or when

[38] Degree information and tenure status was gleaned from the internet or obtained directly in interviews.

they told us about any resistance they encountered in disseminating their expertise, we coded this as "inhibitions." Thus, querying "media AND inhibitions" yielded opinions about why reaching out to the media was inappropriate, while querying "media NOT inhibitions" yielded opinions about why reaching out to the media was appropriate.

Except for van Heerden and Henderson, whose identities would be obvious anyway, I use pseudonyms for all respondents. Moreover, because the sample was overwhelmingly male (30 / 34 = 88% of respondents) and because one of the women requested anonymity, these pseudonyms do not necessarily correspond to the respondent's gender.

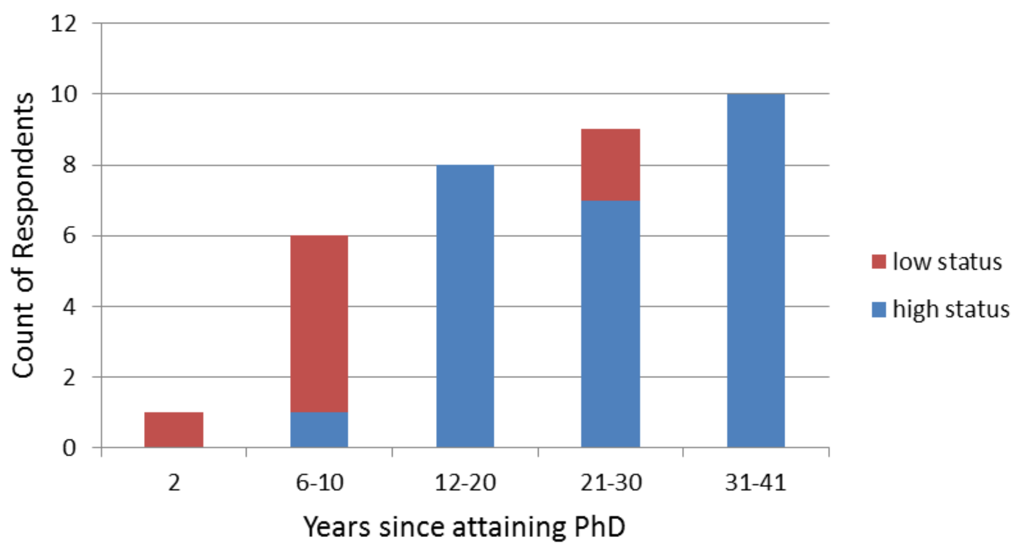


Figure 7. Academic age and status of respondents
*Notes:*
*-"Academic age" was calculated as year of interview minus PhD year.*
*-"Status" is, with some possible exceptions, a measure of whether a respondent had tenure. The possible exceptions include two scientists in the top academic age category, one who was retired and the other who was a university Vice President. While it was unclear in both cases whether the scientist had tenure, the analysis below does not include any quotes from either scientist.*

## Narratives of public science in the academy

I found that scientists’ ability and willingness to conduct policy-relevant research was a key structural underpinning to the public communication of science. That is, in the absence of socially-relevant science, scientists had nothing socially-relevant to say. I also found that scientists were generally ambivalent about public communication, viewing it as unscientific and as a moral obligation. Communication about risk was particularly prone to moral dilemmas.

### Academic constraints to policy-relevant research

Respondents reported that important environmental research had not been conducted and that this lacuna was not the result of happenstance. The logic of academic science often impeded research that was policy-relevant. Scientists described these constraints as being both cognitive and non-cognitive—or, using the language of the ILP, as both symbolic and material.

In terms of cognition, several respondents spoke of the dissonance between pure and applied research. For example, Anthony, a zoologist who earned his PhD 18 years ago, described the stigma against environmental research. “There is this perception,” he said, “that when you do conservation work, it’s not as rigorous—it’s *poorer quality* in general. I would say that’s a common perception.” And Sarah, an ecologist who earned her PhD nine years ago, said that the biology students in her courses typically asserted that “anything that’s applied is garbage—it’s not real science.”

Additionally, scientists lamented that the underlying structure of academia channeled them toward research topics that were not policy-relevant. That is, it was not just that scientists chose to pursue pure research topics, but that the academy did not offer them the choice. Jim, a storm surge modeler who earned his PhD 22 years ago, summed up the situation as follows:

> The nature of environmental work is that it is interdisciplinary. Virtually nothing can be accomplished in the very narrow confines of the discipline. Yet, of course, the academy is set up in that way… It's designed to keep you from wandering too far afield from your disciplines.

Carla, an endocrinologist who earned her PhD 38 years ago, added that part of the problem is that funding agencies, mirroring disciplines, are often broken up into divisions that do not fund interdisciplinary research. "What we're looking at," she said, "is a real-life situation where we have no divisions. There's not a soil and air and water issue. Or it's not biology, engineering, physics, chemistry, social sciences, and behavioral sciences. It's all of that." In short, the academy and its typical funding streams often made it difficult for respondents to gain knowledge about hazards that did not fall into the jurisdiction of a single discipline.

But some respondents spoke about institutional support for applied research. In fact, Carla, in her role as director of a center that fostered environmental research, was a key facilitator of public science. "My premise as a center director," she said, "is that all of our resources are going to be dealing with issues that relate to what New Orleans needs to do to prevent another disaster." "I really don't know what to call it," she conceded, grasping for a term that would adequately summarize her center's emphasis on public safety. "It's like 'responsible research.'" Carla did two things to support her vision of "responsible research," one symbolic and the other material.

Symbolically, Carla sought to overcome the stigma associated with applied research. "The provosts and everybody, they just kept scratching their head. They couldn't figure out what it was we were doing. 'Was it *really* academic? Because it's so community-oriented.'" To counter these notions, Carla routinely distributed copies of *Pasteur's Quadrant* by Donald Stokes (1997). The book uses a 2x2 table to categorize the goals of scientific research—with the "quest for fundamental understanding" (i.e., academic science) along one axis and "considerations of use" (i.e., public science) along the other axis. The research of Louis Pasteur lies squarely in the yes–yes quadrant, i.e., "Pasteur's quadrant." Because the book was written by a former Princeton dean, Carla said that it "wrapped what we were doing instinctively in academic prestige." After she gave the book to the president of Tulane, he instituted a public service requirement for every undergraduate. "He has actually *forcefully* encouraged all faculty to work on the restoration of the city," she commented. Carla thus helped to institutionalize and destigmatize public science at Tulane.

Carla's center also materially supported responsible research. She recounted how she had enticed two young scientists to take on research that might in some way promote the sustainability of New Orleans, offering them various resources, including $15,000 grants and graduate assistants. Carla noted that both projects resulted in high-impact publications. "It helps their career," she said. "It helps their tenure decision."

Because environmental problems are interdisciplinary in nature, Carla also sought to assemble interdisciplinary teams. "No matter what anybody says about 'interdisciplinary,'" she cautioned, "you get penalized for interdisciplinary work when it comes time for a tenure decision, … so you have to have a very, very robust disciplinary portfolio." She explained that the "trick" to successful interdisciplinary teams is ensuring that each team member produces his or her own disciplinary output (i.e., publications).

### Public communication as unscientific

All of the scientists we spoke with knew the importance of communicating their work to their peers, but they differed on the topic of communicating it to the public. Some scientists viewed public engagement as a natural and moral extension of academic science, whereas others rejected it as unscientific, whether inherently or because it resembled policy advocacy.

Henry, a bird ecologist who earned his PhD 28 years ago, exemplified a moral obligation, rooted in his scientific expertise, to positively impact society. He described this in terms of two "threads" that run through his life:

> One is I'm trained as a scientist, and one of my desires it to try and understand how the world works…. The other is that, as I see the world deteriorate through a scientist's eyes, I want to do something about it … to try and have an impact on how people see the world.

Other scientists expressed similar sentiments. Marcy, a toxicologist who earned her PhD 17 years ago, expressed "a *responsibility* for us [academics] to get as good of an answer as we can from our science" as well as a need "to get that message out" to the public. "I think it's always incumbent on us," she said, "to look for areas where our science informs on the policy and find ways of helping." Anthony stressed the importance of engaging in public discourse about environmental conservation. "We cannot as scientists be disengaged from public discourse," he said. "When you see this threat [to the environment] that is eminent and real, how can you not engage in it?"[39] And Elaine, a geologist who earned her PhD 28 years ago, said that, "in terms of vulnerabilities to natural disasters and things like that, I think there is a moral obligation of people to bring forth that information [about vulnerabilities]." In each case, the scientist invoked a narrative whereby having knowledge about a hazard compels some action to communicate that information to the public. Notably, each of these scientists had tenure.

But most scientists, regardless of their status in the academic hierarchy, were also aware of the narrative that public communication was inherently unscientific. For example, several respondents characterized seeking out the media as egoistic and therefore unbecoming of an academic scientist. "Like, you're talking about scientists," said Debapriya, an ecologist who earned her PhD eight years ago. "I don't feel the need as a scientist to go look for exposure." Similarly, Amit, an aquatic ecologist who earned his PhD 19 years ago, described scientists who reach out to the media as "limelight-grabbing." And Peter, an expert on hydrodynamics who earned his PhD 19 years ago, warned that "those in higher education that seek this being in the limelight, it's going to

[39] For example, while conducting research on a national reserve in Mexico, Anthony worried that land developers were beginning to threaten the animals he was studying. As a result, he gave a public talk about what was going on. "I sort of felt morally *compelled*," he said, "to provide some insight, … not only to the townspeople, but also to the government workers who run the reserve."

catch up with you." Given such associations between public engagement and egoism, many scientists were understandably reluctant to communicate their expertise beyond academic circles.

Similarly, most respondents handled advocacy delicately, fearing that it would threaten their image as objective scientists who were aloof from social affairs. John, an ecologist who earned his PhD six years before we interviewed him, told us that advocacy "spoils" science:

> There's a debate within the scientific community about the role of advocacy and how scientists should or should not be advocates. Science, as a discipline, essentially should be a neutral process where you're talking about hypothesis testing and you shouldn't necessarily come in with any a priori assumptions. *Advocacy*, in many respects, *spoils* that process.

Though John felt that his research had policy implications, he saw advocating for any particular outcome as the hallmark of a "bad scientist."[40] Some scientists even avoided being members of advocacy organizations out of fear that other scientists might stigmatize their membership. "You don't want to be an 'advocate,'" explained Debapriya. "If you want to be a 'pure scientist,' you don't want to be associated with the Sierra Club or Nature Conservancy." Elaine's comments regarding advocacy were more caustic. "Write your stuff up, publish it, shut up," she said. "Don't go around playing policy advocate if you really want to be a scientist." Elaine also stated that when engagement more closely resembled advocacy, the specter of egoism grew stronger:

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[40] John brought up an example from his research on invasive species, telling us that questions about the cost and timeline of removing invasive species and about who would benefit or "do poorly" by any particular management strategy were "beyond *me* as a researcher."

> Science means … you gotta be humble about stuff. You gotta be willing to say, "I'm wrong. I'm moving on." In other words, *all* a scientist should be interested in is getting the right answer. … That's the problem with a lot of the scientists that you've undoubtedly already talked to. They're *very* heavily [invested]—they got a bias, they got an opinion.

For Elaine, policy advocacy thus indicated a lack of humility that undermines the scientific process.

In practice, the stigma associated with advocacy colored all public engagement, even in the absence of explicit policy advocacy. Indeed, many respondents discussed advocacy and public engagement almost interchangeably. Consider the following exchange with Debapriya:

> Q: How do you think scientists should relate to policymakers?
> A: I think they should be doing … the stuff that I'm afraid to do.
> Q: So it's only tenured faculty members who should be doing that?
> A: And even they won't do it. … "Just look at how many hundreds of years exist with the scientific method that you don't *sully* it by bringing in the media." I don't *agree* with that, but if I'm going to keep doing the cool research and science that I do and my students do, I have to play by those rules. Maybe I can change them *slightly* and keep writing my popular articles and hopefully spread the word in teaching. … I look forward to the day when I can have the expertise and be the advocate at the same time, but in academia today, it's still dangerous—or at least being a *scientist* [in academia]…
> Q: So what could happen?
> A: [laughs] I'd get fired!

It was thus apparent that the stigma Debapriya and other scientists applied to advocacy also applied to forms of public engagement—media interactions, writing popular articles, and teaching—that were at first blush unrelated to advocacy. Moreover, even though she herself disagreed that advocacy was at odds with academic science, she nonetheless had to contend with the narrative that it was.

## Risk communication as moral dilemma

Scientists' opinions about risk communication were distinct from their opinions about communication more generally. During "crisis situations," scientists generally felt more latitude in going beyond the linear model. This was not the case for less immediate hazards, which were more likely to pose a moral dilemma.

As an example of a "crisis," Mark, a wetland ecologist who earned his PhD 15 years ago, told us about an incident that caused 280,000 gallons of oil to spill into the Mississippi River near downtown New Orleans. "I was really scared by that one," he said. "The stuff that just spilled in the river was fuel oil, and it is *toxic*. And I was very worried about that coming down the river." Concerned that the response company would do a superficial fix, Mark widely disseminated his knowledge about oil remediation.[41]

> I emailed the reporters I knew, I emailed the managers for the refuges down here, I emailed the people in DEQ [Department of Environmental Quality] I knew, I emailed people in the Governor's Office that I knew.... I just kept emailing the bejesus out of people.

Similarly, Gregg Henderson, an entomologist who earned his PhD 20 years ago, told us that he had called a local TV reporter when he first discovered, a few years before

[41] Reflecting on his work as a consultant for a previous oil spill, Mark told us that the response company had been "more concerned about making it *look* clean" than doing what was best for the ecosystem and public health. "You want to leave the vegetation in an oil spill," he told us. "And that's been reported in the technical literature."

Katrina, flood walls infested with termites. "I thought it was a dangerous situation," he said. "I thought it needed to get out immediately."[42]

But scientists were reluctant to sound the alarm about threats that were less immediate. For example, when we asked Mark whether his reaching out to so many audiences was typical, he said that the oil spill was a rather unique situation. "The scale of most of the problems that we deal with just don't lend themselves to being a kind of crisis situation," he said. His research was mainly applied to "much slower acting" things like subsidence and sea level rise. Vikas, a non-tenured plant ecologist who earned his PhD 10 years ago, expressed similar sentiments regarding his research on climate change, comparing it to more "immediate" crises.

> Let's say that it was something, that it was this nuclear meltdown, right? And I was a scientist and I discovered that or found some pretty strong evidence that there was this few events that were gonna cause a meltdown. And I was confident that I was right. Okay, that's one thing, right? Now let's say that meltdown would occur over a 25 year period. It's *different*, right? The meltdown could occur this *weekend*, and it could occur in *New York* or some big urban environment, and that's something people will listen to, *immediately*. But when you tell them, "Hey, the earth is essentially melting down… [trails off]." You know, climate change is gonna be a huge problem, but it might take 10 years for it to really show up in a way that it'll be so obvious.

Even though Vikas felt that climate change was increasing the risk of various catastrophes, such as powerful storms and flooding, he acknowledged that climate

[42] Interestingly, none of our respondents sought out new ties with the media during crisis situations. "I have never contacted a reporter who hadn't contacted me first," Mark said, "because, pff [snide breathing], what are the chances that they're gonna … [trails off]?" And Henderson said that the reporter he had called about infested floodwalls had interviewed him "many times in the past" on other topics. This pattern was partially rooted in the underlying nature of social ties: scientists (and people more generally) are better able to contact someone they know than someone they don't; moreover, reporters who scientists know personally would be more likely to pay attention to what they have to say, take it seriously, and make a story out of it.

change was not itself a tangible, immediate threat that would pique the public's interest. People's attitudes about climate change would inevitably shift, he said, at which point risk communication would simultaneously become more appropriate and more effective.

But scientists generally felt that, unless the connection between catastrophes and "slower acting" threats like climate change is "obvious," then communicating risk would, at best, fall on deaf ears. At worst, they feared that other scientists, the media, or the wider public would peg them as "crackpots" who didn't know what they were talking about. Several scientists invoked the story of Chicken Little, two of whom explicitly told us that communicating information about rare events might place them in the "Chicken Little" category.[43] Such fears left many scientists with a moral dilemma. For example, when we asked Henry whether it was his responsibility to sound the alarm about potential catastrophes related to climate change, he replied as follows:

> I struggle with that question *every* day. I really do see—based on my readings, based on my experience, based on my training as a scientist—I really do see the world unraveling in ways that threaten life as we know it. I mean, that may make me sound like a "Chicken Little," and I know there are people in the press who would love to hear this kind of a statement and make fun of me. There are people who don't believe any of this. But I'm convinced enough that we really do need to do things.

To preserve his reputation, Henry was struggling to discern "outlets that I could be credible in." He feared that the sheer magnitude of the threat posed by climate change,

[43] In the folktale, Chicken Little sensed danger where there is none (in particular, that "the sky is falling!"), and he projected his fears onto others, unwittingly exposing them to an even greater danger. See http://www.pitt.edu/~dash/type2033.html for several variations of "Chicken Little," which has been told in various countries since at least the 19$^{th}$ century.

in the absence of any specific immediate hazard, would make the press call him a "Chicken Little." Hence the moral dilemma, his "struggle": he felt that climate change was "threatening life as we know it," but that disseminating that message widely would diminish his credibility.

