The Expansion of Science Policy in the United States in Three Cases: rDNA Research, The Human Genome Project, and the National Nanotechnology Initiative

by

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DEDICATION

I would like to dedicate this dissertation to everyone who got a late start.

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TABLE OF CONTENTS

LIST OF TABLES

TABLE

LIST OF GRAPHS

GRAPH

LIST OF FIGURES

FIGURE

LIST OF APPENDICES

APPENDIX

ABSTRACT

In recent decades, the agencies tasked with science funding and science policy in the U.S. have increasingly embraced new ideas about the role and duty of science in society. They have opened up to the idea that science and technoscience -- the intersection of science and technology -- have duties to the public beyond simply providing discoveries and innovative technologies. This is reflected in changes in an expansion of science policy to accommodate new concerns, like ethical and societal implications, and new actors, including lay publics. In this dissertation, I trace these changes historically through three emerging technoscientific projects: recombinant DNA, the Human Genome Project, and the National Nanotechnology Initiative. I show that while each of these cases involved a significant expansion in what was considered acceptable science policy, those expansions were met with efforts to constrain the degree of change they brought about for technoscientific development. The constraints were intended to protect scientific authority and autonomy in the face of the changes that the expansion of science policy brought with them. This dissertation questions the degree to which upstream changes to science policy – those motivated from the top-down by scientists and science agency administrators – will bring about critical reflection by policymakers about technoscientific development and governance.

CHAPTER I

Introduction

In this dissertation, I will argue that there have been changes over the last several decades in what scientists and decision-makers at science funding agencies in the United States consider relevant for policymaking around science and technology. Since the 19th century, scientists in the U.S. often saw their work as existing apart from politics and the larger social world, except for the social good that their discoveries provided. With the rise of environmental movements and public activism around issues of science and technology, American scientists and those in the agencies that support them began to change their view. This led to changes in what was considered acceptable science policy, from a purely technocratic focus on scientific concerns to increasingly broader concerns about the ethical and societal implications of research and the role of publics in science policy. Additionally, this led to new programs for investigating these questions and the inclusion of new actors, such as ethicists, social scientists, and publics, who came with forms of expertise that scientists and science agencies had not previously regarded as relevant. These changes could have limited scientific authority and autonomy. However, whether and to what extent which this occurred is an empirical question that this dissertation will answer. Two questions guide the research for this dissertation. First, why did attention to ethical and societal concerns and public engagement emerge for some research projects and not others? Second, how did scientists and science agencies negotiate their autonomy and authority in the context of these new non-scientific actors and non-technical concerns?

This dissertation charts three moments where ideas about what constitutes acceptable science policy shifted. In the Recombinant DNA (rDNA) debates of the 1970s, scientists decided to postpone their research in order to give them time to consider the health and safety issues posed by their research. They did not, however, consider as relevant or legitimate any broader ethical concerns or the participation of non-scientists. In the second case, the Human Genome Project of the 1990s incorporated new actors, bioethicists and scientists acting as ethicists, but fought against the idea of a possible moratorium like that of the rDNA debates. In the final case, the National Nanotechnology Initiative re-negotiated the role of professional ethicists and included another new actor, the lay public. It too excluded any possibility of a moratorium, though for different reasons than the Human Genome Project. In these three cases, we see how ideas about what was considered acceptable science policy has shifted. As I will show in this dissertation, these suggest that scientists have increasingly accepted non-scientific actors and concerns in order to maintain their autonomy against social and political influences.

This chapter introduces the theoretical tools that I will use in this dissertation. I begin by qualifying the data for this dissertation and the discussion to follow as being particularly American. Other national contexts, such as England, France, and Germany, have had their own trajectories in changing science policy. This also helps us understand the important relationship between science and democracy and how they co-construct each other. I then turn to a discussion of credibility, expertise, and autonomy. Contests around credibility and expertise have often been used to set the boundaries of science, which, in the context of this dissertation, have been used to influence science policy and governance. Finally, I will discuss certain key ideas in science and technology studies (STS) scholarship, which I will use to make sense of these policy changes.

Before I move on to the substance of discussion, I should make a note about some of the terms I will be using and how I will be using them. I will refer to the research for each of these cases as “technoscience.” Technoscience acknowledges the inseparable connections between scientific research and the technologies it produces. Technoscience avoids the idea of science as a purely epistemological endeavor and the idea of technology as a purely instrumental one. Where I refer to *the public*, I do so because that is how the actors I study imagined the totality of people outside of their domain. As Dewey argued, there is no one public, but rather many different and overlapping publics who come into existence around issues of public concern (Dewey

[1927]2012). It would be inaccurate, however, to impute this understanding onto actors who evidence a monolithic view of the public.

Expansion of Science Policy

This discussion of expanding ideas about acceptable science policy uses the idea of symbolic boundaries and their capacity to create social boundaries (Lamont and Molnar 2002). This discussion of boundaries should not be confused with boundary work in science (Gieryn 1999). Scientific boundary work determines science from non-science. While the boundary work discussed here does deal with “doing difference” (Bourdieu 1979, West and Fenstermaker 1995), the core activity in scientific boundary work, for the most part I will not discuss the difference between what is and is not science or scientific research. Rather, I will discuss the boundaries of science policy to refer to the ways in which conceptual categories about what is and is not acceptable for consideration as issues for science policy reinforce technical scientific authority (Tajfel and Turner 1985). This is a process whereby a symbolic distinction becomes a social distinction, by using a category to keep out certain ideas or actors deemed threatening to the technoscientific enterprise or to the authority and autonomy of the insiders who maintain the boundaries.

Science, Policy and American Liberal Democracy

In order to understand how and why ideas about acceptable science policy in the United States have changed, it is important to understand the particularly American context in which these emerge. Specifically, it is important to understand how science and American liberal democracy construct one another and the way this has framed science policy. To explore this, I will use theories particularly from two STS scholars, Yaron Ezrahi and Sheila Jasanoff.

Ezrahi argues that all theories of liberal democracy have been informed, in part, by scientific inquiry. This goes beyond the ideology of rational inquiry, the appeal to reason, and a repudiation of arbitrary rule that came out of the Enlightenment. According to Ezrahi, one of the most important aspects of the liberal democratic tradition is a visual culture attuned to the public demonstration of politically relevant facts (1990:74). Unlike monarchical rule, liberal democratic rule is fundamentally oriented towards the public scrutiny of the government. Thus, this requires a new visual culture. The visual culture in monarchical rule was intended to dwarf its subjects in feelings of wonder and awe. In liberal democratic visual culture, however, the intention is to "attest, record, account, analyze, confirm, disconfirm, explain, or demonstrate by showing and observing examples in a world of public facts" (215). He calls this the "attestive visual culture." Central to this culture is "the assumption that reality, including political reality, is inscribed…on the visible surface of the situation" (215). This liberal democratic visual culture clearly borrows from scientific models of credibility and legitimacy. In *Leviathan and the Air Pump*, Steven Shaping and Paul Schaffer (1985) study the battle between Thomas Hobbes and Robert Boyle who fought about the scientific legitimacy of experimentation. Boyle's mode of experimentation only won because his experiments were put on display before a set of respected "gentlemen witnesses" who gave the experiment its epistemological legitimacy. In the visual attestive culture, the experiment has been replaced by appeals to facts and figures and the gentlemen witnesses have been replaced by the governed public. Political actors present and defend their actions in instrumental and technical terms that appeal to a fundamental and objective reality accessible to all, including the public. In presenting themselves to the public, political actors perform their governance publicly in way that gives the appearance, at least, of transparency and openness to public scrutiny[1]. Science policy is no exception. Science policy in America is supposed to give the appearance of consistency with liberal-democratic modes of decision-making and public values (Jasanoff 1990, 2004). The visual attestive culture of liberal democracy supports a more technocratic mode of policy- and decision-making. To some degree, then, all of liberal democracy is informed by the principles of scientific inquiry. Not all liberal democracies, however, manifest scientific reasoning and visual attestive culture in the same way.

Liberal democracy in the United States manifests a particular instance of scientific ideology and visual attestive culture that contextualizes science policy and a

[1] Ezrahi notes that the visual performance may take precedence over the instrumentalism of appealing to facts and reality (121). This "ritualistic instrumentalism," as he calls it, can prove less costly than substantive instrumentalism based on real facts and figures obtained through research.

specifically American mode of liberal democracy. I will highlight two of these that Ezrahi explains. First, American liberal democracy relies on a shared ontological reality and the presentation of public facts to a greater extent than France and Germany. In German democracy, authority not based on appeals to facts plays a larger role, as well as appeals to speculative thought and experiential understanding over empirical knowledge. Second, the American “predisposition for simplicity, plainness, functional realism, and honest explicit serviceability” become the elements of a more technological logic of democracy (1990:132). American democratic bureaucracy is modeled on the ideal of the well-functioning and efficient machine. The steam engine, Ezrahi argues, is the perfect model for early American democracy. Not only is it an efficient machine, but its influence stretched across the vast American landscape, bringing prosperity and taming the wild frontiers. It is not only a model of efficiency, but of civilization, order, and the action at a distance required of large-scale governance. This differs from democracies elsewhere, such as in England, where machines were regarded as rough and without culture. English liberal democracy favored aestheticism, culture, and traditional authority. In American democracy, these look like dirt in the machine. Culture and aestheticism introduce qualities not based in observable reality. As such, they can only clutter the efficient operation of an ideally mechanistic bureaucratic order.

What does all of this have to do with the present discussion about the expansion of acceptable science policy? There are two key points to take away from this. First, the discussion to follow takes place almost entirely in the American political and judicial context. European models of democracy and science policy do come to influence the final case, nanotechnology. For the National Nanotechnology Initiative, administrators in the NSF, in particular, borrow from their European counterparts who, in turn, had been influenced by scholars working in STS, risk studies, and public communication. Otherwise, the decision-making processes about what was considered acceptable from unacceptable science policy developed within the ideological context of these specifically American cultural ideas of democracy and technocracy.

Second, Ezrahi’s discussion sets the stage for a discussion of the tension between democracy and technocracy. We see this tension emerge in attempts to set effective science policy around emerging technoscience research. Following the lead of

Theodore Porter (1995:146), I will use the definition of “technocracy” given by Richard Kuisel (1981). Kuisel writes that technocracy assumes that:

> *Human problems, like technical ones, have a solution that experts, given sufficient data and authority, can discover and execute. Applied to politics this reasoning finds interference from vested interest, ideologies, and party politics intolerable. Its antithesis is decision making through the weighing of forces and compromise. Technocrats thus tend to suspect parliamentary democracy and prefer the ‘rule of the fittest’ and a managed polity.*

The American approach to technocracy is focused on impersonal rules, credible technical knowledge, the power of experts, and the quantification of inputs to decision-making[2]. As Kuisel indicates, technocratic and liberal-democratic modes of decision-making exist side-by-side. Indeed, Porter argues that modern democracy necessitates some measure of technocratic decision-making. Technocratic modes, however, are in tension with more participatory modes of liberal-democracy. This is noteworthy, in part, because American democratic ideology tends to place more value on direct or participatory modes over more representative modes, such as parliamentary democracy (Perrin 2009). This is true even for decisions about technical issues. As Sheila Jasanoff notes, Americans often think “even the most technical policy decisions require a judicious mixture of scientific and non-scientific judgment” (1990:9). Decision-making around science policy must at least give the appearance that it has functioned with democratic legitimacy.

In Ezrahi’s terms, this tension is fundamentally between the degree to which science policy discussions privilege appeal to empirical facts versus the public witnessing and scrutiny of policy actions. Ezrahi explored this tension but did so mostly to problematize how developments in American democracy and science subverted the role of scientific logic in democratic decision-making and power of the visual attestive culture. Particularly, he put the blame on increasingly privatized science and the decline

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[2] Porter contrasts this with the French approach to technocracy, which focused on expertise and managerial authority but did not evidence the preoccupation with quantification that is the hallmark of American technocracy.

in public culture. The public, he argues, views science by private industries as biased and therefore less legitimate for making visual-attestive appeals. Additionally, the decline of public culture has meant that the attestive culture once based on appeal to facts and reason has become mere spectacle. It is the performance itself that drives public opinion rather than the reality, which the performance makes visible to the public. Ezrahi laments this since he views modern science as a model for liberal-democracy much in the fashion of Michael Polanyi's "Republic of Science (1962)." And while we should not adopt Ezrahi's attestive visual culture wholesale, given that it ignores the many ways in which science is deployed to obscure the expression of state power rather than reveal it (Jasanoff 2004, Foucault 1978, Scott 1998), it does set the stage for an important tension that this dissertation will follow. This is the tension between technocratic and democratic visions of science in society. The cases for this dissertation are all different attempts to negotiate the tension between technocracy and democracy in the context of science policy. The scientists and administrators do so by adjusting what is considered acceptable to science policy. In the Asilomar conference, for instance, scientists took a strongly technocratic approach. They adopted what seemed to them a new idea – the postponing of laboratory research – in order to address the technical concerns of their own research. They did so, however, without public scrutiny or the inclusion of outside actors. In the case of nanotechnology, by contrast, funding agencies enrolled social scientists and publics to discover the public's non-technical concerns about the technology. They did not entertain the idea of postponing research. Each formulated ideas about science policy in a way that seemed to fit the social pressures that scientists or funding agencies anticipated.

Sheila Jasanoff explores the tension between democracy and technocracy in greater detail in *The Fifth Branch* (1990), her historical study about the role of science advisers on American science policy. She uses several "flawed decisions" by the Environmental Protection Agency, the Food and Drug Administration, and the National Cancer Research institute, in part, to probe the differences between regulatory science and laboratory science. Regulatory science, she argues, operates under political and democratic pressures that laboratory science does not. To give just one of many examples, laboratory science can take its time getting to new discoveries while regulatory science must acquire knowledge quickly and often moves forward with imperfect information. Ultimately, regulatory science is more open to political and social pressures than laboratory science. In her definition, “science policy” is the hybrid decision-making process that emerges from this tension between scientific and democratic influences, including “both scientific and policy considerations” (49). In *The Fifth Branch*, Jasanoff shows us different regulatory eras, in the balance between scientific-regulatory decision-making and judicial decision-making, and their deference to one another, can lead to “flawed decisions.” A decision may not be flawed only because it leads to some crisis, such as in thalidomide, but because it regulates unnecessarily, as was the case with saccharine. The point, for our purposes, is that science policy is that particular arena in which scientists and policymakers try to strike the balance between democratic and technocratic visions of governance.

Although I will borrow Jasanoff’s definition of “science policy,” it is important to note that this dissertation concerns a different aspect of science policy than that of Jasanoff. The regulatory science that Jasanoff studied functions under different political and judicial pressures than laboratory science and forces it to draw different boundaries than laboratory science, but regulatory science is, nonetheless, still science. In the cases for this dissertation, I do not follow the regulatory science aspect of science policy. There are several reasons for this, but most importantly, we come to each of these cases – rDNA, the Human Genome Project, and nanotechnology – in their earliest stages. At that point, there would have been little for them to regulate, and thus little to study in terms of regulatory science.[3] I study, instead, the early approaches by scientists and administrators to deal, prophylactically, with the democratic pressures they know that they are going to face. I study their attempts to imagine an effective balance between democratic and technocratic visions of their scientific endeavor and the way these play out once their attempts are executed.

In terms of considerations about acceptable and unacceptable science policy, and the balance of democratic and technocratic visions, I argue in this dissertation that

[3] Nanotechnology complicates this somewhat. In some ways, “nanotechnology” became a term to envision future possibilities in science, while it also functioned as an umbrella term that covered research already being done in materials science. Some of those materials could have been subject to regulatory scientific research, but they would not have been “nanotechnology,” properly speaking. Once they were included under the umbrella of nanotechnology, they benefitted from the science-policy boundary setting done for the larger National Nanotechnology Initiative.

scientists and administrators have moved increasingly towards the appearance of embracing the democratic vision when establishing ideas about what constitutes acceptable science policy. As I stated earlier, the Asilomar conference was more strongly technocratic while the NNI was more strongly democratic. The Human Genome Project, unsurprisingly, falls between the two. This project evidenced changes in what non-scientific (e.g. ethical) ideas scientists were willing to consider as well as what non-scientists to whom they would listen. The shift towards a more democratic vision of science, however, seems to have been partly spectacle. In his critique of the decline of public culture, Ezrahi argues that the attestive visual culture in America has given away to mere spectacle. The performance of proper actions, even devoid of meaningful consequences, can often stand in for more substantive and consequential actions. As scientists and administrators adopted more democracy, they also relied more on the appearance of democratic engagement. I will argue, however, that this had more to do with the limitations inherent in the science policy domain – in the case of democratic engagement, administrators not knowing what they wanted democratic engagement to achieve or how to measure its impacts –than bad intentions. Perhaps the most fundamental epistemological limitation to effective science policy, however, is the nature of risk, to which we now turn.

Science and Trust in The Risk Society

Much of the work of science policy boils down to the management of risk and uncertainty. There are historical reasons for this. In the early twentieth century, people had much more faith in science as the arbiter of progress and not a potential arbiter of crisis. Following several high-profile public health crises, particularly in the 1960s and 1970s, most famously as depicted by Rachel Carson's *Silent Spring* (1962), people began to question the sanctity of science. Since then, much of science policy has been directed towards the question "Is this technology safe enough?" (Wynne 2005). Answering this question for areas of emerging technoscience has proven enormously difficult. The German sociologist Ulrich Beck helps us understand why.

Beck argued that risk and uncertainty have emerged as defining elements of modern society (1992). He was a German scholar working in a particularly German context. Germany's experience with Nazi science has given its citizenry an enduring anxiety about uncontrolled or harmful science, particularly when it comes to the biosciences (Gottweiss 1998). Risk, Beck argued, has changed fundamentally from the pre-industrial to the industrial to the modern eras. This change is summarized in Table I.1 (Beck 1992, Zinn 2008). Risks in pre-industrial times were a product of the caprice of nature. No one was to blame, for instance, for the eruption of a volcano. Industrial era risks were better understood and so were both theoretically controllable and therefore a subject of blame for not having controlled them. If factory managers, for instance, can identify dangerous working conditions and fix them, then workers can blame them for failing to avoid an accident. Reflecting a classic liberal ideology, however, Beck argues that workers are always free to work somewhere else than in the factory. Industrial risks are therefore controllable and voluntary.

Table I.1 Temporal Typology of Risks adapted from Zinn (2008)

Risk	Source	Voluntariness	Control
Pre-industrial	God or Nature	Involuntary & Unpredictable	Uncontrollable & Blame-free
Industrial	Man-made	Voluntary & Individual	Controllable & Blame-Worthy
Modern	Man-made	Involuntary & Dispersed Collectively	Not Controllable & Blame-Worthy

Modern risks, on the other hand, are neither controllable nor voluntary. The development of modern science leads to risks that cannot be contained. For this reason, Beck refers to science as a form of "organized irresponsibility" (1992b). Michel Callon (2009) takes the problem a step further. Callon argues that the "term" risk has become a catchall for what are really two phenomena. Risk, he argues, should refer to phenomena about which the possible outcomes are knowable, but their likelihood remains unknown. Uncertainty, on the other hand, should describe phenomena about which we cannot even know the range of possible outcomes, to say nothing of their likelihood. Emerging technoscientific projects more often pose problems of uncertainty rather than simply quantifiable risk.

Beck argues that this sets the stage for a rise in the public distrust of science. The public, he argues, can tell that scientific development subjects them to risks over which they have no control. This leads to a general feeling of alienation from the project of scientific progress at the heart of modernity (Jasanoff 1999). To take a modern example, no one whose Samsung phone battery exploded in 2016 could have known that an exploding phone was even a possibility, much less have made decisions to mitigate their risk. Has the public become more fearful of science? The answer is complicated. In general, it does seem that there has been a decline in trust in science in the U.S. and Europe. One factor in the decline of trust in science seems to be related to one's education in science (Allum et al. 2008, Gauchat 2008, 2010), though the statistical significance for this finding is not strong. Nonetheless, scientists and science agency administrators have used this finding as the focal point for their efforts in bolstering public trust in science. We will turn to this in a moment. A more important factor seems to be one's political and religious beliefs. The decline of trust in science in the U.S. since the 1970s is not just a general decline but is rather far more pronounced among conservatives and more religious people (Gauchat 2012). If alienation from the project of modernity is the root of the public's distrust in science, that alienation is not experienced evenly among the populace. However, it may not be felt evenly within individuals as well. Often people have both fears and hopes about the future of emerging technoscience, leading to a general ambivalence (Cobb and Macoubrie 2004). I have called this a public trust problem, but Beck's insight was that it is really a science and modernization problem. People distrust science because it is like the Golem from Jewish legend: equally powerful and uncontrollable (Collins and Pinch 1993). The key, it seemed, was to try to control risks through quantification.

The effort to manage risk through quantification came from a general desire to stabilize bureaucratic administration and, as such, was imbued with social, political, and institutional values and judgments that led to its development. Risk analysis began as a way to facilitate capital investment in overseas endeavors and the earliest insurance industries (Hacking 1990). Investors were more likely to invest capital if they knew they were covered should the ship sink. For life insurance, both capital investors and customers need to know that the company will make decisions such that it continues to exist (Porter 1995). These risk estimates relied on local managerial knowledge and expertise about individuals, nearby populations, and local conditions. With the expansion of society, and bureaucratic management that increasingly had to operate at long distances, the actors involved did not necessarily trust the expert judgment of someone far away (Porter 1995). The problem of trust over long distances that we see for capitalist ventures like shipping and life insurance was equally true for expanding polities. Calls for objective methods of quantification, in this case cost-benefit analysis, arose in the U.S. in the context of large-scale water projects. The most vociferous calls for objective quantification did so to downplay the validity of expert human judgment at the Army Corps of Engineers. It should come as no surprise that quantification found a more fertile cultural climate in the U.S. than it did in France, with its greater embrace of cultures of expertise, or Germany, with its broader acceptance for different forms of authority (Jasanoff 1990, Porter 1995). The point for our purposes is that supposedly objective methods of quantification, such as actuarial estimates, mortality tables, and risk assessments, cannot escape their basis in human judgment and the institutional, political, and social pressures that influence the gathering and interpreting of such data. Uncertainty and the methods for dealing with it reveal a certain form of politics. Additionally, methods of quantification, despite their admitted power and flourishing capabilities, can never achieve the "perfectly objective, god's-eye view" that it would take to solve the problem of uncertainty (Jasanoff 1999). Finally, the very act of reducing the problems associated with emerging technologies to risk and uncertainty, to the exclusion of all other types of moral and ethical concerns, presupposes a worldview in which technological change and social progress through science are values held above most others. As Beck argues, and as the distrust of science by conservatives should indicate, this worldview is anything but universal.

Given the difficulty in taming risk itself, several approaches have emerged to deal with the public trust problem in science and technology (Fischhoff 1995, Jasanoff 2011). I will highlight three trends in the course of these developments for modes of governance of science, borrowing from examples in the siting of hazardous facilities and from the handling of environmental crises.

In the first mode, scientists and industries, particularly those that pose environmental hazards such as chemical and power plants, simply hope that publics will not scrutinize them too heavily. Industry efforts to avoid a health or environmental crisis boil down to “getting the numbers right” (Fischhoff 1995), meaning they do adequate early risk assessments. It should be clear that his emerged directly out of the cultures of quantification discussed above, which was most prominent in the U.S. In the event of public opposition or worse, an environmental crisis, the relevant industry would turn to public relations for damage control of their public image. That public relations work is largely oriented towards defending whatever had been decided prior to the opposition or crisis. This has been dubbed the Decide, Announce, and Defend (DAD) model (Bennett 2010, Ducsick and Austin 1979, Covello et al. 1988, Renn 2008). The siting of a nuclear waste storage site in Yucca Mountain in Nevada is a good example. The site had been studied by the Department of Energy (DOE) since 1978 and construction was supposed to begin receiving spent nuclear waste by 1998. It was met with bitter local opposition by people from the Western Shoshone tribes and two-thirds of non-native Nevadans (Stern and Fineberg 1996). They argued that they should not have to store spent nuclear waste when Nevada had no nuclear power plants. Congress and the DOE did not anticipate, when making their decision, how local people would respond. They made their decision, announced it, and spent years trying to defend it against opposition groups.

The second mode also involved communicating with the public but moved “upstream.” These efforts were upstream in two senses. In the first sense, these efforts are upstream temporally, in that they came before decisions had been made or a crisis unfolded. Second, this engagement was upstream administratively, in that it was motivated by scientists, regulators, and industry actors instead of displeased citizens, grassroots organizations, or civil society groups (Kasperson and Kasperson 2005). This approach to communicating with the public raised another problem. Many scientists felt that the public did not always understand science enough to respond properly to their communicative efforts. The public, some scientists felt, overestimated the degree of risk, failed to understand the nature of the risk or, worse yet, had value-laden concerns outside of the realm of risk and benefit entirely (Reyna 2004, Tversky and Kahneman 1974). To address this, a branch of psychology arose to figure out what the public thought about science, how they evaluated it, and ultimately to train the public to think in more scientific terms. This came to be known as the “deficit model” of public engagement and gave rise to specialized journals such as the *Public Understanding of Science*. The deficit model is so named because it focuses, as I indicated earlier, on the public as the source of the distrust problem rather than anything to do with how science and technology are developed and implemented (Irwin 2001, Lezaun and Soneryd 2007, Wynne 2005, Yearley 2000). The deficit model is still probably the modal approach in communicating to publics about science. Besides what most agree is an ongoing failure to properly train the public another problem arose. Many of the publics so engaged by these models identified a deficit of their own, this time found in the approach by science and industry. Scientists, they thought, too readily dismissed public concerns, especially ethical and moral concerns that were not strictly scientific or concerned with risk and benefit (Frewer 1999, Frewer & Salter 2002, Hess 2010, Irwin 2001, MacNaughten 2005, 2008, Powell and Kleinman 2008, Scheufele and Lewenstein 2008, Wynne 2005, 2006). The most recent efforts have begun trying to address the problems with the deficit approach.

In the final and most recent mode, scientists and science funding administrators in the U.S. began borrowing strategies from their counterparts in Germany, the U.K. and Denmark, adopting models of public engagement to supplement deficit models of public communication (Habermas 1991). Ideally, this constituted a move away from the "deficit model" to a more inclusionary paradigm. This inclusionary paradigm attempts to take public concerns seriously *in situ*, without trying to pre-determine the relevance of those concerns (Irwin 2006, Pidgeon and Rogers-Hayden 2007). In practice, however, public engagement in the U.S. often breaks from the normative theory that motivated it, with the effect that it still contains some elements of the deficit model that preceded it. For instance, most public engagement events still start with an expert panel or the presentation of scientific information, which frames the range of possible issues for public discussion. Often these informational presentations set a tone favorable to the science in question or even preclude discussion of certain issues. For instance, I participated in a series of NIH-funded public engagement events in 2013 where participants discussed informed consent around biobanking. Biobanking involves the storage of DNA for purposes of research. Participants were supposed to deliberate about a mode of informed consent for obtaining DNA from donors. New to the idea of biobanking, however, participants often wanted to discuss their concerns about storing DNA, to say nothing of the informed consent used to obtain such samples. They were not allowed to do so, however. The informational sessions that began the session framed biobanking in a decidedly positive light, stressing the value of biobanks and the many safeguards they employ to protect DNA samples and donors' identities.