Scientists also feared ridicule from other academics and the general public. Consider the following exchange with Sam, an aquatic ecologist who earned his PhD 39 years ago:

> A: Let's assume that Hurricane Katrina had never happened. And I had preached this.… and I ran out there and did this, and wrote novels and blah, blah, blah and tried to get this message across. And Katrina never happened. What would my reputation as a scientist and a citizen be? I would be a freaking crackpot! … I mean, we're talking about a very, very *rare* event, the probability of that happening. That's where you get in trouble.… Who's the little guy running around saying "the sky is falling"?
> Q: Chicken Little.
> A: Chicken Little. I'd have been in the "Chicken Little" category without any question about it.
> Q: Really? You're a scientist. You're a university professor. You don't have to worry about what other people think…. They're not going to fire you.
> A: [Laughs] Your teaching increases the minute you.... There are other ways to put pressure on people other than tenure.

Mark expressed similar sentiments regarding public communication about high-impact, low-probability events:

> Screaming that the sky is falling, when it doesn't every day; screaming that the levees are going to break, and it doesn't every year—even though it's *true*, you end up losing relevance or credibility or something. It's hard to figure out.

Despite having job security as tenured professors, both Sam and Mark perceived that communicating information about rare events would endanger their reputation as scientists. And Sam was adamant that risk communication would prompt administrators to retaliate by increasing his teaching load. In short, scientists felt that sounding the alarm about things like climate change and sea level rise was personally risky.

## The politics of risk communication

I have just shown, in a somewhat generic way, that scientists may fear retaliation for communicating information about hazards that pose no "immediate" threat. Here I offer a specific example of that retaliation in the aftermath of the Katrina disaster. The Katrina case reveals that the potential for backlash depends not only on the physical or temporal characteristics of a hazard, but also on the broader political context of risk communication. Moreover, within universities, those with an interest in silencing certain risk messages may mask their interests by invoking "academic" norms, framing whistleblowers as having violated the linear model of science.

### Organizational constraints on risk communication

Jim, who worked at LSU as a storm surge modeler, said that there was "a real need to sound the alarm" about vulnerabilities to flooding. However, given the university's close ties to the White House (its chancellor was a Bush appointee), Jim felt that LSU was "uninterested" in shedding light on problems for which the government might be responsible. Rather than help identify and correct the systemic engineering problems that led to the Katrina disaster, LSU's stance was that it was "an act of God" for which no individual or organization could be held responsible. This put him and several of his colleagues "at odds with some of the powers that be." For Jim, the foreseeable public benefits of continuing to blow the whistle on engineering problems did not outweigh the personal risks, so he left LSU to pursue work at an NGO. "I could see what was happening," he said, "and I got out—Ivor's still there."

Of course, Ivor van Heerden had more direct confrontations with administrators. In November 2006, following his comments on the "catastrophic structural failure" of floodwalls, "two LSU vice chancellors called me into one of their offices and … told me that my critical remarks to the media about the levees were hurting LSU's quest for federal funding" (van Heerden and Bryan 2007:246). A few days later, they warned van Heerden in an email that they did not want the university "associated, intentionally or not, with efforts aimed at causation" (AAUP 2011:26).[44] When we spoke with van Heerden in May 2008, he told us that the university was still making him "extremely uncomfortable" and had essentially cut off his contact with the media. He explained that his job description, which for the past 19 years had included 50% "outreach work," had changed. He would now be evaluated solely on the basis of his publications in technical journals. Moreover, his performance would be evaluated annually, rather than every three years. Despite his ardent commitment to public science, van Heerden issued a moratorium on his public communication:

> I've put all the media on notice that I'm not doing any more interviews because I wanna get through the summer. You know, I don't wanna be fired for some reason during the summer until I've sorted out another position. Because I've gotta keep food on the table, and I've gotta pay my mortgage.

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[44] The AAUP (2011:40) asserted that "the LSU administration wanted to have it both ways with Professor van Heerden: to dress him in LSU garb and champion his media appearances when the content of his statements was agreeable, but subsequently to attack him in print while cutting off his access to the media when it disapproved of the content." This was similar to Henderson's experience with the LSU administration, as described above.

In short, the university reversed its support for van Heerden's public science, prompting the hurricane scientist to curtail his outreach. Nonetheless, LSU ultimately informed van Heerden that his contract would not be renewed.

### Peer-reviewed risk communication

Many of our respondents weighed in on LSU's treatment of van Heerden and his colleagues. While some scientists viewed his dismissal as a violation of academic freedom, others supported the administration's decision. What follows is an extended excerpt from our conversation with Peter. The remarks below begin after we asked him if there were any "bad rules of thumb" for how to engage policy makers. It's obvious in his response that he is talking about Ivor van Heerden.

> A: This person's going to remain nameless, but *he*, after Katrina, he's not a structural engineer or a geologist, but he took on the US Army Corps of Engineers. And this was not just like you sitting here talking to me. This went *national* in the press. In fact, I think it went international. … It actually resulted in several contracts that the Corps of Engineers had made to *this* university being put on hold for quite some time. This person, he's not a stupid person, but he just got into something that he's not qualified to do. Consequently, it was a bad, bad deal. … The university got egg on its face, the Corps of Engineers was very upset, and of course the media fed on the whole thing. So that is a bad rule of thumb.
>
> Q: So is it speaking beyond your expertise, or is it just overextending yourself to the media? Being too vocal to the media?
>
> A: Yeah, it's not a "freedom of speech" issue, which we all pride ourselves in academic institutions in having the right to. It's overextending your expertise. … I have two rules: one, you don't get into something that you're not trained to do and you're not, frankly, a world-renowned expert; and, secondly, say things that will hold up in court. … Will this hold up among your peers? Will this hold up among scientists that are reviewing critically your literature in the scientific literature? He didn't do that. That's a no-no. I would advise any *logical* individual not to—what do we say? "Those in higher education that seek this being in the limelight, it's going to catch up with you." Incidentally, that guy was fired. It's not something that I do, and it's not something that 'most all of my colleagues do.

And that's definitely a thumbs-down on that.
Q: So it was something academically inappropriate.
A: *Absolutely*. It's not going to do anything for your career, your pedigree, or for the university, especially if you're taking on a big agency that pumps millions of dollars every year into your institution. The sad thing—again, it's not a function of academic freedom. It's a function of being *sloppy* and uninformed in your interpretations and then taking that to the media. Katrina was a classic example. The failure of the levees, it was sort of bees around honey. A lot of these people just flocked in there and tried to make a name for themselves. But it all comes out in the wash, believe me. He lost his job as a consequence. A couple of the others that did the same thing and fell afoul—accordingly, they left the institution.
Q: So he was not anomalous. There were several others.
A: Yeah, he was not an anomaly, but he certainly was the most vociferous.

Essentially, Peter's opinion was that when LSU fired van Heerden, it was well-deserved.

Parsing the excerpt, he advanced three reasons for why the dismissal was justified:

1) van Heerden was "not qualified" to speak about flood protection systems.
2) van Heerden's goal in communicating with the media was to seek out the "limelight" and "make a name" for himself.
3) van Heerden was slandering an organization "that pumps millions of dollars every year" into LSU.

While the first two reasons were rooted in the linear model of science, the third reason was baldly political. Yet Peter linked these reasons so intimately that it's difficult to see whether any of them could stand on its own as a justification of van Heerden's dismissal.

Regarding the first two justifications for van Heerden's dismissal, Peter felt that van Heerden had violated the linear model. By communicating with the public, van Heerden was being egocentric, something unbecoming of an academic scientist and a sure sign of bias. Moreover, both of Peter's allegations that van Heerden was seeking to further his career immediately followed comments about his lack of technical expertise. But it's unlikely that Peter's opinion would have differed if van Heerden had been a licensed professional engineer or had some other relevant qualification. As the head of the State of Louisiana Forensic Data Gathering Team ("Team Louisiana"), which included nine scientists and engineers, van Heerden had the authority to speak on the team's behalf. Moreover, an independent forensic investigation, funded by the NSF, had corroborated Team Louisiana's account of the causes of the Katrina disaster. Furthermore, Peter (though himself not an engineer) seemed to agree with van Heerden that there were problems with the flood protection system. Earlier in the interview, he had stated, matter-of-factly, that "there were failures in levees" and that "the engineering was all wrong." Thus, that van Heerden was "not qualified" to make similar statements was an unusual argument for why he should have been fired.

Peter's third justification for van Heerden's dismissal was explicitly political and had little to do with any narrative about the nature of academic science: Peter complained that van Heerden had taken on an organization that had strong financial ties to LSU. In stark opposition to his insistence that van Heerden's dismissal was not about "academic freedom," Peter was candid about the ways in which funding agencies and universities work together to shape which studies are conducted and which findings are communicated to the public. Moreover, Peter had stated earlier in the interview that his *own* research was funded by the USACE. Thus, his comment that "several contracts …

[were] put on hold" was not merely about threats to one of the university's major funding sources, but also threats to one of his own funders and, possibly, to his own funding. van Heerden, by communicating unflattering information about the USACE, was thus placing himself in opposition to Peter and to other scientists who relied on the USACE for financial support.

Taken as a whole, our interview with Peter exemplified boundary-work aimed at protecting the USACE and scapegoating van Heerden. Peter began with off-the-cuff agreement with van Heerden regarding engineering failures as the cause of the catastrophe, then shifted to the more sterile narrative that floodwalls had been overtopped, an indication of "weaknesses" in the flood protection system. The difference is between, on the one hand, a situation in which the USACE would be culpable for the catastrophe and, one the other hand, a situation characterized, not by an engineering failure, but by mere oversight. Though Peter admitted that the USACE had done some things incorrectly, he simultaneously granted his funder immunity from the consequences of its failure.

Although most respondents agreed that van Heerden's level of public engagement was atypical for an academic scientist, many of them supported his efforts to blow the whistle on engineering failures. For example, Alastair, a professor of Geology who earned his PhD 29 years ago, conceded that van Heerden differed from "most scientists" in that he "goes out and *seeks*" opportunities to communicate risk. "A lot of people say he oversteps his bounds," he said. But in contrast to critics like Peter, Alastair admired van Heerden's efforts to tell the public about what happened during Katrina. "I think it's important to have somebody like him that can make those opinions and comes off good on TV," he said. "We *need* people like that. He keeps it in the news. It wouldn't be out there if it weren't for him." Marcy expressed similar sentiments. "I wish we had *more* people like him who are willing to get out there and do it," she said. "The critics said he was talking about areas where maybe he wasn't the expert—the world's expert," she added. But she countered this criticism by pointing out that, for various reasons, "other people weren't talking." Marcy viewed van Heerden's dismissal as an abnegation of academic freedom—one that posed a threat to more general efforts to communicate risk to the public. Consider the following exchange:

> A: The thing that scares me to death about his case and his situation is that the young faculty coming up will say, "Why would I bother talking to anybody?"
> Q: Do you think it'll have that kind of a—what is it? There's a phrase for that. Chilling—
> A: Chilling effect! I think it will.… There's pressure not to talk. There's just no benefit in it. The state agencies aren't listening, you're getting criticized by your own university—[those] kind of things. You've got a family and other things. What's the point?

For Marcy, LSU's treatment of van Heerden served as a cautionary tale for scientists who might adopt a policy stance that was in opposition to university administrators.

## A continuum of public engagement, a continuum of retaliation

In this section, I discuss scientists' perceptions of the riskiness of different means of public engagement. Table 5 summarizes my findings, which range from the relatively safe practices of publishing in peer-reviewed journals and teaching to the more contentious acts of reaching out to the media and engaging in overt policy advocacy.

Table 5. Forms of public engagement, arranged by potential risk for academic scientists

| ↓ increasing potential for risk ↓ | |
|---|---|
| | publishing peer-reviewed journal articles |
| | publishing scientific results to the web* |
| | preaching in the classroom |
| | giving talks to non-academics |
| | reaching out to officials at government agencies* |
| | interacting with NGOs* |
| | mass communication<br>- publishing popular articles<br>- accepting media interviews<br>- seeking media interviews<br>- writing white papers or editorials* |

* = not discussed in this chapter

### Publishing peer-reviewed journal articles

Respondents generally agreed that publishing peer-reviewed journal articles is an uncontroversial, but typically ineffective, way to engage the public. It's uncontroversial because peer-reviewed publications are the means for academic scientists to establish their reputation and earn tenure. The phrase "publish or perish" concisely sums up that struggle. João, who had earned his PhD just two years before we interviewed him, framed the situation even more bluntly—as "publish or die."[45] Then, in order to maintain their credibility after tenure has been awarded, scientists must continue to publish at a steady rate.

[45] A review of João's CV in January 2017 revealed the immense pressure he must have felt to publish in the eight years since we interviewed him. Despite having a brisk record of publications—roughly 40 journal articles in that period—"Assistant Professor" appears on his CV at three different universities. It would seem that João's first two attempts to gain tenure were unsuccessful, resulting in a demotion to "Adjunct Assistant Professor" but that the third attempt was successful, resulting in a promotion to Associate Professor.

But success in the academic sphere does not necessarily translate into any broader social impact. Because the audience for academic publications tends to be members of the author's narrow subdiscipline, it's rare that journal articles will reach policymakers or the general public. John summarized the relationship between his scientific output and any ensuing social impact:

> A: There is a *huge chasm*—literally, like wanna talk about Grand Canyon? There's a *big* Grand Canyon between me as a scientist putting out a product, an output—being a scientific paper, being a presentation, or something—and how that thing might effect change. And there are very few mechanisms … [to] close that gap…
> Q: Is there ever the urge to close that gap?
> A: Always. There's just very few mechanisms, *very* few mechanisms.

Marcy advanced two reasons why academic papers are unlikely to have any policy impact. First, "they are just *not* circulated to the people who are making decisions." Second, "they usually are not written in a way that it would even be apparent to a policymaker—if they could read them—how one might apply the information." Similarly, Sam told us, tongue-in-cheek, that "if you publish something, and you have an article on it, you assume people read it and you assume you're educating people." He then exploded in laughter. Sam told us that his research, which suggested that navigation channels managed by the USACE had produced "dead zones," had been ignored. The USACE, he said, was maintaining that the dead zone he had published papers on did not even exist.[46] The consensus among respondents was thus that publishing journal articles was not an effective way to effect change.

As an exception, John brought up the work of Vikas, who had recently published an article in *Science*. The paper showed that Katrina had destroyed hundreds of millions of trees, substantially reducing the area's carbon sink and possibly exacerbating global climate change. "Just having that byline," John said, "*that* injects a certain sense of priority, a certain sense of urgency into this situation."[47] When we spoke with Vikas, who earned his PhD 10 years ago, he detailed the extent of the media's coverage of his work. "It was just a *huge* media outpour," he said. "It was front page at the Washington Post, the LA Times, the New York Times, ABC Nightly News [sic], NPR's All Things Considered, Al Jazeera television—the list goes on and on and on and on." The work he published in *Science* had thus circulated far beyond academic circles.

Although the broader impact of Vikas's work was not typical of academic publications, his attitude toward them was. He repeatedly characterized his work as "basic science" and had not planned for this sort of response. "I was completely blown away at the public interest," he said. Moreover, Vikas doubted that climate change was, or in the near future could be, a serious issue in the public sphere,[48] and he expressed

[46] Sam felt that a more effective way to remediate the dead zone would be to bypass the linear model of science by working with NGOs that would bullhorn his findings to the public and, as a possible result, effect policy change.

[47] In contrast to Vikas, John stated that his own work was "not quite at that level." At the time we interviewed him, John had been an assistant professor for less than two years and had not produced any output that might attract media attention.

[48] With the cost of gasoline at record high levels, he attributed people's interest in his work to their primed mental associations among trees, carbon, climate change, and economics. "Carbon is starting

no interest in seeking out non-academic audiences for his work. Elaine framed her output in a similar manner: "What I do is, I do science. I write papers in journals for my peers. That's what I do. If society wants to use it, God bless them." It wasn't that Vikas or Elaine viewed their research as lacking social relevance, but that it wasn't their job as scientists to communicate with broader publics. Their job was to communicate with other scientists. If a publication ended up being widely publicized, this was an added benefit, but not the primary goal.

David, an emeritus professor of geology, had a different perspective on the social impact of his academic publications. When we spoke with him, he had a paper in press at *Nature Geoscience* on land loss in South Louisiana. In contrast to Vikas, David had *intended* his paper to create a stir and foresaw it getting "a lot of media coverage." The paper, he said, quantified land loss in a manner

> that people who are not geologists could look at the numbers, and they will be shocked by the numbers. This will create not only interest, but it will create controversy.

David's use of *Nature* is in line with the journal's twofold mission, which embraces both academic and public science:

> First, to serve scientists through prompt publication of significant advances in any branch of science, and to provide a forum for the reporting and discussion of news and issues concerning science. Second, to ensure that the results of science are rapidly disseminated to the public throughout the world, in a fashion that conveys their significance for knowledge, culture and daily life. (Nature 2017)

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to become something that we're more intimately familiar with in terms that it's affecting our financial well-being," he said. Indeed, when we interviewed Vikas in 2008, gasoline prices were at record high levels (U.S. Department of Energy 2017 Table 9.4).

David had a clear policy stance on coastal restoration—he advocated massive redirection of the Mississippi River—and he viewed *Nature Geoscience* as a means of advancing knowledge while simultaneously advising policymakers.

In sum, respondents felt that peer-reviewed publications rarely traveled beyond academic circles. High-impact journals like *Science* and *Nature* were the exception to the rule.

### Preaching in the classroom

“I got into this research because I thought I could change the world,” said Mark. “But before I was even done my PhD, I realized … I’ll probably make a bigger impact in the classroom than I will in a journal publication.” As university professors, our respondents viewed teaching as an obvious way to positively impact society. Scientists discussed three ways of using the classroom to effect change while maintaining their credibility as scientists.