Scientists, funding agency administrators, and scholars in the social sciences and humanities have voiced their support for this turn to public engagement, albeit probably for different reasons and different understandings of public engagement. Scientists see it as one solution to their public trust problem, policymakers and funding administrators use it to symbolically evidence their commitment to public accountability, while scholars see it as making science more democratic (Jasanoff 2003, Wynne 2006). While few argue against the democratic intentions of engagement, several argue that it fails in practice. Jasanoff (2003), who advocates for the inclusion of citizens in technoscientific development, critiques these engagement practices as too narrowly focused on technocratic considerations, to the exclusion of normative questions like the very purpose of technoscientific development. The "technologies of hubris," that exclude moral and normative concerns, should become "technologies of humility." Alan Irwin (2006) argues that the new modes of governance are too tainted by older modes to constitute a new paradigm. Brian Wynne (2006), original critic of the deficit model, says that the new engagement models do not allow their own science-policy culture to become a matter of dialogue – one of the core components of Habermas's theory of deliberative democracy. Instead of fostering trust, it is more likely to foment distrust of that practice. Cooke and Kothari (2001) call this turn to engagement the "Tyranny of Participation." According to them, engagement is simply a new means of enforcing the will of industries and state governments, with the added benefit that they can dress up that enforcement in the trappings of democracy. With a somewhat less insidious focus, Kasperson and Kasperson (2005) consider engagement a solution ill fitted to its problem. Engagement has become a bandwagon upon which policymakers and scientists have blindly jumped without knowing what makes engagement effective or when it has successfully improved public trust. U.S. policymakers adopted public engagement as the most recent strategy for fostering public trust and securing support for technoscientific development by performing their accountability to the public. In its ideal form, public engagement fosters more democratic science development and even helps train citizens in civic engagement. In its less than ideal forms, however, it is little more than a spectacle, in Ezrahi's terms, meant to bolster support for technoscience without changing the substance of its development.

Each of the cases for this research is a technoscientific subfield or research area in the U.S. fearful of public anxiety and backlash. In each, scientists and funding administrators must deal with the risk and uncertainty of science and the public distrust to which those elements give rise. In each case, scientists or administrators have used one or a combination of these strategies to fix their trust problem and, ultimately, protect their autonomy from too much public scrutiny or regulatory overreach. In so doing, scientists in each research area or scientific subfield studied here worked to maintain their autonomy, in part, by solidifying the power of their scientific credibility and expertise. The fact that their research finds itself embroiled in the concerns of the risk society, however, presupposes an influence from outside of science itself. Thus, their practices for dealing with public concerns wound up re-defining credibility and expertise in light of public concerns, in order to protect their autonomy.

Autonomy

The motivation to secure public trust was first and foremost motivated by the need to protect scientific autonomy, which seemed threatened by the deconstructing potential of too much public scrutiny (Irwin 2006, Jasanoff 1990, 2011, Slovic 1993). Autonomy is of central concern to scientists, who have had to fight for it since the dawn of science (Gieryn 1999). What does it mean for science to have autonomy? Robert Merton seemed to reflect scientists' preferred vision of their enterprise when he framed autonomy as freedom from external influence. Concerned about what he saw as anti-science viewpoints would do to science, he wrote that "the social stability of science can be ensured only if adequate defences are set up against changes imposed from outside the fraternity itself" (1938: 328). Two elements of this stand out. First, the autonomy Merton describes is conditioned upon exclusion. Changes emanating from without science require defenses. Second, insufficient autonomy threatens the very existence of science. In this, Merton reflects many earlier thinkers, such as Condorcet, Bacon, and Saint-Simon, who thought that scientists should be left alone, not only so that they could get on with their work but also because the empirical and objective nature of scientific work provided the model of an ideally rational society (Turner 2008).

Although scientific autonomy has never been free from political and social influences (Guston 2000, 2004, Jasanoff et al. 2006, Pielke Jr. 2004, Shapin and Schaffer 1985), there is a pervasive sense, particularly among scientists, that science once enjoyed far greater autonomy. It is now a matter of scientific lore that science enjoyed a Golden Age of peak autonomy in the 1950s, much like that of medicine of that same era (Guston and Keniston 1994, Lewenstein 1999, Parsons 1951). Scientists and doctors had near total authority to conduct their work as they saw fit. For scientists, this usually harkens back to a speech by Vannevar Bush in 1945 titled *Science, the Endless Frontier* that led to the passage of the National Science Foundation Act of 1950[4]. With the passage of the NSF Act came a new "social contract for science" in which the federal government would fund basic research and, in return, science would produce "a steady stream of discoveries that can be translated into products, medicines, or weapons" (Guston and Keniston 1994:2). In this arrangement, "the federal government provided the money" while "science provided the discoveries and kept its own house in order" (13). David Guston and Kenneth Keniston show how this was never really the case. Even in the supposed Golden Era, federal funding did not mean a "blank check" and science was never apolitical or free from social influences. The patronage of the federal government came with significant strings attached. Those strings led most directly back to science funding and regulatory agencies, however. As I will argue in the context of the Asilomar conference, scientists in the U.S. did not yet always see the force of what Jasanoff has called "civic epistemology," the larger set of institutions, agencies, and concerned public who evaluate the results of scientific practice. This recalls Elliott Freidson (1970) who argued, in the context of medicine, that autonomy is not something established from within a field but rather as a product of the patronage and protection of the state, which effectively grants such autonomy. Michel

[4] Daniel Kevles argues that the idea for the NSF actually came earlier, from a now-forgotten New Deal senator, Harry Kilgore, Jr. (Kevles 1977). Nonetheless, even recent scholarship treats Bush and his speech as the seminal moment for the NSF.

Callon (Callon 1987, Callon et al. 1986) and Bruno Latour (1987) developed this into a larger theory of enrollment. The most successful scientists, they argue, effectively enroll a variety of non-scientific actors and material resources that make scientific work possible.[5] Enrollment works both ways, however. Enrolling an actor into a project means being enrolled yourself by that actor as well. Susan Cozzens (1990) interpreted enrollment as a challenge to the Golden Age view of scientific autonomy. The most successful scientists are often the ones most enrolled by those whom they enroll. In the social contract for science, the federal funding agencies became the most important single actor for individual scientists or research projects to enroll. Additionally, Latour observed that enrollment works both ways. Scientists who enroll important actors and resources are in turn enrolled by those same actors and resources. Accepting federal research funding means also adopting state goals and being subject to state scrutiny.

Despite his ideal vision of scientific autonomy, Merton actually understood this process of enrollment. He saw it, however, more as a problem to be avoided or corrected rather than simply the way science is done. Using Nazi Germany as an extreme case, Merton argued that the politics of the nation state intervene on science, infecting it with non-scientific values and goals. The public, fooled by "myths…clothed in scientific jargon," can only hinder scientific progress by forcing scientists to deal with their irrational fears (1938: 333). Science, in Merton's view, deserves more autonomy than it receives. Scientists generally agree. It was not until the controversy around rDNA research, the first case of this project, that scientists began to accept what Guston and Keniston (1994) call the "populist tension" between science and democracy. This tension arises from the fact that while democracy must pay heed to citizens' concerns, science traditionally has not had to do the same. Although scientists and their political allies often make discursive use of the scientific Golden Age and the ideal of Mertonian autonomy, the cases for this research show moments where they have embraced the populist tension and what it means for scientific autonomy.

Susan Cozzens has described scientific autonomy as "mastery or control over the relevant environment" which "combines resource acquisition and defensive policy

[5] We may even see parallels in the social movement theory of resource mobilization (McCarthy and Zald 1977).

clout along with self-governance" (1990:167). She defines scientific autonomy by dividing it into four types, common to STS and social science scholarship:

1. The condition of a collectivity that has established a social identity, a relatively stable resource base, and a system of internal control.

2. The exercise of discretion by individuals in their work.

3. The ability of scientists to set long-term career or project goals and have the resources to carry them out.

4. A condition of science as a whole or the science of a particular nation.

Cozzens's motivation for this typology came from a paradox she identified in Latour and Callon's concept of enrollment. From an enrollment perspective, the most autonomous science would be the least successful and the most successful science would be the least autonomous. She wanted to understand how the most enrolled scientists retain any autonomy. To explain this, Cozzens borrows from Weberian concepts of power and authority. She offers two ways in which scientists' power and authority help them retain a modicum of autonomy: influence and competitive edge. Influence is the respected afforded to authoritative proclamations, like the scientific advice of a science adviser. Competitive edge is the ability to exert one's will against the will of others. In those domains where scientific expertise enjoys a high degree of authority, such as within the laboratory or in science funding agencies, influence and competitive edge function best. For scientific work, then, a scientist may enroll others while still acting from a privileged position. These expressions of power and authority function less well, however, for domains that only intersect with science, or where scientific authority is not at the top of the authority hierarchy. In science policy domains, for instance, scientific authority runs up against the populist tension discussed above. This does not stop scientists from trying to treat the science policy domain as if it were strictly a scientific domain where their base of authority can ensure greater autonomy.

Autonomy plays centrally for this dissertation for two reasons. First, autonomy remains one of the chief concerns of the scientists studied here. It is fundamental to scientists' sense of themselves, the importance and value of their work, and their confidence in the health of science overall. The pursuit of autonomy, therefore, occupies much of their thought and actions. Second, this research challenges a common view of autonomy as either: (1) simply a process of excluding the influence of non-scientists or (2) something that only scientists and their immediate allies can maintain. This research suggests that autonomy is no longer solely concerned with the strict demarcation between the inside and outside of scientific inquiry. Rather, scientists win autonomy through a carefully managed process of inclusion and exclusion. Scientists and science agencies have become aware of the populist tension and the situated nature of their autonomy in a world where science and its modes of governance co-produce one another (Callon et al. 2009, Funtowicz and Ravetz 1995, Jasanoff 2004). Although I will discuss co-production in detail below, it is enough to say here that science is not developed purely in laboratories free from the influence of other institutions of governance and even public opinion. Rather, science develops in a larger social and political context of which, I will argue, scientists and science agency administrators have become increasingly aware. In this dissertation, I argue that, in order to accommodate this, scientists and administrators at funding agencies have increasingly enrolled non-scientific actors and non-scientific ideas into technoscientific development. By non-scientific actors, I mean not just non-scientists broadly. Scientists enroll a common set of non-scientists into the usual development of technoscience, particularly policymakers, representatives of private industries, capital investors, and regulators. This dissertation studies the enrollment of social scientists and the lay public, whom scientists and administrators historically have not directly enrolled into non-medical laboratory research. By non-scientific ideas, I refer to moral and ethical concerns that scientists have historically discounted as too value laden to warrant consideration for technoscientific development (Proctor 1991). To accommodate these new elements, scientists and administrators introduced changes in how they defined acceptable science policy and adopted a view of their own autonomy that sees it as a more negotiated process rather than simply being left alone to do their work.

Credibility

Credibility is one of the foundations for the scientific claim to epistemic authority and autonomy. Credibility practices, such as peer review, are largely internal to the scientific field in question[6]. It is one ways that science "keeps its house in order" under the social contract for science. Scientific practices for earning and maintaining credibility, argues Steven Shapin, transform what would otherwise be unsubstantiated belief into authoritative knowledge (1995). Latour and Woolgar similarly framed credibility as playing a central role for scientific knowledge-production. They viewed science as a system of "literary inscription," a social process whereby laborious research leads, most importantly, to publications. In this process, earning, maintaining, and recognizing credibility serves as scientists' main motivation ([1979]1986). So, credibility shapes the production of scientific knowledge but how does it shape the boundaries of science?

Thomas Gieryn (1999) gives us three ways in which credibility supports autonomy. It does so through processes of: (1) exclusion, (2) expansion, and (3) expulsion. These function variously to maintain a boundary in place or to move the boundary. As a practice of exclusion, credibility stops anyone from entering into the scientific domain who does not have the requisite training, certification, and experience. As I have indicated earlier, the rise of citizen science and "maker-spaces" (informal experimental spaces) in recent years have challenged this exclusion to some degree (Bonney et al. 2004, Irwin 2002, Lindtner 2014). It would take a broad recognition by many scientific communities to change, fundamentally, what counts as credible for these to be fully recognized as science, however. Taken on its own, credibility functions, in part, to keep out those individuals and ideas that have not gone through well-established vetting procedures or successful negotiation of worksite practices in the laboratory for inclusion into science (Latour and Woolgar 1979). Second, credibility helps scientific subfields expand into new areas when scientists in those fields identify

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6 Kinchy and Kleinman (2003) have argued that even scientific credibility intersects with public values and thus is not strictly internal to science. They give the example of scientists engaging the public. The public introduces ideas of "purity" and "utility" in their own ideas of scientific credibility. They deem scientists with deep ties to industry as less credible. While true, I think this has limited application here since their case involved direct contact with the public that scientists try to avoid. In other words, whether or not there is a social element to scientific credibility, scientists try to keep credibility practices internal to their domains.

problems to which they can contribute or even solve. This expansion is similar to the practice of medicalization (Conrad 1990, Freidson 1970, Zola 1972). Physicians attempt to expand the power of medicine by applying it to previously non-medical realms. The more social worlds over which an institution claims authority, the more power that institution enjoys. The most common example is alcoholism. Alcoholism was effectively medicalized once it became a disease rather than a moral failing. There are limits to the value of such expansion. Being enrolled into ethical debates, for instance, can threaten a scientist's credibility because doing so violates the fact-value divide that is key to the epistemic authority of science (Proctor 1991). Jonathan Beckwith, bioscientist and early member of the activist group Science for the People, reflected upon this. "A rule of the culture of science until then [the 1960s to early 1970s] had been that scientists didn't contaminate themselves with public contact." He continued, "Those of us who raised concerns about the social consequence of science were mistrusted by other scientists" (Beckwith 2002:157-158). This is less true for high-status, late career, or otherwise well-established scientists, but can negatively impact early career scientists. Carl Sagan, for instance, took early criticism for getting involved in the popularization of science before he had secured his professional credibility (Gregory and Miller 1998). Finally, scientists who fail credibility contests can either have their research expelled as scientific knowledge or even find themselves expelled as scientists. Andrew Wakefield's article attempting to show a connection between vaccines and autism is one example of knowledge that was eventually expelled (Godlee et al. 2011). Gieryn gives us an example of expelled scientists in the chemists Martin Fleischman and Stanley Pons. Fleischman and Pons made headlines in 1989 when they announced to major media outlets the early results of their research into "cold fusion." The discovery would have meant essentially limitless energy at almost no cost. They violated scientific norms of credibility, however, by turning to the news media about their research before it had been properly vetted through any kind of peer-review. They were vociferously criticized by their fellow scientists, some calling them outright frauds. Their research could not be replicated. The cold fusion energy revolution never came to fruition. After this, both of them left the United States to work for a Toyota research laboratory in France. Fleischman and Pons' crucial mistake was to externalize what should have been an internal credibility contest. They used the media and public excitement to get attention for their research before it had been given the scientific stamp of approval. They so violated the norms of credibility that they had to leave academic science.

This dissertation shows instances where scientists and administrators deploy the rhetoric of credibility when (re)negotiating ideas of acceptable science policy. Importantly, science policy is not science, not even the regulatory science that Jasanoff describes, so these are not credibility contests as Gieryn defines them. Nonetheless, scientists and administrators borrow from the institutional logics with which they are most familiar, including the language of credibility and expertise, when arguing about what non-scientific ideas and actors should be considered acceptable for science policy.

Expertise

While credibility refers to a social vetting process, expertise refers to the authority afforded certain epistemological or experiential training. In the context of science, credibility is used to exclude or expel people whose research or training are not recognized by other scientists as appropriate or relevant. Expertise is both an element in that determination – whether one has credible expertise to be deemed credible – and a product of that – the social recognition of one's expertise is partly dependent on community recognition of the authority and credibility acquired by the institutions provided one's training (Jasanoff 2004, 2005, Ottinger 2013, Wynne 1996). Thus, expertise and credibility are intimately connected. Harry Collins (1985) argues, for instance, that if you try to trace expertise to its roots, you wind up with credibility. The expertise we afford someone who has conducted an experiment boils down to the trust we have that their experiment was conducted faithfully and accurately.[7] At some point, we trust the experiment and the expert because they have built up past credibility. Similarly, Peter Dear (2004) argues convincingly that expertise cannot be separated from one's scientific authority, which is also a product of credibility. Expertise is constructed with networks of people who not only hold heterogeneous levels of knowledge, but also power and authority (Ottinger 2013). This is not to say that

[7] Lorraine Daston and Peter Galison (2010) have written about the difficulties in reproducing many experiments which, according to scientific method, is supposed to be the source of faith in an experiment.

expertise has no basis in rarefied knowledge and skills or even reality. Brian Wynne (2003), in his critique of Collins and Evans, admits that some kind of realism is unavoidable when talking about expertise. Expertise, however, is contextually dependent. One has expertise *in* some field or body of knowledge and expertise in scientific domains does not necessarily translate to expertise in the science policy domain. We will say more about this momentarily. There are two salient points for now, though. First, expertise is socially constructed, but often is not treated as such, especially among more technocratically-minded scientists, administrators, and policymakers in the U.S. When the social construction of expertise goes unacknowledged, it comes to look universally authoritative for all questions of science in society. Scientific expertise renders scientists the only relevant actors in science, even for ethical issues, political issues, and issues related to the public good. Other kinds of expertise are treated as irrelevant. Second, whereas credibility is a social process more often deployed by scientists to police the internal boundaries of science against other scientists, they use expertise to police the external boundaries of science against non-scientists. This is not a strict rule, however, as a scientist in one area may argue that a scientist in another area lacks the expertise to have credible insights into the former's field. Furthermore, scientific credibility is not a monolithic type that a scientists either has or does not have, but rather a process continually negotiated in the laboratory, in conferences, and in peer review, among other venues (Frickel and Moore 2006). Nonetheless, these credibility processes are directed more at scientists and scientific claims rather than non-scientists.

One of the key threads of STS scholarship on expertise are the difficulties it creates for the ideology of liberal democratic governance (Blok 2007, Ezrahi 1990, Jasanoff 1992, 2007, Sclove 1995, Weinberg and Elliott 2012). Scientists enjoy a privileged place in an epistemically unequal arrangement. When the issues are purely technological or scientific, such as whether vaccines really do cause autism, then strictly scientific expertise is relevant. When the issues intersect with ethical or moral concerns or democratic values, however, then strictly scientists can extend their authority into realms in which they do not have sufficient expertise (Kleinman 2000, Sclove 1995, Wynne 2003). As Brian Wynne (2003) notes, in commenting on Collins and Evans, there is a significant difference between scientific expertise extended to the public domain for “technological artefacts” and issues related to the scientific discipline on the one side and “wider interventions” that issues of public meaning, including narratives of science in society and networks of power. Scientific expertise cannot answer these questions for a democratic society as these have more to do with the distribution of resources, national priorities, and commonly held values. Nonetheless, scientific expertise usually enjoys a privileged position in these decisions as well.

While having expertise often gives scientists a privileged position in policy- and decision-making domains, not having it justifies the exclusion of non-scientists from those same domains. Studying breast cancer and life-form patent activism, Shobita Parthasarathy (2010, 2017) calls this the “expertise barrier,” which she defines as "the formal and informal rules of a science and technology policy-making domain which make it difficult for those without technical expertise to engage as equals" (2010:355). Parthasarathy joins other scholars in showing how activists have overcome this expertise barrier (Epstein 1996, Frickel and Moore 2006, Frickel et al. 2010, Hess 2007, 2010, Kinchy 2012, Moore 2009, Tesh 1988). Brian Wynne call these forms of activism “uninvited participation” (2007). The fundamental lesson, besides the fact that the expertise barrier can be overcome, is that it usually must be overcome in the first place. Scientists were not inviting these individuals into the domains of research or policy to share their opinions. If an activist overcomes the credibility problem by successfully proving their relevance to a scientific issue, usually by making themselves a problem for science or industry in some way, those resisting their efforts will erect the expertise barrier, arguing that they do not have sufficient expertise to participate.

Furthermore, in deploying strategies to overcome the expertise barrier, activists do not always challenge the fundamental power and authority of scientific expertise. Instead, they either make themselves scientifically credible or supplement scientific expertise with expertise of another kind. Steven Epstein’s AIDS activists, for instance, acquired sufficient scientific expertise to gain credibility and legitimacy in medical domains. They essentially gained entry into the technocratic domain on terms set by that domain. Brian Wynne’s Cumbrian sheep-farmers supplemented physicists’ somewhat flawed knowledge of radioactive decay with knowledge about the behavior of sheep. Policy around sheep farming in the wake of the Chernobyl disaster focused only on physicists' knowledge about soil and radioactive decay (1996). It failed to take into account sheep-farmers' expertise about sheep and land with the result that many sheep were unnecessarily tainted with radioactive fallout. Neither of these approaches fully supplant the privileged role of scientific expertise.

Activists have used strategies that have presented a more fundamental challenge to scientific expertise. Parthsarathy (2017) gives us three examples of this in her research on life-patent activism: (1) introducing new kinds of facts, (2) introducing new policy-making logics, and (3) attacking bureaucratic rules. For each of these, activists have obviated the power and relative privilege of scientific experts by moving the discourse away from scientific expertise. David Hess (2010) gives another example. His research into scientific counter-publics shows cases where social movements have been able to affect what scientists research. They use the power of large-scale public activism to supplant the authority of scientists in making these decisions. Nonetheless, scientific expertise is something to overcome, side step, or add onto in order to carve out a space for other kinds of knowledge, other kinds of claims, or other kinds of actors.

There has also been a strain of theory about expertise that attempts use it to justify the inclusion of non-scientists in decision- and policy-making. This, ultimately, was Brian Wynne's argument in his research into the Cumbrian sheep-farmers (1996). He wanted to show that even for an issue as seemingly purely scientific as the irradiation of sheep following the Chernobyl nuclear disaster, other forms of expertise proved relevant and consequential. Harry Collins and Robert Evans turned this into a general principle with their somewhat contentious theory of expertise. They argue that the problem of public distrust in science, discussed above, is essentially a "legitimacy problem" in scientific governance whereby the political legitimacy and authority of scientific expertise has diminished. They give us three types of experts: (1) contributory experts who contribute to the production of new scientific knowledge; (2) interactional experts who may understand some set of scientific ideas well enough to teach them or speak intelligently about them, but cannot contribute to the production of new scientific knowledge – science teachers, museum employees, and even informed social scientists of science may be included here – and (3) non-experts, who simply have no scientific expertise. This leaves them irrelevant for purposes of science policy- and decision-making. Collins and Evans (2007) later sub-divided the "no expertise" category to reflect gradations of scientific knowledge among the lay public. They called this "beer mat" knowledge and the knowledge that comes from paying attention to informed public discourse. Robert Evans and Alexandra Plows tried to turn this theory of expertise into an argument for the inclusion of lay people in science policy- and decision-making. Their theory, which has a surface resemblance to Wynne, defines lay people as experts in "the good life" (2007). Wynne has critiques Collins and Evans, however, for arriving at a theory of expertise that ignores its social construction and the practices of credibility and authority that give it its power (Jasanoff 1998, 2004, Ottinger 2013, Wynne 1996). Additionally, Wynne counters that the "legitimacy problem" that Collins and Evans identify in terms of respecting expertise is really a problem of public meaning. Scientists, administrators, private industries, and policymakers do not always respond to people's reasonable concerns about the development of technoscience. That takes us back to the problem of public trust, however, which we have already covered. For our purposes, it is important to note that Collins and Evans' theory of expertise gives scientific expertise a privileged place. Technoscientific expertise is only challenged by other forms of expertise and it is the benchmark against which other forms of expertise are measured and valued. In the cases for this dissertation, we will see many instances where scientists use an idea of expertise like that of Collins and Evans to justify their own authority over issues of science policy, even if those issues are not strictly technological. Similarly, they use this idea of expertise to question the relevance of non-scientists or non-allies in science.

In terms of exclusion, expertise functions as a way to exclude non-scientists. This may be the wholesale exclusion of all scientific non-experts, where expertise is treated as binary, or the exclusion of some subset of individuals lacking the hierarchical gradations of expertise identified above. In the latter case, scientists may see an industry representative as relevant if they have the appropriate "interactional expertise." The expertise barrier is only overcome by possessing another form of relevant expertise that can compete with or supplement scientific expertise, by either redefining the terms of discourse away from a monopolistic focus on scientific expertise or by exercising political power in such a significant way that expertise loses its privileged position in policy- and decision-making domains. Two important features stand out about these struggles between science-experts and non-science-experts. First, they take place in domains far removed from those in which scientific knowledge is produced. These struggles take place in courts, in public meetings, and in the streets. Where they do influence scientific knowledge-production more directly, they do so in terms determined by scientists who adjudicate the credibility of public inputs. Scientific standards determine whether the data citizens have collected counts as evidence (Frickel et al. 2010). Second, these struggles are usually motivated by non-scientists seeking to redress some perceived wrong. Sympathetic scientists may support a public cause, but they are rarely the ones leading the charge. Groups like the Union of Concerned Scientists are a noteworthy exception.

Political Imaginaries, Civic Epistemology, and Public Reason

The research for this dissertation follows changes in what was considered acceptable boundaries for science policy for each of the cases studied here: the Asilomar conference for recombinant DNA research, the Human Genome Project, and the National Nanotechnology Initiative. These policy changes were a product of changing conditions in the social and political context in which each of these technoscientific projects developed. I will use several concepts from Sheila Jasanoff to understand how and why science policy expanded and how science agency administrators and scientists controlled this expansion. Particularly I will use her concepts of civic epistemology (2005), co-production (2004), and public reason (2012). I will situate these within Yaron Ezrahi's concept of the political imaginary.

Jasanoff used civic epistemology to understand how publics perceive and receive the development of technoscience in democratic societies. Hers is a culturally specific politics of biotechnology in Britain, Germany, and the U.S. It is a theory of public knowledge production and engagement in technoscience that stands in contrast to the public understanding of science (PUS) model discussed earlier. She criticizes this model, more commonly referred to in STS scholarship as the deficit model, for assuming what citizens ought to know or do know about science and turning this into a *de facto* theory of democracy based on "ignorant publics…in need of rescue" (2005:254). While her theory contains normative elements about science, policy, and democracy, it is grounded in the practicalities of public perception in general, and of technoscientific development in particular. She takes seriously the fact that people live in overlapping systems of meaning making of which scientific "understanding" is but a part. Far from being dupes, publics mobilize both impressive cognitive faculties and cultural repertoires as they make judgments about not only the technical aspects of an emerging technoscience but its moral value, its social purpose, and how it should be governed. In short, "civic epistemology refers to the institutionalized practices by which members of a given society test and deploy knowledge claims used as a basis for making collective choices" (Jasanoff 2005:255). Publics are not the passive recipients but active participants in the development, deployment, and governance of technoscience.

Civic epistemology in the U.S. is marked by several factors that differentiate it from other cultural contexts. Due to the large and heterogeneous nature of the American citizenry, public reasoning in the U.S. is pluralist and based on interested parties – actors from non-governmental organizations, industry, and academia, to name a few (Ezrahi 1990, 2012, Jasanoff 1990). For biotechnology, at least, publics began from a position of distrust (Bennett 2010, Nisbet et al. 2003). Decisions about governance and regulation of biotechnology in the U.S. were adversarial and publics often turn to courts to adjudicate these issues (Hurlbut 2017).[8] Publics in the U.S. are persuaded by scientific claims when they are put into technical arguments backed by numbers. These serve as the bases for objectivity, similar to what Theodore Porter observed.

Although I will use the concept of civic epistemology to help make sense of the cases for this dissertation, this is not a project on civic epistemology. To do so, I would have to follow Jasanoff's lead and provide a sustained inquiry into the reception of each technoscience and the social and political conditions of its development and governance. Instead, I use civic epistemology as a tool for understanding how scientists

[8] In an earlier work, Jasanoff argues that regulatory agencies in the U.S. are often stymied when there is an improper balance between scientific and juridical authority (2004).

and agency administrators effected changes in what was considered acceptable science policy – what ideas and actors they considered credible or relevant at different times and for different technologies. In other words, in changing what was considered acceptable science policy, scientists and science agency administrators were responding to an imagined civic epistemology. They worked in the realm of imaginary for two reasons. First, we take each of these cases at the emergence of a new technoscientific field or research project. When they began to think about how to approach rDNA research or nanotechnology in terms of policy, there was nothing like a broad public idea about them. Second, scientists and administrators felt that they did not know enough about the public in general. They drew, as best they could, on precedents from other contexts – other fields and other countries – and from the literature on the public understanding of science. In the end, though, they had to imagine how the real public might response, or civic epistemological process, would play out.