The most straightforward way to effect change was to teach students about hazards, in the hopes that, when in the path of a hazard, they would evacuate. Alastair referred to his popular course on natural disasters as his “first outreach.” In his lectures, both before and after Katrina, he warned his students that “New Orleans is a disaster waiting to happen.” When asked if he had shared his warnings with the general public, he explained that doing so was beyond his role. “I mean, I’m a professor at a university,” Alastair said. “I really don’t expect myself to be a preacher standing up on a pulpit warning the rest of the world."[49] Sam had a similar philosophy. He told us that before Katrina, he tried to communicate to his students the possibility that New Orleans might one day have to be abandoned. "I call it preaching, as opposed to, you know, sound science," he said. "It was beyond really the scope of the course and the science of the course. And I was expressing something out of the realm of science, … involving feelings, beliefs, and things like that." Sam told us that he had never shared warnings with anyone besides his students. "That wasn't my job as a biologist," he said. As noted above, he feared that issuing warnings more widely would place him in the Chicken Little category.[50]

A second use of the classroom was to have students complete projects that had a public service component. Before Katrina, Henry had not done much to combine his academic and political interests. But the disaster served as "a wakeup call that hurricanes, global warming, and sea level rise—all those things—they threaten all of us." Following Katrina, he began seeking out ways to communicate that message to broader audiences. When Tulane (with Carla's nudging, as described above) started

[49] When probed, Alastair elaborated on the "preaching" metaphor:

Q: But the classroom is not a "pulpit."

A: Well, in a sense it is. [laughs]

Q: In what sense? [laughs] In *what* sense?

A: … Some of it is facts and … I think what we're *all* trying to teach them is … [to] think about them and put them in some kind of logical way that is relevant to your life and is relevant to the rest of the community that you're trying to help.… It's the same thing with a religious pastor: "Here's the facts, and this is what you should do." We all do that to some extent.

[50] Notably, some government regulations have institutionalized classroom advocacy. For example, Marcy described a Louisiana Department of Natural Resources (DNR) requirement that any engineering firm participating in coastal restoration must have trained coastal engineers on staff. The regulation, she said, drove engineers to seek out coursework on coastal restoration, and LSU responded by crafting new curricula. In these courses, policy advocacy becomes more closely intertwined with the material; talking about coastal restoration is not "beyond" the curriculum, but an essential part of it.

requiring all students to participate in public service, Henry viewed this as the opportunity he had been looking for. He began to teach service-learning courses, which he viewed as a form of "activism." Henry told us that he now had students who were "out in the community" effecting change, working with the mayor's office and various NGOs, like the Gulf Restoration Network. Similarly, Anthony encouraged his post-graduate students to collaborate with policymakers when developing their research projects. As exemplary, he mentioned that a student, whose research was on invasive species management, was collaborating with the director of a nature preserve.

Anthony's efforts also aligned with the third strategy that professors used to effect change—instilling in students a culture of public service that might extend beyond their pedagogical relationship. In contrast to his own PhD advisor, who had not offered much guidance with respect to public service, Anthony told us that his goal was to foster public service among the next generation of academics. "I am trying to impress upon my post doc and my graduate students that this is a critical thing," he said. And Mark geared his preaching toward undergraduates, particularly non-science majors, advocating lifestyle changes, such as paying more money for environmentally sustainable products (e.g., "bird-friendly coffee"). Science majors "already care" about the environment, he said, so teaching them about sustainability was "preaching to the choir." But teaching non-science majors was "extremely important" for Mark, particularly education majors, because they might one day preach sustainability to students of their own. Marcy adopted a more direct approach to advocacy— colonizing government agencies with her students. "The most effective way to get things out there," she said, "is that one of our former students, who we have some personal relationship with, lands over there in some kind of position that we can more one-on-one, personally interact with them." Marcy felt that such personal connections fostered willingness to accept policy advice because former students "know that you're not trying to sell them something."[51] Reaching out to non-students at the agencies would be "a cold call," she said, and therefore less effective.

Using the classroom was thus an acceptable means of effecting change. Although scientists generally felt that they lacked role models for linking research to public service, the Katrina disaster goaded them into becoming role models for future generations of academic scientists.

### Giving talks to non-academics

Like journal publications and teaching, scientists viewed giving talks as an acceptable form of risk communication. However, the more closely that a talk resembled policy advocacy, and the more direct its link to the public, the more contentious the talk might be. The audiences for talks included civic and professional organizations, industry groups, government agencies, and the general public. Talks often had a formal structure with two parts: scientifically-framed problems and potential solutions. For example, Jason, a tenured geologist who earned his PhD eight years ago,

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[51] Marcy did point out, though, that another reason to have students working in government agencies was an increased possibility of obtaining funding from the agencies.

said he would first discuss "the facts" about land loss, then get "up on the soap box a little bit" by offering suggestions for how to maintain the coast or manage a retreat. Crystal, a geologist who earned her PhD 22 years ago, described the same format, calling it "the standard talk."[52]

Many scientists told us that they had attempted to communicate risk to civic and professional organizations. For example, Mark told us that "every 18 months or so," he would give a talk to the local Mortgage Bankers Association.[53] "I'd explain wetland loss and subsidence and why we're having it," he said. Echoing his sentiments about teaching non-majors, Mark said that giving talks to civic organizations about land loss was "extremely important," particularly so because "those people vote." Bob, an ecologist who earned his PhD 30 years ago, expressed similar reasons for giving talks to various civic organizations, such as Rotary Club. "You want to get the public involved," he explained. "Without them backing wetland restoration, it will never happen." Analena, a zoologist who earned her PhD 32 years ago, was among the most prolific communicators. She told us that she had given "*thousands* of talks at Rotary Clubs, conventions, Garden Clubs, you name it—just going to people and carrying the message of the *need* for coastal protection."

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[52] Scientists also discussed giving talks at their own professional societies' meetings. Like the typical journal article, these talks were catered to a highly specialized audience of peers. However, respondents mentioned that government employees sometimes attended academic talks.

[53] Mark told us that his wife was a member of the MBA, and this presumably helped him to get invited. After moving to a new area, with a new local MBA, he did not have as much luck getting invited to talks. "I keep putting my name up on the speaker's thing, but nobody's inviting me to go talk to Mortgage Bankers or Lions Club or whatever. I'm not on that list yet."

Scientists expected that the social impact of talks to private-sector organizations would be indirect. Consider the following exchange with Jason:

> Q: Did these efforts to communicate this sort of impending doom of the wetlands, did anyone want to act on this, … people who were empowered to do something?
> A: Sorta, not really. I suspect over the years there variously have been some people in the audiences who are influential in terms of trying to enact or act on something. But it's not as if anybody ever came up to me and said, at the end of the talk, "You know what, I'm going to take this to the congressional committee meeting that I have next week and make sure everybody knows." So there wasn't anything like that.

Jason told us he would have been "absolutely *shocked*" if his talks inspired such a direct policy response. Perhaps because of this separation from policymakers, no one reported any negative consequences of giving talks to private-sector organizations. "I think it was encouraged really," Bob said, reflecting on the opinions of his peers and the administration at his university. Giving talks to private-sector organizations was thus an acceptable and relatively safe way of engaging the public.

In contrast, scientists who gave talks to audiences in the public sector often performed boundary-work to maintain their credibility as academics. For example, Alastair's "second outreach" was to offer fieldtrips, free of charge, on "the geology of the Katrina disaster." He estimated that he had given 250 fieldtrips between November 2005 and August 2008, an average of 8 every month. "I do a lot of weekends," he said. "I want to *make* time. I'm passionate about it. I really feel it's important." He rattled off some of the groups he had taken into the field: middle school, high school, and university students; AmeriCorps groups; numerous professional societies that came into town for conventions; wedding parties; and even the New Orleans Office of Emergency Management. On the one hand, it was obvious that Alastair viewed these fieldtrips as a politically significant form of risk communication:

> A: If you go on my fieldtrip, I bang the drums there.
> Q: You do? You tell people what ought to happen there?
> A: I say, "Look. Now we've talked about the geology. Remember, I'm a geologist. Here's my opinion on what's going on. Some of it's based on geologic fact, some of it is just my opinion—it's political."

On the other hand, he described the fieldtrips in neutral, apolitical terms:

> My approach is, people who come to me, I'll try and explain it [risk] to them in a logical, scientific way.… I'm not a politician. I don't have an *agenda* except to educate people. That's it. [pause] I'm not an "activist"—I really never have been an activist. … They're probably not going to hear it from me unless they *accidentally* come on one of my fieldtrips.

Thus, Alastair's strategy for navigating the potential contradiction between his identities as an academic scientist and as a public intellectual was to frame the fieldtrips as a series of passive, non-"activist" encounters with the public. That is, whereas "activists" seek out opportunities to advance a political agenda, Alastair hyperbolically removed agency from both himself and those who go on his fieldtrips, saying that they do it "accidentally." In effect, what Alastair had done was to create a venue where the public could come to him for information about risk so that he would not have to seek them out. This allowed him to portray as normal a remarkably high number of public talks.

While Alastair described an upper bound to activism, beyond which his peers might view his outreach as too active, several scientists described a lower bound to activism. For example, Teja, an aquatic ecologist who earned her PhD 27 years ago, described a strategy in which she would sometimes "stay quiet" on politically sensitive issues regarding risk:

> This is what I find that distinguishes myself from a lot of other scientists.… It's not that I say something that's *inaccurate*. I just stay quiet. I compromise myself to stay in the game, to be at the table.

But she told us that this strategy sometimes left her on "very uncomfortable ground" among her peers. Other scientists, she said, had criticized her lack of activism on several occasions. One scientist, she recounted, "came to my office, sat down with me, and challenged my ethics…. He even went to the chancellor of the university and complained about me … that I was not challenging the system—[that] I was *part* of the system." As an example of when she felt goaded into action, she told us about a public talk that she had recently given as part of a panel. The mayor hosting the panel kicked off the meeting by saying "We will not consider relocation," which put Teja in an awkward situation. On the one hand, she could "stay quiet" and not challenge the mayor. On the other hand, as an expert on coastal dynamics, she felt that relocation "has to be on the table," especially for those in vulnerable areas. In this instance, the more contentious act for Teja would have been to go along with the mayor's framing of risk, so she abandoned her prepared statement to "lash out" at him. She recounted her comments as follows:

> Sea level is going to change the dynamics of risk. Future generations are going to have increased risk no matter what we do relative to today. In other words, Mayor, you can't go around saying and being as staunch as you are about the fact that we will not relocate. In my mind, you're putting the next generation at higher risk.

The mayor chided Teja after the meeting, saying she had "no business scaring people." But as an academic scientist who studied the coast, Teja felt compelled, both socially and morally, to challenge the mayor, even if it tarnished their relationship.[54]

In general, giving talks to non-academics was a relatively uncontroversial way of communicating risk to the public. Although some scientists communicated information that upset their audience, no one reported incurring any reputational damage or career risk.

### Mass communication

In contrast to the relatively safe means of risk communication discussed this far, scientists were ambivalent about mass communication, which includes things like publishing popular articles and appearing in the news. Resistance to the mass communication of risk was sometimes rooted in boundary-work about the nature of science; at other times, it was rooted in broader political battles regarding the specifics of what was communicated.

Scientists articulated various negative experiences related to mass communication. Recall that Debapriya, having told us that "too much stuff is going wrong" and that "you can't just be a pure scientist," wanted to do more public science. Yet she had never reached out to the media and was careful to throttle the number of

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[54] Her background with the mayor was also a factor in her decision to publicly contradict him. "When I first started talking to him three years ago, I could see him listening to me. Lately, he's just completely condoned science and what we're saying. I just felt I had to lash out at him. I got pissed with him."

popular articles that she wrote. Her reluctance was rooted in the fear that popularization would prompt university administrators to deny her tenure.

> It's something I've heard department heads and deans say: "You're putting out these non-peer-reviewed articles. That could have been a peer-reviewed article, so you're not getting tenure." I swear to God, that happens all over the place… It's a *risk*. "That's not a real scientist. … You need to do pure science. If you're piddling your time talking on the television or making magazine articles in aquarist publications…."[55] I can't publish articles in there. … It's a *reeeally* thin line that, at my stage in my career, I'm afraid to go across right now.[56]

Debapriya's public engagement was thus constrained by the narrative that mass communication was inherently unscientific and might decrease her chances of tenure. Notably, her anxiety about popularization existed in the absence of any broader political context; she feared that administrators would censure her if she increased her output of popular articles, regardless of the substance of what she communicated to the public.

Other scientists feared backlash for popularization that was rooted in political interests. Henderson, for example, feared that LSU would somehow prevent him from publishing a popular article that he had written about the role that termites had played in the Katrina disaster. He also expected that the journal's editor would reject the article "for political reasons and because of the type of science it was." Although it was ultimately accepted by the journal and widely publicized, with more than 800 media outlets covering the article, some of Henderson's fears materialized when he received

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[55] "I'm dealing with invasive fishes," Debapriya explained, "and a lot of times, the people in the aquarium industry are the ones who are curious about it."

[56] While Debapriya did not mention any threats that were directed specifically at her, she told us that other scientists had confronted her about one of her students who had written popular articles. She recounted the criticism as, "Why is he publishing in that when he should be getting a *scientific* publication out?"

"some comments from administrators and some comments from government agencies that were not real happy with my article." When asked to elaborate on this, he said that "it was always hearsay, … somebody saying that this group is upset with you, USDA is upset with you, that you published this." Although Henderson was unsure why the USDA would be upset with him, the hearsay was troubling, as the agency was a key funder of research on termites and one of his own funders.

With respect to media interviews, as discussed above, scientists had to contend with the narrative that interacting with the media "sullies" science.[57] Scientists sometimes invoked this narrative in subtle ways. For example, consider Analena's description of the "number one reason" why scientists "don't like talking to the press":

> It takes them away from the bench where they're doing their research. And once they do it once, they get more and more calls, and then they feel that that's a distraction from what they're about: … the quality and quantity of their science.

Similarly, Debapriya said that—in addition to her fear that appearing the news would "sully" her image—her decision not to reach out was "a time thing."

> Non-scientists don't understand how much scientists work.… I'm trying to keep the ship afloat over here. I'm trying to get tenure. I'm trying to publish. We need to be reaching out and be more interdisciplinary, but it's a hard profession to keep all those balls in the air.

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[57] Respondents also expressed many of the same inhibitions to media interviews as have been reported in prior research, such as the fear of being misquoted. "It's really hard to trust them to do something right," said Debapriya. She recounted an experience where she was quoted by the local media as saying that "the dead zone could actually be *good* for fisheries." The problem was that she hadn't even been interviewed! They correct attribution, Debapriya said, was to a professor in another department at her university.

And Tatiana, a geologist who earned her PhD 16 years ago, said that the media "demand your time, and it very often leads to nothing." Sometimes, she lamented, the same reporter would interview her multiple times, but not publish a story.

At first blush, the argument about time seems generic: doing one thing means that you can't do another thing at the same time. But Kreimer et al. (2011:43), who found similar responses in their survey of Argentinian scientists, has argued that such responses are naïve, since the amount of time that reporters might demand is, in the scheme of things, trivial. An alternative explanation for why scientists say they lack the time for media interviews reflects a more subtle form of the narrative that media interviews are unscientific. Indeed, it was apparent that being "too busy" for the media was a euphemism for the judgment that scientists *ought* to limit their interactions with the media. For example, although Carla told us that communication was a "critical element" of her center, she was aware of the potentially transgressive nature of mass communication within academia. "Every once in a while, we say, 'Okay, let's chill out a while, cause it's starting to *piss* people off that we got three stories in a row.' That element is always there in science." Because too much media attention might violate academic norms of communication, Carla was thus careful to throttle her center's visibility in the public sphere.

Whereas Johnson et al. (2014:92) found that some scientists viewed public engagement as normal when relegated to personal time, scientists like Elaine drew no such distinction. "It's *hard* to do science," Elaine said, "and you certainly don't have enough time if you get on TV." Interestingly, Elaine had spent roughly two hours with us (our second longest interview)—in the middle of her workday at that—yet she gave no hint that our conversation was making it more difficult for her to do science. Also interesting was the fact that Elaine reported being a frequent resource for the media. She justified this, and avoided the impression that she was out to make a name for herself, by adopting a passive orientation to media interviews. Elaine was one of many scientists who, under ordinary circumstances, refused to reach out to the media, instead waiting until the media contacted them. The following exchange with her summarizes the stigma that many scientists feel about media interviews and the role of passive outreach in buffering themselves from that stigma:

> A: I am not involved in public policy, at least overtly, like a lot of people are. I do politics, but I do it behind the scenes….
> Q: So you said that you did do some politics. Not openly, not like going out sounding the alarm, but what exactly *did* you—or *do* you—do?
> A: Well—I mean, the one thing—in being—[clears throat] yeah. I've been before the media a number of times: I've been on Anderson Cooper, and I've been on Glenn Beck, and I've been on all these—every newspaper in the world interviewed me. But I don't go out of my way. There are people on this campus that I think had in their head a plan that they were going to run around and tell people that they were, you know, "the man who knew."[58] Everybody knew it was gonna happen 20 years ago.

In short, though Elaine claimed that "every newspaper in the world" had interviewed her about Katrina, the key difference between her and other scientists was that she did

[58] Reading between the lines, it's easy to see a thinly-veiled attack on van Heerden, whom *Nova* had billed, in the weeks after the storm, as "The Man Who Predicted Katrina" and "The Man Who Knew" (Lewis 2005). Elaine's assessment of van Heerden's outreach was typical. Even Alastair, who viewed van Heerden as performing an important public service, acknowledged that he was an "outspoken" scientist who "sought out opportunities" to communicate with the public.

not go "out of her way" to be on the news. Similarly, when we asked Bob about his relationship with the media, he said, "I kind of shy away from it."