Imagination is an important component and worth explaining in more detail. The actors in these technoscience projects had to respond to an imagined public and an imagined civic epistemology. Imagination became a tool in acting at a distance. The distance in this case was not only geographic, but also epistemic and cultural. In his *Imagined Communities*, Benedict Anderson (1991) argued that the nation-state must constantly engage in practices of "nation-making," through the strategic use of symbols like the flag, and technologies like the census, to establish its authority and its ability to represent a large and disparate set of individuals. Nation-states engage in imagination work, in part, because of the problem of distance for a large and heterogeneous society. Citizens cannot see their communities, their representatives, or the centers of power and authority. But in providing an imagination of itself, the nation-state must imagine its own citizenry as well. Decisions about the design of the national census, for instance, operationalize one imagination of the citizenry over others.

In *Imagined Democracies*, Yaron Ezrahi builds on his argument about the role of public witnessing (1990) to argue that imagination work constitutes "the fabric of political world-making, the core of the political order, and the clue to its formal architecture and informal dynamics" (2012:38). These *political imaginaries*, as he calls them, refer only to imaginaries that have the power and authority to drive regulation or structure the political order. Political imaginaries help create modes of governance. In the context of science policy, this makes imagination work a form of co-production. Sheila Jasanoff (2004) showed that the ontological work of scientific knowledge production produces the simultaneous construction of governance systems. The technical and democratic orders – the ontological and the normative, what *is* and what *ought* to be – co-construct one another. Although not scientific knowledge production *per se*, science policy nonetheless provides one link between scientific work and the modes of governance in which that work is embedded and which it helps constitute through various political imaginaries. It is one domain of the co-production of science and governance, if not the most visible one. How is this relevant for this project?

Scientists and administrators for the research projects for this dissertation engaged in crucial imagination work by deciding how to approach the potential ethical issues of their research and, later with nanotechnology, the public perceptions of it. They imagined both a public and a mode of governance when they made decisions about which health and safety hazards were worth pursuing and which were not. They imagined a public when they held hearings and workshops in which they tried to discern what might frighten people about their research and how to address those fears. They imagined a mode of governance when they promoted one vision of science and science policy over others. These are exercises in what Stephen Hilgartner has called "vanguard visions' (Hilgartner 2015). Vanguard visions are a subset of Jasanoff and Kim's concept of the sociotechnical imaginary (Jasanoff et al. 2006, Jasanoff and Kim 2015). Sociotechnical imaginaries are "collectively held, institutionally-stabilized, and publicly performed visions of desirable futures, animated by shared understandings of forms of social life and social order attainable through, and supportive of, advances in science and technology" (Jasanoff 2015:4). Vanguard visions are similar to sociotechnical imaginaries but differ in that they have not yet achieved the same stability and widespread public recognition. They are visions of the future, often the very far-flung future, which visionaries hope will structure and motivate contemporary actions. The promoters of these visions wanted to define the future of both scientific development and modes of governing science. For this reason, they are a form of future-making. Since these vanguard visions are usually a negotiated process among scientists, science agencies, policymakers, industry, and publics, I refer to this as "imagination work." Imagination work describes the effort of promoting one vision of science and the future against others.

We should not see imagination work as coming from a monolithic set of scientists or science agency administrators. Consistent with past scholarship on American science policy, this dissertation shows that imagination work is an adversarial competition to promote different imaginaries of science, science policy, and the public by different actors with the authority to do so (Jasanoff 1990, Laurent 2017, Parthasarathy 2017). The actors I present here promoted their own ideal imaginaries about the authority and autonomy of science, the role of publics in science, and the modes of governance that constituted acceptable science policy. The prevailing imaginaries were constituted, in part, by the prevailing social, political, and cultural conditions of the time, as they always are. They often evidenced, however, a deep anxiety and ambivalence about both the nature of the public and its power. By this, I mean that prevailing imaginaries about these emerging technoscientific fields and their governance did not always fully account for how publics would perceive and receive these fields. Scientists were often surprised by what people feared and what they did not. The effect of this was that each subsequent effort to imagine publics and modes of science policy was conditioned, in part, by what came before or what was happening elsewhere. Scientists and NIH administrators for the Human Genome Project borrowed from their own past experience with the Asilomar Conference and its fallout. Administrators for the National Nanotechnology Initiative, particularly at the NSF, borrowed from the Human Genome Project and from Western Europe's experience with public responses to biotechnologies.

This borrowing led to an overall expansion in what was considered acceptable science policy but one limited in key ways by those doing the imagination work described above. The expansion involved the inclusion of new ideas about what issues scientists and science agencies should consider, what actors they should include, and what practices they should engage in when funding and regulating science. I refer to this as an expansion because the cases for this dissertation evidence a trajectory from considering only technical issues, to accepting ethical issues, to accepting a role for social scientists, and eventually lay publics in issues of science policy. These changes came with new ideas about expertise and relevance. Social scientists, for example, were accepted in the National Nanotechnology Initiative as experts in public perceptions of science. Although the trajectory among the cases was one of expansion, it was also an exercise in the bureaucratic production of public reason. This limited the scope of the policy expansion by producing technically credible concerns and rational publics. I follow Sheila Jasanoff who uses public reason to refer to the “institutional practices, discourses, techniques and instruments through which modern governments claim legitimacy in an era of limitless risk – physical, political, and moral” (Jasanoff 2012:5). Public reasoning is a way to understand the political response to the risk society. It refers to the political and institutional production of modes of governance as well as the logics for justifying those particular modes of governance. Jasanoff is clear that she does not follow the definition of public reason according to John Rawls, who sees it as the rational deliberative process of a citizenry, in a sense very similar to the Habermasian ideal (Habermas 1991, Rawls 2002). J. Benjamin Hurlbut, a student of Jasanoff’s, has shown how governmental ethics bodies, science policy actors and science funding agencies deployed the Rawlsian ideal of public reason to determine what “right reasoning” should look like (2017:27). We will see a similar process in the case of nanotechnology, once publics were invited, they were constructed in a way that was consistent with the Rawlsian concept of the reasonable deliberator. This was not the only way that public reason, in Jasanoff’s use of the term, was used to narrow the boundaries of science policy even as they expanded to include new ideas and actors.

In several ways, the public reasoning of science agencies, Congress, and key scientists were used to determine what constituted elements of science policy discourse and how those elements should be discussed. Hurlbut, again, has described as disciplining discourse (2017). He used the American political and bioethical struggles to define the embryo to show that it also involved a tacit set of arguments about how publics should reason. Ideas such as the pre-embryo – an attempt to define the first two weeks after fertilization as not an embryo – were attempts at disciplining discourse around the embryo. One could not speak about anything prior to this two-week period as an embryo, a term heavily imbued with moral and ethical claims. At least one could not do so in any context that the term pre-embryo was taken as real. This has clear affinities with the Foucauldian notion of power-knowledge, where the construction of knowledge becomes an exercise in power, making certain truth claims more prominent and others unthinkable (1972). I will borrow the concept of disciplining discourse to show how scientists, funding agency administrators, and eventually social scientists worked to contain and stabilize the expansion of ideas about acceptable science policy. In their minds, an unstable science policy boundary constituted a threat to the autonomy and authority of science. Indeed, the very fact that the boundary was expanding at all, to include these new issues and new actors, looked threatening to many. Thus, for them, it needed conditioning.

I will show three key ways in which discourse was disciplined. First, discourse was disciplined by determining what ideas were relevant and credible. These ideas most often concerned which issues were worthy of consideration and which were not. Issues related to environmental health and safety, for instance, were the most acceptable while the distributive justice of a technology's risks and benefits, essentially who gets the benefits (e.g. people who can afford a new product) and who gets the risks (e.g. poor people who live near a factory or dump site), was a more contested concern. Apocalyptic fears were entirely discarded as science fiction. Second, discourse was disciplined by determining the kinds of actors that could participate in the newly expanded boundaries of science policy and the conditions of their participation. Social scientists were included in the Human Genome Project and lay publics included in the National Nanotechnology Initiative. Publics had to reasonable, in Rawlsian terms, and both had to be supportive of technoscientific development and commercialization. Finally, discourse was disciplined by what funding agencies and policymakers would accept from these new actors and ideas. In other words, it was up to the science agencies to move forward on an issue of concern identified by an ethical body looking into the societal implications of a technology or a public deliberation. Simply not doing anything with a report is an easy way for science agencies or policymakers to discipline public discourse, assuming that the issue identified does not get socially amplified in some other way, such as the actions of a committed civil society group (Hess 2008, Slovic et al. 1993).

These modes of disciplining discourse involve normative claims about credibility and expertise. Sometimes these take the form of scientific boundary work. We already saw an example of a credibility contest above with the example of Fleischman and Pons and their research into cold fusion. In these cases, we see a credibility contest between engineer K. Eric Drexler and physicist Richard Smalley to define what is technically possible for the emerging technoscience. Drexler had introduced a particularly frightening environmental crisis and Smalley wanted to define it away. Drexler eventually loses the contest and, with it, his credibility as a scientist. Their argument was technical, but they were both aware that what they were really fighting for was the ability to define nanotechnology and its future. In other places, we see similar exercises of authority that do not constitute boundary work as described by Thomas Gieryn. In other words, they are not used to define science. They are used, however, to stabilize what is considered acceptable science policy. Scientists and agency administrators made claims about expertise and credibility, for example, to exclude publics or even just certain publics from policy discussions even as they gave broad vocal support to the inclusion of lay publics in policy, broadly speaking.

The Study and Research Design

Research Design

The concepts that I have reviewed provide the theoretical scaffolding for answering my research questions: why attention to ethical and societal concerns and public engagement emerged for some research projects and not others and, second, how scientists and science agencies negotiated their autonomy and authority in the context of these new non-scientific actors and non-technical concerns. I chose three cases to answer these questions because they exemplify upstream attention to non-scientific concerns for high-profile research. I began with the National Nanotechnology Initiative (1999-2015) because literature about deliberative democracy heralded it as a "unique opportunity" to realize the potential for democratizing science in the United

States (Barben et al. 2008, Davies 2006, Delgado et al. 2010). I chose the Human Genome Project (1989-1999), with its program in Ethical, Legal, and Social Implications because it was the next most recent high-profile, large-scale research initiative with significant attention to non-technical concerns and which treated a certain sub-class of non-scientists, social scientists and professional ethicists specifically, as relevant actors. Additionally, the National Nanotechnology Initiative followed, temporally, directly on the conclusion of the Human Genome Project. I wanted to analyze the degree to which the NNI borrowed from the HGP in designing its upstream approach. Finally, Asilomar (1973-1977) is framed as one of the earliest attempt by scientists to deal with the non-technical concerns of science in an upstream manner (Abels 2005, Hurlbut 2015). Asilomar was not yet a major scientific initiative but became one over the course of the debates about it.

These cases, then, offer an informative temporal perspective on the evolution of approaches to upstream handling of non-technical issues for science and, with that evolution, the negotiation of scientific autonomy and authority with changes that each research endeavor faced. Specifically, this dissertation analyzes changes in the pressures these initiatives faced and in what scientists and science agencies recognized as relevant concerns, expertise, and the role of publics. These analytical categories are summarized in Table I.2. Research on rDNA faced little outside pressure from concerned publics. However, it did face pressure from other bioscientists concerned about the safety of rDNA research. Since they came from other scientists, these concerns were purely technical in nature and, as such, did not warrant the inclusion of non-scientific actors or non-technical concerns. The whole Asilomar Conference was an exercise in protecting a strong version of scientific autonomy and authority. The Human Genome Project faced some external pressure from people and groups concerned about what would be done with a completed human genome. The Human Genome Project codified attention to non-technical concerns with its program in Ethical, Legal, and Social Implications (ELSI). Although publics still had no role, other than as objects of research, social scientists and ethicists were invited to participate in policy-making around the project. Their inclusion brought new kinds of expertise into what would have previously been a strictly scientific discussion. This should have made scientific autonomy and authority a more negotiated process, as scientists no longer had ultimate authority in determining what mattered about their research and how to go about doing that research. Finally, the National Nanotechnology Initiative represented a further expansion of concerns, publics, and expertise. The program in Societal and Ethical Implications of Nanotechnology (SEIN) was motivated by a strong fear among scientists and science agency administrators of a public backlash against nanotechnology. The SEIN program included exercises in upstream public engagement, giving a direct role to publics in the initiative and further expanding the kinds of expertise considered relevant. The inclusion of publics and their concerns, along with consideration of ethical concerns and societal implications, should have motivated a further renegotiation of scientific authority and autonomy. Again, since we take these research initiatives in their early stages, most of their approaches to these concerns, the role of publics, and expertise constitutes imagination work.

Other cases I considered, but decided not to pursue, were contemporary research into synthetic biology and geoengineering and the Super-Conducting Supercollider project of the 1990s. While synthetic biology and geoengineering evidence exercises in upstream public engagement, those efforts have not been funded in any way comparable to the National Nanotechnology Initiative. Furthermore, as of the time of this writing, they are not high profile or large-scale initiatives like the Human Genome Project and the National Nanotechnology Initiative. The Superconducting Supercollider constitutes an interesting case in failing to deal with non-technical concerns in an upstream manner. The $4.4 billion project that was already 20% finished and employed 2000 people was cancelled, in part, because scientists failed, in their imagination work, to show Congress how the project would benefit the United States either economically or in terms of scientific innovations (Appell 2013). It would have

Table I.2 Cases and analytical categories

		Dominant Imaginaries			
	Pressures	**Relevant Concerns**	**Relevance of Publics**	**Relevant Expertise**	**Scientific Autonomy**
rDNA / Asilomar	Little direct outside pressure on research. Strong inside pressure from scientists.	Only technical concerns.	No role for lay publics.	Only scientific. No new actors.	Strong scientific autonomy and authority.
HGP/ELSI	Concern about possible outside pressure. Strong inside pressure from scientists.	Technical plus ethical concerns and societal implications.	Publics an object of concern, but no role for lay publics.	Scientific and ethical/social. Inclusion of new actors: ethicists and social scientists.	Negotiated autonomy with the inclusion of new actors.
NNI/SEIN	Strong concern about outside pressure (public backlash). Little inside pressure.	Technical plus ethical concerns, societal implications, and public opinion.	Publics an object of concern. Role given to lay publics.	Scientific, ethical/social, publics/lay-publics. Inclusion of new actors: lay publics.	Autonomy further negotiated with the inclusion of more new actors.

been an interesting case showing the value of an upstream approach to the non-technical issues of science. I think that the Asilomar Conference and its backlash makes that point sufficiently, however, without introducing another case just for that purpose.

Methods of Data Collection

I collected different types of data for each of these cases. For the rDNA debate, I relied entirely on secondary sources. These included books and articles about the Asilomar conference including two books written by journalists who attended the conference (Rogers 1977, Wade 1977).[9] For the Human Genome Project, I also relied heavily on secondary sources, again mostly books and articles, several by people with direct experience with the project and its ELSI program (Andrews 1999, Beckwith 2002, Duster 2002). For the Human Genome Project, I supplemented this with data on all of

[9] These citations, and the ones below for the Human Genome Project's ELSI program, do not constitute a complete bibliography for these initiatives but only reference those people who were directly involved.

the funded projects in the Ethical, Legal, and Societal Implications program (n=186). I read the abstracts for each of these grants and categorized them according to two sets of criteria. First, I categorized them according to whether they involved research on people (e.g. studying public opinion through surveys), ethical issues, institutional practices, the institutions themselves (e.g. historical research), or research involving people (e.g. observational research about people's decision-making in a hospital setting). Second, I categorized those grants that dealt with the public according to the knowledge each tried to obtain of the public. These grants were intended to increase understanding of public perceptions, public actions and reactions, education, best practices in dealing with the public, other outcomes (a catch-all category).

For the National Nanotechnology Initiative, I analyzed 17 Congressional hearings related to nanotechnology, 38 NSF workshop reports, the National Science Foundation budget documents from fiscal year 2000 to fiscal year 2015, and six pieces of proposed legislation dealing with nanotechnology. To deepen my understanding of upstream public engagement for nanotechnology, I studied five upstream public engagement exercises around nanotechnology. I obtained the background documents given to participants for two of these events and full transcripts for one of them. I analyzed published documents and interviews with the people involved for all five of these exercises.

I interviewed a total of 33 people associated with the National Nanotechnology Initiative and its Societal and Ethical Implications of Nanotechnology Program. Interviews were semi-structured and tailored to the experiences and background of the particular person being interviewed. To prepare for each interview, I read all relevant publications or accessible presentations by the interviewee. Although I tailored each interview to the respondent, interviews shared several broad themes. These included how a participant became involved with nanotechnology, the details of the upstream public engagement events they organized or helped conduct, how they interacted with other scholars involved in the engagement events, their personal and professional goals for public engagement, and any insights about the relationship between the project and science funding agencies. Interviews gave me important insights into these aspects of upstream public engagement that official documents could not reveal. I solicited interviews from anyone who had been involved in these public engagement exercises, in the Science in Society Centers that organized them, or in the relevant administrative agencies tasked with the oversight of the Science and Society Centers. I obtained the names of my interviewees from publications and from the list of co-primary investigators listed on the NSF grants for this upstream public engagement work. I solicited interviews from agency administrators at the NSF and DOE who oversaw this public engagement work. I solicited a total of 51 people with a resulting 33 interviews for a response rate of 64%.

The 33 interviews I conducted, included interviews with persons directly involved in the public engagement exercises (n=28). These interviews focused primarily on the engagement exercises, respondents' role in organizing or moderating them, and their assessment of how well the exercises worked and the challenges of organizing them. These interviews also included questions about respondents' professional backgrounds and involvement in the NNI, as well as broader questions about public engagement. In addition, I also interviewed representatives of administrative agencies overseeing the Science and Society Centers (n=2), including two former NSF Program Directors, focusing more generally on the role of the NNI in fostering public engagement and their views of the successes and challenges of these efforts. Finally, I interviewed several individuals serving in a leadership role for a civil society group (n=3) that had been recruited as part of an initiative to encourage engagement in issues of science policy, and my interviews focused on their experiences with this initiative.

I conducted interviews over the phone or using a video conferencing program such as Skype or Blue Jeans. Interviews lasted between 35 minutes and two and a half hours with the average interview lasting one hour. The interviews were audiotaped and transcribed. With the exception of the individuals from civil society groups, who permitted me to reveal their identities, all interviewees have been given pseudonyms to protect their identity. This project was approved by the University of Michigan IRB.

I coded the interviews, event transcripts, background materials, and Congressional hearings in Atlas-Ti at the paragraph-level according to several themes. These themes included upstream public engagement, ethical and societal concerns, modes of governance, research logics, and outcomes, among others. I did not code the other public documents to the same degree of specificity but rather performed a close reading of them.

I was unable to include sufficient qualitative data for the Human Genome Project and its ELSI program or for the Asilomar conference because I was unable to interview enough people who had worked on those projects. None of the individuals associated with the ELSI program returned my requests for an interview. Three of the people I interviewed for the National Nanotechnology Initiative had experience with the Human Genome Project, but I did not deem that sufficient to include them as data for that section.

Summary of Dissertation Chapters

In this dissertation, I will use public documents and interviews to understand how and why ideas about acceptable science policy were expanded and how policy-relevant discourse was disciplined for these cases. I have already referenced the story to come several times as I explained the relevance of the ideas brought in here to this dissertation. Therefore, this summary will be brief. I should say, however, that I focus more on the emergence of the public in the National Nanotechnology Initiative and the cases prior to this are mainly used to illustrate the novelty of this expansion in ideas about acceptable science policy.

In Chapter II, I explore the debate about recombinant DNA (rDNA), with a particular focus on the Asilomar Conference and the Ethical, Legal, and Social Implications (ELSI) program of the Human Genome Project. The data for the rDNA debate come from secondary sources. The data for the Human Genome Project and its ELSI program also come mainly from secondary sources, but I also obtained the records of all funded ELSI projects from 1990-1999 to have a better understand of what kinds of research took place. I show how, for rDNA research, ideas about acceptable science policy came to include a new idea, the voluntary moratorium, while excluding new participants. Working in a fully technocratic mode of governance, bioscientists researching rDNA trusted only themselves to identify relevant issues with their research. They hoped that their actions would serve as evidence, in a manner reflective Ezrahi’s visual-attestive culture, the safety of rDNA research and their commitment to doing responsible science. The Human Genome Project expanded this boundary further. With the creation of the ELSI program, the National Institutes of Health (NIH) and the Department of Energy (DOE) included a new set of actors: social scientists. This is a hybrid technocratic order comprising both science and social science, whereby social scientists were supposed to bring a new set of concerns based in new kinds of expertise – particularly legal, ethical, and sociological – but were supposed to do so in ways that did not fundamentally challenge the value or legitimacy of the larger initiative. This mode does not upend the privileged place of technical experts in favor of any direct public witnessing, but rather adds a new kind of expertise into the technocratic mode.

In Chapter III, I turn to the National Nanotechnology Initiative (NNI) to understand how and why social scientists and public engagement became acceptable as science policy. The data for this chapter come from mainly from public documents, including Congressional hearings, National Science Foundation (NSF) reports, budget documents. There were several reasons for this. In part, it was a response to the latent potential of nanotechnology. Advocates for nanotechnology made incredible promises and the public seemed to pose the most important threat to realizing this promise. The inclusion of lay publics was also a case of institutional learning on the part of science agencies from happenings in Western Europe. Crises of public trust around genetically modified organisms (GMOs) and the response of European agencies served as a warning to American science agencies about what can go wrong if you do not effectively imagine the public and its responses to new technologies. Finally, the inclusion of lay publics also came from battles within the emerging field of nanotechnology about its societal implications. Efforts to discipline the discourse of those within the field of nanotechnology led to the realization, among science agency administrators and policymakers in Congress, that it might be necessary to include lay publics in policy discussions, if only to discipline their discourse around nanotechnology.

In Chapter IV, I follow the public engagement work for the NNI. The data for this chapter come almost entirely from 33 interviews I did with the social scientists whom the NSF enrolled to do this engagement work, two former NSF program directors, and three representatives of science-related civil society organizations. I show that while this engagement work was an innovative expansion in ideas about acceptable science policy, it also represented a certain disciplining of discourse around nanotechnology. Discourse was disciplined in three important ways. First, it was disciplined by the organization of public engagement by the NSF. Second, it was disciplined by the social scientists themselves in constructing the deliberative publics for their engagement exercises and in the issues that would be discussed. Finally, discourse was disciplined by the impact these engagement exercises were allowed to have on science policy.

CHAPTER II

The Asilomar Conference and Human Genome Project

Introduction

This chapter deals with two different approaches in changing ideas about acceptable science policy: the rDNA debates of the 1970s, culminating in the Asilomar Conference, and the Human Genome Project's Ethical Legal and Social Implications (ELSI) working group. In terms of inviting new ideologies into a scientific domain, the Asilomar Conference of 1975 and its related meetings and events remains one of the most interesting and noteworthy cases. The conference dealt with recombinant DNA (rDNA) research. Scientists, not publics, began to worry about the public health risks associated with the way that this research was being conducted. This alone makes the case noteworthy. Usually it is only because public concerns begin to pressure scientists or industry that the latter responds to risk concerns. With rDNA, scientists were the ones calling for attention to this. They had clearly begun to learn some of the lessons of the Risk Society, that a crisis of public health can sink public support for research. Additionally, scientists decided to postpone their research through a voluntary moratorium until they discovered the risks associated with rDNA research.

These were new ideas to the scientists in the rDNA debate. They represented a creative way of securing scientific autonomy for a potentially controversial new line of biological research. The bioscientists working in rDNA embraced a moratorium and even the possibility of total relinquishment of the research because it gave them time to address the most fundamental problem of the risk society: that scientists cannot control the risks involved with their own work. The moratorium was intended to protect scientific autonomy by keeping outsiders out of the scientific domain and thereby reinforcing the privileged place of scientists to determine the nature of their own work. Over the course of the rDNA debate, the ideas of moratoria and relinquishment were eventually deemed blasphemous.

The Human Genome Project and its program in the Ethical, Legal, and Social Implications (ELSI) of the genome represented a different approach. For the ELSI program, moratoria and relinquishment were considered blasphemous. Discursively framing them out of the ELSI mandate was the first activity of the ELSI working group and a key component of disciplining bioethical discourse around the project. Instead of deferring research, so that scientists could address all of the problems, the Human Genome Project enrolled a new set of experts tasked with addressing the ethical, health, and safety issues so that the science could move forward unimpeded. There were two types of new experts: social scientists, from outside the fields of the biosciences, and ethically minded scientists. Nonetheless, their approach to these issues reflects a technocratic logic of solving social issues like regular research questions.

Each of these cases represents a different imagination about the public context for these emerging technoscientific projects and different approaches to science policy and governance. Whereas the Asilomar Conference attempted a purely technocratic approach to the potential issues of rDNA research, the Human Genome Project still took a technocratic approach to ethical and societal issues. It expanded the boundaries of science policy to include new actors with expertise in social and ethical issues, but ultimately expected those actors to treat social issues as if they were technical ones: objectively discoverable like natural facts.

rDNA Research and the rDNA Debate

Recombinant DNA research involves cutting DNA at some point in the chain and reassembling it. Usually scientists wanted to insert a new sequence of DNA into a cell's genome, but they may also simply want to excise a sequence. This was the start of genetic manipulation by means other than sex-selection. It was the first direct manipulation of a DNA strand rather than generational change. The process at that time was not easy by the standards of technology today.[10] Scientists used an enzyme to "cut" DNA at a certain point in the strand. That cut leaves what scientists colloquially call "sticky ends," because of their propensity to attach, or stick, to their complementary base pair (Rogers 1977). Doing this once, however, would only yield a single strand of altered DNA, which by itself, is not very useful. In order to alter the form or function of an organism, the altered DNA must replicate throughout the organism. A single strand of altered DNA would not propagate in an organism because even if that single altered cell replicated well, it would be drowned out by the abundant replication of non-altered cells in the organism. To get the new DNA to propagate well, biologists had to use either a virus shell or a bacterial. These would "infect" surrounding cells with the newly altered DNA. Viruses act like little hypodermic needles, inserting new DNA into the cells around them. Bacteria have a natural mechanism, the plasmid, which allows them to share DNA. This is what makes bacteria so effective at mutating in response to anti-biotics. It only takes one bacterium to mutate in such a way as to render an antibiotic ineffective. That bacterium can then share its mutation with nearby bacteria via plasmids, without having to wait to reproduce as almost all other organisms would. The rDNA research discussed here involved propagating a new DNA sequence in bacteria using the plasmid method.

While these kinds of genetic transfer might spark concerns for many people, this is not what ultimately sparked concern among scientists or the rDNA debate. Rather, scientists became concerned about the choice of DNA to alter and the choice of bacterium in which to propagate the new DNA. Scientist do not use just any DNA or any bacteria when they engage in experiments like these. Instead, they use those organisms and DNA that are most often used for other experiments by other scientists. This makes research easier, since much is already known about the idiosyncrasies of the materials used. They also give a degree of scientific control to the otherwise complex and messy nature of biological research.[11] Thus, when Stanford biologist Paul

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[10] CRISPR makes the direct manipulation of DNA much easier than the rDNA method described here.

[11] This can also lead to problems for research. The fruit fly *drosophila* is the most common object for genetic research. Having been propagated in the lab for hundreds of thousands of generations, however, it no longer resembles its counterpart in the wild. When scientists want to use other organisms, such as Barbara McClintock's work on corn genetics, they can be met with scorn by their colleagues (Keller 1984).

Berg wanted to insert eukaryotic (i.e. non-bacterial) DNA into a bacterium, he chose a DNA source and a bacterium that were already well studied. For the DNA that he wanted to insert, he chose Simian Virus 40 (SV40). Not only had it been the subject of a great deal of research, but it also responded particularly well to existing tools for mapping DNA. This made it more likely that it would respond well to being cut and inserted into a new organism. When he chose a bacterium into which he would insert SV40, he chose *Eschelerius coli* (E. coli). It had also been the object of a great deal of research, more than any other bacterium. Why should this raise concerns? To start, SV40 is a viral tumor-causing agent found in primates. Scientists reasonably assumed that it would cause tumors in humans as well. Furthermore, the bacterium E. coli is found in every human gut. Scientists feared that when SV40 was inserted into E. coli, Berg might inadvertently unleash a public health disaster in the form of a communicable tumor virus. Moreover, this risk came in the same year that marked the so-called War on Cancer. Thus, this risk went against the grain of the most important biological research at the time. Furthermore, scientists working with rDNA had research showing that laboratory workers exhibited immune responses to the materials they worked with, suggesting that containment was anything but perfect. Thus, there were real reasons for concern.

In the Asilomar Conference of 1975, scientists hoped to fully identify and address this concern and others for rDNA research. It is now regarded as a seminal event in the development of scientific ethics. Only a few at the time had a sense of its real importance. The Asilomar Conference was actually two conferences that were part of a larger set of meetings among scientists and, later, federal agencies about the risks of recombinant DNA (rDNA) research from around 1971 to 1978. The first Asilomar conference in 1973 was narrowly focused on the hazards of the SV40 virus. When I discuss *the* Asilomar Conference, I refer to the larger and later one in 1975 that concerned rDNA research more broadly.

A Voluntary Moratorium

Although historical accounts often begin at temporal beginnings simply for the sake of natural storytelling, the way in which the rDNA debates began is actually one of its most important features. Paul Berg had, as explained above, begun experiments to insert DNA from SV40 into *E. coli*. The earliest concerns came from other scientists when one of Berg's graduate students, Janet Mertz, discussed their work at a workshop on cell culture at Cold Spring Harbor Laboratory in June of 1971. Mertz presented the research matter-of-factly, unaware that anyone might have concerns about it. The organizer of the workshop, Robert Pollack,[12] had already expressed concerns about the "laissez-faire" distribution of SV40 to biologists seemingly for the asking (Friedberg 2014, Wade 1977). There were no safety protocols or even a set of agreed-upon best practices for the use of this potentially dangerous virus. Pollack and Joseph Sambrook, scientists also present at that 1971 meeting, had written a lengthy memorandum about the use and availability of SV40. Their memorandum included a section titled "Are there any good experiments using human cells and viruses that should not be done?" (Friedberg 2014:207). This would prove a groundbreaking question and one which would become central to the entire rDNA debate: should even good science that it was possible to do ever *not* be done? Furthermore, should it not be done for reasons that had nothing to do with science itself? Berg had introduced laboratory biology to the ideas of a moratorium and even possibly relinquishment.

Pollack responded with surprise and incredulity when Mertz described the work she and Berg were planning. He wanted to know how close they were and if they had done the work already. Mertz recalled, "I told them that all the technology other than that needed for actually generating recombinant DNA molecules was already available in Paul's laboratory and that we were almost at the point where we should be able to amplify mammalian DNA in *E. coli*." Pollack, in his own words, "had a fit" (Wade 1977:33). He took a day to calm down and gather his thoughts, after which he admonished Mertz, "Do you really mean to put a human tumor virus into *E. coli* — a gut bacterium? Don't you know that SV40 is a tumor virus?" (Friedberg 2014:206). Pollack was not the only one concerned. Berg began to receive calls from others who had attended the Cold Spring Harbor workshop. Berg remembered their comments, saying, "I think I was upset by the criticism at first, but then I went out and started to talk about

[12] W. Emmett Barkley, Edwin Lennette, and even James Watson also thought that there should be more stringent controls on SV40 particularly and on potentially dangerous reagents in general (Wade, 1977: 30)

the problem with a lot of people" (Rogers 1977:37). He came to realize that the concern was more widespread than the few vocal people who had actually contacted him. Berg decided to postpone his work on the experiment and suggested that others doing similar work follow suit. In the early days of the debate, before the issues fully caught on, many scientists would repeat Berg's process of denial and realization. Many, who were at first annoyed at what they saw as personal criticism of their work and scoffed at being asked to consider non-scientific concerns, later came to accept that these concerns were legitimate and needed wider consideration. Although not in a policy context yet, the imagination work around rDNA research began here. The people doing the research and those concerned about it represented different imaginaries about scientific research, its consequences, and the duty, or lack thereof, of scientists to deal with these issues in the course of their normal workday.

For those who would later come to participate in the Asilomar Conference, Berg's moratorium was groundbreaking. There had been voluntary moratoriums on *publishing* the results of potentially dangerous science but, to their minds anyway, never one on doing the research in the first place. Berg remarked at the time "It is the first time in our field. It is also the first time anyone has had to stop and think about experiments in terms of potential hazard" (Rogers 1977:37). One could argue with Berg on a number of points here. Medical experiments, for example, have often been deferred for ethical or moral reasons. Experimental heart transplants in the era before anti-rejection drugs were postponed, for instance, when it became clear that heart recipients simply were not surviving beyond a few weeks or, in the best cases, a few months (Fox and Swazey 2002). To be fair to Berg, though, medical experiments have direct emotional impacts on both patients and experimental surgeons. As Fox and Swazey note in their ethnography of experimental surgeons, these surgeons treat their patients as "personal associates and professional colleagues" in the experimental treatment process (2002:95). This process can be long and very intimate as well. Together, the patient and experimental surgeon "must bear the emotionally and time-consuming burden of constant surveillance of his condition" (107). Biologists in the laboratory are unlike experimental surgeons in this respect. They are far removed from the eventual outcomes or consequences of their research. In any case, In Berg's mind laboratory research that scientists were capable of doing and which had been fully funded had never before been voluntarily deferred for non-scientific reasons. That Berg decided to stop his work, even temporarily, for uncertain consequences well removed from the context of his own laboratory was unprecedented. Indeed, many of his colleagues feared that it would set a precedent.

The voluntary moratorium by Berg's lab did not avert concern by itself. In part, this was because it did not receive the same media attention as the work itself. In the October 1972 edition of the *Proceedings of the National Academy of Sciences* (*PNAS*) David Jackson, Robert Symons, and Paul Berg published an account of their rDNA work, specifically the extraction of DNA from SV40 and the "sticky ends" of the DNA that would allow them to implant it into an *E. coli* bacterium. A biology correspondent from *Nature* saw the same problems that Pollack and others had seen. In a response to the *PNAS* account, the correspondent wrote:

> *What would be the consequences if the reagent Berg and his colleagues have made somehow infected and lysogenized E. coli in someone's gut as the result of an accident? This possibility, remote thought it may seem to be, can hardly be ignored, and it will be most interesting to learn what criteria the group adopts when it decides whether or not the scientific information that might be obtained by continuing the experiment justifies the risk. Perhaps those involved will decide that the game is not worth the candle. (Nature 1972 240:73).*

The public information director at Stanford, Spyros Andreopoulis, having read the *Nature* piece approached Berg to ask whether they should make a public announcement of the experiment to avoid possibly negative press and public response. Berg responded indignantly that he had already stopped doing the experiment and was upset that *Nature* had somehow not known or failed to report on that (Friedberg 2014:203). To be fair, the correspondent described the tumor-virus concern as the "fly in the ointment which no doubt Berg and his colleagues are well aware of" (Nature 1972 240:73). Whether or not *Nature* should have known that they had deferred their research is not clear. What is clear is the tension between displaying efforts to respond to scientific uncertainties with public health implications and the consequences of doing so. Berg and his team had not made public their concerns about their research nor their prophylactic moratorium to deal with those concerns. To do so would have invited scrutiny from those either outside of or not allied with their scientific domain and potentially reduce the autonomy of Berg and his team. Unwittingly foreshadowing the Asilomar Conference to follow, he tried to do the ethics work behind closed doors and present the scientific work to the wider scientific world.

Berg's personal decision to defer his research translated into a general call for all rDNA researchers doing potentially hazardous work to do the same. This started in June of 1973 at the annual Gordon Conference. The Gordon Conference was an annual three-day meeting for discussing highly technical issues in biological research. Issues of risk and uncertainty were not central to the Gordon Conference. In fact, these issues came up at the 1973 meeting as an afterthought. Younger scientists, unnamed in the historical accounts of the rDNA debate, approached conference co-chair Maxine Singer of the NIH and Dieter Söll to suggest attention to the safety issues of rDNA research. Singer and Söll set aside fifteen minutes on the last day of the conference to discuss these concerns. Around one-third of the participants had already left. The majority of the ninety who remained, however, agreed that a letter should be drafted about the safety issues or rDNA research. The Singer-Söll letter, as it came to be known, was sent to the National Academy of Sciences (NAS) and published in the September 21st issue of *Science*. When Philip Handler, President of the NAS, asked Singer for guidance on how to proceed, she directed him to Paul Berg who, by that point, spent the most time of any single individual thinking about the issues. Berg, however, saw himself as no expert on ethics or even lab safety. He convened a group at MIT to discuss the issues thinking that this would be the end of his role. "I naively believed that the advice of the group would be passed on to [NAS President Philip] Handler, and once it was in his hands I would be free of additional involvements" (Friedberg 2014:240). Berg could not have been more wrong. Far from ending, the debate was just getting underway. Additionally, since no one was looking outside of the scientific domain for guidance on the ethical, health, and safety issues of rDNA research, Berg became the default expert on these. That he had no expertise in ethics, and had never professed any, did not seem to matter.

The catalyst for the larger debate came out of the MIT group. In 1974, they drafted and unanimously signed a letter suggesting a voluntary temporary moratorium on certain types of research until the risks of that research could be better assessed. Several prominent scientists who had not been present at the MIT group wanted their names added to the letter. One such person was James Watson, the brash and mercurial figure who would later denounce both the letter and the entire concept of a moratorium as not only useless but also downright dangerous. The letter was almost immediately dubbed the “Berg Letter,” because it was an extension of Berg’s voluntary moratorium. The letter also established Berg’s role as the de facto, and completely reluctant, spokesperson for the bioethics of rDNA research. Although this broader moratorium still only applied to a handful of laboratories, all of which were in the U.S., those labs constituted the only ones capable of inserting SV40 into *E. coli*. This effectively made the moratorium a global one. Returning to Pollack’s question, should even good research ever not be done, Berg and the MIT group had answered firmly in the affirmative.

Asilomar

On its surface, the rDNA debate was about the uncertainties inherent in an emerging technology. The uncertain consequences of rDNA research led to uncertainties about how to pursue and govern such research. In reality, the debate was about much more. If it were simply about the research itself or its consequences, then the temporary moratorium would have been a mere technical matter to be solved like the cleanup work of any piece of Kuhnian normal science. Rather, bitter in-fighting erupted around the moratorium between those who thought that uncertainty warranted prompt regulatory action and those who thought that uncertainty could not justify any regulatory action. For those involved, the stakes were not simply technical, the research of only a few labs around the world, but would set a precedent for the biosciences in a larger sense. Implicit in the rDNA debates was a debate about the autonomy of the biosciences. By meeting and setting the guidelines for their own self-regulation, they hoped to give both the lay public and their political representatives no reason to have a role or even to want one. The goal for most of the organizers by that point was to put an end to the voluntary moratorium still in place, but to do so in a way that protected public health and safety from a crisis caused by rDNA research while protecting science from intrusion. They saw Asilomar as a zero-sum game. Success meant autonomy. Failure meant the intrusion of federal regulation, a frightening outcome for many bioscientists.

The Asilomar Conference not only had the goal of protecting scientific autonomy but also was itself organized as an exercise in scientific autonomy, leading to several problems for the success of the conference. For instance, the organizers of Asilomar quite unwittingly and with no prior experience, tried to invent a deliberative consensus conference. A deliberative consensus conference is a particular mode of democracy made popular by Jurgen Habermas (1991). Essentially, people deliberate a set of issues together and arrive at a consensus about how to move forward. Organizers of Asilomar wanted to pursue this democratic model, but with only scientists present, no one had any experience in doing so. Nothing indicates that they were aware of Habermas or the theory of deliberative democracy. The way they describe Asilomar, however, is exactly the definition of a deliberative consensus. It is a testament to their sense of scientific autonomy, particularly the irrelevance of non-scientists that they did not think to look beyond their own domains of expertise when organizing and running Asilomar. No one suspected that something like Asilomar already had a great deal of theory and practice behind it. Instead, they tried to reinvent the deliberative consensus conference and in so doing, made some novice mistakes. David Baltimore, one of the chief organizers of Asilomar, began the conference by trying to coerce consensus preemptively. He warned that they had to reach a consensus because there was no higher authority to turn to in the matter of rDNA. Failure to do so would invite formal regulations that nobody wanted. When asked how they would achieve such a consensus he responded, obliquely “The procedures by which the consensus will be determined will be largely determined by the extent of the consensus” (Wade 1977:43). In other words, there was no real plan to achieve consensus. If one did not emerge more or less organically from discussion, then they would have to improvise. Unsurprisingly, consensus did not emerge easily. They wound up having to improvise. One reporter marveled at the lack of organization and the difficulty of consensus. He recalled the trouble they were having getting discussions going. Then "with almost audible creaking, did the wheel of discussion begin to turn. And it proceeded to run right downhill into chaos. Odd, I thought at the time, that a roomful of leading minds on a leading edge of science can't agree on how to run a meeting" (1977:60).

In another example of a novice mistake for a deliberative consensus conference, certain high-status actors, specifically James Watson and Joshua Lederberg, unduly influenced the meeting. James Watson was a Nobel-laureate and co-discoverer of the double-helix structure of DNA. Joshua Lederberg was a microbiologist and a Nobel-laureate. He received his Nobel Prize for discovering the process by which bacteria can share DNA. Watson and Lederberg were the two primary voices opposed to any substantive considerations of risk. So loud and vociferous was their opposition that journalist Nicholas Wade, in attendance throughout the conference, described them as "*enfants terribles*" (1997:43). At one point, towards the end of the conference, several people had been calling for a vote to gauge the sentiment in the room regarding an ongoing moratorium of rDNA research. Watson and Lederberg loudly and angrily protested, saying that the room was intractably split. It was not until Sydney Brenner, a well-liked Cambridge molecular biologist, called for an impromptu vote that it became clear just how minority a position Watson and Lederberg represented. Support for an ongoing moratorium was near unanimous. A puzzled Paul Berg, Chairman of the meeting, said:

> *It was then that we realized that we'd been listening to the wrong people. A few people were doing all the talking, and a lot of people had been quiet. And the quiet ones were in favor of coming out with something just as we had, and it was the Lederbergs and Watsons and a few others who were doing all the talking and confusing us. We thought they were reflecting what everybody wanted and felt* (Rogers 1977:86).

This shows the intersection of status and power in driving the imagination work going on at Asilomar. The two dominated discussion at Asilomar despite not representing anything close to a majority position. That it took almost the entire conference to figure this out shows how much they were learning as they went about consensus conferences and deliberative decision-making.

The question of who got to attend Asilomar also reveals it as an exercise in preserving scientific autonomy. Krimsky reports that the first Asilomar conference in 1973 was attended by “individuals from a broad range of disciplines within the biological and health sciences” (1982:64). No one from outside those sciences was in attendance, including the media. Indeed, the only thing to report would have been the speculative nightmare scenario of a communicable tumor virus, which no one in attendance wanted to advertise to the public. In the lead-up to the second Asilomar Conference in 1975, however, the situation changed. In the two years since the first conference, the news media had discovered rDNA research. This was unsurprising. As Krimsky observed, rDNA research “has all the prerequisites for a science-fiction thriller: new dread diseases possible; cloning; human genetic engineering; human gene maps; interspecies hybrids” (1982:100). Recombinant DNA had the key elements for mass public concern. Yet few seemed to realize the extent of this potential, especially not so far as to consider the lay public, or any non-scientists, in any way relevant for the conference itself. A pamphlet titled *The Health Hazards of Gene Implantation* published by the civil society organization Science for the People criticized the conference organizers for inviting “predominantly research directors” (Krimsky 1982:109). Nicholas Wade described Asilomar attendees more colorfully as “not quite monarchs, but at least the paladins of their own special world” (Wade 1977:41). These paladins gathered to define, for themselves, the united front that they would present to the larger world regarding rDNA research and its uncertainties.

Some wanted to broaden the scope beyond simply research scientists but those people were disregarded. Early suggestions that the conference should have a public engagement component or that representatives from the “broader public” should attend were not taken seriously by organizers. Organizers feared that including these outsiders would make consensus difficult or even impossible in the limited time that they had. The closest thing to broader engagement was an invitation to Jonathan Beckwith, a Harvard microbiologist and a member of Science for the People, to speak to the social implications of the research. Having to decline, he suggested his colleague Dirk Elseviers, a post-doctorate researcher whom, he suggested, might represent the views of Science for the People. Berg rejected the suggestion saying that they wanted Beckwith's opinion not that of Science for the People. Beckwith had been invited as an insider, another paladin of that special world. His participation in Science for the People may have given him a certain added expertise about the social implications of science, but that perspective was only valuable by virtue of his status as an insider. Anyone else from Science for the People was simply an outsider. The group represented something more radical and potentially heretical than organizers of Asilomar were willing to entertain. Had someone from Science for the People attended they would have been the only representative of a civil society organization at the conference. Incidentally, Beckwith later became a founding member of the Human Genome Project's ELSI working group fifteen years later. By that time, though, he was, by his own admission, much less radical and no longer supported moratoria on scientific research (Beckwith 2002).

Although Asilomar was an exercise in scientific autonomy, consisting mainly of insiders, it did include two important kinds of scientific outsiders. These were journalists and lawyers. The decision to include them was not without controversy. Scientists did not trust journalists, whom they thought would misunderstand and sensationalize the hazards posed by rDNA research. Others thought it might be more dangerous to exclude them, since the proceedings would have no transparency to the public. In Ezrahi's terms, they needed journalists to perform the all-important role of public witness to the proceedings (1990, 2012). The first vote at Asilomar, then, concerned the question of whether to allow journalists to participate. The vote determined that journalists could attend, but only if they came every day, stayed until the end, and did not report on anything until the conference was finished. They were not invited to serve as participants but only to bear witness. Many scientists worried about their capacity to do this as well, fearing that the same lack of understanding that led scientists to question their presence in the first place would lead the journalists to report on Asilomar in a negative light. Despite the fact that they were physically present, the, journalists were not there as insiders. They were tolerated outsiders. In terms of information, Asilomar was to be contained within a hermetic seal until it was over. In one way that hermetic seal remains. Organizers had decided to record the entire proceeding but put a 50-year embargo on its public release. At the time of this writing, there are still nine more years to go before those tapes become available.

The inclusion of lawyers was less contentious. This may have something to do with the fact that one of the organizers, Maxine Singer, was married to Daniel Singer, a lawyer whom she suggested as a participant. Many of the organizers and scientists in attendance already knew Daniel through Maxine and, presumably, trusted his commitment to scientific autonomy. Maxine's credibility as a scientist and supporter of rDNA was partially transferred to her husband and thereby to the inclusion of lawyers more broadly. She made him something more than just another outsider. Nonetheless, the participation of lawyers at Asilomar was still discretely demarcated from the rest of the discussions. At Maxine Singer's suggestion, they were given their own plenary session to discuss the legal and ethical issues of rDNA research. And like the inclusion of ethical issues at the Gordon Conference, the inclusion of lawyers was something of an after-thought to the planning of the main conference. Furthermore, the scientists at Asilomar seemed to think the lawyer's plenary session would constitute a small addendum to their meeting, simply showing them how to avoid lawsuits. Instead, the lawyers' presentations marked a sobering moment for the scientists in attendance[13].

The lawyers' presentations forced scientists to recognize that the social implications of uncertain research like rDNA came with strong political and practical limitations to their professional autonomy, particularly their capacity to retain control over issues of regulation, safety, and broader social concerns. The lawyers introduced three types of external considerations: the deeply problematic nature of uncertainty, the role of ethics in science, and the relevance of non-scientific actors, particularly the lay public. Singer was the first to speak. He challenged the assumptions that the risks of rDNA research were remote and that it would be morally wrong to place significant restrictions on research. In essence, he was defending some measure of long-term moratorium. He argued that scientists had a responsibility to prove that their research was safe to the public and not only to themselves. He also warned scientists to accept the fact that some experiments that, in Krimsky's words, were "elegant and intellectually

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[13] The lawyers were, in order of their presentations, Harold Green, affiliated with George Washington University, Daniel Singer, affiliated with the Hastings Institute and working for a law firm in Washington D.C., Alex Capron from the University of Pennsylvania, and Roger Dworkin from Indiana University.

satisfying, may be morally reprehensible” (Krimsky 1982:139-140). Despite arguing for greater public engagement and relinquishment, his presentation seemed not to raise many hackles. This may have been due to his measured and philosophical tone. Singer also did not say anything that some of the scientists in attendance had not said already. In a sense, the very fact of Asilomar was a testament to their attempt to take his points seriously.

Alex Capron, on the other hand, raised many hackles. Capron told the scientists in attendance “they could no longer ignore public participation” (Krimsky 1982:139-140). The publics he imagined were laboratory workers and the “broader public.” Both were subject to risks from rDNA research over which they had no control. The lab workers, of course, most immediately subject to these risks. Capron did not have any real suggestions for how to address these various publics. Rather, his argument stemmed from a philosophical and pragmatic legal conviction about the limits of intellectual freedom and a scientific duty to serve the public good, or at least avoid unnecessarily harming it. He reminded the scientists in attendance, as Krimsky summarizes, that “freedom of thought does not imply freedom to cause physical injury to others” and that restraint is necessary where “irreversible harm is threatened.” Capron said bluntly that “this group is not competent to assign overall risk” (Rogers 1977:140-142). Instead, he argued that “it is the right of the public, through the legislature, to reach erroneous decisions — while it remains your right, and is probably your obligation, to lobby against such decisions but to abide by them in the meantime” (141). Rogers describes Capron’s discussion as a “merciless ‘outsider’s analysis’ that within moments had jaws dropping all over the chapel” (77).

Capron brought into question the very nature of scientific autonomy. He violated what Krimsky called the “key value at Asilomar” (1982:152). In so doing, Capron put into doubt the ability of scientists to determine the content of their field and to direct its course free from unwanted interference. Essentially, he told the scientists at Asilomar that the conference would inevitably fail to achieve what they wanted. This seemed to elicit shock and dismay among the scientists in attendance. They had already allowed some previously unthinkable ideas into the scientific domain, specifically the moratorium of laboratory research, but now they were being told that they had no choice but to invite outsiders into their own domain. This constituted a double-threat to their autonomy. If Capron was right, then not only would outsiders cross the science-society boundary, but scientists had no choice in the matter. Furthermore, Capron did all of this as an outsider himself, one of only five invited to participate within the scientific domain.

The last lawyer to speak, Roger Dworkin, gave a pragmatic and cynical spin to Capron's otherwise normative philosophical argument. In a manner consistent Jasanoff's (1990, 2004) observation about the particularly American intersection of legal authority in issues of regulatory science, Dworkin assured the gathered scientists that, given their present course, it was only a matter of time before they were the subject of a "multi-million-dollar lawsuit" (Krimsky 1982:152). He too argued for greater transparency to the public, as "any appearance of self-serving will sacrifice the reservoir of respect that scientists have, and will bring disaster upon them" (Wade 1977:50). These were the consequences, according to Dworkin, if scientists continued to hide behind their "traditional immunity from being called to account" (Wade 1977:49). Not only was their scientific autonomy at risk but, if Dworkin was right, it was pathological as well. According to Dworkin, scientific autonomy was the source of public distrust and any attempt to maintain or protect it might paradoxically threaten it further still. The one-two punch of Capron and Dworkin's presentations left many scientists in shock. Notably, James Watson was unfazed by the lawyers. He assumed that a multi-million-dollar lawsuit was inevitable. For him, it was simply the cost of maintaining their autonomy. Better to continue as they were and pay a few million dollars here or there than invite outsiders whose presence might fundamentally alter their most important value.

The question of how to proceed with rDNA research, particularly whether to extend or lift the moratorium on research, was ultimately decided by a vote among conference attendees. Attendees voted on proposals that had come out of various smaller breakout meetings among conference attendees. The breakout groups were defined by their area of shared scientific expertise. For instance, there was a virus group and a plasmid group. The constitution of the groups is not relevant here except to note that ideas for the final report of Asilomar were developed in them and voted on by the larger set of constituents. This allowed the idea of an ongoing moratorium, and even the possibility of total relinquishment, to flower outside of the vocal influence of people like Watson and Lederberg. The results of the voting were to be incorporated into a final report submitted to the NIH to guide policy regarding rDNA research. Paul Berg, Maxine Singer and a few others stayed up all night drafting the final report for the final day of the Asilomar Conference. The final report included one of the breakout groups' suggestion for a three-tier classification schema of high-, moderate-, and low-risk rDNA research. The levels of risk were determined by the feasibility of containing hazardous biological material and would require different safety protocols. By a majority vote, the scientists at Asilomar also included and approved a proviso in the report that "some experiments should not be done even in the highest containment presently available" (Krimsky 1982:146). The bulk of scientists in the rDNA debate, then, supported not only a moratorium of research but also relinquishment, the unprecedented notion that some research should simply not be done. Others, like vocal dissenters James Watson and Joshua Lederberg, thought that they had just set a dangerous precedent for the future of scientific autonomy.

In the short-term, at least, the Asilomar conference was a resounding success. It achieved its two main aims. First, it ended in a rough consensus. In some ways, it was a consensus that pleased no one, as consensuses often do. Nonetheless, Asilomar had met its goal, since the meeting ended with a set of suggestions for the NIH about how to regulate rDNA research. Furthermore, they were able to draft arguments about why the NIH should be their main oversight body, arguments that went unchallenged in subsequent years. Scientists at Asilomar saw the NIH as an ally and the government entity least likely to interfere in their desired autonomy.

One way to view the rDNA debate is as an experiential lesson into the nature of science in the modern Risk Society. Scientists thought that they could identify any relevant concerns and fully address them before anyone became aware of what they were doing. They did not yet understand that uncertainties proliferate and overflow the discursive work to contain them when you cannot be sure either of the risks involved or even how to identify them (Callon 2008). In response to such uncertainty, scientists at Asilomar generally took one of two positions. The first is consistent with the Precautionary Principle. In the face of uncertainty and risk, do everything possible to ensure safety for the public and those working in laboratories. This view justified calls for an ongoing moratorium and possible relinquishment of rDNA research. Others took the opposite approach. In the face of uncertainty, no action was justified, since you could not be sure that it would help. Watson vocally supported that stance. At one point, when talking about what to do about the risks of rDNA research, he shouted, “We don’t even *know* the fucking risks [italics in original]!” He was angry at the possibility that they might be regulated, and the science impeded, for risks that no one could enumerate, and which might not even exist.