> If they catch me, great. If they don't catch me, I'm not necessarily one to be, to want to be… [trails off]

Debapriya articulated a similar philosophy. "I am more of the type to wait and see when they want my expertise," she said. "If they contact me, I'll be happy to help them." And Alastair told us, adamantly, "I don't seek to be on the news. The news finds me." Such a passive orientation helped scientists to mitigate the impression that they were using the media, and the public, to advance their career.

A few respondents described how they used the rule of passive engagement strategically, redirecting interviews about their published work toward unrelated topics. Analena, for example, had an overt policy agenda: "to try to alert the public about the vital importance of coastal restoration." So when an NPR reporter sought out her expertise for a story on frogs, she "connected the dots" for the reporter, explaining that frogs rely on cypress trees for cover from predators and that humans rely on cypress trees for protection against storm surge. The result was a five-minute story about the value of cypress swamps for both frogs and humans. "It was a brilliant piece," Analena said. "I was just *very* impressed." Similarly, Henderson said that when the media contacted him about one issue, he felt no qualms about steering the story toward some other issue:

> It could be about something very different from the reason they were calling me, but it's a big issue to me…. They've got an idea, they've got a story, and they want to make it interesting. Well, I make it *more* interesting for them…. I

> *convince* them that's it's a very *interesting* [chuckles] direction for them to publish, and they do. It's generally very *easy* to get somebody to be interested in publishing something that you're convincing them is of public importance.

In the immediate aftermath of Katrina, several reporters had reached out to Henderson regarding his research on how termite colonies handle submersion. But since he suspected that infestations had played a role in the collapse of floodwalls, he redirected the story. "I'm pretty *quick* to start filling them in on other concerns I have related to termites and drowning and the flood wall issue," he said.

Media interviews were sometimes constrained by political forces unrelated to academic science. Several respondents reported specific instances—in the absence of any critique of mass communication as being "unscientific"—where colleagues, administrators, or politicians took umbrage at what they had to say. Consider the following exchange with Henderson:

> A: I have been told I don't need to be on the front page of the newspaper all the time by administrators, which is kinda hurtful.
> Q: Why do you think they said that?
> A: Politics. Doesn't matter what the article is—maybe they're gonna get a phone call about it, and maybe it's gonna be a negative phone call, and they don't want those phone calls.… They'll take the good, but don't want any bad that comes from it.

Because Henderson had little control over how his comments might be injected into various political contexts, the LSU administration's solution was that he remain apolitical by curtailing his media appearances. This was in line with the university's handling of van Heerden's media appearances.

## Discussion

To summarize my findings, many of the scientists we spoke with described a moral obligation to communicate risk to the public. However, the ease with which they enacted that obligation varied according to the availability of information about a hazard, the nature of the hazard, its broader political context, and scientists' method of public engagement. In politically treacherous contexts, scientists tended to view more direct forms of public engagement as personally risky. This was particularly the case for scientists who lacked tenure.

### The lack of policy-relevant research

A key constraint to public engagement that emerged from the interviews is the paucity of incentives for conducting policy-relevant research. Hess (2009, 2010, 2016) has dubbed this lacuna "undone science." Undone science constitutes a "problem," write Frickel et al. (2010:445), when areas of research with "potentially broad social benefit … are left unfunded, incomplete, or generally ignored." Much environmental research has potentially broad social benefits. It was precisely those applications that prompted some scientists to cast off environmental research as "not real science." The narrative that applied research is unscientific is in line with prior studies on scientists' perceptions of pure vs. applied science (Johnson et al. 2014).

Aside from these cognitive constraints regarding "real science," respondents also described structural and material constraints to pursuing policy-relevant research. Scientists lamented that the academy and funding streams are structured to answer research questions that are disciplinary in nature, limiting their ability to choose applied research projects, which tend to be more interdisciplinary. While centers like the one that Carla directed helped to foster public science, they may be somewhat of an exception in academia. At the time of this writing, her center no longer exists.

Additionally, in certain political contexts, powerful actors may actively hinder research on environmental hazards. Raymond Seed (2007), the head of UC Berkeley's forensic investigation into the Katrina disaster, reported witnessing

> a significant, and still ongoing, effort to promulgate misleading studies and statements [and] to subvert appropriate independent investigations that would provide useful second opinions … to literally attempt to change some of the critical apparent answers regarding lessons to be learned in the wake of this major disaster.

With restricted access to relevant data and/or with access to falsified or misleading data, scientists will be unable to reach conclusions and communicate information that might prevent future disasters.

### The technical and political aspects of risk communication

Even when policy-relevant research has been conducted, it may still go "unheeded" (Hoffman 2013; see also Frickel et al. 2010). Respondents described communicating with the public about chronic processes like climate change and sea level rise as relatively difficult compared to acute crises like oil spills and looming hurricanes. This finding is in line with prior research showing that "tangible evidence" is a powerful factor in shaping people's perceptions of risk (Scammell et al. 2009). For example, the general public is more likely to perceive global warming as real and politically relevant during heat waves (Ungar 1992) and as fake and politically irrelevant during cold snaps (as indicated by Donald Trump's tweets: Trump 2017, 2019). In a similar vein, Pielke (2007 ch. 4) has argued that engaging the public about immediate threats is straightforward because everyone has the same goal—to get people out of harm's way (see also Sarewitz 2004). In contrast, not everyone agrees on the best way to handle less "obvious" threats.

Negative attitudes about communicating with the public about intangible threats came from two main sources—one rooted in the logic of academic science and the other in broader political forces. By putting it this way, I'm not implying that "academic" constraints are apolitical. I'm emphasizing that academic scientists who engage in political activity must contend with the narrative, maintained by scientists and non-scientists alike, that they ought to be apolitical. The concern underlying this narrative is that being "interested" in a particular policy outcome might undermine a scientist's objectivity (Kinchy and Kleinman 2003:878). Risk communication is inherently political, with the ostensible goal of reducing risk. Even those scientists who viewed science as compatible with risk communication nonetheless had to contend with the narrative that it was not.

Broader political forces also constrained risk communication. In the aftermath of the Katrina disaster, several respondents sought to prevent another Katrina-like event by first discovering what went wrong. But by shedding light on flaws in the hurricane protection system, they were forced to assume the role of "whistleblowers." As Alford

(2016) points out, the most important determinant of whether pointing out organizational failures constitutes "whistleblowing" is how others respond. Venting federal pressure to shut down forensic investigations, administrators and several of van Heerden's peers at LSU responded forcefully. "That an administrator would instruct hurricane scientists not to seek causes of hurricane destruction," stated the AAUP's (2011) report on van Heerden's dismissal, "is difficult … to comprehend." LSU's dismissal of van Heerden, a final violation of his academic freedom, prompted the AAUP to place the university on its censure list.[59]

Other universities took a different approach to handling federal pressure. Raymond Seed wrote that, like Team Louisiana, his team

> was also approached in an inappropriate manner…, but such untoward pressures were simply rebuffed. That, in the end, probably goes right to the heart of what really separates a top-flight university [like UC Berkeley] … from a university like LSU. (Seed 2007)

Although the two forensic investigations reached similar conclusions regarding engineering failures, it would thus seem that UC Berkeley promoted open inquiry whereas LSU stigmatized inquiry that might yield politically inconvenient findings.

### A repertoire of activist practices

A clear pattern emerged with respect to scientists' perceived riskiness of different form of public engagement. In line with prior research (Crawford et al. 2016), respondents articulated norms of communication for academic scientists in which more

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[59] The university suffered further woes in 2013, when it paid van Heerden $435,000 to settle a lawsuit, after having already spent more than $457,000 on the case in attorney fees (Schleifstein 2013).

direct involvement in the policy process was less acceptable. Notably, scientists expressed fewer constraints in venues that were more traditionally "academic"—publishing peer-reviewed journal articles and teaching. In these venues, scientists had a great deal of control over their audience and how their message was received. However, journal articles were an unlikely path to policymakers or policy action, the exception being those rare gems that the media disseminated widely. While teaching held little promise to shape government policies, some scientists effected direct change by incorporating public service into coursework. At the level of graduate education, scientists who supported applied research helped to set the stage for the next generation of public engagement.

When giving talks to non-academics, scientists transferred their teaching skills from the academy to the public sphere. Scientists typically gave talks to receptive audiences who had invited them to speak about risk—a controlled environment quite similar to a lecture, but with a policy tagline. The more that the audience of a talk reflected the general public, however, the less control that scientists had over their how their message would be received. Some scientists performed boundary-work in their talks, explicitly distinguishing "science" from "politics," which allowed them to engage in overt political activity while maintaining their credibility as scientists.

Mass communication with the public, particularly through media interviews, was more contentious. While no one reported any reputational damage or career risk from publishing in academic journals, teaching, or giving talks, scientists who appeared in the news had to contend with academic norms as well as broader political forces. One strategy for appearing normal was to throttle their number of media appearances. Another strategy was to do media interviews by request only. The more that scientists appeared to "seek out" the media, the more readily they might be accused of having deviated from academic norms. When reporters did call them about their research, some scientists successfully redirected news stories toward other topics. Of course, this strategy only works if a reporter initiates contact with a scientist. The prerequisites here are that the reporter must know of the scientist and have some reason for reaching out. Even then, the reporter might not publish the scientist's version of the story, or any story at all. Nonetheless, those respondents who redirected news stories viewed it as an effective way of reaching the public without incurring any damage to their reputation.

### Moral dilemmas in the academy

In recent decades, sociologists have become increasingly less likely to discuss human motivations for action in terms of "morality" (Hitlin and Vaisey 2013), instead favoring what the ILP calls "logics" or what cultural sociology calls "norms." This transition is in line with the emergence of what Tooby and Cosmides (1992) call the "standard social science model," which views people as having an unbounded ability and willingness to internalize norms. In the context of research on such rule-following behavior, morality and moral dilemmas are noticeably absent. Indeed, Roger Friedland (2013:44)—a founder of the ILP—has lamented how the "cognitivism" that characterizes the contemporary ILP literature fails to account for the moral aspects of institutional life. In Weber's (1978:24–26) terms, sociology has come to be characterized by instrumental rationality, which bears the moniker of "agency," and traditional action, which is associated with "structure." Sociologists have viewed the role of value rationality as increasingly less relevant.

But, following Freud, Wrong (1961) has argued that within every person exist "motivational forces bucking against the hold [of] social discipline" (p. 188). Socialization does not strip away all altruism, nor does it squelch all egoism.[60] Many respondents spoke of risk communication as a moral obligation and, particularly in politically toxic environments, a moral dilemma. On the one hand, they felt that, as a result of intangible threats like climate change, the entire planet was at risk. On the other hand, they feared that saying so would prompt colleagues, administrators, or the general public to ridicule them as crackpots who didn't know what they were talking about, as "Chicken Littles."

Recent calls for scientists to speak out about hazards have ignored the possibility of such retaliation (Maibach et al. 2014). Although Kotcher et al. (2017) argued that scientists have "considerable latitude" to engage in policy advocacy without risking harm to their credibility, they offered no empirical support for this conclusion. On the contrary, they found that advocating specific policies, such as building more nuclear

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[60] While Wrong's focus was on egoism (e.g., "material interests, sexual drives, and the quest for power"), he did mention altruism in passing—that there is such a thing as "moral man," who "in his nature" is an "angel" (pp. 190–191). But, as evidence of the pervasiveness and durability of the standard social science model, Fligstein and McAdam (2015:55–56) stared down Wrong, baldly asserting that any "card-carrying sociologist" would accept an "oversocialized conception of man" as an accurate conception.

power plants to address climate change, might negatively impact a scientist’s credibility. If they had surveyed people whose opinions matter—namely, scientists, who may stigmatize one another, or administrators, who have it in their power to censure individual scientists—Kotcher et al. likely would have obtained different results.

While I have not argued that “academic science” is always what pushes scientists away from public engagement, the narrative that scientists who engage the public are “politicized,” “unscientific,” and “limelight-seeking” did offer critics a readymade vocabulary for justifying censure. Ironically, the narrative that engaging the public is all about “making a name for oneself” goaded some scientists into egoism, putting their own wellbeing ahead of public health by remaining silent. For other scientists, though, the decision to “stay quiet” was a strategic maneuver to maintain their credibility and “stay in the game,” that is, an attempt to foster the greater good by choosing their battles wisely.

Different universities established different rules for the game. In the aftermath of Katrina, Tulane normalized public science, establishing an explicit goal to “strengthen and expand the connections between academic study and public service” (Tulane University 2005; see also Giegerich 2008). This new mission afforded some scientists with opportunities to effect social change without losing their credibility as scientists. In less supportive universities, like LSU, moral dilemmas were more common. Several scientists at LSU feared that administrators or peers might censure them if they went public with politically inconvenient information about hazards. Although discussing risk posed little career risk for high-status scientists, they still did so gingerly, if at all. Several low-status scientists failed to play by the rules. In communicating risk to the public, these scientists acted against their own self-interests, incurring various forms of retaliation.

## Appendix: Interview Schedule

SCRIPT: I'd like to move in roughly chronological order, beginning with the sort of work you did prior to Katrina. Then I'd like to talk a little about your reaction to Katrina. And finally we'll talk about your work in the aftermath of Katrina. But feel free to jump in if you have something else to share. Is this format okay?

**Before Katrina**

1. Could you describe the nature of your work prior to Katrina?
    1. Would you say that your work was "collaborative"?

       *Probe:* How so? On what projects did you collaborate?
    2. Would you say that your work was "interdisciplinary"?
    3. Thinking about your work at that time, did it address potential disasters in any way?
2. Prior to Katrina, did you ever make any predictions based on your expertise?
    1. What was your prediction?

       [If yes] Could you give me a few examples of such predictions?

    [Follow with questions below, even if respondent says "no" (but alter them accordingly)]
3. What, in your opinion are the usual/typical/traditional ways that people in your profession/organization sent messages about danger prior to Katrina?
    1. What would have been an example of an unusual venue for sending messages about danger?

[For 4 and 5, characterize according to discipline, academic vs. the public, governmental vs. nongovernmental, other type of group, frequency]

4. To what groups/audiences did you *typically* present your predictions?
   *Probe:* Could you tell me about this?
5. Were there exceptional occasions when you presented your predictions to another group/audience
   *Probe:* Could you tell me about this?
6. Were your predictions ever published in an academic journal?
   *Probe:* Could you tell me about this?
7. Did you ever present your work to the popular media (tv, radio, internet)?
   *Probe:* Could you tell me about this?
8. What sorts of responses did you get from those who heard your predictions?
   [Associate responses with groups listed above, as applicable.]
9. Were there divisions within your own field about these predictions?
   1. Were these divisions made public?
   2. What do you think were the effects of these divisions on the reception of your predictions?
10. Did you offer suggestions about how to respond to this potential danger?
    *Probe* for examples.
11. Still thinking about things prior to Katrina, were any of your audiences able to take action on your predictions?
    [Get at both whether they were *able* to take action and whether they *did.*]
    1. [If they were able to do something and they didn't] Why do you think the warnings were insufficiently heeded?
    2. [If they were able and they did] What did they do?
    3. [If they were unable] Who would have been able to take appropriate actions, and how would you go about getting in touch with such a person?
       *Probe* [if applicable]: Did they adopt any of your suggested responses?
12. Did you ever feel frustrated in your attempts to share and apply your expertise?
    *Probe*: Tell me more about this.
13. Can you name someone you knew prior to Katrina, perhaps another expert, who would have had enough clout to be heeded?
    [Record any "experts" or other individuals.]
14. In retrospect, is there anything you that could have done to increase the likelihood that policy-makers would have paid attention to your predictions?

**Katrina**

1. Could you walk me through what you did during the days leading up to Katrina?
    1. What did you think when your first learned of the coming storm? (primary concerns, ideas of what to do, how to warn, etc)
    2. Were you in contact with others? Who were they/from what field?
2. Did you use your prior predictions during the events immediately preceding Katrina?
    1. Who did you feel was your constituency? (Do you feel as if there was anyone – in public or private spheres – who was relying on your scientific work?)
    2. Who did you think was most important to warn?
    3. What was most and least effective ways you tried to take action/warn?
    4. What other strategies did you consider for getting the word out, but ultimately decided against?
        i. Why did you not choose these?

*Probe* to continue narrative through storm.

3. Did you develop any new professional relationships because of these events?

**After Katrina**

1. In what ways does your work differ from what you were doing before Katrina?

    *Probe* with the following to identify differences from answers given above.

    1. Would you say that the work that you do now is "collaborative"?

        *Probe:* How so? On what projects do you collaborate?

    2. Would you say that your work is "interdisciplinary"?
    3. Does your current work address potential disasters in any way?

        *Probe:* Could you tell me about this?

2. Have the usual ways that people in your profession/organization send messages about potential danger changed in the wake of Katrina?
    1. Could you give an example of an unusual or untraditional venue for sending messages about danger?
3. Does your current work involve making predictions based on your expertise?

    *Probe* with the following questions, even if respondent says "no" (but alter them accordingly)

4. To what groups/audiences do you *typically* present your predictions?

   [characterize according to discipline, academic vs. the public, governmental vs. nongovernmental, other type of group, frequency]

5. Do you publish your predictions in academic journals?
   1. About how often? Would you say that this has changed since Katrina?

6. Do you present your work to the popular media (tv, radio, internet)?
   1. About how often? Would you say that this has changed since Katrina?
   2. Could you tell me about this?