In addition to this disagreement, scientists also disagreed about the fact that Asilomar had been closed off to all but scientists. Erwin Chargaff, a biochemist at Columbia University, had criticized Asilomar for being exclusive to rDNA scientists. He wrote:

> *It was with a feeling of deep melancholy that I read about the peculiar conference that took place recently in the neighborhood of Palo Alto. At this Council of Asilomar there congregated the molecular bishops and the church fathers from all over the world, in order to condemn the heresies of which they themselves had been the first and principal perpetrators. This was probably the first time in history that the incendiaries formed their own fire brigade* (Chargaff 1975:21, Wade 1977:104).

Chargaff’s charge of exclusivity and self-dealing at Asilomar would come to haunt the discussions of rDNA research and, in some cases, science more broadly after Asilomar. Partly because of this, arguments emerged that the public deserved a more significant role in scientific policy- and decision-making.

The Public Emerges

The whole point of Asilomar was to keep the discourse about rDNA research within the confines of the scientific domain. Asilomar had been an attempt by rDNA researchers to keep their own house in order, consistent with the traditional social contract for science, and to thereby obviate the need or even the justification for outsiders like the public, regulators, politicians, and the media, to participate in the discourse around rDNA research (Guston and Keniston 1994). Scientists were surprised and somewhat dismayed, then, at the public response to rDNA research during and after the Asilomar conference. They had been unable to maintain the boundary in such a way as to keep scientific outsiders on the outside of policy discussions about rDNA research.

There was no immediate aftermath, but rather a slow percolation of newspaper and magazine articles, television programs, and localized public hearings. The coverage as not all negative, but some of it took on a critical or concerned tone that scientists found objectionable. As Friedberg (2014:268) writes, Paul Berg expressed special appreciation for one journalist, David Perlman, simply because of Perlman's "alarm-free tone and avoidance of gratuitous sensationalism and hyperbole in writing for a conversant lay public." Perlman's approach, to Berg at least, was not the modal approach of journalists to rDNA research.

Asilomar led to a great deal of public attention mostly at the local level. Nine cities considered local ordinances on genetic research (Krimsky 1982:295). Two of these cities stand out. The first was the city of Ann Arbor, Michigan, where the University of Michigan resides. A group of people in the University of Michigan medical school formed the Ad Hoc Committee for Microbiological Safety. In the wake of the Berg Letter, the group requested that the Vice President of the university support research into the issue of microbiological safety at the university. The result was the Committee on Microbiological Research Hazards. One of its subcommittees was supposed to investigate "broad policy aspects" of research, including "social and ethical issues [of] rDNA research at the university" (Krimsky 1982:295). Although none of the members of that subcommittee were scientists, the committee was stacked with people that the Committee for Microbiological Safety thought would be proponents of rDNA research. Additionally, the university had promised that it would not move forward on building facilities for hazardous microbiology research like rDNA until it received that subcommittee's report but wound up doing so anyway. Furthermore, the committee did not reach out in any way to the larger community. The mayor of Ann Arbor, who actually held a PhD in public health and microbiology from the University of Michigan, expressed his displeasure that the university had not included city residents in its decision-making process. University of Michigan scientists had innovated on the approach taken at

Asilomar. Instead of trying to contain the review and decision-making processes of rDNA research within their own scientific domain, they instead enrolled sympathetic outsiders. The reviewing subcommittee protected rDNA research at the University of Michigan and the scientific autonomy of researchers by giving the decision-making process the legitimacy afforded by appearing independent and disinterested.

The second city of Cambridge, Massachusetts, provides a contrasting case. Sheldon Krimsky, who wrote the most comprehensive history of the rDNA debates, participated in the citizens' committee that formed there and so gave a rather detailed account of its process. I will only note a few things here, however. First, the context for the Cambridge case was different from that for Ann Arbor. Unlike in Ann Arbor, the city of Cambridge already had strained relationships with the various universities in the city's confines, particularly Harvard and the Massachusetts Institute of Technology (MIT). This set the stage for a more protracted debate. Additionally, in Cambridge, scientists did not present a united front regarding rDNA research. Indeed, some scientists actively opposed rDNA research, even joining a group called the Genetics and Social Policy Group, a subsidiary of Science for the People. There was also some broad support for holding a public meeting, even by those scientists who did support rDNA research. This is very different from the situation at Asilomar in which scientists could not imagine how or why the public would be relevant to what they saw as essentially a technical and scientific deliberation. In Cambridge, this led to a two-day public hearing in a special meeting of the city council on June 23 and July 7, 1976. Krimsky writes that:

> *For the first time the public had control of the forum. This dramatic displacement of authority caused considerable unease among university researchers and administrators. The self-governance of science was concretely and symbolically threatened at this town meeting* (1982: 301).

At the conclusion of the hearing, the mayor proposed a two-year moratorium on rDNA research. This was shot down, but the council decided that a more thorough public decision-making process was needed. It formed the Cambridge Experimentation Review Board to review rDNA research. The review board spent over 100 hours hearing from experts and debating the relative risks and merits of rDNA research. The seven-member group had two medical doctors, who were broadly supportive of rDNA research, but the other members were neither experts in medicine nor biology. Logistical precautions were put in place to ensure that the doctors did not have undue influence on the group's decision-making process. In the end, the Cambridge Experimentation Review Board did not support either a moratorium or relinquishment. Instead, they suggested safety and containment measures in addition to those laid out in the NIH guidelines that had been informed by Asilomar. The city council enacted the suggestions in February 1977 leading to the first local DNA legislation in the United States (Krimsky 1982:157). Cambridge served as the example for the nine cities and states that adopted local ordinances.

This type of active public response was exactly the kind of thing that Asilomar attendees had wanted to avoid. Asilomar was supposed to protect scientific autonomy by obviating the need for publics, either directly or in the form of their representatives, from taking an active role. Scientists had incorporated new ideas, moratoria and relinquishment, into their policy discourse with the hope that doing so would discipline discourse in a way that let them retain control of the discourse. Despite their best efforts, the public began to assert itself and put pressure on the boundary they had tried to stabilize between themselves and scientific outsiders. Advocates of rDNA research, particularly at the NIH, were increasingly presented with the problem of how to approach these publics. Additionally, moratoria and relinquishment became increasingly contentious. Not only had they not worked as intended, but also they stymied research in the process.

The issues of public engagement, a moratorium, and relinquishment came up in three important contexts at the NIH. These issues came up in the NIH-sponsored Recombinant DNA Advisory Committee, which was created in 1976. After Asilomar, the NIH became the focal agency for further debate about rDNA research and regulation, just as Asilomar scientists had intended. The Recombinant DNA Advisory Committee was intended to help draft regulatory guidelines for the NIH regarding rDNA research and later any research in genetics with potential ethical concerns. The question of self-dealing and the place of non-scientists came up early for the committee. The committee's meetings were only open to scientists when they first began. At the very first meeting, however, several members expressed concern about the lack of public members. The committee's Chairman, DeWitt Stetten, said that he "was…a little surprised to find out…that the members of the committee wanted to have at least one non-scientist present" (307). He resisted this expansion of the committee, questioning the value that nonscientists could add to what he saw as fundamentally scientific issues. Chairman Stetten clearly viewed the scientific autonomy of the committee in the same way that attendees had viewed Asilomar. The resistance of committee members eventually won out, however, and non-scientists were eventually included on the committee. It took a little over a year to find the nonscientists who would serve on the committee. These were LeRoy Walters, a philosopher-theologian and direct of the Center for Bioethics at the Kennedy Institute at Georgetown University and Emmett Redford, a professor of government at the University of Texas, Austin. Later Elizabeth Kutter was also included. She was a biologist, but came from a small liberal arts college, Evergreen State College in Washington, rather than one of the research institutions with close ties to the NIH. While not exactly from the lay public, given her scientific background, she represented a different viewpoint by virtue of her independence from the NIH and any large laboratories. In other words, she was less enrolled by institutions like the NIH, which made her something closer to a member of the public in the eyes of the committee.

The enrollment of committee members was tied, in the minds of scientists critical of the committee, to the issues of a moratorium, relinquishment, and the place of non-scientists in scientific policy- and decision-making. Committee Chair DeWitt Stetten appointed Stanford geneticist David Hogness to chair the subcommittee that would draft the actual guidelines. Hogness received an NIH grant in 1972 to use rDNA on fruit flies to develop what would later become genomic research[14] (Burtis et al. 2003). Some thought that, as a geneticist working in rDNA research, Hogness' leadership presented a conflict of interest. His committee's proposed guidelines effectively downplayed the safety concerns and, in large part, ignored the suggestions that had come out of Asilomar. Specifically, the guidelines dropped any mention of a moratorium or relinquishment and called for less constraining safety precautions than those proposed

[14] Genomic research studies an organism's full DNA profile instead of a smaller genetic subset.

by Asilomar attendees. Scientists responded angrily. At a meeting at Cold Spring Harbor in 1975 that Elizabeth Kutter had organized around safe cloning procedures, discussion turned to the issue of the Recombinant DNA Advisory Committee's proposed guidelines. The result was a petition that outlined the chief issues with the guidelines. Of the three points elaborated in the petition, the first urged a reinstatement of a moratorium, saying that "the most hazardous experiments be curtailed until there was an experimental determination of risks (Nature 257:637, 1975)." Kutter was instrumental in leading the opposition despite having not been appointed to the committee. With respect to non-scientists, the committee remained relatively closed to outside groups until 1979. In that year, it opened itself to environmental groups and some civil society organizations related to science.

Moratoria, relinquishment, and the exclusion of non-scientists in scientific decision-making were issues being tackled outside of the NIH as well. These issues came up in a senate subcommittee meeting chaired by Senator Edward Kennedy two months after the Asilomar conference, a few months before the Recombinant DNA Advisory Committee's guidelines issue would be settled (Genetic Engineering 1975:1-12). Asilomar was not the sole focus of the senate meeting, but it was clear the media attention around Asilomar had been what motivated the meeting to take place. The issues driving the meeting went far beyond rDNA or any single type of research. Among the focal questions, Kennedy asked, "Was it proper for scientists alone to decide to stop and then resume the research? How could nonscientists participate in the process… [and] what should be done now in terms of public policy in this area?" Kennedy was aware of scientists' concerns. In his opening address he acknowledged as legitimate scientists' fears of an anti-science sentiment among the public, increasing federal regulation of science, and oversight that would might cause "a resultant drop in [scientific] productivity" (Genetic Engineering 1975:1-12). The total lack of public involvement at Asilomar, however, also seemed like a problem. Kennedy's key criticism was the exclusive nature of the conference. Neither the public nor any representative of the public had any capacity to influence decision-making at Asilomar. Kennedy was also concerned about scientists' desire to end the moratorium of rDNA research. He wanted to know what regulatory mechanisms would follow such an action. Essentially, Senator

Kennedy took issue with the primary purpose of the Asilomar conference: to retain scientific authority by means of effective self-regulation by scientists and only scientists.

One defender of Asilomar, Stanley Cohen, particularly stands out in that hearing. Cohen was an rDNA researcher who had attended Asilomar. He spoke in defense of Asilomar, particularly the exclusion of the lay public. He also argued for no restrictions on rDNA research. Cohen was polite but persistent in his position that the public, depending on how one defined it, either had been included at Asilomar or was impossible to include. With respect to the former, Cohen said that the meeting had been open to "public scrutiny" because they had "communicated openly to the public at large" through the presence of journalists (Genetic Engineering 1975:1-12). Cohen expounded on this by saying that Asilomar was "carried out under full public scrutiny with public participation," because "one of every eight attendees…was a representative of the press" (Genetic Engineering 1975:1-12). Cohen invoked the public witnessing that journalists provide to give the legitimacy to the proceedings (Ezrahi 1990). The fact that this public witnessing happened only after the meeting had concluded, the decisions had been made, and the moratorium had been lifted on terms set exclusively by rDNA researchers themselves did not seem to matter. According to Cohen, the public was made present in absentia by virtue of the public witnessing of the media.

When Kennedy did not accept this formulation of "public participation", Cohen espoused his second argument, that public participation was impossible. Cohen argued "There was no way to quickly call in the public and get their agreement for a moratorium of the experiments" (Genetic Engineering 1975:1-12). He again pushed his view of public scrutiny as sufficient for public participation but also added that future "public involvement in the process would occur by the currently available mechanism of nonscientist participation on the National Councils concerned with the funding of scientific research" (Genetic Engineering 1975:1-12). The National Council meetings are nominally open to the public in the way that the later Director's Advisory Committee meetings were. They are open, but effectively only to people who are physically in Washington D.C., informed enough to know that the meetings are happening and, interested enough to go. In other words, lay people technically *can* participate but, effectively, almost never do. Both Kennedy and Halsted Hollman, a Stanford geneticist who testified at the meeting, readily conceded to the point that broad public participation was logistically impossible for Asilomar, given how quickly it was organized and executed. Cohen, however, wanted to take this a step further, essentially arguing that public engagement was never necessary. Kennedy's reason for excusing Asilomar was logistical but Cohen's reason was the nature of scientific credibility and expertise. How could publics ever contribute to discussions about fundamentally unknown technological issues? Cohen's approach reflects the technocratic mode of governance that most rDNA researchers at Asilomar seemed to operate under, irrespective of their conviction that the technology had important implications for public health and safety.

Contradicting both of his own positions, however, Cohen also seemed to argue that more *was* being done to foster greater public participation. Perhaps he had begun to realize, as Asilomar veterans like James Watson later would, that a measure of public participation was politically necessary. In any case, he argued, "The goal of the [National Academies of Science] committee was to rapidly obtain a moratorium on potentially hazardous kinds of experiments, in order to allow time for a full public discussion of the issues" (Genetic Engineering 1975:1-12). Cohen discursively recast the entire NAS committee effort as a valiant effort in public engagement and democratic science. In reality, it was only intended to figure out the level of threat posed by rDNA research and to organize the Asilomar conference to discuss that threat. Not only was public participation not the nature of those meetings but rDNA researchers were actually quite terrified of public discussion. After having been pressured by Kennedy about the insufficiency of a purely voluntary model of self-regulation, Cohen responded by saying "at this point formal mechanisms are in the process of being established, with public participation, for insuring that compliance [with the new NIH guidelines] continues to occur" (Genetic Engineering 1975:1-12). He never gets the chance to explain what this participation looks like and whether it is different from the *post-hoc* public scrutiny model he earlier espoused. Furthermore, the NIH guidelines did not contain anything about public participation at that point. He might have been referring to the DAC meetings that would commence in the following year, but it is impossible to know.

Recombinant DNA researchers took an important lesson away from Asilomar and its aftermath in the public and civil spheres: do not, yourself, invite trouble. Many scientists did not share Cohen's optimistic opinion regarding even public scrutiny, much less public engagement. Krimsky (1982) writes that even those who had called for a longer moratorium and even total relinquishment on certain kinds of research began to "close ranks" once they saw public and political concern fomenting. Increasingly rDNA researchers began to question the wisdom of Asilomar, especially for voluntarily addressing issues of risk. Norton Zinder, an rDNA researcher at Rockefeller University, said that many scientists said to him "If you guys hadn't opened your mouth, nothing would have happened. It would have all blown away" (Wade 1977:100). Many scientists felt the victim of a great injustice, in that in trying to do the right thing they became the villains. One scientist, in the aftermath of the Director's Advisory Committee meeting, lamented, "We acted responsibly and now the public is over-reacting (Krimsky 1982:179)." Others, like James Watson who would later come to direct the Human Genome Project and establish its ELSI program, considered it a colossal waste of time and resources. In a 2008 interview, thirty-three years after Asilomar, he said, "I was depressed about Asilomar because I couldn't see when we were going to be able to start our experiments. Was it going to be a six-month delay, several years? As it was, it turned out a couple of year delay…and for a couple of years I just thought about regulations, not about science" (*Meetings that Changed the World: Asilomar 1975* 2008). Scientists thought that Asilomar would be a one-time detour into the unscientific realms of ethics, laboratory safety, unknown hazards, societal implications and, later, politics and public opinion. These issues, they hoped, could be quickly dealt with so that they could get back to work. They would then return to what Capron had called their "historical immunity" to such considerations. When that did not happen, it left many embittered and skeptical about those extra-scientific issues and those who had espoused their importance.

In an interview in 1975, Maxine Singer still thought that the Berg letter had been necessary, despite the controversy it raised among her colleagues. She expressed regret, however, at the public attention the letter had engendered about rDNA research.

> *None of us had any way of knowing whether the concerns that we had were real…and, therefore, to raise in the public's mind the notion of possible dangers…was premature and might not be productive. It might*

make some people worry and concerned about something about which there is no need to worry. And it's very hard to undo such things once you do them (Krimsky 1982:74).

Paul Berg, in the same set of 2008 interviews, seemed to take a more optimistic approach. He said, "There is a lesson in Asilomar for all of science: the best way to respond to concerns created by emerging knowledge or early-stage technologies is for scientists from publicly funded institutions to find common cause with the wider public about the best way to regulate — as early as possible" (*Meetings that Changed the World: Asilomar 1975* 2008). Essentially, Berg learned that they could not protect scientific autonomy and the progress of contentious research by drawing a solid barrier between science and society. He learned that scientists had to employ other boundary-stabilizing practices that included some kind of public participation.

The Importance of Asilomar

The Asilomar conference marked an important moment for modern biosciences in the U.S. Scientists engaged in rDNA research realized, on their own and without a motivating public crisis, that their research posed important potential risks for public health and safety. They then set about to deal with those risks prophylactically, both to protect public health and safety as well as the autonomy of their work. In so doing, they re-defined the interior of their own scientific domain to include the possibility of a moratorium or relinquishment, postponing or entirely foregoing an area of research, respectively. These concepts stem from a different set of ideological commitments than those that laboratory bioscientists in the U.S. commonly embrace. They represent an expansion of acceptable science policy. Bioscientists in the U.S. had never, to their own thinking at least, postponed or given up on otherwise good research. The ideology behind moratoria and relinquishment does not put science at the top of the social value hierarchy. Instead, science was put into the service of society. Scientists recognized that they had to incorporate something from outside of the scientific domain in order to protect the autonomy of their work.

Yet Asilomar is as interesting for what bioscientists did not advocate in their expansion of the boundary into acceptable science policy. Although the scientists at Asilomar incorporated these new ideas and the ideology underpinning them, most of them resisted the inclusion of any non-rDNA researchers into the discourse. Actually, with respect to the public, it goes even further than that. It did not even occur to the organizers of Asilomar that the public might have any role in the discussion of rDNA and its risks. Stanford biologist Paul Berg, whose research got the whole debate started, became the *de facto* bioethicist at the center of the rDNA debates. This was not because he had any expertise in ethics or public health safety. Rather, it was simply a consequence of it being his research and him being the first convert from the idea that there was nothing to worry about with rDNA research to the position that it had important health and safety concerns. With Asilomar, there was no expansion in what scientists saw as relevant expertise. Scientists did include journalists, but only because of the public witnessing component that journalists could offer, and which would give the proceedings democratic legitimacy (Ezrahi 1990, 2012, Jasanoff 2004). The absence of media would bring into question their commitment to safe and ethical science. It was not until after Asilomar, when the conference became the subject of criticism, that scientists began actively thinking about how they would include non-scientific actors.

Additionally, discourse was eventually disciplined around rDNA by the attempt to expel the ideas of moratoria and relinquishment, elements that were previously central to the discourse. The scientists, who once adopted a moratorium and relinquishment as new elements of ethical science and, implicitly, expanded ideas about acceptable science policy, turned on these ideas when they saw the consequences these concepts had for their research. The moratorium of rDNA research did not appease the public or politicians and their concerns about health and safety. If anything, it made it look like rDNA posed extraordinary risks that most scientists thought exaggerated any true danger. For this reason, they later closed ranks to protect the progress and autonomy of science against intrusive regulation and public interference.

The ELSI Working Group of the Human Genome Project

We now turn to the next case in which acceptable science policy expanded. The Ethical, Legal, and Social Implications (ELSI) program of the Human Genome Project performed early boundary setting to de-legitimize the value of a moratorium and relinquishment. Instead, the ELSI program actively enrolled new non-scientific actors, social scientists and ethicists, and hybrid scientific actors, scientists with interest or experience in ethical issues, and new ethical ideas consistent with the goals of the larger project. Space had to be made, however, for these new actors and ideas, in order to convince other scientists and science agency administrators that they were safe for inclusion in the larger project.

The National Human Genome Research Initiative (herein Human Genome Project) officially began in 1990 and since its beginning, evidenced a public commitment to ethical issues and social implications. The project funded a grant program in Ethical, Legal, and Social Implications and a working group dedicated to identifying ELSI issues as they arose for the Human Genome Project. The overall ELSI program represented the culmination of more than a decade of lessons learned about uncertainty, publics, and science. Furthermore, these were lessons that came directly out of Asilomar, for the simple fact that most of the original organizers of the Human Genome Project were the same players from the rDNA debates. David Baltimore and James Watson championed the project against resistance from other bioscientists. James Watson became the project's inaugural director. Paul Berg served as Chairman of the Scientific Advisory Board of the project for a time after Watson left. Jonathan Beckwith, who had worked with Science for the People and had been forced to decline his invitation to Asilomar, served on the inaugural ELSI working group.

To understand why the Human Genome Project exhibited such a commitment to ELSI issues, we need to understand the institutional and science-cultural context in which it emerged. The goal of the project was to make a map of the entire human genome. No one knew how big the human genome was, and so nobody really knew how long the project would take or how much money it would cost. In 1988, the National Research Council and the Office of Technology Assessment estimated that it would cost $3 billion over 15 years, or $200 million per year (NHGRI 1991). Bob Moyzis, Johns Hopkins biochemist and advocate for the Human Genome Project, wrote "by the standards of the biological community, the Project's funding makes it seem very much like Big Science, and as such it's been a target for criticism" (Cooper 1994:82). The project met with early opposition that came not from outside groups fearful of genomic research or potential health and safety crises, but from other bioscientists who feared a burgeoning culture of Big Science. Bioscientists whose research did not fall within the purview of the Human Genome Project worried about reduced federal support for their own research. Scientists critical of the project thought that it would absorb all bioscience funding for those decades. Worse still, if the former CEO for Biogen, Walter Gilbert, was right, then the genome project would not even employ that many scientists. He proposed that thirty scientists could spend the first decade sequencing the most important 1% of the genome. After that, the process could be automated, given advances in sequencing technology that were certain to follow, with a ten- to hundred-fold increase in efficiency to sequence the rest of the genome the following decade. Thus, the Human Genome Project could eat up the biggest portion of the federal budget for the biological sciences for twenty years and support just a handful of scientists (McElheny 2010:42-57). Scientists like Bruce Albert at U.C. San Francisco worried that the shift to larger labs would mean less contact between senior scientists and their graduate students or post-docs, changing the very culture of American bioscience over the long-term (McElheny 2010:42-57). They also worried that it would lead to a permanent shift in the nature of federally-funded biological research from a high number of smaller research projects in universities of all sizes distributed across the country to just a few highly-funded, large-scale projects centralized in a handful of elite universities. Additionally, seeing all of the political wrangling involved in funding the Human Genome Project, many scientists feared that this too would become the new norm if research funding shifted towards such large-scale projects (McElheny 2010:42-57).

Essentially, many bioscientists were concerned that the Human Genome Project would bring a significant loss of autonomy. The threat to autonomy was two-fold. First, federal agencies might have too much power to direct research, threatening the ability of scientists to determine the content of their field and direct their own work. In a way, these scientists were operating on the myth of the old social contract for science, in which federal funding agencies passively supported scientific research without directing it (Guston and Keniston 1994). Although, as we saw in Chapter I, this had never been the case, the Asilomar Conference helped bolster the power of the NIH in directing biological research. Scientists at that time saw the NIH as their best option, an ally among federal agencies, once it was clear that they could not simply continue rDNA research free from public interference. With the Human Genome Project, however, many bioscientists resisted NIH Director James Wyngaarden's attempts to house the project within the NIH. They wanted to leave it to the Department of Energy, which had already funded some of the research into the human genome. Second, large-scale science threatened scientific autonomy by forcing scientists to become too highly enrolled by funding agencies. So not only might those agencies actively direct their research, but scientists might increasingly pursue research projects in line with whatever those agencies were likely to fund. A good deal of basic scientific research (i.e. research without immediate potential for application in technologies or commercial products) or research with applications not valuable to the military or private industry might go unfunded (Frickel et al. 2010, Hess 2010). The internal conflict around the nascent Human Genome Project was likely one reason for its early support of the ELSI program. With such significant internal opposition to the project, outside opposition from concerned publics, civil society groups, or politicians would have almost certainly killed the project before it got started.

James Wyngaarden was very aware of this fragility when he asked James Watson to lead the project. James Watson was, of course, half of the Nobel Prize winning duo behind the discovery of DNA and the *enfant terrible* from the Asilomar Conference (Wade 1997:43). He wrote *The Double Helix*, one of the most famous and widely-read books on the history of science and one which many regarded as gossipy and overly acerbic, just like Watson himself. Watson had a strong personality that alienated a great many people from him. Yet the genius of his work, his ability to discover talented young scientists, his skill at organizing others, and his political savvy with those in funding agencies and Congress made him an indispensable player for the fledgling Human Genome Project. He was an early supporter of the project but had never intended to lead it. Indeed, he had been readying himself for retirement. Two things changed his mind. First, NIH Director James Wyngaarden and David Botstein, Watson's friend and colleague, entreated Watson at a conference to lead the project.

Wyngaarden needed Watson's personal charisma and his credibility among bioscientists to quell the internal opposition to the project. He needed Watson's star-power, recognized by well-informed publics and policymakers, to give the nascent project a face they associated with groundbreaking scientific discoveries. That alone might not have convinced Watson. In addition to Wyngaarden's efforts, Watson's son began showing signs of schizophrenia. Watson, who had no doubts that schizophrenia was largely, if not entirely, a genetic condition, wanted the genetic treatments that the Human Genome Project promised, and he wanted them immediately (McElheny 2003, 2010). Thus, he postponed retirement to become the first director of the Human Genome Project.

Watson took charge with the authority that everyone expected of him. He started by quelling the fears of Big Science. "We are all small scientists," he said. "Big Science is no good. We have to give the money to bright people. The program has to be run by scientists, not by NIH administrators" (McElheny 2010:76). Then, in a move that seemed to surprise everyone involved, Watson single-handedly decided to create the Ethical, Legal, and Social Implications program as a key add-on to the main initiative. Watson had been extremely ambivalent when it came to ethical concerns and societal implications in science. At times, he was a vocal champion of the ethical concerns and societal implications of bioscience. This is why he had signed on to the Berg committee's original moratorium letter, for instance. At other times, however, he combated those people who wanted to pay attention to these concerns, seeing such attention as a hindrance to the progress of science. If the people he fought were scientists, then he attacked their credibility. In the context of rDNA research, Jonathan Beckwith said that Watson labeled scientists with whom he disagreed as "kooks" or "second-rate scientists" (Beckwith 2002:158). Thus, in October 1988, it came as a surprise to many when Watson announced the creation of the *Ethical, Legal, and Social Implications of Human Genetics Research Program* (herein ELSI program). He dedicated 3% of the total project budget to the ELSI program, later rising to 5%. Furthermore, he seemingly made this decision without consulting his colleagues or anyone else at the NIH, even Director Wyngaarden (Cook-Deegan 1994, McElheny 2010, Wexler 1994). He seemed to learn the lesson from the aftermath of the rDNA controversy: that people's concerns mattered irrespective of how scientifically credible they were. Explaining why he announced the ELSI program, Watson explained that:

> *Deep down, I think that the only thing that could stop our program is fear; if people are afraid of the information we will find, they will keep us from finding it. We have to convince our fellow citizens somehow that there will be more advantages to knowing the human genome than not knowing it* (Kevles and Hood 1992: 173).

Watson imagined a public with the power to destroy the initiative and a mode of self-governance, the ELSI program, capable of avoiding this potential. In his imaginary, Watson did not give up on the most essential component of the Asilomar conference, that scientists could effectively self-regulate and thereby avoid public scrutiny and backlash. He did seem to expand his idea of governance to include expert non-scientists. These were experts in ethics, law, and societal implications. Still, their discourse would be disciplined, in part, by the fact that they were to conduct their work within the confines of the Human Genome Project's supporting agencies, the DOE and the NIH. Furthermore, the ELSI program was to serve the interest of the larger Human Genome Project. Eric Juengst, the first director of the overall ELSI program, said that its purpose was to "aid the virtuous genome scientist's professional ethical question: 'What should I know in order to conduct my (otherwise valuable) work in a socially responsible way?'" (Juengst 1996:68). Unlike for rDNA research, that the science should move forward or that scientists were virtuous was not a question.

Policy Expansion: New Experts

Since the rDNA debates, it seems that bioscientists in the U.S. began to realize that something like ELSI as necessary, at least politically if not for ethical and moral reasons. Beckwith writes in his memoir, "It had become accepted wisdom that scientists should at least pay lip service to the concept of social responsibility" (Beckwith 2002:159). They had learned from the rDNA debates and the debacle that had ensued that early attention to ELSI issues was the key to avoiding a repeat of negative public attention. One thing, however, did not change. Those appointed to the ELSI working group were an elite group of experts. While many scientists viewed ELSI as heretical to the ideology of objective, value-free science, it was a heresy firmly embedded within the scientific culture of the NIH and DOE. One of the benefits of embedded heresy for the Human Genome Project was that Watson could strategically choose his heretics. Watson clearly felt that he had to allow non-scientists to cross over into the scientific domain. He used the power of his position to choose the people he thought would be supportive of the nascent Human Genome Project.

The ELSI program had four components: the NIH/DOE Joint Working Group on Ethical, Legal, and Social Implications (ELSI working group), the Extramural ELSI Branch, the Office of Policy Coordination, and the Intramural Genome Ethics Office (NHGRI ELSI Program Review 1996:1). This dissertation focuses mainly on the ELSI working group for two reasons. First, as described by the Human Genome Project itself, the ELSI working group “provided the overall guidance to the NHGRI [National Human Genome Research Initiative] and the other ELSI programs” (7). The ELSI working group was supposed to determine the issues of concern that would help set the agendas of the other ELSI endeavors. The second reason is practical and methodological. More critical accounts have been written by people who served in the ELSI working group than by people in the other branches of ELSI for the Human Genome Project.

Watson started by appointing clinical psychologist and disease research advocate Nancy Wexler to chair the ELSI working group. Wexler had many qualities that made her an attractive choice. Primarily, she had expertise and credibility that biologists recognized. Although her PhD was in clinical psychology, Wexler had experience with Huntington’s chorea and the ethics of genetic testing, having had her mother die of Huntington’s. When a genetic test for Huntington’s was later developed, she grappled with whether or not to be tested. She used her own struggle as the impetus for a psychological study of people facing the same decision (Wexler 1979). Her goal for the research was to inform genetic counseling services around Huntington’s and other tragic genetic disorders like it. Huntington’s disease and genetic testing became the focus of her career. She became an advocate for families with Huntington’s. Later she helped lead a multi-year study on the gene for Huntington’s disease in Lake Maracaibo, Venezuela. Her participation was an enormous help to the geneticists on the project, since her personal experience with the disease gave her a degree of credibility and legitimacy with the people of Lake Maracaibo that the geneticists could not replicate. As a social scientist by training, a genetics researcher by experience, and a member of the stake-holding public by virtue of her personal experience with Huntington's and her advocacy work, Wexler was the proverbial total package to lead the ELSI working group. Particularly the fact that she was a social scientist, and not a geneticist, would help alleviate some of the concerns of insider bias that plagued the Asilomar Conference.

Wexler and Watson's selections for the rest of the ELSI working group skewed heavily in favor of scientific expertise. In addition to Wexler, the original group included Victor McKusick, a geneticist from Johns Hopkins University, Robert Murray, a clinical geneticist at Howard University, Patricia King, a lawyer from Georgetown University, Robert Cook-Deegan,[15] a medical doctor by training who later focused on health policy, Thomas Murray, a social psychologist and bioethicist who would come to head the Hastings Center, and Jonathan Beckwith, a molecular biologist and member of Science for the People. The ELSI working group would later include more social scientists, but the inaugural group were almost all insiders to the biological sciences adopting the role of ethical overseers to the project. Only Beckwith had been in any way an outsider by virtue of his participation with Science for the People. By working primarily on ethical issues, however, they were supposed to represent a different kind of expertise.

Although the group was comprised mostly of bioscientists, they nonetheless put their scientific credibility on the line, even risking being labeled heretics to their colleagues in the Human Genome Project. They were often seen as "second-rate scientists" and the working group as a "welfare program for ethicists" (Beckwith, 2002: 199-200).[16] This was not peculiar to the Human Genome Project. This is a common risk for scientists who vocalize ethical, moral, or other value-laden concerns about science or technology (Gregory and Miller 1998).

Watson's choice of people to serve on the ELSI working group helped discipline

[15] Robert Cook-Deegan also wrote the most comprehensive history of the Human Genome Project. Yet despite being a member of the inaugural ELSI working group, he did not write much about it other than its origins.

[16] Beckwith observes, pointedly, that scientists never see initiatives like the Human Genome Project as "welfare for scientists."

the ethical and societal implications discourse around the Human Genome Project. They focused on concerns that did not threaten the future of the scientific work. Although the ELSI group was clearly concerned with certain types of ethical concerns, particularly how newly discovered genetic information might be used by individuals and by medical industries, they did not entertain any potential moratorium or relinquishment of genomic science. In fact, Wexler and Watson were quick to preempt the introduction of these into the ELSI working group. In 1990, David Baltimore hosted an informal meeting at his home for the new Human Genome Project leadership. The meeting was a forum to discuss the emerging initiative for a book that would begin the historical account of the Human Genome Project (Cooper, 1994). The meeting, and the proto-historical account that came out of it, was quite obviously a clever piece of marketing for the then-fledgling Human Genome Project. It nonetheless offers a useful glimpse into the values espoused at the beginning. They closed their meeting with a discussion of ELSI. Nancy Wexler, by way of introducing the new ELSI working group to those present preempted the legitimacy of relinquishment.

> *It would be foolish to try to slow down the advancing science—the advances promise better treatments for disease, better quality of life and health for society. Rather than slow the science, we need to accelerate the creation of a social system that will be more hospitable to new information about our genes, our heritage, and our future* (Cooper 1994:167).

She returned to this idea – that we should not focus on issues surrounding the science but rather on fixing society and preparing it for the science – several times throughout their meeting. There are two important things to notice here. First, and most importantly, the science came first. The scientific progress of the genome project would take priority over any potential ethical or social implications of that project. Second, the scientific knowledge was sacrosanct. It could not be blamed for any problems that emerged out of the Human Genome Project. Rather, they were indications of an already-flawed society. Any problems that might arise in the future would require the tweaking of society to match and accommodate the science rather than changing how these technoscientific developments were developed or deployed into society. As David

Galas, the Director of the Office of Biological and Environmental Research in the Department of Energy's Office of Science, said at that same sit-down meeting:

> *There are two important things to remember when we think about ethical and social issues in terms of the Genome Project. First, there are no new problems. Issues concerning privacy, confidentially, and discrimination will become much more pressing. The basic problems, however, are not new—they will simply be exacerbated. The second thing to keep in mind is that many ethicists, lawyers, and social scientists who speak out about the implications of the Genome Project are often somewhat ignorant of the fundamental science of genetics. We need everyone to learn and understand [genetic testing and genetic probability]* (179)

Galas here deflected blame for any ill after-effects of the Human Genome Project and sets the conditions for participation: acquiring scientific expertise. The potential for the Human Genome Project to raise old problems in new ways or to more extreme degrees did not count for Galas since these issues were not new.[17] One might reasonably have argued that, from a certain perspective, no problem is ever new. There was another reason why the Human Genome Project could not harm, according to Galas.

> *I've heard people say—including people in Congress and even some scientists—that the public can be hurt by genetic information. It's true that in the past that information has been used against people. But genetic information itself is not going to hurt the public; what could hurt the public is existing social structures, policies, and prejudices against which the information can ricochet* (Cooper 1994: 304).

The Human Genome Project could not cause harm because it was simply information and, by itself, information cannot harm. It may harm but only if it "ricochets"

[17] He also seemed to contradict himself. At the same meeting, he criticized a study on biological reductionism because, he argued, genetics is no more reductionist than biology more generally. He stressed, instead, the issues emerging around genetic testing for cystic fibrosis. It seems, then, that he does not support reductionism as a topic because, unlike cystic fibrosis testing, it is an old topic.

off flawed social structures, which should themselves be blamed. Galas' sentiment treats the entire Human Genome Project as pure science, a simple a quest for knowledge, and not the unavoidably social and political technoscientific project that it was. Paradoxical in this view of ELSI is the fact that, if ELSI was driven at all by substantive purpose and was not simply a cynical political maneuver, then at its core lay an acknowledgment of the socio-political nature of the Human Genome Project.

These comments are especially curious given that Wexler did understand the dangerous power of simple information. Her mother had made a "serious suicide attempt" after being diagnosed with Huntington's (CBS 1986). In recounting her work in Lake Maracaibo, she wrote about how she and her team often had to council people about their life with a genetic condition. Many times, they had to try to talk people out of committing suicide after those people discovered that they carried a fatal disease. Wexler and her team made the ethical decision not to test children. They saw no positive use for that information but several negative ones, like parents withdrawing resources such as food or even attention from children almost certain to die (Wexler 1979). Much of Wexler's work explored the psychological impacts of genetic testing for Huntington's (Wexler 1979, 1980, 1985). Clearly, she knew about the negative potential impact of what Galas frames as mere information but was an enrolled outsider, willing to accept and promote the science-first vision of the larger Human Genome Project.

Furthermore, the very actions of the fledgling ELSI group evidence an understanding about the power of information. By 1989, a genetic test for cystic fibrosis had become available. The test, however, was very complicated and its results difficult to interpret. Although there was a single cystic fibrosis gene, different mutations elsewhere in the genome altered how it was expressed and resulted in degrees of severity difficult to predict. Genetic counselors and clinical geneticists began to raise the alarm about making such a test widely available. They were outnumbered by laboratory geneticists who thought that simple information could do no harm. Wexler, however, "courageously steered the ELSI working group straight into the [cystic fibrosis] storm (Cook-Deegan 1994: 245)." The efforts of the working group resulted in seven grants funding research at six centers to study the effects and approaches to clinical cystic fibrosis testing. Oddly, though, cystic fibrosis testing quickly became one of the few uncontroversial areas of ELSI research among the scientists on the project. This is probably because it focused on what people did with information rather than the process of scientific discovery. Cystic fibrosis testing did not threaten the progress of the scientific research or scientific autonomy. Either way, Wexler and her ELSI group were clearly aware, on some level, that simple information was incredibly powerful and potentially dangerous.

Ethical scrutiny may have been embedded in the Human Genome Project, but those engaging in it were not exactly radical in either the actions they pursued or the values they espoused. Wexler and probably even Galas were aware of the potential impacts but discursively minimized them to ensure that the larger project moved forward. Even before the ELSI program or Working Group got started, Wexler joined the vocal opposition to a moratorium. Even Jonathan Beckwith, once a self-styled radical scientist who had made many enemies among his colleagues for his advocacy of scientific ethics, had had a conversion by the time Wexler invited him to the ELSI working group. When some of his activist friends criticized his participation in the new ELSI working group, saying that there was no consideration for slowing the project to consider ethical issues or those of social impact – essentially no consideration of a moratorium – Beckwith responded that he "had long since rejected the idea that the best way to prevent the harmful results of scientific developments was to stop the progress of science (2002: 194)." While the very existence of ELSI may have looked like some measure of heresy to scientists in the Human Genome Project, it was not the embedded heresy of Asilomar. There were no troublemakers in the group, no one like Harold Greene to argue, "society can more easily tolerate a postponed, or even a lost benefit than it can an actual injury (Krimsky 1982: 139)." If there had been, their credibility and legitimacy would likely have been challenged.

After Watson: Disciplining ELSI

The way Watson and Wexler designed that inaugural group set the stage for all of the work to follow, at least in the sense of who was included and what kinds of topics the group would address. Certain topics took precedence at different times, as we saw with the transition from cystic fibrosis testing to BRCA testing, but the core range of issues remained relatively stable. As already discussed, they attended to concerns like privacy and discrimination while ignoring issues like genetic engineering or those that might have come from the public.

Perhaps more importantly, though, I have focused on the beginning because it was while Watson was director that the ELSI working group had any real autonomy, according to Jonathan Beckwith and Lori Andrews, the chair of the working group after Nancy Wexler. This is not to say that Watson necessarily had any special love for ELSI. Even his intentions in forming the group are difficult to discern. Lori Andrews, chair of the ELSI working group from 1994-1996, was extremely critical of Watson. In her memoir of that time she wrote that:

> *Watson implied that the ELSI working group had been created not to set ethical standards but to let the science proceed unimpeded. "I wanted a group that would talk and talk and never get anything done," Watson said, "and if they did do something, I wanted them to get it wrong. I wanted as its head Shirley Temple Black'"* (Andrews 1999: 206).

Presumably, Watson was referencing Shirley Temple Black's role as an American ambassador, by which he seemed to mean a nominal role only. Jonathan Beckwith, the only other insider to publish criticism about the early ELSI group, said that Watson was not an obstructionist. Indeed, Beckwith said that Watson took little interest in what the group did and never hindered their projects or their funding. Watson may not have been a champion for ELSI, he may have considered it a waste of resources despite establishing it, but neither was he an active obstacle to the working group. For her part, Nancy Wexler may have also sided with science as an ultimate good, but she was more than just a nominal leader without purpose or the capacity to realize that purpose.

All of this changed once Francis Collins became the director of the Human Genome Project in 1993. Depending on the account, Watson either was fired or quit the Human Genome Project on April 10, 1992. The official reason given by the NIH was that Watson had a conflict of interest, due to his stock holdings in biomedical corporations (Brown and Gladwell 1992). Watson, however, said that he was willing to sell the stock.

The reason he gave, and which histories of the project usually recount, was an increasingly bitter and public dispute between Watson and then NIH Director Bernadine Healy about the patenting of DNA sequences, a proposition he called “sheer lunacy” (Scott 1992).

For whatever reason, Watson was out, and it was difficult to imagine who might replace a celebrity-scientist like Watson. Francis Collins, a University of Michigan researcher, was an obvious choice. Lori Andrews describes him as “an immediate hit in Congress” (1995:188). He attended prayer breakfasts with conservative lawmakers and had ready biblical justifications for the work of the Human Genome Project. He bridged a divide not only between science and politics generally but specifically between a science project that raised a lot of moral and ethical anxiety and those whose religious convictions often made those anxieties consequential in the form of funding.

Unlike Watson’s approach of benign neglect, Collins actively tried to take control of the ELSI working group and to direct the overall program. Lori Andrews, who took over in 1995 after Wexler’s term ended, realized too late that she made a mistake by allowing Collins to participate in the working group’s meetings. Collins made sure that the group studied only what he wanted, which meant a strong focus on studies about testing for the cystic fibrosis gene and a breast cancer gene (see Graph II.1). He regarded psychology, Nancy Wexler’s background, as “too mushy” for consideration (Andrews 1995:200, Beckwith 2002:204). He also changed their budget structure so that they had no budget of their own. The working group had to appeal to Collins and his staff for any funding. Eventually, he would not even fund regular meetings.

Several ethical crises emerged within the NIH and the DOE that further demoralized the Working Group. The now infamous book *The Bell Curve* was published in 1994. It sold 400,000 copies and was a convincing argument, to many, about race and the genetic basis for intelligence. People used it to argue for defunding education in predominantly minority communities since, if the book’s argument was correct, educating minorities was a waste of resources. The ELSI working group drafted a memo arguing that “since the lessons of genetics are not deterministic,” decisions about education should be seen as “moral, social and political ones” not biological ones (Nature 378:529, 1995). Their own staff in the NIH stonewalled them, however. It is still not entirely clear how or why. It took eight months and eventually strong-arming the staff to send out their memo for publication (Andrews 1995:196-197, Lehrman 1995: 96). Collins himself caused a crisis when he held “a massive press conference” in September of 1995 to announce the discovery of a mutation that caused breast cancer in Ashkenazi Jewish people (the 185delAG mutation). Women began calling to be tested for the gene. Some women even wanted prenatal testing so that they could abort fetuses possessing the gene. According to Andrews, doctors were telling patients that it had an 86% chance of expression leading many women to elect for prophylactic double-mastectomies. It was later found to have only a 50% chance of expression. Some still did not get the importance. Dr. Harvey Stern from the Genetics and IVF Institute said, in 1996, that "the actual number" did not matter. "If the number is 50, 60, 70, 80, [percent] it doesn't matter. It's still much higher [than for women without the gene]” (Andrews 1999:97). In another scandal, the Department of Energy (DOE) was sued by its own African-American employees for testing them for sickle-cell anemia and cystic fibrosis without their knowledge. In its defense, the DOE argued that since they there was no evidence of discriminatory action following its obtaining the results, that there was no harm (Andrews 1999:97). Still, it demoralized those in the ELSI working group to see that even one of their parent agencies could not act entirely ethically when it came to genetic information.

Lori Andrews and Jonathan Beckwith have both said that they were continually marginalized and hamstrung by the larger project. When they were told by the NHGRI that there was not enough money to fund their three annual meetings, a total cost of $20,000, working group members became irate. The working group was funded separately from the 5% of the total Human Genome Project budget that went to ELSI work. Lori Andrews set a meeting with Francis Collins in December of 1995 to discuss the ELSI group’s lack of autonomy and funding. She says that he stood her up to attend a prayer breakfast with a member of Congress. She quit two months later. Troy Duster took over for her but said that he did not intend to be the new chair (Lehrman 1995). He and Dorothy Nelkin, both sociologists, later repeated Andrews’ complaints that Collins was purposefully obstructing the activities of the group. Nelkin told a journalist from Nature “members feel that they are sometimes being used to legitimate the genome project, rather than to explore critically issues related to its impact” (Lehrman 1996). The other members of the ELSI group eventually followed Andrews in quitting.

Collins represented more than just himself in how he approached the ELSI group did. He may have had an undue personal influence, especially in his commitment to genes for cystic fibrosis and breast cancer, but his general orientation towards the ELSI working group and ethical concerns in general represents a larger contingent of scientists. Indeed, Jonathan Beckwith, Troy Duster, and Lori Andrews all suggest that Collins represented the majority opinion. Beckwith said that Collins’s approach “appeared to reflect the dissatisfaction of genome scientists with the ELSI program” that there should be “no more ‘soft science,’ ‘prurient speculation,’ or ‘vacuous pronunciamentos of self-styled ethicists’” (2000:204).

To scientists like Collins and seemingly Watson as well, ELSI research did not count as worthwhile knowledge to pursue. Since ELSI research had no direct visible impact on genomic research, it was difficult to measure its value. Unsurprisingly, genome scientists and agency administrators began to complain that ELSI research was taking too long. Many could not appreciate the relatively glacial speed of the policymaking process. Duster (2002) recounts an experience in 1996 in which a member of the NIH ELSI Review Panel told him that all of the other advisory panels had completed their work “in a year or two” and asked him why ELSI was “’still in business’ after seven years.” Duster’s response to the NIH ELSI Review Panel brought up three arguments, only two of which concern us here. First, he said that “while molecular biologists might produce an across-the-board solution to a technical problem and complete their work in a year; ethical, legal, and social issues frequently emerge out of the very solutions other disciplines propose” (2002:69). Secondly, he brought up the vexing problem of race in America. While scientists agreed that “race is not a concept of any scientific value,” race is nonetheless consequential for people’s everyday lives.

> *We may be 99.9 percent alike at the level of DNA, but if that were the end of the story, we could all pack up and go home after a year of technical advice. The Ethical, Legal, and Social Issues advisory committee is still “in business” for a good reason* (Duster 2002:69).

Duster's point seems to have been that ELSI research would never be done. We can read this as a flipped version of David Galas' assertion, at the very beginning of the ELSI working group, that the genome project would introduce no novel problems but rather would only be mapped onto existing ones. Duster seemed to argue that as long as problems like racial discrimination in the U.S. persist, then there was always going to be reason to understand how research like the genome project was being either mapped onto those problems or adding to them.

Imagining Issues

In this section, I transition from using secondary data collection to analyzing project data for the National Human Genome Research Initiative's (NHGRI) Joint ELSI program between the NIH and the DOE. I do so to understand what kinds of work the larger ELSI program funded and to what degree ELSI research reflected a critical gaze back at the larger project. It is also to get a measure of the ELSI program's commitment to public engagement and compare this to the technocratic approach that I argue was the modal approach for the ELSI program. Public engagement was still a relatively new concept for American bioscientists, but it was listed in the goals and mechanisms of the ELSI program's five-year plan. Including public engagement would have been another expansion in the ideas about acceptable science policy for the Human Genome Project.

This ELSI research still took a technocratic approach to ethical issues and societal implications. In this model, experts in ethics and in the social sciences tried to discover ethical issues and societal implications like they might discover universal laws. As Richard Kuisel observed, the program operated as if "human problems, like technical ones, have a solution that experts, given sufficient data and authority, can discover and execute" (Kuisel 1981 in Porter 1995:146). The difference here was that the experts included social scientists rather than the Asilomar model, which gave the ethics work to scientists. Additionally, with some few exceptions, there was little recognition that ethical and moral issues might arise from people and institutions who make technology both meaningful and thereby actionable. In Brian Wynne's terms, researchers did not fully appreciate that science has a meaning problem when it comes to its interactions with publics. For the most part, when people did factor into their analyses, it was nearly always as objects of research. In other words, how people thought and what people did were not seen as constituent elements of an overall universe of ethical concerns, but as additional problems to be understood and solved on the way to scientific and medical advances. There were some noteworthy exceptions. In 1995, for example, Morris Foster at the University of Oklahoma was awarded a grant to study ELSI concerns of two Native American tribes. For the most part, however, ethical issues were seen as “out there” in the proverbial ether where technical experts could anticipate them.

This section uses reports for the NHGRI and project data from all of the funded ELSI proposals from 1985-2000 (n=203) — including grants, fellowships, and activities. I use these to show what was promised, what was done or not done, and how successfully funded researchers imagined the issues for the Human Genome Project and how to investigate them. I had to combine two separate sources of NIH data. The NIH RePORTER database only has grant information going back to 1993. Additionally, in most cases the results did not provide grant abstracts. The NHGRI website has its own database of ELSI grants, which provided abstracts but not funding data.[18] I combined these two data sets using NIH project numbers to ensure no projects were double-counted. I chose the 1989-2000 time frame because it covers the period from the inception of the ELSI working group to the completion of the genome. Only in this time frame, therefore, could anyone have raised issues that might have influenced the course of discovery.

According to the Human Genome Project’s first 5-year strategic plan, the ELSI program began with four goals and five mechanisms for accomplishing these goals. Below is a list of these goals where *n* refers to the number of funded grants associated with each category. Of course, I have added the counts to these categories from the strategic plan. At the time of the report, there would have been no count.

1. Address and anticipate the implications for individuals and society of mapping the sequencing the human genome (n=103).

2. Examine the ethical, legal, and social sequelae of mapping and sequencing the human genome (n=71).

[18] https://www.genome.gov/17515632/elsi-research-program-abstracts-and-activities-database/

3. Stimulate public discussion of the issues (n=16).

4. Develop policy options that would assure that the information is used for the benefit of the individual and society (n=28).

The five mechanisms for accomplishing these goals were the following.

1. Stimulate research on these issues through grants (n=136).

2. Refine the research agenda through workshops, commissioned papers and invited lectures on specific topics selected by the working group (n=48).

3. Solicit public testimony from the community at large through town meetings (n=12).

4. Support the development of educational materials for all levels (n=34).

5. Encourage international collaboration in this area (n=5).

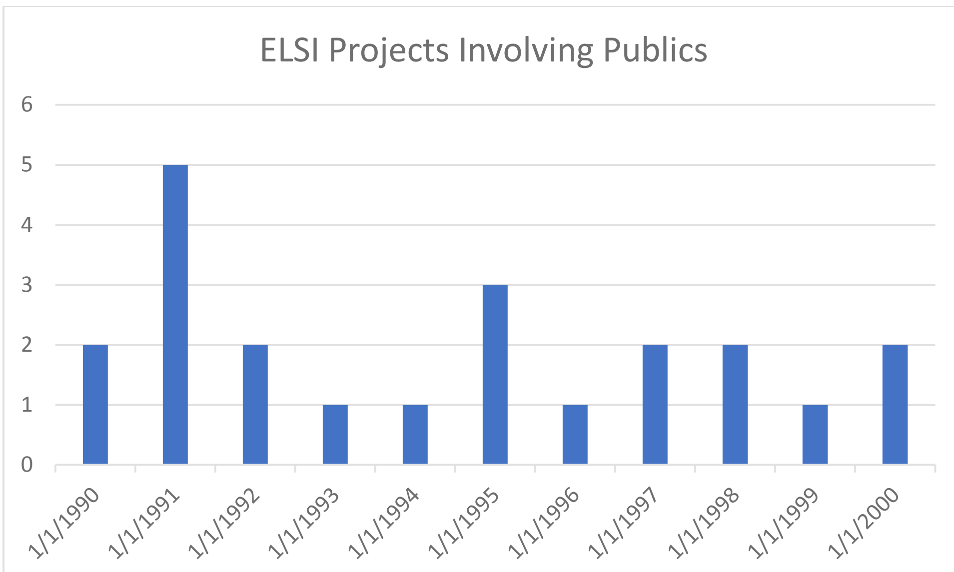
Like the scientists in the rDNA debate, those leading the ELSI apparently did not see the relevance of the public for its work. Although they solicited public testimony in other ways, usually through academic conferences with a public component, not a single town meeting, as listed in mechanism three, was funded from 1989-2000. Because I found nothing in the grant data, I searched the archive of NIH bulletins and the annual ELSI program reports to see if any mentioned a “town hall” or “town meeting” as described in the five-year goals. The first activity with a significant town hall type meeting came almost a year and a half after the genome was complete. It was held on November 9-11, 2001, titled "The Human Genome Project Conference: The Challenges and Impact of Human Genome Research for Minority Communities" at the Renaissance Hotel and at Shiloh Baptist Church in Washington, D.C. The stated purpose was to reach out to minority community members about the implications of the newly completed genome. Francis Collins also used the opportunity to discuss the “unfortunate walls” that had developed between science and the Christian faith. According to the NIH bulletin, the town meeting aspect involved 50 community members who were free to discuss their concerns about the completed genome. Waiting until the genome was complete to conduct anything like a town hall limited the impact that public opinion might have had on the research and, obviously, made any discussion of a moratorium or relinquishment irrelevant. Not only was this meeting the first town hall but seemingly the only one for many years as well. The next town hall seems to be a 2006 program, which was also described as a pilot program on public engagement. Jean McEwen, a director of the NHGRI's ELSI research program in 2006, introduced it by saying:

> *It is important that researchers begin working with the U.S. public now so that, in the event these projects are launched, public input and concerns about issues like patient privacy and informed consent can be incorporated into the design and implementation of such studies* (NIH News 9/28/2006).

Apparently, public input on the design and implementation of a study was not a key concern for the Human Genome Project itself. This town hall, and McEwen's sentiment here, came at a time when other branches of science and technology were re-discovering the importance of public engagement in science. This process was driven, in part, by the politics and market consequences for public resistance to genetically modified organisms in Europe, and the response by European science policy agencies. The next chapter discusses this in detail. For now, it is only important to note that there were few efforts to bring the lay public into the work of the ELSI program and the development of the Human Genome Project.