7. What sorts of responses do you get from those who hear your predictions? Has this changed since Katrina? Why do you think that is?

   [Associate responses with groups listed above, as applicable.]

8. Are any of your audiences able to take action on your predictions? [Get at both whether they are *able* to take action and whether they *do.*]
   1. [If they are able to do something and they don't] Why do you think the warnings are insufficiently heeded?
   2. [If they are able and they do] How do they take action?
      *Probe:* Do you think that there is a "Katrina effect"? Do you think that this is temporary or not?
   3. [If they are unable] Who do you think *would* be able to take appropriate actions, and how would you go about getting in touch with such a person?

9. Do you ever feel frustrated in your attempts to share and apply your expertise?

   *Probe:* Tell me more about this.

10. Are you a member of any professional organizations (e.g. The Union of Concerned Scientists)?
    1. Does your membership in these organizations change the way you do your work?
    2. Have your affiliations changed in the aftermath of Katrina?
       *Probe:* In what way? (More scientific, political?)

**General questions**

1. Do you think scientists should be in the business of "sounding the alarm"? Why?
    1. [If yes] How might they do that?
    2. [If no] Can you think of occasions where, exceptionally, scientists should "sound the alarm"?
       *Probe:* What about in the face of a catastrophe?
2. What difference do you think it would make if scientists worked more with each other across interdisciplinary fields? [May have been addressed above.]
    1. Do you think that an increase in interdisciplinary work changes the ways in which scientists communicate with one another?
    2. What about how they communicate with the public?
3. How do scientists think scientists should relate to governmental bodies?

   *Probe:* ways to improve this, changes due to Katrina, etc.
4. In what ways do you think policy shapes science? (Do you feel that policies shape the sort of work that you can do?)
   *Probe* for examples
5. How do you think communities of scientists get built?
    1. Could you give me some examples?
    2. What do you feel communities of scientists should ideally look like? Why?

# Chapter 4

# Manslaughter and Scientific Immunity: International Responses to the Trial in L’Aquila, Italy

*In no way was there a justifiable ground to turn this into a criminal case against scientists.*
—Euroscience

## Introduction

On 30 March 2009, a moderate (4.4 $M_W$) earthquake tore through central Italy.[61] Fearing an aftershock, many of those living in fragile buildings spent the night outside or took refuge in sturdier structures. The following day, the *National Commission for the Prediction and Prevention of Great Risks* (CGR) met in L’Aquila to assess the risk that a more powerful earthquake would occur. A message that emerged was that because moderate earthquakes are “favorable” and “discharge energy,” there was “no danger” of a more powerful earthquake. Five days later, when a moderate (4.0 $M_L$) earthquake struck, several people recalled what the scientists said and broke their tradition of evacuating. A few hours later, they were asleep in their beds when a powerful (6.3 $M_W$) quake struck. The earthquake killed over 300 people. According to Judge Marco Billi, the members of the CGR had conducted a “superficial, approximate, and generic” risk analysis and given the people of L’Aquila “incomplete, imprecise, and contradictory

[61] All earthquake data are from the European-Mediterranean Seismological Centre (2013).

information." He convicted them of manslaughter in connection with the deaths of 29 of the victims and sentenced each defendant to 6 years in prison. In addition to their jail terms, each was also banned from future government work and fined 8 million Euros.

This chapter examines the unprecedented response that the trial elicited from the scientific community. To my knowledge there has never been such widespread discussion in the scientific community about scientists' legal accountability in the context of risk communication. Part of the reason for this may be that scientists have never been held legally accountable in this way. The closest parallel may be the "Climategate" scandal. In that case, some emails sent among climatologists had been hacked, then leaked to the public, revealing some biased peer review practices and some questionable handling of Freedom of Information Act (FOIA) requests. At least eight investigations ensued, none of which placed much blame on any of the climatologists (Anon 2018; Grundmann 2013). But had the climatologists been convicted of evading FOIA requests, the worst penalty would have been a fine, not a prison sentence.

This chapter proceeds as follows. I first briefly review some literature on boundary-work at the science–society interface and apply the notion of boundary-work to the context of risk communication. Then, after detailing the social and geophysical context of the CGR's risk communication in the days leading up to the deadly earthquake, I provide some background information on the legal basis of the trial. In so doing, I offer translations from Italian of the CGR's risk communication, laws relevant to the trial, and the judge's explanation of the verdict. Finally, I analyze the scientific community's response to the trial. I conclude the chapter by introducing the notion of "scientific immunity," a doctrine in which scientists who communicate risk cannot be held legally accountable for what they say, even if what they say is incorrect. I also suggest that most defenses of the CGR, whether advocating the CGR's innocence or its immunity, were rooted in the lack norms for crisis communication about intangible threats.

## The public accountability of scientists

In the previous chapter, I probed the moral foreground of science by examining scientists' perspectives on risk communication. This chapter delves into morality from another angle: public accountability. How do scientists respond when the public attempts to hold them accountable for what they do and do not say in the context of crisis communication?

As discussed in the dissertation's introduction, the literature on public science has focused mainly on the topics that scientists choose to research. Scientists, the literature has shown, sometimes view particular research topics as moral or immoral (Kempner et al. 2011, 2005). The idea of "dual-use research" highlights how such concerns relate to science communication. As defined by the National Science Advisory Board for Biosecurity (n.d.:4), dual-use research has "the potential for both benevolent and malevolent applications"; relevant ethical issues include whether dual-use research should be done and, if so, to whom its results should or should not be communicated.

For example, in the midst of the Cold War, the National Academies responded to proposed governmental restrictions on scientific research and communication by publishing "Scientific Communication and National Security." Gieryn (1983:791) coined the term "boundary-work" to describe how the report's authors, many of them high-ranking academics, strategically defined "science" in ways that advanced their interests. Gieryn found that they characterized research that the government might wish to control as "basic" rather than "applied," thus distancing scientific research from any negative applications. Their goal, wrote Gieryn, was "immunity from blame for undesirable consequences of non-scientists' consumption of scientific knowledge" (1983:789). Yet the authors simultaneously trumpeted those applications of science that the public might view favorably (see also Pielke, Jr. 2007:12).

The L'Aquila case helps to extend the notion of boundary-work to the context of legal accountability. In an unprecedented trial, scientists were held accountable for their risk communication, and the scientific community gave an unprecedented response. Dozens of science organizations issued public statements that highlighted the positive attributes of science and scientists (including some of the defendants) while downplaying the specifics of what was said in the days before the disaster. Although Gieryn (1999:23) framed boundary-work as "strategic practical action," my analysis reveals that scientists' boundary-work surrounding the L'Aquila trial was not always deliberate. In line with Kinchy and Kleinman's (2003) findings, much of the boundary-work that took place may have relied on the presumption that the scientists on the CGR had done nothing wrong.

## The social and geophysical context of the CGR meeting

From November 2008 through March 2009, hundreds of earthquakes struck central Italy. Though most of these tremors were relatively weak, many people began to wonder whether they might be precursors to a much more powerful earthquake. Adding to their fears, Giampaolo Giuliani, a technician at Gran Sasso National Laboratory, gave journalists specific predictions about when and where more powerful earthquakes would strike. Though he was not a seismologist, had never published any research in seismology, and had made several predictions that were incorrect,[62] there were so many tremors plaguing central Italy—with as many as 17 occurring on January 23rd (Hall 2011:268)—that some of his predictions were bound to be somewhat correct. As a result, his alarms gained credence.

Officials in Italy's Department of Civil Protection (DCP) felt that it was necessary to counter Giuliani's claims. On 30 March 2009, they cited him for *procurato allarme* (being an alarmist) and ordered him to stop making predictions (Hall 2011:267). Later that day, a moderate (4.4 $M_W$) earthquake struck central Italy. To discredit any associations between Giuliani's predictions and powerful earthquakes, Guido

[62] An international commission that investigated Giuliani's predictions noted that at least two of them were incorrect in terms of the predicted strength, location, or timing of earthquakes. "No evidence," reported the commission, "indicates that Mr. Giuliani transmitted to the civil authorities a valid prediction of the mainshock before its occurrence" (Jordan et al. 2011:323).

Bertolaso—the head of Italy's DCP—scheduled an "emergency" meeting of the *National Commission for the Prediction and Prevention of Great Risks* for the following evening.[63] The DCP issued a press release stating that the goal of the meeting would be to provide people with "all the information available to the scientific community about the seismic activity of recent weeks" (Billi 2012:94).

But in a phone conversation with Daniela Stati, a local DCP official, Bertolaso revealed that the true goal of the meeting was to silence any "imbecile" who thinks he can predict earthquakes and thereby "calm the public."[64] The CGR normally met at the DCP's headquarters in Rome (Billi 2012:88), but this time Bertolaso wanted them to meet in L'Aquila. Furthermore, instead of a closed meeting as was typical, public representatives would be in attendance, including Stati and Massimo Cialente, the mayor of L'Aquila. "It's more of a media operation," he told Stati. "The leading experts on earthquakes will say: 'It's a normal situation… It's better to have 100 earthquakes at 4 on the Richter scale than no earthquakes at all, because these 100 shocks will release energy, preventing a more powerful earthquake from striking.'"[65] Just before the CGR's meeting, Bernardo de Bernardinis—the head of the CGR and Bertolaso's immediate

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[63] According to Italian law, members of the Commission are to be given "notice of at least ten days, except in cases of urgency or emergency in which it can be reduced to one day" (Billi 2012:87).

[64] Nigg (2000:146–48) describes a similar case, but a different official response, at least initially. In response to predictions that a major earthquake would strike in about a year, the US National Earthquake Prediction Evaluation Council "resisted conducting a formal hearing on the prediction because its members mistakenly believed that by evaluating the prediction they would give it more credibility."

[65] Bertolaso's conversation with Stati was wiretapped as part of a separate investigation. I took some liberties in translating Bertolaso's choppy description of how the scientists would describe the situation into smoother prose. See Billi (2012:150–52) for a transcript of the full conversation.

subordinate—met with reporters.[66] In language quite similar to his boss's directive, de Bernardinis wielded the CGR's credibility and implied that weak or moderate earthquakes decrease the chance that a more powerful earthquake will occur. In a brief but memorable statement, he told the media that "there is no danger. The scientific community continues to confirm that it [the recent seismic activity] is in fact a favorable situation, an ongoing discharge of energy." When the media replayed snippets of this interview, the message that emerged was that more tremors means less danger (Billi 2012:593; Hall 2011:268).

That message, however, was incorrect. According to the scientific consensus, a powerful earthquake is around 100 times more likely to occur within two days of a moderate earthquake, raising its probability to roughly 2% (Grandori and Guagenti 2009:61; see also Grandori, Guagenti, and Perotti 1988; Hall 2011:267).[67] Console et al. (2012:42)—employees at the Istituto Nazionale di Geofisica e Vulcanologia and, thus, colleagues of some of the defendants—estimated that during the week preceding the 6 April mainshock in L'Aquila, a powerful earthquake was only 30 times more likely to strike, up to a 0.39% chance.[68] While both of these probabilities may seem low in

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[66] The interview, which may be seen at http://www.youtube.com/watch?v=kLIMHe0NnW8, was transcribed and is available in court documents (Billi 2012:105–6).

[67] The key assumptions of earthquake forecasting are that "(1) the future will look like the past, at least probabilistically, and (2) the past is accurately known and accounted for" (Hanks, Beroza, and Toda 2012:760).

[68] The numbers in the main text refer specifically to central Italy, but seismic risk may vary by geographic context. In southern California, Jones (1985:1675) estimated that a moderate earthquake increases the chance that a powerful earthquake will occur within a five-day timeframe by a factor of 500, up to a 6% chance. For reviews of the literature on time-dependent earthquake forecasting, see Jordan et al. (2011) and Woo and Marzocchi (2014).

absolute terms, risk research has shown that people tend to be more sensitive to small changes in risk when the change is at the bottom of the spectrum—for example, a change from 0 to 1% compared to 36 to 37% (Patt and Schrag 2003:18–19). Under such circumstances, if people living in fragile buildings were told that moderate earthquakes *increase* the risk that a powerful earthquake will strike rather than *decrease* it, they may have been more likely to spend the night elsewhere.

The CGR met on 31 March 2009 at 6:30pm. Interestingly, according to the draft version of the meeting minutes (Billi 2012:99–103), but not the official version (pp. 95–96), Barberi stated that he had heard the head of the DCP tell the press that seismic sequences decrease the risk of a more powerful earthquake. When he asked for comments on this notion, no one provided a direct reply. Stati testified that, at the meeting of the CGR, one of its members said something similar to what de Bernardinis had said during his interview with the press, but she could not remember who (pp. 112–113). Selvaggi (one of the defendants) testified that he did not recall hearing the notion that earthquakes discharge energy at the meeting. “I would have jumped on my chair,” he said, “if such a thing had been put forth” (pp. 137–139). Bertolaso testified that that he had made similar statements on numerous occasions when seismologists were present, including members of the CGR, and that none of them had ever disputed his claims (pp. 257–259).

As recorded in the official minutes, the CGR had two objectives. First, it aimed to provide an “objective assessment of the seismic events taking place in relation to a possible prediction." Boschi said that in the days following a moderate earthquake in a seismic swarm, a more powerful quake might occur. "It's improbable," he said, "but the possibility cannot be completely excluded." Professor Eva agreed: "the region around L'Aquila is seismically active," he said, "so it's not possible to say that there will not be an earthquake." Selvaggi highlighted cases in which strong earthquakes were preceded by seismic swarms, but he also said that "many sequences have not resulted in strong earthquakes." Boschi and Barberi both stated, somewhat ambiguously, that swarms are "not precursors" to strong events.[69] No one on the CGR mentioned any specific numerical probabilities or discussed the way in which a moderate earthquake in a seismic swarm might affect the probability of a larger earthquake.

The second item on the CGR's agenda was to "discuss and provide information on alarms common in the population." Barberi stated that specific predictions about the time, place, and strength of earthquakes—including predictions based on measurements of radon gas levels (Giuliani's method)—were "scientifically unfounded."

At several points throughout the meeting, members of the CGR stated the need to reinforce fragile buildings. At the conclusion of the meeting, the draft version of the minutes—but, again, not the official version—records Stati as thanking the members of the CGR for giving "statements that allow me to reassure the population via the media."

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[69] Strictly speaking, saying that seismic swarms are "not precursors" means that they do not affect the chance that a more powerful earthquake will occur—that the risk is the same as at any other time. It might also connote, however, that they are "not *sure* precursors," meaning that swarms do increase the chance that a more powerful earthquake will occur, but they do not *always* signal that a powerful earthquake will occur. Some sources (e.g., Jordan 2013:5) quote the scientists as saying "not sure precursors," but this is not what is recorded in the minutes.

Barberi, de Bernardinis, Calvi, and Dolce attended a press conference that followed the meeting. Barberi essentially repeated what had been said in the meeting, saying that seismic swarms "very rarely" precede powerful earthquakes. He did not discuss how moderate quakes affect the probability of a more powerful quake, and, as in the meeting, he did not state any numerical probabilities (pp. 104–105).

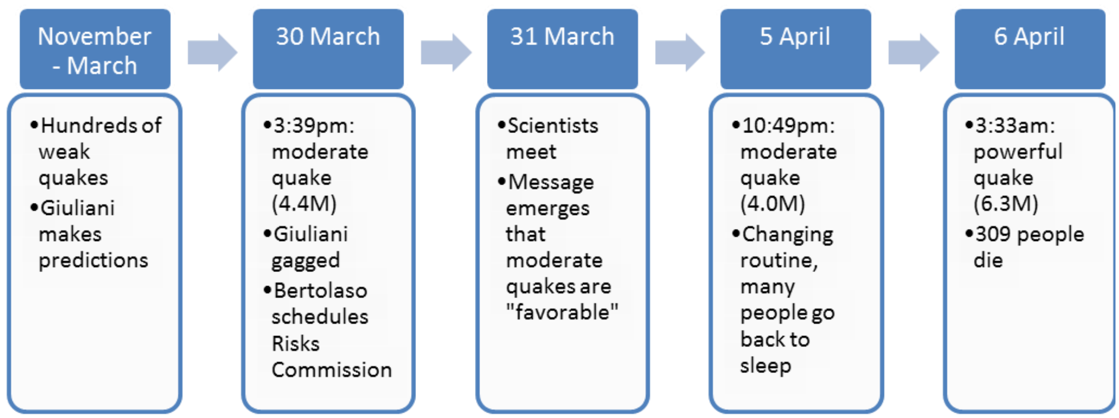


Figure 8. Timeline of events leading up to the 6 April 2009 disaster in L'Aquila

The events leading up to the disaster in L'Aquila are summarized in Figure 8. Leading up to the earthquake, the DCP had essentially constructed a "media echo chamber," that is, a venue with "the potential to both magnify the messages delivered within it and insulate them from rebuttal" (Jamieson and Cappella 2008:76). By gagging Giuliani, the DCP ensured that for the immediate future, its interpretation of the seismic activity in the region would be the only one, and by claiming that it was presenting "all the information available," it asserted that its expertise was authoritative.