Since I found no town hall meetings from 1989-2000 I decided to broaden the criteria for "solicit[ing] public testimony" by counting conferences and other meetings which claimed to give non-expert publics an opportunity to have their voices heard by counting research using open-ended focus group discussions. The ELSI program's major innovation over the rDNA debates was the inclusion of certain ethics professionals. With these data, however, I wanted to see the degree to which the ELSI program included the lay public. I only counted events which were open to anyone who wanted to participate regardless of knowledge or professional affiliation and where there seemed like an opportunity for feedback from the lay public. Essentially, I hoped to see to what degree they satisfied Goal #3, to stimulate public discussion of the issues. Graph II.1 shows the frequency of these events by year. Work involving some sort of public access peaked in 1991, the first full year of the ELSI program. It declined in 1992-1994. This may have to do with the battle between Human Genome Project director Jim Watson and NIH director Bernadine Healy at that time around patenting genetic information with Watson leaving the project in 1994.

Graph II.1 Funded ELSI Projects Involving Publics

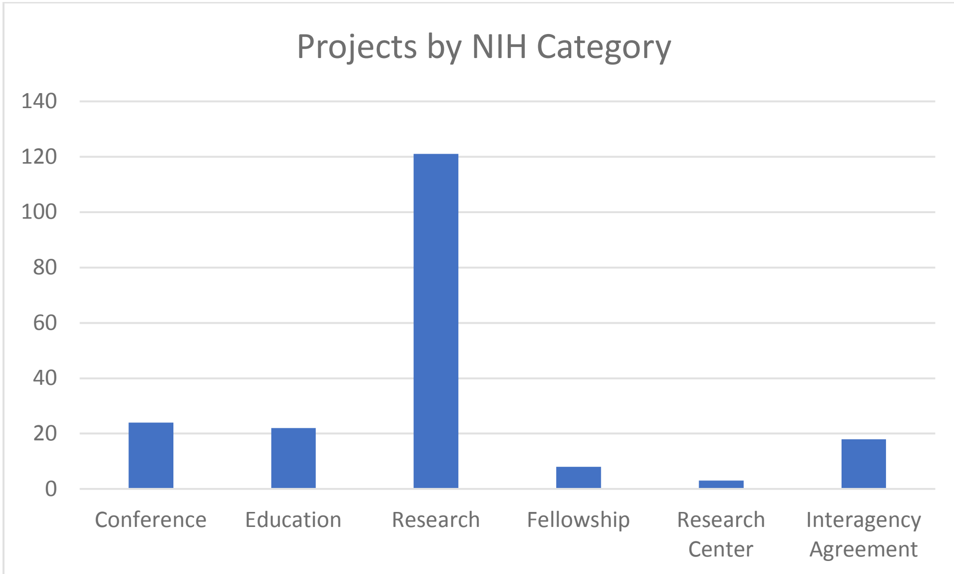


Only a handful of projects had any public involvement. Again, these were mostly academic conferences in which publics could participate if they knew about them. If there was little public engagement, then what did happen? Using the NIH project numbers, and the classification system to which they correspond, it seems that roughly 62% of ELSI grants supported research (n=134), 11% supported educational initiatives (n=24), another 11% supported conferences (n=24), and 8% were NIH Inter-Agency Agreements (n=18). The rest of the categories were represented by only a few cases. The Inter-Agency Agreement classification is odd in that it does not capture the nature of the activity but rather a mode of funding. It describes any activity for which funds are transferred from the NIH to another agency within the DHHS (n=5) or funds transferred outside of the DHHS altogether, to “acquire products, services, or studies” (n=13). These were mostly conferences (n=7) and research (n=9) but also included a PBS television series and a single individual’s book project. The NIH used this funding mechanism mostly from 1989-1991 (n=14). According to NIH classifications, then, the ELSI working group mostly funded standard academic activities: research, education, and conferences. These are consistent with practices of scientific knowledge-production and clearly borrow from those modes of producing credibility and legitimacy. While this is not surprising in itself, it further indicates that public engagement did not then fit within the institutional logics of federal science funding agencies. Not only do they literally lack the funding category for public engagement, but it also seems difficult to fit public engagement into existing funding categories.

In addition to not showing public engagement, the NIH categories also do not capture the degree to which these projects addressed ethical concerns. To determine this, I coded the project descriptions according to the five goals laid out in the NHGRI’s Five-Year strategic plan for the join ELSI program. There is overlap among categories since, unlike NIH project numbers, these categories were not mutually exclusive. For each of the NIH goals listed above there is a corresponding *n* showing the number of total projects in that category.

The number of projects might not matter as much, however, if each of the projects were sufficiently well funded and engaging. It is difficult now to know the degree to which each project engaged the public. Funding data is the best approximation. Unfortunately, though, the NIH RePORTER tool, which provided funding data, only goes

Graph II.2 Number of ELSI Projects by NIH Category



back to 1993. This is unfortunate given the internal battles that began taking place in that year. It would be valuable to see how the Human Genome Project's funding commitments changed with the changing of the Human Genome Project director from James Watson to Francis Collins. It is precisely this period for which we do not have funding data.

Since research is the largest category, I coded those abstracts again in order to identify trends in the focus of those research projects. I began open-coding but quickly discerned four common types. The first was research on *people*. Typically, this research asked how people understood, or misunderstood, genetic information and what factors motivated them to seek or avoid genetic screening for a possible condition. For example, Susan Spear received a $200,435 grant in 1999 to field a survey to patients with increased risk of Alzheimer's disease to see how well they understand the genetic factors in Alzheimer's and to identity their attitudes towards pre-symptomatic genetic testing. This category of research falls within the definition of public understanding of science discussed in Chapter I. Although PUS has historically focused on communicating with the public, it also includes research to identify how publics think and what they know, in order to facilitate science communication. Brice Laurent calls this the move from public understanding of science to a scientific understanding of publics (2017:49).

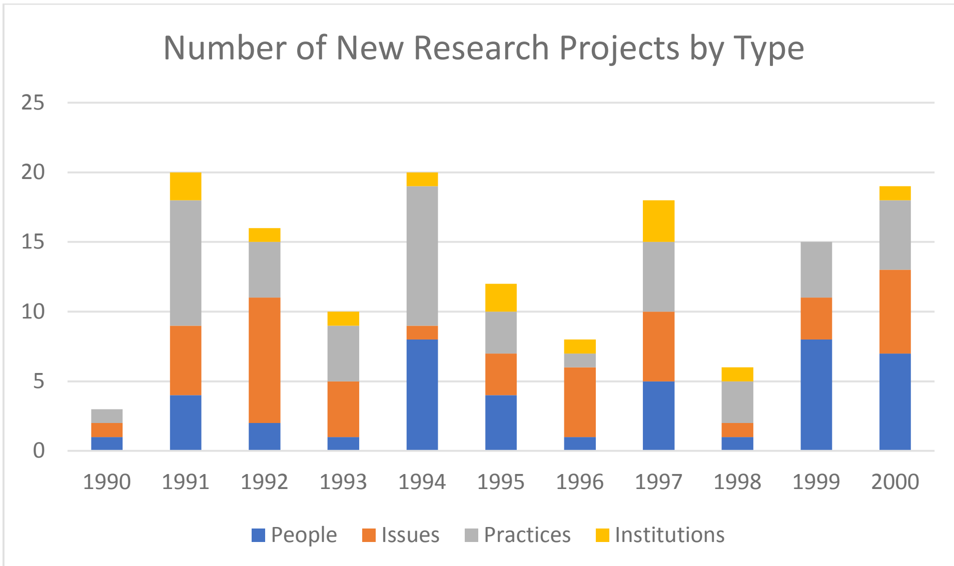
The research category of *people* overlapped strong with the second area of research, which concerned medical *practices*. The most common examples of this research focused on informed consent, genetic counseling, and communication with patients. Research projects that combined these two categories were often testing a treatment condition. For instance, in 1991 Dorothy Wertz at the Eunice Kennedy Shriver Center for Mental Retardation received $135,420 for a project that fielded a 20-question post-treatment survey to test the impacts of genetic counseling on people in the U.S. and Canada.

The third category was research on *issues*. This research track was more often directed towards identifying the potential impact of issues already defined by ELSI working group members or prominent scientists like Francis Collins. In 1993, for example, Albert Jonsen was awarded $129,966 for his study *Paradigm Approach to Ethical Problems in Genetics*. Jonsen's study proposed to develop a "comprehensive and systematic framework for ethical issues" by pairing diseases with known a genetic component to a known list of ethical issues and testing these categories against patient records in genetics clinics at the University of Washington. This category best reflects the technocratic vision of the Human Genome Project's ELSI research, which treats ethical issues as objective, technical objects that scientists can discover in the technology itself.

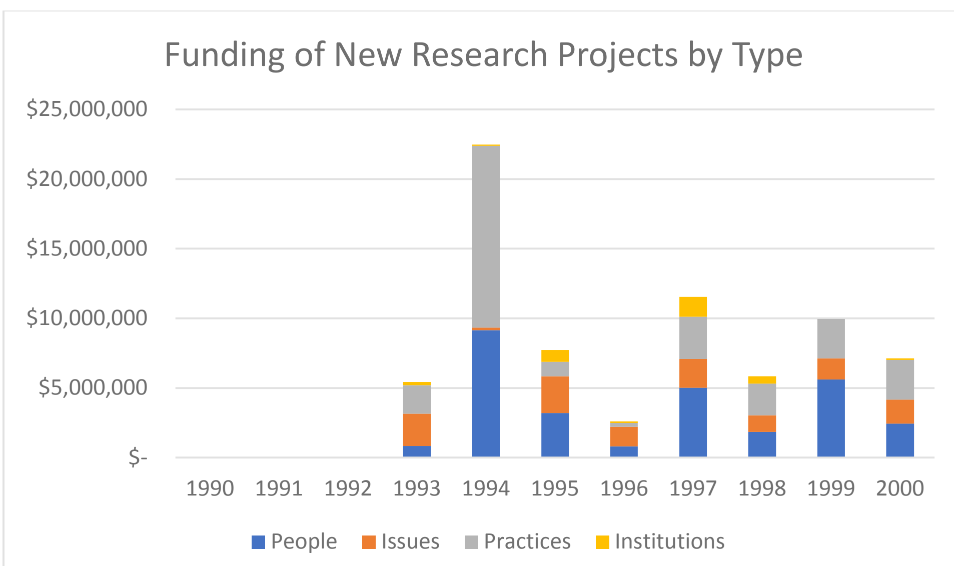
The final category of research focused on *institutions*. These projects, which were the fewest in number, investigated the history of scientific disciplines, the cultures of science, and the broad impacts of genetic knowledge on institutions of law, insurance, medicine, and healthcare. For example, in 1992 David Blumenthal was awarded $100,000 to study changes in academic-industry relations brought about by the Human Genome Project. The study was supposed to help academic-industry relations, in part, by testing whether the Human Genome Project would "enhance the commercial and academic productivity of university investigators (including current or potential HGP grantees), or…reduce communication in ways that may compromise scientific progress and the university environment." This category came the closest to recognizing the power of civic epistemology, meaning here that technoscientific discoveries are changed, applied, and otherwise made meaningful by the institutions with which they interact (Jasanoff 2005).

Given the logic of the ELSI program, we might have expected the largest category to be research on issues. Even if Galas was right, and the Human Genome Project raised no new social issues, research into the issues against which the Human Genome Project might “ricochet” was still a major component of its mandate. Issues research does grow steadily from 1990-1992 before it bottoms out in 1994. Issues research resurfaced briefly in 1996-1997. In an inverse relationship, we see research on people and practices. These issues see a spike in 1994, decline briefly, and then come back in 1998. Although I cannot be sure, the 1994 spike may have been a product of the 1993 cancellation by Congress of the Superconducting Super-Collider planned in Texas. The cancellation of that $4.4 billion project sent shockwaves through science agencies. The lesson was that they had to communicate better with people and with Congress in order to show the value of scientific research (Guston and Keniston 1994). The overall funding picture tells a similar story about the rise and decline of people and practices research, though it shows that such research actually came back in 1996 and remained strong. The funding picture also reveals that the 1994 spike is stronger than the number of projects alone shows. In terms of funding, the majority of projects dealt with practices and about half investigated people. Research on issues nearly disappears. This was likely an artefact of the Watson, with his hands-off approach to ELSI, being replaced by Francis Collins, who began directing the study of the ELSI group more towards the four major issues identified early by the ELSI working group. These were the cystic fibrosis gene, the BRCA gene mutations for breast cancer, issues of informed consent, and issues related to genetic testing.

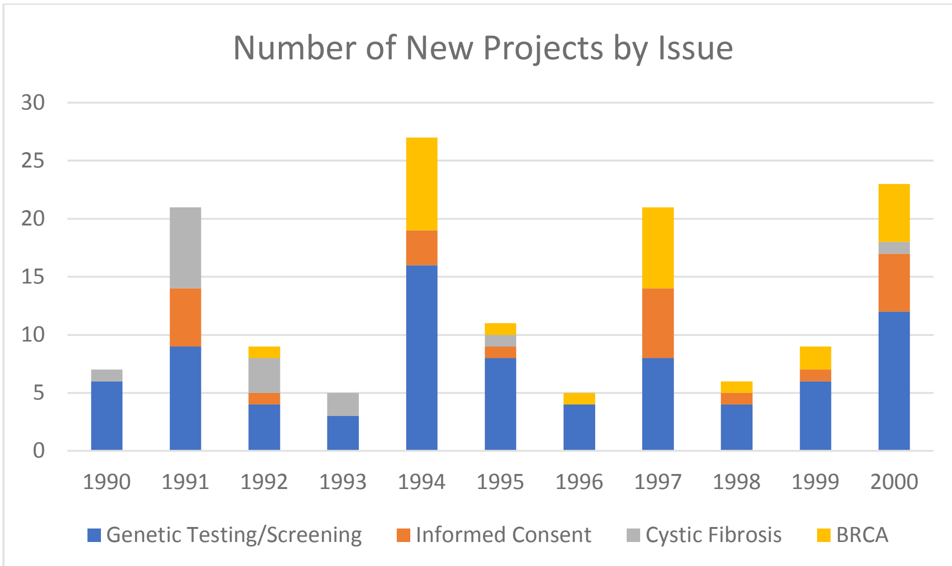
Graph II.3 Number of Annual New ELSI Projects by Research Type



Graph II.4 Funding of New ELSI Projects by Research Type



Graph II.5 Funding for Core ELSI Issues Defined by NHGRI



Conclusion

These two cases, the rDNA controversy with a focus here on the Asilomar conference and the Ethical, Legal, and Social Implications program of the Human Genome Project, two different ways in which ideas about acceptable science policy were constructed. Each represents a different technocratic orientation to science and policy and technoscientific development in a democracy. Each one also protected scientific autonomy in a different way.

In the case of the rDNA debate, scientists focused almost solely on making their research safe, first in the laboratory, and secondly for society as a whole. The issues were technical ones, having to do with the infectiousness of the SV40 virus inserted into E.Coli bacteria. The solution were similarly technical, centering mostly on containment. They introduced what was, for them, a new idea into the science policy domain: a moratorium and possible relinquishment. Whether or not the ideas of a moratorium and relinquishment were really new, it was revolutionary to them. Furthermore, it was a move that even many supporters of it later came to regret, as it did not seem to quell public concerns with rDNA research. If anything, it looked to many like biased self-dealing by scientists to avoid substantive regulatory policies around rDNA research. And really, that was fairly accurate. They hoped that their efforts would obviate the need for a more substantive role by regulators. When that seemed impossible, they tried to make sure that the NIH, which they considered an ally, would oversee rDNA research and its safety.

The scientists at Asilomar, for the most part, kept to an authoriative idea of scientific expertise. Non-scientists were not invited to participate in the discussions. Furthermore, rDNA researchers' collective imaginary about proper science and science policy did not include publics or ethics experts as even a possibility. Journalists were allowed in, but only because they served the role of public witness to give the proceedings democratic legitimacy. Even their presence was cause for a vote by rDNA researchers at the conference. Attorneys were also brought in and here we see the only kind of non-scientific expertise allowed. They were to report on the possible legal-scientific intersection that might happen with rDNA research. They were not there to speak to the issues, but only to possible outcomes. The attorneys spoke outside of their role, however, by introducing the idea that publics had a role and should have a voice in science. If scientists accepted these ideas, they did not immediately put them into practice.

The Asilomar conference, and to some degree the larger rDNA controversy, was an object lesson in drawing the boundaries of science policy in such a way as to exclude those without scientific expertise. Consistent with theories of risk and uncertainty, the efforts of rDNA scientists to address all of the potential issues with their research was bound to fail. In their defense, they tried to draw a line around what was relevant by directing their efforts at health and safety concerns. In the first place, though, there was no way that they could have foreseen all of these concerns. Secondly, their focus on scientific expertise as the ultimate arbiter in rDNA policy did not take into account the fact that lay publics might have ethical concerns falling outside the bounds of health and safety. Something like rDNA raised difficult questions about the nature and sanctity of life and fear of science run amok. However irrational and irrelevant those concerns might seem to scientists, they can have serious consequences nonetheless.

The ELSI program of the Human Genome Project marks another moment of expansion and disciplione in ideas about acceptable science policy. The ELSI program expanded ideas about acceptable science policy by including a new set of experts. These were people with expertise in social sciences, law, and medical ethics. At the same time, there was work to narrow those newly expanded boundaries and thereby to discipline the ethics discourse in the Human Genome Project. The first effort was to get rid of the idea that had been the science policy expansion in the rDNA debates, the possibility of a moratorium on human genome research. The second effort was to enroll a new set of experts who would not problematically deconstruct, in Jasanoff's terms (1990), the work and goals of the Human Genome Project. Although some of them were outsiders, in the sense that they were not experts in science, they were enrolled into the larger project. Finally, the ELSI working group took a technocratic approach, treating ethical and social issues like technical ones and increasingly using the new set of non-scientific-experts to make those discoveries.

Additionally, both the rDNA debates and the ELSI program of the Human Genome Project were responses to an imaginary of social pressure and possible backlash. It was fear for the project that motivated the attention to ethical, social, health, and safety issues for these research projects. Those imaginaries proved insightful for accurately identifying the fact that public perceptions mattered. Neither, however, fully embraced the idea that public engagement was important for addressing ethical and societal issues. Even the Human Genome Project, which espoused the value of public engagment, did very little in this regard. For that, we turn now to the National Nanotechnology Initiative, in which the funding agencies involved expanded what was deemed acceptable science policy to include public engagement. Of course, this came with its own modes of disciplining discourse.

CHAPTER III

The National Nanotechnology Initiative: Societal and Ethical Implications of Nanotechnology

Introduction

The previous chapter introduced the two comparative cases of this research, showing two different approaches to ethical, health, and safety issues as well as societal implications. This chapter turns to the main empirical case of this research, nanotechnology and the National Nanotechnology Initiative (NNI). The data for this chapter comes from an analysis of 17 Congressional hearings related to nanotechnology, 38 NSF workshop reports, the NSF budget documents from FY2000 to FY 2015, six pieces of legislation dealing with the National Nanotechnology Initiative, and occasionally quotes from the 33 people I interviewed where such quotes are relevant. When the NNI began in 2001, it was supposed to include its own version of the ELSI program, the Societal and Ethical Implictions of Nanotechnology (SEIN) research program.[19] This chapter explores the conditions of emergence for the SEIN program and its efforts at engaging lay publics. It is this upstream public engagement that, for our purposes, most differentiates the National Nanotechnology Initiative's SEIN program from the Human Genome Project's ELSI program. Upstream public engagement was the major expansion in acceptable science policy under the National Nanotechnology Initiative.

The condition of upstream public engagement's emergence gives an insight into the political imaginaries and public reasoning that underlay those efforts. Although

[19] SEIN was also often called ELSI. In the interest of avoiding confusion, however, I am choosing to refer to the NNI's program as SEIN and use ELSI only to refer to the Human Genome Project's program. Where interviewees or official documents use ELSI to refer to the NNI program, I will put "SEIN" in brackets to clarify.

upstream public engagement was a significant expansion in what was considered acceptable science policy, the way it was imagined also served to discipline the discourse in terms of who was invited and what they could talk about. The emergence of the SEIN program and the boundary work that defined nanotechnology, discussed in this chapter, shows how some of the early discourse around nanotechnology was disciplined.

In ways similar to rDNA research and the human genome project, the SEIN program was intended to protect the funding of the program as well as the autonomy and authority of scientists. Unlike the other two cases, however, the stakes seemed much higher for nanotechnology. If not, then the hyperbole was stronger. The development of nanotechnology came to symbolize the standing of American scientific development and its economic future. Looking at the conditions of emergence for the SEIN program will also help us understand, in the next chapter, how the discourse of the lay public was later disciplined. In the lead-up to the passage of the National Nanotechnology Initiative and the development of its SEIN program, several battles took place to define nanotechnology and narrow the scope of acceptable concerns about it. In this story, we see exercises in public reasoning (Hurlbut 2017, Jasanoff 2012) that discarded certain concerns as science fiction while embracing others as acceptable. The results of these struggles informed the public engagement work that followed.

Nanotechnology and The NNI

What, exactly, is nanotechnology? This question is surprisingly difficult to answer, but not for scientific or technical reasons. One reason is that “nanotechnology” may be thought of as more of an umbrella term for several areas of technoscientific research than a field unto itself. For instance, micro-electronics and materials science both fall under the “nanotechnology” despite the fact that both technosciences pre-date the American use of the term. The other reason is that scientists have a preferred myth about the origins of nanotechnology that exclude its founder, Eric Drexler. Nanoscientists came to see Drexler’s concerns about the social implications of nanotechnology as a threat to the field, and so they labeled him a heretic to science. He engaged in a protracted battle with Nobel laureate physicist Richard Smalley to define nanotechnology. Their debate set the stage for the determination of what were seen as the legitimate social concerns about nanotechnology and, thereby, influenced what were deemed legitimate matters for science policy. For now, though, it is enough to know that the signified to the signifier of "nanotechnology" has never been entirely clear.[20] Nonetheless, it will be more useful to have even a flawed working definition of nanotechnology than none at all.

Most definitions of nanotechnology are based on its unimaginably small size. The prefix "nano" refers to one-billionth of a meter. The common referrent for this is the human hair, which is 10,000 times thicker than the smallest nano-particle. Nanotechnology, using size as its defining factor, is any science having to do with materials or machines at that scale. Carbon nanotubes, for instance, are cylinders with walls one carbon atom thick. They are many times stronger and lighter than steel. A nanowire, another nanotechnology structure, is a nano-sized crystalline structure that conducts signals in micro-transistors. Nanotechnology, then, usually refers to materials or shapes between 1nm-100nm that are used in technological or commercial products.

There are two things to know about nanotechnology for this discussion other than its size. First, is that nanotechnology is in a lot of consumer products from the microprocessor chip in a cell phone, to the coating on stain-resistant clothing, to nano-sized zinc oxide particles in clear sunscreen. Second, is that nano-materials often behave differently than their larger-sized counterparts with the result that they can be more toxic. For instance, the element gold, which is famed for being chemically non-reactive, becomes reactive and, thus, toxic at the nano-scale. This is due to the increased ratio of surface area to volume for the nano-particle. There are two reasons to be aware of this. First, these surprising qualities, in which usually non-toxic elements become toxic, increases the uncertainty of nanotechnology, potentially exacerbating risk society style concerns about the proliferation of technological risks and the inability of scientists to deal with them (Fitzgerald and Rubin 2010). The other is reason this is important is that much of the disciplinary work around nanotechnology seemed to avoid

[20] Even scientific definitions of nanotechnology have proven elusive, which has made nanotechnology difficult to regulate. It is not clear to regulators if nanotechnology refers to a certain size of particles in commercial products or to various kinds of uses. Toxicologist Andrew Maynard argues that regulators should not try to define nanotechnology at all, since such a definition would be a "'term of art', rather than science" (Maynard 2011).

acknowledging this fundamental and concerning uncertainty. Basing a definition on nanotechnoloy on size, then, elides much of the complexity surrounding nanotechnology. It is simply too far beyond the scope of this project to arrive at a more sensible definition of nanotechnology.

Nanotechnology has always had something of an identity crisis that, at various times, threatened its stability as a field and thereby its funding. This identity crisis starts at the very beginning of nanotechnology. The story that scientists now tell about the birth of nanotechnology credits the idea to Richard Feynman's 1958 lecture *There's Plenty of Room at the Bottom*. Feynman's lecture, the story goes, lay the conceptual groundwork for what would later be called nanotechnology. However, as many scholars have since shown, this is actually a re-imagining of nanotechnology to exorcise its founding father cum heretic, Eric Drexler from its history (Benett and Sarewitz 2006, McCray 2013, Selin 2011). As an undergraduate at the Massachussetts Institute of Technology (MIT), Drexler created a new major for himself which he called "molecular engineering," a term borrowed from MIT physicist Arthur von Hippel (Charriere and Dunning, 2014). Drexler would go on to advocate for the promise of molecular engineering, eventually borrowing the term "nanotechnology" from Japanese physicist Norio Tanguchi. Drexler taught undergraduate courses in nanotechnology mostly focused on what might one day be possible, given established principles of science and engineering. This is one of the many ways in which nanotechnology was unique as a modern science. It emerged as a discipline before it could do anything in the laboratory. It "arrived" in 1989, when scientists at IBM's Almaden Research Center used a converted electron scanning tunneling microscope (STM) to arrange individual argon atoms to spell out "IBM."[21]

The mantle of nanotechnology, which was often seen as something closer to alchemy than science while led by Drexler, was slowly taken over by physicists such as Richard Smalley. It achieved full legitimacy with the creation of the National Nanotechnology Initiative (NNI) in 2001. President William J. Clinton announced the

[21] In a bit of random coincidence, I was present just after this moment of discovery. I wound up taking a personal tour of that Almaden facility in 1989 at the age of 13 with a physicist named Hanz Coufal. I was briefly introduced to Donald Eigler, one of the scientists who arranged these atoms. Showing me the electron microscope image of "IBM" spelled in argon atoms that they had taped to a wall, Dr. Coufal told me that they had probably just embarked on the most exciting discovery in modern physics.

creation of the NNI in a January, 2000 speech to an audience of scientists at Caltech. In his Caltech speech, Clinton referenced three of the most common innovations promised by nanotechnology: materials lighter and stronger than steel, high-capacity storage media, and tools for detecting early cancers. The excitement and hope around nantoechnology was unmatched at that time. As a 2003 editorial in *Nature* magazine described it:

> *Not since John F. Kennedy's announcement of the US government's intentions to put a man on the moon has scientific discipline so captured the imaginations of politicians, venture capitalists and pulp-fiction writers alike* (Nature Materials 2003:127).

The NNI became a federal initiative which grew to include twenty-five agencies, all collectively developing nanotechnology research. Its initial budget of $464 million in 2001 grew to $1.6 billion by 2013 (Carim 2013). Of these federal agencies, the most prominent were the NSF, the DOE, the DOD, and NASA. While all of these agencies were responsible for a certain degree of SEIN research, the NSF became the central agency for SEIN and upstream public engagement.

In a matter of decades, nanotechnology went from a matter of speculation by an intrepid undergraduate at MIT to one of the most highly-funded and highly-hyped areas of technoscience in U.S. history up to that point (Berube 2006). There is little wonder, then that it was difficult to set policy for nanotechnology and that doing so involved significant re-imagining of science policy and its relation to the civic epistemology.