## The legal basis of the trial

Under Italian law, the DCP is tasked with "protecting the integrity of life, property, settlements, and the environment from damage or risk of damage from natural disasters, catastrophes, or other major events" (Law 401 of 9 November 2001). For the tasks of prediction and prevention, the DCP's advisory body is the *National Commission for the Prediction and Prevention of Great Risks* (CGR; Law 225 of 2 February 1992). Judge Billi argued that if the defendants had expressed on 31 March 2009 "purely personal" opinions—or even opinions "as scientists, experts, or scholars"—then the relevance of what they said would have been purely academic, but because they had expressed opinions as members of the CGR, their statements must be evaluated in light of legal obligations to predict and prevent various risk scenarios (p. 295).[70]

To hold the members of the CGR accountable for manslaughter, Judge Billi argued that they had assumed some of the duties that typically fall to the DCP. The normal procedure, he wrote, is for the CGR to provide the DCP with information "and only later (after selecting any useful information and after identifying the most suitable means of communicating with the media), the DCP shall inform the population" (p. 213).

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[70] Italian law defines prediction and prevention as follows:
Prediction consists of activities directed at the study and determination of the causes of hazardous events, the identification of risks, and the identification of geographical areas subject to those risks.

Prevention consists of activities to avoid or minimize the possibility of damage … based on knowledge acquired as a result of predictions. (Law 401 of 9 November 2001)

But Judge Billi identified two reasons why the CGR had "knowingly and willingly assumed the burden" of public communication on 31 March 2009. First, in allowing others to attend the meeting, including Daniela Stati and mayor Cialente, the CGR had made "immediately public, without any filter, each stage of the discussion and each topic" (p. 205). Second, the presence of several of its members at press conferences before and after the meeting had "constituted a sort of guarantee of the authenticity of the information disclosed to the media by local DCP officials and the fact that this information came from the CGR" (p. 210). Billi noted that Barberi and de Bernardinis, in the presence of Calvi and Dolce, had been active participants at the press conference after the meeting. For these reasons, Judge Billi argued that the CGR should be held responsible for anything they said that detracted from the DCP's mission of "protecting the integrity of life."

Judge Billi agreed with the public prosecutor that the members of the CGR had failed in these duties. Moreover, he agreed that as a result of their failure, they were guilty of crimes covered under Articles 589 and 590 of the Italian Penal Code—that is, of causing death (589) and injury (590).[71] Because the members of the CGR were being tried as a group, Article 113 was also relevant; it states that when multiple people cooperate in a crime, "each of them is subject to the penalties established for the crime itself." Regarding causality, Article 40 of the Penal Code states that a person may only be punished if the "harmful or dangerous event" that constitutes his crime is "a

[71] The Penal Code is available at http://www.altalex.com/index.php?idnot=36653

consequence of his act or omission." Article 40 also states that "not preventing an event, which one has a legal duty to prevent, is equivalent to causing it." The key premise of the trial, therefore, was that the members of the CGR had, through commission and/or omission, brought about injury or death.

Billi found particular fault with the statements by de Bernardinis at the press conference. "It's a normal situation," he wrote is "highly ambiguous." Along with its connotations of "ordinary" and "usual," he wrote that normal "can be well understood in the parallel meaning of something that is of no concern" (pp. 248–249). He also stated that the phrases "there is no danger" and "favorable situation" were "unmistakably reassuring."

> To say there is no danger means to exclude categorically, emphatically, the possibility of any future events that are negative, of events that are capable of producing damage. (p. 251)

The image of energy being "discharged," Billi stated, had further "reinforced the reassuring effect of the forecast" (p. 251). Through such statements, Judge Billi argued that the defendants had "consciously and uncritically" participated in the "media operation" that Bertolaso had orchestrated, ultimately increasing the chance that people would be harmed by the earthquake (p. 906).

Regarding the meeting itself, Billi ruled that the defendants' "superficial, inadequate, and ineffective" analysis of various risk indicators constituted "negligent conduct by omission" (p. 390). Consider Billi's critique of Boschi's statement that a powerful earthquake was “improbable … but the possibility cannot be completely excluded.”

> It does not signify any concrete contribution to the risk assessment, as the criterion of analysis is devoid of references or hooks to data enabling an even embryonic quantification of the probability or improbability. In science, and particularly in the study of earthquakes, … all arguments are articulated in essentially probabilistic terms … [and] nothing can be ruled out absolutely. Therefore the phrase quoted appears trivial and commonplace, … used as mere filler. (pp. 234–235)

Despite the fact that one of the two items on the CGR’s agenda was to assess what the seismic swarm might portend, no one present at the meeting mentioned any specific numerical probabilities or gave any indication of how a moderate earthquake might affect the probability of a more powerful earthquake. Yet Billi pointed out current seismic activity ranks among the best available predictors of future seismic activity. In a review of 40 possible precursors by the International Association of Seismology and Physics of the Earth’s Interior, he noted that 3 of the top 5 possible precursors involved seismic anomalies (p. 74). He also highlighted the work of Dolce, which stated that “at present, the most promising approach seems to be predictions based on specific variations, within a delimited area, of background seismicity” (p. 70).

A more complete analysis of risk, Billi argued, would have taken into account different dimensions of risk that are commonly used in risk research (including publications by several of the defendants), such as the *probability* that an event of a particular magnitude might occur at a particular time and place, and the *vulnerability* of people and buildings to that event. The possibility of a more powerful earthquake, he wrote, was "anything but remote" (p. 350). Moreover, he stressed the importance of scenarios that were more likely to occur, including earthquakes of equal or lesser magnitude than the ones that had already occurred, as well as the impact of these earthquakes on different types of buildings. He pointed out that by the time of the CGR meeting, moderate earthquakes in the seismic swarm had already visibly damaged some buildings (p. 350).

Contrary to sensationalistic media (and scholarly) reports, Judge Billi did not fault the members of the CGR for their inability to predict earthquakes. Billi repeatedly stressed that his ruling was not based solely on the *ex post* criterion of whether the earthquake had occurred. In fact, he wrote that "the deterministic prediction of earthquakes, according to the current state of scientific knowledge, is definitely not possible" (pp. 68–69), a point reiterated at least 10 more times throughout his explanation of the verdict. Instead, Billi applied the criterion of "posthumous prognosis *ex ante*" (pp. 220, 233), which refers to *attempted* criminal activity that may or may not have a particular result.[72] An *earthquake*, he wrote, is "an unpredictable natural phenomenon," but the *risk of an earthquake* is "a potentially analyzable situation" (pp. 295–296).

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[72] Billi used the example of a single unprotected sexual encounter and ensuing transmission of the HIV virus (p. 387). Moreover, according to Di Amato—Professor of Law at the University of Naples—people are only criminally liable under the criterion of posthumous prognosis *ex ante* if they *intend* to commit the crime in question (Amato and Fucito 2016:110). Continuing the above example, this would describe individuals who *knowingly* put their sexual partners at risk of disease. Thus, the defendants' guilt depended on their intention to place others in a dangerous situation.

Of the 944 pages in his explanation of the sentence, Judge Billi allocated 454 pages (48%) to the topic of causality. For each alleged victim, he detailed (a) the conduct of each victim in relation to earthquakes that occurred before the CGR's meeting, (b) each victim's knowledge of the outcome of the CGR's meeting, and (c) the conduct of each victim after becoming aware of the meeting's outcome (pp. 398–402).[73] For example, the sons of one of the victims testified that ever since they were children, their family had a "culture of evacuation" (pp. 637–651). After the 30 March earthquake, their terrified mother had evacuated her home, which had been constructed before 1900 and was, as her children put it, "decrepit." Following the 5 April earthquake, however, she resisted their pleas to spend the night with one of them. Apparently reassured by the CGR, she told her sons that any aftershocks would not be more powerful than the one that had just occurred. In this manner, Judge Billi linked the CGR to the death of 29 out of 37 alleged victims and to the injury of 4 out of 5 alleged victims (pp. 902–904).

## Science organizations respond to the trial

To analyze the ways in which the scientific community responded to the trial, I collected public statements issued by science organizations. Compared to statements by individual scientists, organizational statements are the result of discussion among

[73] Judge Billi also attempted to account for various confounding factors for why a victim may have remained inside, such as having a sick child, having to get up early the next day, or that the weather outside was uncomfortable.

multiple scientists or, at the very least, claim to speak on behalf of multiple scientists. I thus view them as better able to tap into norms of how "scientists" viewed the trial.

In the weeks after the 3 June 2010 indictment, four science organizations issued statements about the trial.

When the members of the CGR were convicted of manslaughter on 22 October 2012, many other science organizations issued statements. A particularly useful resource for collecting post-verdict statements was a website created by Italy's Istituto Nazionale di Geofisica e Vulcanologia (INGV), which had the stated purpose of "collect[ing] documents, articles and comments from the scientific community about the trial, … [an] event which has had and still has a major impact on the interaction between the scientific community and civil society both in Italy and abroad." Their site contained a comprehensive archive of legal documents surrounding the case. Of particular interest to this chapter was a page titled "National and International Support," on which the INGV requested that scientists send in letters of support for the CGR. To limit "the detrimental consequences" that the guilty verdict would have "on science and its role in modern society," The INGV called for "clear and prompt action with massive support." At the time of my analysis, the page contained links to 25 post-verdict statements about the case that were issued by science organizations, including university departments, private research centers, professional associations, national academies, and international bodies.

I chose not to analyze 10 other post-verdict statements on the INGV's website—9 by individuals and 1 that was ambiguous in terms of whether it reflected the opinions of a science organization or an individual. In that case, Evgenii Gordeev used his position as director of a large prestigious science organization to establish his credentials, but he did not explicitly say that he was speaking on behalf of his organization, and he repeatedly wrote in the first person singular throughout his letter. Here is how the letter began: "As Director of the Institute of Volcanology and Seismology, the Russian Academy of Sciences, … I am deeply anxious and astonished by the verdict…." In contrast, Alan Leshner—the CEO of the AAAS—made it clear that he was "writing on behalf of the American Association for the Advancement of Science, the world's largest multi-disciplinary society," and thereafter wrote in the first person plural ("we"). In any event, Gordeev's letter did not substantially differ from other statements that I discuss in this chapter.

I found many other statements through Internet searches, nine of which I chose to analyze. I excluded statements written by science organizations in the spirit of news coverage. I also excluded a statement written by the International Seismic Safety Organization (ISSO), which was the only one I found that was overtly critical of the members of the CGR. Gabrielli and Di Bucci (2015:994) point out a series of coincidences regarding the ISSO and its statement on the trial:

> the headquarters of this organization are located … in the office of Wania Della Vigna, one of the lawyers of the victims' relatives. Three out of the 13 founding members were witnesses of the indicters represented by Della Vigna. The organization was founded just before the sentence was delivered, and in its

position statement, issued on August 6, 2012, it explicitly questioned the interpretation of the indictment of the "Seven" [i.e., the seven defendants].

Because the ISSO appears to have been founded with the primary purpose of aiding the victims by giving "scientific" credibility to the prosecution's claims, I believe that its statement may not reflect broader scientific norms.

Thus, I analyzed a total of 38 statements by science organizations, 4 following the indictment and 34 following the verdict. The organizations that authored these statements are listed in the appendix. Several of these statements were published in French and Italian. An undergraduate research assistant (Mark Rodriguez) and two graduate students (Jessica Lauren Criales and Raymond Dansereau) assisted me by translating these documents into English.

Some comments are necessary regarding the INGV and its role in shaping the scientific community's response. Among institutions that study earthquakes, Thomson Reuters (2010) ranked the INGV third in both number of papers produced and in number of citations. Because of its familiarity with seismic activity in central Italy, it was not surprising that the INGV would issue prompt and authoritative statements about the case. Indeed, it was the first organization to speak out, both after the indictment and after the verdict.[74] But the INGV also had a clear stake in the outcome of the trial; Enzo Boschi (the president of the INGV) and Giulio Selvaggi (the director of the INGV's National Earthquake Centre in Rome) were among the accused, and the INGV pointed

[74] The INGV's first letter is undated, but references the 3 June 2010 indictment as being "two weeks ago."

out this fact in its plea for "letters of support." Thus, since this chapter relies heavily on statements published on the INGV's site, it's important to note that these statements may not reflect the "true" feelings of the scientific community about the L'Aquila trial. I presume that more politically active scientists were more likely to respond to the INGV's call for "clear and prompt action" and, furthermore, that scientists who agreed with the status quo (i.e., the guilty verdict) were unlikely to bother drafting a collective response that would support the status quo. Yet the INGV did publish one statement, jointly drafted by two national academies, that was agnostic about the culpability of the CGR.

Below I describe three common responses to the trial: (a) misconstruing the nature of the trial, including its legal basis and the current state of relevant scientific research, (b) using the credibility of "science" to frame the guilty verdict as a verdict against science writ large, and (c) distinguishing responsibilities in the prediction process that are scientific from those that are political.

### Misconstruing the charges as failure to predict earthquakes

All four organizations that responded to the indictment stated that the CGR was charged with failing to predict the exact time, location, and magnitude of an earthquake. In a letter to the Italian President, the INGV wrote that "[t]he basis for the indictment" is that the CGR "did not provide a short-term alarm to the population." The letter, which was posted on their website, received 5,165 signatures, many from geoscientists. Similarly, the International Union of Geodesy and Geophysics (IUGG) stated that the indictment was "based on the failure of the Committee to warn the population,” and the American Geophysical Union (AGU) stated that the CGR was being prosecuted “for failure to warn the city of L’Aquila, Italy, before an earthquake.” The American Association for the Advancement of Science (AAAS) added a slight caveat, stating in a letter to the Italian President that the basis of the changes “*appears to be* that the scientists failed to alert the population of L’Aquila of an impending earthquake” (emphasis added).

Each of these statements then defended the CGR by pointing out that it’s currently impossible to predict earthquakes. The allegations were “completely unfounded,” stated the INGV, because “there is currently no scientifically accepted method for short-term earthquake prediction that can reliably be used by Civil Protection authorities for rapid and effective emergency actions.” The scientists were being prosecuted “for failing to do something they cannot do yet—predict earthquakes.” The AAAS stated that the charges were “unfair and naïve” because scientists cannot “credibly” alert the public about earthquakes (perhaps hinting at the incredulity of Giuliani’s warnings), and the AGU stated that the allegations were “unfounded” because “it is not yet possible to accurately and consistently predict the timing, location, and magnitude of earthquakes before they occur.” The IUGG wrote that the CGR had “expressed a scientific opinion based on the available knowledge, which is always limited.” Each of the four responses to the indictment thus framed the trial as being about the current state of scientific knowledge regarding earthquake predictions.

While the organizations correctly pointed out that scientists cannot currently predict the exact time, location, and magnitude of earthquakes, they neglected the fact that de Bernardinis actually made such a prediction, albeit implicitly; apparently following Bertolaso's orders, he told the people of L'Aquila that there was "no danger," i.e., that there would not be a more powerful earthquake anytime soon. Moreover, even though a powerful earthquake is more likely to occur after a moderate earthquake, all four organizations ignored that de Bernardinis told the people of L'Aquila that they were *particularly* safe—that there was less risk, not more. In short, each of the four responses to the indictment focused primarily on the technical aspects of earthquake prediction, simultaneously ignoring that what de Bernardinis told the citizens of L'Aquila was technically incorrect.

It might be argued that the statements by the AAAS, AGU, and IUGG were the product of busy scientists who lacked the time and linguistic skills to fully understand the nature of the charges. Perhaps these organizations relied on the INGV's framing of the indictment. In the ensuing 16 months between the indictment and the guilty verdict, however, enough information had been published in English that even a cursory Internet search would have added more nuance to the trial. In 2011, for example, Stephen Hall had published an article in *Nature* that, according to its editor, "revealed the real issue at the heart of this story … failing to evaluate and communicate the risks of a major earthquake" (American Geophysical Union 2012). In the wake of the guilty verdict, it was apparent that the article (titled "At Fault?") shaped the responses of several science organizations. For instance, three months before the verdict, the AGU had bestowed on Hall an award for the story. Not surprisingly, in its post-verdict statement, the AGU retreated from its position that the trial was about failing to predict an earthquake, instead stating that it was about "manslaughter *in connection with* the L'Aquila earthquake" (emphasis added). Moreover, Hall's article was one of several sources that the Japan Association for Earthquake Engineering referenced in its statement. Whether as a result of Hall's article or other resources, several other science organizations portrayed the nature of the charges against the CGR more accurately.

Surprisingly, just over half of statements issued in the wake of the guilty verdict continued to misconstrue the nature of the charges, stating that the case was about the CGR's failure to predict earthquakes. Some organizations continued to assert this fallacy explicitly. For example, organizations like the European Association for Earthquake Engineering (EAEE) stated that the conviction was "based on the failure of those accused to warn the population" and that that earthquakes "cannot be predicted with our present capabilities and knowledge." The International Association for Earthquake Engineering (IAEE) adopted the same interpretation of the charges—that the CGR had "failed to warn the public." The International Association for Volcanology and Chemistry of the Earth's Interior (IAVCEI) concisely linked this interpretation of the charges to legal accountability: "The scientists did not cause the earthquakes, they could not prevent them, nor could they predict them, so how could they be guilty of manslaughter?"

Perhaps because information to the contrary was readily available, other organizations were more circumspect about misconstruing the nature of the charges. A common strategy was to produce statements about seismology that were true, but not directly relevant to the charges. Consider the following paragraph, excerpted from the middle of the INGV's press release about the verdict:

> According to the international scientific literature, it is impossible to deterministically predict an earthquake. For this reason, asking INGV to indicate how, when and where the next earthquake will strike is not only useless but also wrong because it feeds, in an unjustified way, the expectations of people living in earthquake prone areas.

While the press release does not explicitly state that the trial was about deterministic predictions, this is the clear implication. Why else would the INGV speak about such predictions and make the claim that they are "impossible"? Similarly, the IAEE stated that earthquake science is not "a magical tool that allows anyone to state with any degree of credible reliability when and where an earthquake with a prescribed size will occur."[75]

A related strategy was to acknowledge the nature of the charges, but to reject the distinction between probabilistic forecasts and dichotomous predictions in terms of any practical action. For example, the EAEE acknowledged that "the conviction was not based on the scientists being unable to predict the earthquake but for having issued a

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[75] The IAEE issued two statements after the verdict. The one quoted in the previous paragraph (the CGR had "failed to warn the public") was from one statement and the quote in this paragraph was from the other.

forecast of 'no eminent risk.'" But it then argued that this distinction was specious and misleading:

> The truth is that being unable to predict an earthquake and failing to properly assess the risk are essentially the same thing…. Clearly, the authorities in charge can either evacuate a city (or province) or not; 'a la carte' evacuations are simply not a real option and any such suggestion by the authorities only creates confusion and further tension in a situation that is already critical enough.

Similarly, IAVCEI distinguished between the "prediction of timing, magnitude and impact" of natural hazards and the "estimated statistical likelihoods" of possible scenarios. Yet it defended the CGR with post hoc dichotomous reasoning, stating that scientists "cannot get it exactly right every time." The implication is that if a powerful earthquake did not materialize (the most likely outcome), then the message of "no danger" would have been correct and there would have been no trial. These statements thus framed the trial in terms of Type I vs. Type II errors—that the CGR's options were either to declare "danger" (with a ~98% chance of being wrong) or "no danger" (with a ~2% chance of being wrong). Government officials, however, did have other options besides "to evacuate or not to evacuate." For better or worse, communicating probabilistic forecasts would have given the citizens of L'Aquila more control in managing risk. As well, it's possible that no risk communication whatsoever would have led to a lower body count than the dichotomous prediction that was offered.

### Defending scientists, defending science

Many science organizations defended the CGR by focusing on favorable personal or professional attributes of particular members of the CGR (i.e., appeals to authority).

The IAEE, for example, stated that the defendants were all "well known in their respective professional fields, and enjoy[ed] the respect of their peers." Other organizations singled out members of the CGR as "brilliant," "eminent," "prominent," "highly respected," or "internationally renowned." The authors of the IAEE statement then distinguished the members of the CGR from less reputable individuals. Weak earthquakes, they wrote, have never been linked to deterministic predictions of powerful ones, "except in the imagination of publicity seekers with no scientific credentials or on account of pure coincidence." Similarly, the Italian Physical Society (IPS) noted that only "charlatans and buffoons" believe they can predict earthquakes.

As opposed to appealing to the authority of individual scientists, some organizations defended the CGR by appealing to the authority of science itself. For example, the Società Italiana di Fisica (SIF) wrote that the sentence appears, "on the basis of all available scientific knowledge, to be a serious mistake." Similarly, Austria's Central Institute for Meteorology and Geodynamics wrote that the verdict "must be utterly rejected as it lacks scientific reasoning." Though the statement did not explain what "scientific reasoning" was or how it might exonerate the scientists, it "endorse[d] the content of the statement issued by the INGV"—which, as shown above, subtly conflated deterministic and probabilistic risk assessment. It was enough to say that "science" proved the CGR's innocence. The Austrian letter was only three sentences long.

Another approach to the L'Aquila trial was to frame it as an attack on science and scientific values. Following the indictment, the AAAS wrote that the Italian courts were "subjecting scientists to criminal charges for adhering to accepted scientific practices." Such concerns were particularly salient in the wake of the guilty verdict. The SIF put it succinctly: "the sentence imposed in L'Aquila is … a condemnation of the scientific method." The Italian courts, declared the International Association of Seismology and Physics of the Earth's Interior (IASPEI) and the Center for Disaster Management and Risk Reduction, had prosecuted "the free expression of scientific opinions." And the Geological Society of Japan wrote that the members of the CGR were found guilty for their "willingness to help in the risk assessment." Such comments were glosses regarding the nature of the L'Aquila trial. Side-stepping the actual complexities of the case, these organizations framed the verdict as the successful criminalization of benevolent and mundane scientific activities.

Layered upon the specific threat that the trial posed to the CGR, many organizations discussed the trial's broader implications on science–society relations. The AAAS, for example, warned the Italian president that the trial would have a "chilling effect on researchers," and the INGV wrote that the sentence "risks to undermine the foundations of scientific research." Other science organizations warned that the verdict would set a "terrible," "disturbing," and "dangerous" precedent. "There is now NO incentive," remarked the Seismological Society of America, "for Italian geoscientists to participate in national efforts to 'forecast and prevent' major risks." "Which scientist," asked the INGV, "will express her/his opinion being consciously aware he/she could go to jail for doing so?" In essence, these organizations asserted that the conviction would have a very specific negative effect—scientists would no longer be able to do science.

## Delineating roles and responsibilities in the risk-management process

Several organizations invoked the idea of a "chain" of responsibility, in which scientists and government officials play distinct roles. The statement by the Geophysics Research Group at the University of Ulster, for example, outlined "a sequence of measures which begins with scientists but ends with local and national government":

> 1) the careful scientific identification and assessment of the hazard
> 2) the detailed zonation of this hazard using the best current scientific principles
> 3) the development of rigorous building codes based on the assessment of the hazard
> 4) the scrupulous application of these codes not only to the construction of new buildings but to the retrofitting or demolition of old ones
> 5) planning and education for the threatened citizens which prepares them in every way possible for future events

The Geophysics Research Group then stated that "the failure in L'Aquila … came at the end of the chain where the responsibility moves away from the scientists and on to those who are responsible for planning rather than providing the scientific basis for planning." IASPEI wrote similarly of different "roles and responsibilities in the earthquake mitigation chain," and the Scientific Council of the Observatory of the Coast d'Azur wrote of responsibilities along a "chain of decisions."

Such references to a "chain" served two related purposes. First, they distinguished between various roles and responsibilities in the risk-management process—between scientists, who do risk analysis, and government officials, who make decisions based on that analysis. Scientists, stated the AAAS, "can identify areas of risk and inform government preparedness plans and policies." Governments, on the other hand, are responsible for "the execution of such plans." Similarly, IASPEI stated that scientists "have the duty to provide the state of knowledge in a comprehensive and unbiased fashion, to enable authorities to take the required mitigation actions." And the INGV stated that "prevention actions" fall under the jurisdiction of "national and local authorities."

The notion of a "chain" also placed these roles and responsibilities along a timeline, with scientists and government officials both having clear long-term responsibilities (items 1–4 in the Geophysics Research Group's typology), but only government officials having clear short-term responsibilities (item 5). Regarding long-term responsibilities, many science organizations argued that the CGR had done its part to reduce mortality but that the government could have done more. In particular, several statements mentioned the fact that members of the CGR had done exemplary work by producing a seismic hazard map that identified central Italy as at high risk for powerful earthquakes (items 1–2). But many organizations noted that the government, by not enforcing adequate building codes, had abnegated its responsibilities in the middle of the chain (items 3–4). The INGV, for example, blamed the tragedy in L'Aquila on "seismically unreinforced buildings." IASPEI stated that casualties were due to "the collapse of poorly built or maintained buildings." And the Royal Astronomical Society stressed the need "to reduce the vulnerability of buildings to shaking in earthquakes."

Regarding short-term responsibilities ("planning and education" in the Geophysics Research Group's typology), science organizations focused instead on communication from scientists to government officials, then from government officials to the public. Euroscience's comments on communication were typical:

> When public hazards are at stake, the authorities take advice from scientists. Scientists [have] the responsibility to scrutinise scientific evidence and to arrive at the best possible advice …. To issue evacuation orders and to ensure that the public knows about and is trained in preventive and damage reducing measures, is the responsibility of the public authorities at national, regional or local level. It is not the role of scientists.

The goal here was to distance the CGR from any accountability for what they said by arguing that public communication was not part of their job.

A potential difficulty for science organizations was that the CGR had issued a public statement that there would not be an earthquake. As described in the previous sections, some organizations side-stepped issues related to risk communication, instead focusing on risk analysis or appealing to the authority of science or scientists. Another strategy was to acknowledge that the CGR had communicated some problematic statements, while belittling any impacts of what was said. The Geophysics Research Group, for example, cast off the CGR's poor risk communication as "an inappropriate comment in communicating a basically sound scientific conclusion." Similarly, Euroscience stated that the CGR had communicated what anyone could "reasonably" state about seismic risk—"that there is a *likelihood* of a big earthquake not too far in the future" (emphasis added). "The issue," continued the statement, "is about what justified or unjustified reassurance is instilled by such communications in the public, and that is certainly not an area for criminal prosecution." Instead, Euroscience situated any blame on public authorities, who bear the responsibility of communicating risk in an intelligible manner.

Yet another strategy was to split the CGR into those who were innocent and those who might be guilty. In particular, several organizations neglected to defend de Bernardinis—the CGR's sole "engineer," its head, and the government official responsible for public communication.[76] The International Council for Science and the Geological Society of America took this one step further, writing out the names of the six scientists and stating that an injustice had been done to them. The Japan Association for Earthquake Engineering (JAEE) was more direct; the JAEE declared that, at the CGR meeting, "the six experts" fully enacted their "responsibility … to provide information" and "to support decision-making," whereas "the [DCP] and the local government," in their media interactions, "downplayed the seismic risk." The judge, continued the JAEE statement, had "shifted the responsibility of the Department of Civil Protection and the City of L'Aquila to the experts of the [CGR]." The implication is that the scientists had fulfilled any short-term responsibilities that they might have had, whereas de Bernardinis and other DCP officials had failed in their short-term responsibilities to protect the public.

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[76] As an exception, the statement by the National Academy of Science of Ukraine only mentioned Selvaggi and Boschi by name, declaring that the judgement against them was "erroneous." The Ukrainians may have singled out Selvaggi and Boschi because they viewed an explicit defense of the two defendants who were INGV employees as particularly relevant to a letter addressed to the INGV.

Although most statements presumed that the scientists’ had fulfilled their short-term responsibilities, there were some exceptions. In a joint statement, the German and French National Academies suggested that the events preceding the earthquake be “reconstructed comprehensively, precisely and objectively” to assess “whether the persons involved performed their duties appropriately in the situation in question.” Such agnosticism was rare. Perhaps organizations that were unsure of the CGR’s culpability were less likely to issue statements that criticized the verdict than organizations that presumed the CGR’s innocence.

A few statements stood apart from the others in that they made explicit a doctrine of scientific immunity, whereby scientists should not be held legally accountable for their short-term risk communication, even when they say something that is scientifically unfounded. For example, the statement by the Seismological Society of Japan (SSJ) began by acknowledging that “the charges were not based on the inability to predict the earthquake, but instead on negligence in the analyses of the situation and insufficient communication of information.” It also stated the CGR had issued a “safety declaration” and that “as a result of the notice there were many victims of the earthquake.” Nonetheless, the SSJ asserted that when scientists “make honest contributions to efforts in natural disaster reduction, they should not be held liable for crimes.” Similarly, the International Council for Science (ICS) explicitly stated that both government officials and scientists might have played a role in endangering the citizens of L’Aquila. In particular, the ICS acknowledged that “lives might have been saved if the public authorities had reacted differently before the event" and that "the role of scientific advice in the decision-making processes prior to the earthquake is a legitimate area of enquiry." But, as mentioned above, the ICS explicitly defended the six scientists on the CGR, but not the one government official. Like organizations that invoked a "chain of responsibility," the ICS asserted that "blaming scientists and scientific advice for the deaths that occurred in L'Aquila is a grave error." Thus, the difference between the statement by the German and French National Academies and these statements was that the former left open the possibility of blame, whereas the latter closed it off, even as they acknowledged that harm had been done.

## Discussion

My main contribution in this chapter has been to show how the scientific community responded to the L'Aquila trial: misconstruing the nature of the charges, appealing to scientific authority, and stating that the scientists on the CGR had fulfilled their responsibilities. Here I discuss these responses in terms of "innocence" vs. what I call "scientific immunity." I also discuss the responses in terms of a wider theme in this dissertation—scientists' lack of norms for crisis communication.

### Innocence vs. scientific immunity

Although a few science organizations produced disinterested statements about the criminal liability of the members of the CGR, the typical response was to argue, based on information not directly relevant to the trial, that the manslaughter charges were groundless. A long line of sociological research has explored scientists' efforts to bolster their credibility as individuals or their profession more generally (Bucchi 2017; Gieryn 1983, 1999; Hilgartner 2004; Kinchy and Kleinman 2003; Kotcher et al. 2017; Latour and Woolgar 1986; Mulkay 1976). So it was no surprise that science organizations appealed to authority, whether of the particular scientists on the CGR or to the profession and method of science writ large. Emphasizing the credibility of the "scientists" while discounting that of "charlatans" was a textbook case of boundary-work.

A more surprising finding was that so many science organizations claimed that the trial was about the CGR's failure to predict earthquakes. One tempting explanation for this claim is that it, too, was rooted in "boundary-work," with scientists saying whatever it took to overturn the verdict, even if that meant *deliberately* omitting unflattering evidence against the CGR or misconstruing the nature of the charges. Boundary-work is, as Gieryn (1999:23) put it, "strategic practical action," and scientists surely had an interest in protecting one another from criminal liability.

But most defenses of the CGR may not have been so deliberate. Similar to Kinchy and Kleinman's (2003:875) observations of how some scientists approach advocacy, commentators may have engaged in boundary-work "routinely and relatively unreflectively" (see also Bourdieu 1991). Consider the striking similarity of statements. Many organizations invoked identical values, such as "freedom of investigation," that they viewed as relevant to the trial. The AAAS copied verbatim, without citation or quotation marks, portions of the INGV's post-indictment statement. And some statements were brief, only long enough to endorse some other statement (typically that of the INGV). As such, I suspect that many organizations formed their opinions about the trial through "mimetic isomorphism" (DiMaggio and Powell 1983) and not through independent deliberation on the particulars of the case.

As defined by DiMaggio and Powell (1983:151), the key driver of mimetic isomorphism is "symbolic uncertainty," and there were two factors that left many scientists unsure of how to respond to the L'Aquila trial. First, because scientists had never been criminally prosecuted for their utterances in the context of crisis communication, there was no model for evaluating the CGR's potential liability. (I say more about this in the next section.) Second, all source materials (trial documents and courtroom video) were in Italian. Thus, science organizations were confronted with an unprecedented situation that, for most of their members, was playing out in a foreign language.

To reduce these sources of symbolic uncertainty, scientists may have looked to one another for guidance in crafting their responses. Even though it would have taken minimal effort to seek out nuanced coverage of the trial in other languages, scientists may not have bothered to consult materials that challenged their preconceptions. Organizational responses to the L'Aquila trial may thus have reflected the "general dispositions of participants in the scientific field rather than calculated efforts" (Kinchy and Kleinman 2003:870). That is, through confirmation bias, scientists may have consumed and published information that ascribed absurdity to the trial, namely, that it was premised on the mistaken belief that earthquakes can be deterministically predicted in the short-term. Particularly illuminating was the fact that several organizations issued public statements denouncing the verdict while also explicitly acknowledging their ignorance of the full context of the charges. This seems to suggest that these organizations were guided by a *pro scientia* bias, that is, predispositions that science and scientists are generally "good."[77] The point here is that although commentators, scientists and non-scientists alike, used mischaracterizations of the trial as grounds for asserting that the CGR should be exonerated,[78] such mischaracterizations may or may not have been deliberate. In either case, the result was an argument for the CGR's innocence.

In contrast, a few science organizations explicitly acknowledged that the CGR's risk communication had caused some loss of life and simultaneously advocated the notion that they should be immune from any legal accountability. This was a novel argument for a novel situation. Similar to the way that diplomats are immune from being prosecuted while traveling in foreign nations, the main tenet of what I call "scientific immunity" is that scientists cannot be held legally accountable for what they communicate to the public, even when what they communicate is scientifically

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[77] Regarding the general public's positive dispositions toward scientists, 93.2% of Americans agree or strongly agree that "scientific researchers are dedicated people who work for the good of humanity" (Smith et al. 2012), and 73% of Italians feel that "politicians should rely more on the advice of expert scientists" (European Commission 2005). For research on scientists' dispositions about science, see Anderson et al. (2010).

[78] See Yeo (2014) for a survey of mischaracterizations more generally, including media coverage. Also see Pielke (2012).

unfounded. The last clause of the definition is what distinguishes immunity from innocence. An argument for innocence implies that no wrongdoing took place. Immunity, on the other hand, functions like a get-out-of-jail-free card, which is played after guilt has been established.

Although only a few statements explicitly advocated scientific immunity, many others nonetheless furthered it, even if their authors did so unwittingly. Comments about the trial’s “dangerous precedent” or its “chilling effect” on science–society relations promoted the idea that, as Jackson (2012:14) put it, “scientists may refuse to share their expertise unless they themselves are held harmless.” Such a doctrine of immunity would grant scientists protections that other professions lack. Within medicine, for example, claims of “malpractice” are a routine bureaucratic process, with physicians in various jurisdictions (e.g., some states in the US) being required to purchase malpractice insurance. As I showed in Chapter 2, scientists tend to use the phrase “misconduct,” not “malpractice,” to describe wrongdoing. Moreover, norms of conduct have been institutionalized in the context of study implementation and scholarly communication, but not in the context of public communication.

### The lack of norms for crisis communication

The L’Aquila trial showed that whether the CGR should have said something different was a legal and moral question. But the scientific community’s response revealed that this was also a normative question: to what norms can scientists appeal to hold one another accountable for short-term seismic forecasts?

Table 6 summarizes what science organizations identified as roles and responsibilities throughout the earthquake forecasting process, by the temporal "reach" of forecasts.[79] With respect to long-term forecasts (on the order of years), science organizations described two communication threads in which scientists might play a role. Borrowing Pielke's (2007 ch. 1) terminology, I dub these roles the "Pure Scientist," who "focuses on research with absolutely no consideration for its use or utility," and the "Science Arbiter," who provides technical advice to decision-makers to help answer specific policy questions. Both roles are part of a linear model of science, in which information flows from scientists to the public, mediated by government officials. For example, in the context of earthquake forecasting, the Pure Scientist might produce a seismic hazard map and publish it in an academic journal.[80] Seismologists become "Arbiters" when they are called upon to provide government officials with such maps or with some other characterization of long-term risk. In either case, both communication threads make it clear that government officials, not scientists, are tasked with acting to improve public health, typically through building codes.

Table 6. Scientists' roles and responsibilities in the earthquake forecasting process, according to science organizations, by temporal reach of forecast

| **Temporal Reach of Forecast** | |
|---|---|
| **<u>Long-term</u>** | **<u>Short-term</u>** |
| Pure Scientist → Knowledge → Govt. → Public<br>Govt. → Science Arbiter → Govt. → Public | Charlatans → Govt. → Public<br>Charlatans → Public |

[79] "Reach" is one of Mische's (2009) nine dimensions of future-oriented thinking.

[80] Judge Marco Billi (2012:333) pointed out that members of the CGR had done just this in their scholarly publications: Boschi et al. (1995:1481) had predicted, with "a P virtually equal to unity" (i.e., a 100% chance) that a M ≥ 5.9 earthquake would strike the L'Aquila region within the next 5 years.

With respect to short-term forecasts, science organizations generally focused on yes/no “predictions” of whether an earthquake would strike a particular area in the immediate future (hours to days). Although only one organization used the word “charlatan,” numerous organizations echoed the famous words uttered by Charles Richter—that those who make such predictions are “charlatans, fakes or liars” (Sharbutt 1971). The strategy here was to align the members of the CGR with “credible” scientists, that is, those who would never make short-term predictions regarding seismic risk and, therefore, would not have any role in communicating such predictions to government officials or the public.

But there are several reasons why the complexity of what happened in L’Aquila does not fit neatly into Table 6. First, while it’s true that the members of the CGR were tasked with being Science Arbiters, the meeting was an “emergency” one (as defined by Italian law) and their advice was intended for immediate, practical use: what should people do if they feel a tremor? In other words, the public was not interested in a long-term discussion of risk but a short-term one. They were expecting the members of the CGR to be Science Arbiters in the context of crisis communication.

Second, although the public received some advice that they could apply immediately, that advice did not flow linearly from scientists to the public, mediated by government officials. In contrast to a “linear model of science,” in which scientific advice shapes decisions, the risk communication that took place in L’Aquila might be better characterized by a “linear model of politics,” in which political machinations shape scientific advice. Or, as Scolobig et al. (2014) has argued, scientific advice and decision-making in L'Aquila were "co-produced."[81] Regardless, the *official* short-term prediction process (in response to Giuliani's unofficial "charlatan" prognostications) more closely resembled these communication threads:

Govt. → "Scientists" → Public
Govt. → Issue Advocate → Public
Govt. → Science Arbiter → Public

Notably, all three threads begin with government officials and place scientists in direct contact with the public. In the first thread, I have placed "scientists" in quotes to emphasize that the CGR's presence in L'Aquila was more symbolic than substantive, giving credence to an already-established talking point. Indeed, the key message that emerged, which was produced before the CGR even met, was aligned with Bertolaso's framing of risk. The "Issue Advocate" in the second thread is another role in Pielke's (2007 ch. 1) framework, describing experts who use technical information to advance a specific political agenda. This thread holds true to the extent that the scientists on the CGR were complicit in Bertolaso's "media operation … to calm the public." The third thread is similar to the model that most science organizations proposed in the context of long-term earthquake forecasts, but acknowledges that the members of the CGR communicated a short-term forecast to the public.

The point here is that although the L'Aquila trial probed scientists' responsibilities in the context of short-term forecasts, science organizations emphasized

[81] Jasanoff (2004), who popularized the idiom of "co-production," wrote that "society cannot function without knowledge any more than knowledge can exist without appropriate social supports."

responsibilities in the context of long-term forecasts. That their emphasis was on long-term responsibilities, for which scientists are unlikely to be held accountable, is telling. As Grundmann (2012) has pointed out, the longer the time gap between problematic statements and the events that prove them wrong, the easier it is to draw attention to other factors that may have caused harm, e.g., to inadequate implementation of building codes.

Another reason why organizations tended not to discuss scientists' short-term responsibilities for public communication of risk may be attributed to a lack of norms. As Sellnow et al. (2017:1) have pointed out, finding "the right words … has vexed the scientific community of earthquake experts … for a long time." While some of this difficulty is rooted in technical uncertainty,[82] some of it is rooted in normative uncertainty. Consider that, regardless of what scientists say, the public may interpret any risk message (or lack thereof) as "danger" vs. "no danger." On the one hand, if scientists emphasize that the probability of a disaster is still relatively low, the public may place its concerns about earthquakes alongside a host of routine issues that demand attention (Best 2012; Downs 1972; Hilgartner and Bosk 1988), such as heart disease, teenage pregnancy, and potholes. On the other hand, if scientists emphasize that seismic risk is higher than usual after a moderate earthquake, but nothing happens,

[82] Researchers in communication have shown that different ways of communicating risk—numbers vs. words and the choice of words—yield different understandings of risk and, thus, different decisions (Budescu, Por, and Broomell 2012; Harris and Corner 2011). Thus, whether a different choice of words would have saved lives in the L'Aquila case may be a counterfactual question with an empirical answer.

they may be accused of being "alarmists" who produced false positive statements. As Hough (2009:220) has noted, "the line between responsible communication of concern and overly alarmist rhetoric is fine to the point of invisibility." Moreover, public reactions to information about risk may depend not only on what is said, but also on things like the trust that the public has in government officials and the existence of public statements by counter-experts.

Given these complexities and without the benefit of hindsight, would other scientists in the CGR's position have said anything different? The uncomfortable answer is that scientists lack a clear vision of what crisis communication about earthquakes should look like. One reason why the trial proceeded as far as it did was that calls for public accountability for crisis communication preceded the emergence of professional standards.

## Appendix: Science organizations that responded to the trial

Academy of Sciences of the Czech Republic
*American Association for the Advancement of Science
**American Geophysical Union
Australian Academy of Science
Center for Disaster Management and Risk Reduction Technology
Central Institute for Meteorology and Geodynamics, Department of Geophysics (Austria)
European Association for Earthquake Engineering
European Geosciences Union Division on Seismology
European-Mediterranean Seismological Centre
Euroscience
Geophysics Research Group at the University of Ulster
German National Academy of Sciences Leopoldina, French Académie des sciences
Helmholtz Centre Potsdam - German Research Centre for Geosciences
**Istituto Nazionale di Geofisica e Vulcanologia (Italy)
International Association for Earthquake Engineering

International Association for Volcanology and Chemistry of the Earth's Interior
International Association of Seismology and Physics of the Earth's Interior
*International Union of Geodesy and Geophysics
Japan Association for Earthquake Engineering
L'Association Française du génie ParaSismique
Le Conseil Scientifique de l'Observatoire de la Côte d'Azur
National Academy of Sciences of Ukraine
National Academy of Sciences (US), The Royal Society (UK) (coauthors)
National Survey of Seismic Protection (Armenia)
Royal Astronomical Society (UK)
Seismological Society of America
Seismological Society of Japan
Società Italiana di Fisica
Swiss Seismological Service
The Geological Society of America
The Geological Society of Japan
The International Council for Science
Yerevan State University of Architecture and Construction

Notes:
All statements, unless otherwise indicated, were published after the guilty verdict. The European Geosciences Union (EGU) and the International Association for Earthquake Engineering both issued two statements about the verdict. The EGU's second statement, published on 3 March 2013, is the most recent one in the sample. All other post-verdict statements were published between 22 October 2012 and 24 November 2012.
*Only published a statement after the indictment
**Published a statement after the indictment and after the guilty verdict

# Chapter 5

# Conclusion: The Scientist's Dilemma

*Thou shalt not kill, but needst not strive*
*Officiously to keep alive.*
—Dr. Colenso Ridgeon, quoting Arthur Hugh Clough's poem "The Latest Decalogue"
within George Bernard Shaw's play *The Doctor's Dilemma*

## Chapter Summary

In Chapter 2, I explored the "moral background" of science, that is, factors that "facilitate, support, or enable" moral judgments and beliefs about science (Abend 2016:16–17). To determine which issues are "on the menu" and which are not, I examined consensus reports published by the National Academy of Sciences. By examining two groups of reports—those that discuss the nature of science and those that address risk communication—I found that some moral issues are addressed more explicitly than others. In particular, the reports emphasize the importance of ethical considerations throughout study design and implementation, but say rather little about the ethics of public communication. Notably absent from *On Being a Scientist*, a guidebook on the nature of science, are any professional standards for public communication.

One reason for the lack of standards is that, depending on the technical and political context, the public communication of science may be controversial. Under such circumstances, the incentives for reaching out to the public may be overpowered by disincentives, such as being accused of bias or losing funding. Indeed, while earlier editions of the guidebook framed public communication as an unproblematic “responsibility,” the most recent edition frames it as a “right” whose invocation may result in scientists being perceived as biased advocates. This suggests either that public communication became more politically charged over time or that the report writers became more attuned to potential moral dilemmas that existed all along.

In Chapter 3, I explored the “moral foreground” of science by asking academic scientists about appropriate and inappropriate ways of communicating risk to the public. Scientists, particularly those who lacked tenure, tended to view more direct forms of public engagement as personally risky. However, this varied according to the availability of information about a hazard, the nature of the hazard, and its broader political context. Regarding available information about hazards, many scientists discussed structural and material constraints to conducting policy-relevant science, resulting in a paucity of information to communicate. In particular, they lamented that the academy and its funding streams are structured to answer research questions that are disciplinary in nature, which tend not to have any immediate applications.

Respondents generally felt that it was appropriate to reach out to the media to communicate information about immediate threats like looming hurricanes, but expressed reluctance to do so about seemingly intangible threats like climate change. Even though climate change has been linked to more frequent and more powerful storms, they noted that the public may not view climate change itself as a crisis—if they even view it as real. Scientists feared that by reaching out to the media about intangible threats, university administrators, their fellow scientists, or members of the general public might accuse them of seeking the limelight or of being biased. Notably, this occurred in the aftermath of Hurricane Katrina. To communicate information about what went wrong and why, several scientists had to assume the role of “whistleblowers.” As I noted in the chapter, whether risk communication constitutes “whistleblowing” depends on other people’s response. Various forensic investigations into the Katrina disaster received varying levels of support from their universities. Whereas UC Berkeley supported its forensic team, “Team Louisiana” received a cold response from LSU. Apparently fearing threats to the university’s funding streams, administrators at LSU tried to prevent scientists from communicating politically inconvenient findings.

Chapter 3 could be summed up as an examination of scientists’ fears about being placed into the “Chicken Little” category for exaggerating risk. I explored the opposite phenomenon Chapter 4—the public’s response to scientists accused of downplaying risk. In an unprecedented trial, the State asserted its control over scientists who communicated risk to the public, convicting seven scientists and engineers of manslaughter. In an unprecedented response, science organizations from around the world defended the experts’ poorly-worded statements. They did so in three ways: (a) misconstruing unflattering information, (b) appealing to the authority of “science” writ large, and (c) distinguishing responsibilities that are scientific from those that are political. I framed these responses in terms of “innocence” vs. “scientific immunity.” Additionally, contributing to the overarching theme of the dissertation, I argued that scientists were responding to a situation that made them uneasy. The lack of norms for crisis communication blurred the line between appropriate and inappropriate remarks. Organizations that advocated for immunity pushed back against what they viewed as encroachment into the definition of, and responsibilities associated with, public science.

## Discussion

By focusing on moral discourse within the scientific community—at the level of individuals (Chapter 3), organizations (Chapter 4), and institutions (Chapter 2)—I have drawn the institutional logics perspective (ILP) back toward its value-rational roots. With one notable exception (Moore and Grandy 2017), the contemporary ILP literature has not addressed the ways in which moral issues shape people’s actions. Instead, ILP researchers have sought to explain human behavior in terms of the rational pursuit of goals. Thornton et al. (2012:2–3) have stated that different institutional logics shape “how rationality is perceived and experienced” and that “rationality varies by institutional order.” But I have argued that institutional logics, as transmitted by organizations, also shape how morality is perceived and experienced and that morality varies by institutional order. This is in line with the “new sociology of morality,” which points out that “religions, occupations, generations, educational categories, organizations, and social movements can all have their own moralities” (Hitlin and Vaisey 2013:53).

By introducing the logic of "public science," I have helped to bring issues of morality back in to the ILP. For academic scientists, the public communication of risk falls into the moral category of "supererogatory acts," which, according to Heyd (2016), go "beyond the call of duty"—to that which is not "obligatory," yet still "morally good." To clarify the distinction, Heyd contrasted firefighters, whose job it is to rush into burning buildings to save people, with untrained bystanders who do the same thing. A firefighter might invoke the expression "I only did my duty," but bystanders who say the same thing might come off as insincere or haughty because it was not, in fact, their "duty." The ILP can account for the firefighter's actions by invoking the instrumental attainment of organizational goals, but it cannot account for the bystander's actions, which are driven by value-rationality.

Academic scientists who communicate with the public about intangible hazards are more closely related to bystanders than to firefighters. In terms of obligations, the chief duty of academics is to act in accordance with the logic of "academic science" and publish journal articles. It's what they are trained to do, and the number, quality, and "impact" (i.e., citations) of publications go hand-in-hand with a successful career in academia. In contrast, academics have no obligation to do "public science," and they often have no training or incentive to do so. Indeed, as Oreskes (2013) put it, researchers are "a group singularly ill-equipped to communicate effectively to ordinary publics, particularly about issues that trigger alarm or fear." As I showed in Chapter 3, public communication also meets the other criteria for supererogation. That is, even though universities typically provided little support for public communication, some academics nonetheless viewed it as "good," particularly when human lives were at stake.

I took this one step further by showing how supererogation produced moral dilemmas for academic scientists. As Burawoy (2014:279) has noted, public communication can be a "precarious engagement" for academics:

> It is precarious because of the time it requires, the sacrifices it demands and the professional hostility it can arouse, all of which can jeopardize an academic career. It is precarious because on entering the political field, the academic faces a game with very different rules and sometimes no rules at all. It is playing with dynamite. Finally, it is precarious because in disturbing common sense it can incite vicious attacks, public humiliation and even death threats.[83]

I found that the nature of a hazard and the temporal reach of a forecast shaped the extent to which scientists viewed risk communication as precarious. Whereas scientists had few inhibitions to communicating about immediate threats like hurricanes and oil spills, communicating with the public about abstract and intangible threats like climate change posed a moral dilemma, prompting some scientists to remain quiet about what they perceived as threats to public health.

Comparing the Katrina and L'Aquila cases helps to elaborate now norms of communication vary by hazard and the temporal reach of forecasts. In the long term, earthquakes and hurricanes are similar in that they both tend to materialize where they have in the past, which allows scientists to produce hazard maps. But because there is

[83] Burawoy was writing specifically about "public sociology," but he hinted at the fact that his observations apply to academics more generally.

no immediate threat, academics may have difficulty communicating with the public about both hazards, unless a disaster has recently occurred (Birkland and DeYoung 2013).

In the short term, it’s more difficult to communicate about earthquakes because scientists can’t predict them as readily as hurricanes. While it’s typical to receive a reliable warning, several days in advance, of when and where a hurricane will make landfall, seismologists are only able to provide, at best, a few minutes’ advanced warning to people who are far from the epicenter of an earthquake. As a result of these differences in the actionability of crisis communication, the public may be less interested in information about earthquakes than hurricanes, making crisis communication about earthquakes more difficult.

The aftermath of a moderate earthquake offers an important exception. In the L’Aquila case, some people were concerned about the possibility of an “aftershock,” and experts were called on to provide a short-term forecast of seismic risk. For a variety of reasons, communicating the scientific consensus—that the probability of a powerful earthquake was higher, but still relatively low—was difficult. Yet the experts, as members of a government-appointed committee, were more like firefighters than bystanders. It was their job, and their legal responsibility, to understand and communicate risk.

My primary interest in the L’Aquila case, however, was not about the actual legal responsibilities of the experts, but about how the scientific community framed those responsibilities. As I showed in Chapter 3, ILP researchers have generally treated moral discourse (about duty, responsibility, right vs. wrong, etc.) as a means to an end. That is, the literature has shown how some organizations engage in discourse about "social responsibility" primarily to present an *image* of social responsibility. The L'Aquila case elicited the opposite phenomenon: discourse about the *absence* of social responsibility. Science organizations depicted the defendants and scientists more generally as bystanders in the context of crisis communication. Scientists should not be held accountable for any negative events that occur as a result of what they say, the organizations argued, because public communication goes beyond their duty as scientists.

While science organizations may have issued statements with the specific purpose of exonerating the defendants, the broader argument that scientists are not responsible for crisis communication is partially rooted in the lack of norms for public communication. Thus, mitigating moral dilemmas for scientists would entail transforming risk communication from a supererogatory act to a mundane duty. This transformation would require more training in how to communicate with the public and incentives for doing so, perhaps by making popular articles and media appearances "count," in a positive way, in tenure decisions. To the extent that scientific information helps people to manage risk, such efforts to normalize public communication would ultimately improve public health.

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