Figure III.1 Agencies in the National Nanotechnology Initiative (Ota Wang 2009)

Societal and Ethical Implications of Nanotechnology (SEIN) and Public Engagement

Similar to the Human Genome Project and its ELSI program, the National Nanotechnology Initiative was supposed to attend what was called, in the NNI, the Social and Ethical Implications of Nanotechnology research. The concept of SEIN is not so different from ELSI and they are often used interchangeably by people in the field of nanotechnology. I will use SEIN here mainly to differentiate it from the Human Genome Project's ELSI program, but the reader will not be far off if they think of SEIN as nanotechnology's ELSI program. Again, SEIN was a part of the NNI before it even got started. In his aforementioned speech, President Clinton stressed the importance of ethical scientific innovation under the NNI.

> *It's up to all of us to figure out how to use the new powers that science and technology give us in a responsible way. Just because we can do something doesn't mean we should. It is incumbent, therefore, upon both*

scientists and public servants to involve the public in a great debate to ensure that science serves humanity -- always -- and never the other way around.

On this campus nearly 70 years ago, Albert Einstein said, "Never forget this, in the midst of your diagrams and equations: concern for man himself and his fate must always form the chief interest of all technical endeavors." Today, at the dawn of this new millennium, we see for all of you, particularly the young people in this audience, an era of unparalleled promise and possibility. Our relentless quest to understand what we do not yet know, which has defined Americans from our beginnings, will have more advances in the 21st century than at any other time in history. We must be wise as we advance (Clinton 2000).

One could say a great deal about this quote. Clinton’s political imaginary here embraces a mode of science policy governance that includes a possible moratorium. He also frames scientists not only as inherently social actors, even in simply conducting their research, but as fiduciaries of public trust and wellbeing. These particular political imaginaries did not ultimately frame the SEIN program. The essential point that nanotechnology should be pursued responsibly, however, did seem to frame the effort. Early Congressional hearings and NSF workshops also stressed a commitment to SEIN research. For instance, in the 2000 NSF *Societal Implications* workshop, intended to address the importance of societal and ethical concerns, it states that “Advances in nanoscience and nanotechnology promise to have major implications for health, wealth, and peace in the upcoming decades” and that, “The study of the societal implications of nanotechnology must be an integral part of the NNI” (Roco and Bainbridge 2001:iii).

In part, this commitment followed directly from the example set by the ELSI program of the Human Genome Project. It was the only real model of a federally-funded initiative for attending to the ethical and societal issues of a larger scientific initiative in an upstream manner. Its value lay in the fact that it seemed to address the necessary issues without resorting to the blunt mechanism of a moratorium, used unsuccessfully during the rDNA controversy. It was, however, only a model for the feldgling NNI. As Edward Hall, one interviewee for this project involved in the development of the NNI, explained that “as we were beginning to formulate what this should look like, we actually made repeated references to the [ELSI] program…but it was the idea rather than the mechanics” (Hall Interview 5/19/2018).

The NNI borrowed from the Human Genome Project the idea that something like ELSI was necessary and that it should begin early in the life of the initiative. As we will explore in more detail below, the NNI was also influenced by public backlash against genetically modified foods in Europe and the efforts to deal with that backlash. In terms of ELSI, though, the ethical work for nanotechnology borrowed from the concept of ELSI but did not take the form of ELSI. For instance, nothing quite like the ELSI working group was ever implemented for the NNI.[22] Perhaps more saliently, unlike the Human Genome Project, the NNI did not immediately begin attending to societal and ethical issues despite a lot of rhetoric to that end.

The NNI could not make such a boast. Three years into the initiative and little to nothing had yet been done in terms of SEIN research or upstream public engagement. As Mnyusiwalla et al. (2003:R10) point out, although the NNI had supposedly set aside $16-28 million for SEIN research, less than half of that had been spent and none of it funded even a single social science project. Much of this funding supported other ways that laboratories could meet the requirement regarding ethical issues, including research into laboratory safety. This is research that they might well have done anyway using their primary research funding. The first report to Congress by the National Academies of Science (NAS) on the NNI confirmed this problem (*Small Wonders, Endless Frontiers* 2002). It put the blame partially on the Nanoscale Science, Engineering, and Technology (NSET) subcommittee of the National Science and Technology Council (NSTC)[23] for several problems in its funding structure and partly on

[22] In 2006 the National Nanotechnology Coordination Office (NNCO), tasked with coordinating the NNI’s twenty-five agencies, did create the Nanotechnology Public Engagement and Communication (NPEC) working group. However, it did not function like the ELSI working group for the Human Genome Project. The NPEC did not deal with societal issues itself but rather organized the efforts of the various agencies and tried to turn their SEIN and engagement work into a standardized approach to publics and public engagement. Chapter IV covers this in greater detail.

[23] See Figure III.1 for the organization of the NNI from 2000 until 2004. To avoid confusion, I will not often refer to these subcommittees unless necessary.

the social science proposals themselves which were not funded because they “were not at centers judged meritorious enough to warrant funding” (2003:34). Since there is no record kept of unfunded proposals, it is impossible to know where these applications came from or what was considered “meritorious” for these purposes. According to the report, the three structural problems were (1) a funding structure that pitted social science proposals against science and engineering proposals, (2) that social science work was only one of six ways in which the NNI could qualify as societal implications research (others including “international collaboration, shared experimental facilities, systems-level focus, proof-of-concept test beds, and connection to design and development activities”), and (3) there were no social scientists on the review panels for these grant proposals (Societal Implications Hearing 2003:5).

That report was not the first time these issues had been raised. The Chair of the review committee that produced the National Academy of Sciences report, Dr. Samuel Stupp, had voiced these concerns previously in his testimony at the September 2002 Congressional hearing.

> *Even though there was the intention of the NNI to look into issues of societal implications, the reality is, it really has not happened to a great extent, and so we are recommending that the NNI implements a new strategy to make sure that those programs do take place* (*Nanotechnology* 2002:53).

The rhetoric around early attention to societal and ethical implications of nanotechnology had not been met with funding to realize that rhetoric. The new strategy he recommended was a funding mechanism that set aside funding solely for SEIN research and public engagement. This funding structure did not immediately change. In part, this was because it seemed to meet some partisan resistance in Congress. Of all the amendments proposed in May of 2003 to finalize the 2003 Nanotechnology Research and Development Act of 2003 (Nano R&D Act), intended to fund a multi-agency research effort in nanotechnology, the only two not passed were one by Representatives Brad Sherman (D-CA) and Chris Bell (D-TX) “to require that not less than five percent of the total appropriations be set aside for research on societal and ethical implications of nanotechnology" and one by Representative Eddie Bernice Johnson (D-TX) "to require that the Advisory Committee convene citizen panels of nonscientific and nontechnical experts to consider and make recommendations on the societal and ethical concerns arising from the development of nanotechnology" (Nano R&D 2003:11-12). The only SEIN related amendment to pass was one proposed by Representative Chris Bell that ensure that "interdisciplinary research centers include activities that address societal and ethical concerns" (Nano R&D 2003:11-12). In some ways this resembles the Asilomar approach in that these centers were housed in research in scientific research facilities. Alone, this amendment would have placed ethical and social implications work within centers of technoscience discovery.

Three other factors had been bubbling in the background while all this had been taking place. First, tthroughout the 1990s, while the ELSI program struggled to influence the larger Human Genome Project, scholars mostly in STS published a great body of research critiquing what we already discussed as the "deficit model" for risk research and public engagement (Alaszewski and Brown, 2007, Davis 2011, Irwin 2001, 2014, Jasanoff 1998, Leach et al. 2005, Lewenstein and Brossard 2006, Rogers-Hayden and Pidgeon 2007, Sarewitz 1997, Stilgoe 2014, Toumey 2006, Wynne 2006, Ziman 1991). The response by scholars in the 1990s, particularly in science and technology studies, was what has now been dubbed the "participatory turn" or sometimes the "analytic deliberative turn" (Fiorino, 1990; Fischhoff, 1995; Irwin and Wynne, 1995, 1996; Pidgeon, 1998; Stern and Fineberg, 1996; Renn, 1992).[24] I will stick here with the former term. Those working in the participatory turn advocated for more upstream public engagement and participation instead of top-down informal science education and communication. A key figure in one of the Science and Society Centers saw this as one reason for the risk of SEIN research and public engagement under the NNI.

> *I think I also see it as part of the analytic deliberative turn which was, in part, really a global deliberative turn and then, a little further on, the*

[24] I have combined literatures from the psychometric paradigm in decision-making research with STS scholarship. While both advocated for public participation as an antidote to ineffective public communication strategies, the two had very different motivations. Those in the psychometric paradigm saw public participation as a better way to inform and correct public misperceptions, essentially an extension of the deficit model, while STS scholars wanted to eradicate the notion of the misperception deficit altogether (Pidgeon and Rogers-Hayden, 2007).

> *responsible innovation turn in the EU. In the risk world, or at least the risk perception/risk communication world, the analytic deliberative turn happened, you know, in the four or five years before the onset of this [the NNI]. So, I say that it was partly the moment in time* (Jackson Interview 4/13/16).

Second, several Western European countries had been dealing with issues of public trust in science brought about by certain public health crises, like the Bovine Spongiform Encephalitis (BSE), or "Mad Cow" disease, in the U.K. and public response to genetically modified foods (GM foods) all over Europe. The BSE crisis has become a cautionary tale about how not to foster public trust. Aware that BSE might be able to jump species, Minister of Agriculture John Gummer went on television to assure the public that British beef was safe to eat. In an odd exercise in public witnessing, he tried to get his daughter Cordelia to eat a beef burger. The burger was too hot for Cordelia, so he wound up eating it himself. When a public health problem did emerge, British citizens' faith in British regulatory science had been shaken (Bennett 2010, Jasanoff 2004). Opposition to GM foods, likely linked in the U.K. case to the BSE scare, gave rise to discourse like "Frankenfoods" and "Mutant Crops." These discourses proved resistant to either the technocratic assurances about the safety of GM foods or the charge that GM opponents simply did not understand the science (Frewer et al. 2002, McDavisen 2008, Shaw 2002). Those discourses only served to alienate further a public who already did not trust the products of technoscience (Wynne 2001). The public opposition to GM foods resulted in commercial losses that neither Americans nor Europeans science agencies wanted to repeat with nanotechnology (Gaskell et al. 1999, Laurent 2017).

Finally, upstream public engagement was beginning to emerge for other sciences contemporaneously with the National Nanotechnology Initiative. The National Research Council published a report in 2001 that called for "broad national dialogue on the societal, religious, and ethical issues" in which scientific experts would lead public deliberation about the issue of human reproductive cloning (Hurlbut 2017:191). This effort was qualitatively different from nanotechnology, however, in that it centered public engagement on scientific experts rather than enrolling social scientists to do upstream public engagement. It was also not an ongoing program, but a single set of events to deal with a politically contested issue around the use of the term “embryo” in conjunction with cloning research. Nonetheless, it is evidence that a broader process of learning both from the Human Genome Project’s ELSI program and from experiences in Western Europe was at work in the early construction of the National Nanotechnology Initiative’s SEIN program.

While scholarship like this may have had an influence on the development of the SEIN program, it would only have served as an example for scientists and science agencies in the U.S. to borrow from rather than a direct influence. Bennett and Sarewitz (2006) have shown that social scientists were almost entirely absent from the early development of the SEIN program for the National Nanotechnology Initiative. They came to nanotechnology when invited, with the lure of funding to conduct their research. With the single exception of a toxicologist working in nanotechnology, who turned later to risk research and public engagement, those interviewed for this project all confirmed this when asked how they got involved in nanotechnology. As Raquel Jackson, a prominent scholar who came to lead one of the centers for social science of nanotechnology remarked.

> *In 2000, … I was already interested in deliberative mechanisms for engaging people about science and technology, when these opportunities [from the NNI] just simply came up. … The level of attention to nanotechnology [by social scientists] was off the charts and really continues to be, you know, well beyond what would happen on its own [without NNI funding]. But, you know, I think a lot of the same types of questions would have been asked by social scientists around emerging technologies. But the focus on nano, in particular, was definitely driven by the funding mechanisms* (Jackson Interview 4/13/16).

Academics working in the participatory were already working on projects other than nanotechnology. Indeed, most had never even heard of nanotechnology before the NNI began enrolling them. Jackson acknowledged this as well.

So, for me personally this really was in response to the funding call. I had no background in nanotech and so this was really initiated by one of the leading nano scientists on our campus calling me in and asking me to help put together a group of people who could respond to this [call for research from the NNI]. In fact, our initial reaction was "sounds interesting but we don't know anything about that." (Jackson Interview 4/13/16).

The scholars who eventually came to do upstream public engagement for the SEIN program came after the funding had been secured for SEIN research and upstream public engagement and after having been invited to conduct the work they had already been doing elsewhere. As mentioned earlier, many of them had an indirect influence on the NNI through the upstream public engagement work they had done in Europe. In 2009, for instance, one interviewee, Hugh Davis, had been a part of the first pan-European World Wide Views conference on global warming. That conference helped legitimize large-scale public engagement in science in the U.S. Davis would later come to lead upstream public engagement projects under the NNI. People at the NSF, particularly the first Director of the National Nanotechnology Initiative, Mihail Roco, were aware of this body of research, especially the European work on public engagement. These scholars, however, had no direct influence on the initial discussions about SEIN research and public engagement in the NNI.

Ultimately, funding was set aside for SEIN research and public engagement. The 2003 Nano R&D Act, with its partisan battle over amendments about SEIN research funding and the convening of citizens' panels, wound up not making it to the Senate for a vote. The 21st Century Nanotechnology Research and Development Act opf 2003 (21st Century Act), which borrowed language from the 2003 Nano R&D Act, did pass and was signed into law in December of 2003. It created the National Nanotechnology Coordination Office (NNCO) to organize the several science funding agencies supporting the National Nanotechnology Initiative. It also codified SEIN research and upstream public engagement, in part, "by the convening of regular and ongoing public discussions through mechanisms such as citizens' panels, consensus conferences, and educational events, as appropriate" (21st Century Act §2(a)(4)(D)(10)). This did not happen immediately, however, or simply because SEIN research and upstream public engagement were seen as the right thing to do. To understand how and why SEIN research and public engagement were taken seriously, we must understand the value of the NNI to scientists and funding agencies and the threats they saw facing the initiative.

The Value of the NNI

The Societal and Ethical Implications of Nanotechnology program and upstream public engagement were ultimately funded in order to protect the enormous potential for scientific knowledge-creation, wealth-production, and American scientific identity that nanotechnology promised. To understand SEIN research and public engagement, then, we have to understand the basis for these claims about its potential.

The value of SEIN research was a direct product of the perceived value of the nanotechnology program itself and the perception that SEIN would protect the NNI. Nanotechnology's advocates framed it as imperative for three reasons. First, it promised a funding windfall for a struggling National Science Foundation (NSF). Second, they framed it as the single most transformational emerging technoscience that science-funding agencies could support. It offered nothing less than a second Industrial Revolution. Finally, nanotechnology's advocates argued that it promised to secure America's position as the global leader in scientific development. Together, these made funding the NNI an imperative for American science.

As its first source of value, Nanotechnology promised a massive expansion in the funding of non-biological sciences, such as materials science and physics. In the years leading up to the NNI, funding for these disciplines had been stagnant or had even decreased. It was in this context that Mihail Roco brought the idea of nanotechnology to President Clinton. Mihail Roco is the most important single figure for nanotechnology in the late 1990s and early 2000s. He was the founding chair of the National Science and Technology Council's subcommittee on Nanoscale Science and Engineering. He was nanotechnology's biggest advocate and later became the first Director of the NNI. He was more commonly known then as the "Czar of Nanotechnology." He brought nanotechnology to President Clinton's attention at a time when the NSF was trying to figure out ways to expand its budget. Years of stagnant funding for the NSF budget in absolute numbers meant a real decline when considering the impact of inflation.

Especially annoying to some was the relative disparity in funding between the NSF and the NIH. The NIH had been working with a budget more than double the size of the NSF. An article in the March 2001 issue of Scientific American described the situation aptly:

> *Biologists sometimes stand accused of physics envy: a yearning for irreducible, quantifiable laws sufficient to explain the complex workings of life. But the jealousy goes both ways. Physicists, chemists and other non-biologists have long suffered from what can only be called NIH envy: the longing for the hefty increases in research funding that seem to go every year to the National Institutes of Health* (*Megabucks for Nanotech* 2001:8-10).

This disparity made little sense to those at the NSF and its supporters in Congress. The NIH had a narrow focus on biology and public health whereas the NSF had to fund every other scientific discipline as well as science education. As Representative Eddie Bernice Johnson (D-TX) reminded House Committee on Science[25], "[the] NSF funds 36% of the total academic research in physical sciences, 50% in engineering, and 72% in mathematics" (*The NSF Budget: How Should We Determine Future Levels* 2002:10-13). Graph III.1 shows this relative funding disparity over a thirty-year period.[26] The NSF's supporters in the House Committee on Science called attention to the disparities in the amounts of the average grants awarded within the two agencies and the average length of those grants, where the NIH was able to award larger grants for longer periods. Representative Nick Smith (R-MI), Chairman of the Subcommittee on Research, said what scientists at the NSF apparently could not say.

> *One critique I do have of course is the continued funding disparity … between NIH and NSF. In this budget, for instance, just a slight reduction*

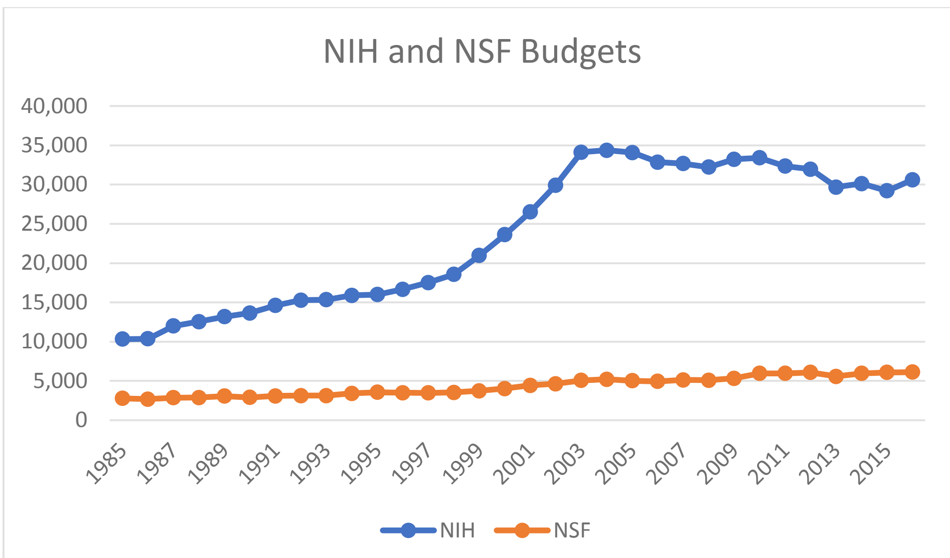
[25] This committee has undergone several name-changes. As of the time of this writing, it is currently the Committee on Science, Space and Technology.

[26] The research budget for the department of Health and Human Services (HHS) was combined with the NIH for several years making it impossible to disaggregate their budgets for those years. For this reason, these data, from the AAAS, combine NIH and HHS budgets for all years.

in NIH funding because of the size of that funding would make a huge difference in terms of increased funds being available in the National Science Foundation (*The NSF Budget: How Should We Determine Future Levels 2002:10-13*).

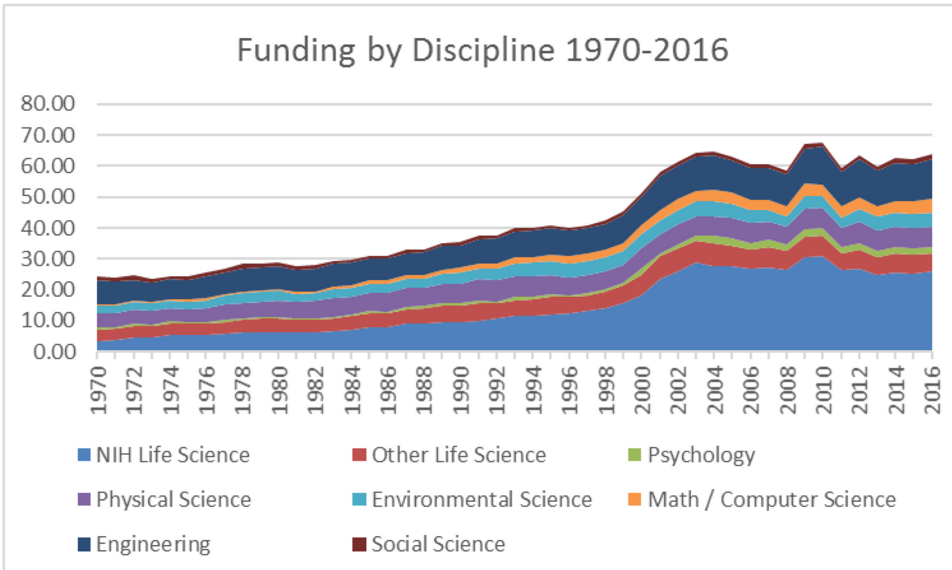
In all of the funding documents for the prior 10 years, there were no calls for a reduction of the NIH budget to fund an expansion of the NSF budget. Administrators at the NSF clearly did not want to call for a decrease in funding for their fellow-agency despite suffering budget-wise themselves.

Graph III.1 NIH and NSF Budgets from 1985-2015 in 2015 Constant Dollars



This funding disparity was not simply between the NIH and the NSF, however, but represented a wider trend in the growth of the “biomedical industrial complex” relative to other scientific disciplines (House 2016). Graph III.1 shows the relative budgets of scientific disciplines over a 36-year period. Engineering and physical science enjoyed slight funding advantage relative to the life sciences throughout the 1970s to early 1980s. Starting in the mid-1980s, though, life science funding began to grow. By 1996 the life sciences became the most well-funded scientific discipline. One justification for expanding the NSF budget, then, was based on issues of fairness and value. It was unfair to have such a funding disparity between the NSF and NIH since the NIH created no more value in terms of commercial technologies and the social value of science than the NSF created.

Graph III.2 Science Funding by Discipline 1970-2016 in 2016 Constant Dollars



Although seemingly everyone acknowledged the disparity as a problem, no one had a long-term solution. In 1999, the Clinton Administration promised a 10% increase in the NSF budget that was supposed to come from a settlement between the federal government and the big tobacco companies. Tying the NSF budget increase to what was essentially a gamble, however, made many policymakers uncomfortable. In his opening statements for a 1998 hearing of the House Committee on Science, Chairman Pickering expressed this anxiety.

> *Although the Science Committee is generally supportive of the increases in R&D proposed by the Clinton Administration, there are real concerns that the proposed budget increases for scientific research relies too heavily on tobacco smoke and mirrors* (*Budget Authorization Request: National Science Foundation* 1999:2-47).

Then-NSF Director Neal Lane was diplomatically circumpsect in his opinion about the tobacco settlement. When asked his opinion by Representative Gil Gutknecht (R-IA) about having his agency's funding "tied to an anchor which may or may not float," Dr. Lane gave a long and general answer about the importance of research and the difficulties of setting budgetary priorities. "Gosh, we'd love to invest more" Lane said, identifying with the Congressional subcommittee and their struggles to make tough budgetary decisions, "but we have to make a decision on our own priorities." At the end of Lane's statement Mr. Gutknecht, seeming confused, asked "So I take it…you'd prefer not to be tied to the tobacco settlement?" Lane merely replied "I look forward to working with Congress," drawing laughter from the committee (NSF Budget Request 1999:2-47).

Director Lane acted diplomatically. Although Lane had compared the NSF to the NIH in terms of number of grants funded, average funding per grant, and the average length of grants to show the disparity between the two agencies, he never called for a reduction in NIH funding to help the NSF. He also did not disparage a potential funding windfall, even if it presented a stop-gap measure to a long-term problem. In the end, the gamble paid off . The settlement did eventually give the NSF a short-term funding windfall, but the deeper problems remained. Another windfall like the Big Tobacco Settlement might never come along again. The NSF needed a longer-term solution.

The usual mechanisms the NSF used to justify its funding clearly were not allowing it to keep pace with inflation much less the NIH. The NSF budget request to Congress asks for funding based on regular expenditures, like administration and facilities, and "research and related activities" (NSF Budget Request 2002). Facilities might justify modest budgetary increases but usually administration costs do not, as high administration costs are viewed negatively. Budget documents usually contain language to assure that budgetary increases will not go to administration costs. By themselves, research and related activites, proved insufficient to justify major budget increases. Research and related activities refers to work in core disciplines like chemistry and mathematics (NSF Budget Request 2002). It seems that the most effective way for justifying budgetary increases were to have research initiatives that combined the activities of several disciplinary fields. The NNI started as one of these smaller initiatives. In the years prior to the NNI, there were several of these initiatives.

The Knowledge and Distributed Intelligence (KDI) initiative was an initiative to fund computer scientists and facilitate computational knowledge infrastructures. The Life and Earth's Environment (LEE) initiative helped fund biologists, geologists, and other scientists workin in areas related to environmental science. The Education for the Future (EFF) initiative funded informal science education in schools and museums. None of these, however, could justify funding all of the varius work that the NSF did. Nanotechnology, on the other hand, by virtue of its fuzzy definition and hype, could do just that. As Neal Lane's successor, NSF Director Rita Colwell, told Congress in February of 2000:

> *[Nanotechnology] involves biology, math, physics, chemistry, materials, engineering, information technology, and all the different ways they connect to each other at the nanoscale. I cannot emphasize enough the strong connections between these initiatives.*[27] *For example, we often say you need nanotech to go further in infotech (Fiscal Year 2001 Budget Authorization Request: National Science Foundation* 2000:7*).*

Not only did nanotechnology cover all of the core research areas but, as Dr. Colwell frames it here, nanotechnology was also the lynchpin for the success of every other initiative that they had proposed. In the budget proposal for the previous year (FY2000), nanotechnology had been framed as an aspect of the Biocomplexity in the Environment (BCE) initiative and helped justify a $25 million increase for the NSF (*Fiscal Year 2000 Budget Authorization Request: National Science Foundation* 1999:194). The total increase requested by the NSF for all of these projects for FY2000 combined amounted to $117 million. By comparison, the FY2001 budget that Dr. Colwell discusses above requested $200 million just for the new nanotechnology initiative.

Nanotechnology promised a budgetary windfall that would right the wrongs of the funding disparity between the NSF and the NIH. The NNI, as an inter-agency initiative, promised even greater funding for the NSF and looked to narrow the gap between it and

[27] She refers here to the other initiatives she discussed at this hearing, including Information Technology Research – which had morphed out of KDI, Biocomplexity and the Environment, the 21st Century Workforce, and the Nanoscale Science and Engineering Initiative.

the NIH. An NSF report to Congress in 2000 promoting the NNI promised that nanotechnology would justify a budget increase of 124% to the NSF and 300% to NASA, while only promising the NIH a 13% budget increase (*Nanotechnology: The Next Industrial Revolution* 2000). Additionally, the report promised that 70% of the additional funding will go towards funding university grants. This had been the main complaint about the funding disparity between the NIH and NSF, that the NSF could not fund university grants at the same level and duration as the NIH. The NIH could offer more grants to university researchers with higher levels of funding that lasted on average two to three years longer. The NNI looked like the budgetary solution scientists and administrators at the NSF had been hoping for.

That was the immediate value of the NNI to the NSF, but nanotechnology's advocates said that nanotechnology held promises for Americ as a whole. Nanotechnology, they said, promised unrivaled economic benefits and came to define America's dominance in science and technology.

In terms of the economy, nanotechnology's advocates promised that it would both create new markets and reshape existing ones. Nanotechnology, to a greater degree than the other initiatives at the time, was framed as nothing short of the the next industrial revolution. Indeed, in a report to Congress titled *Nanotechnology for the 21st Century: Leading to a New Industrial Revolution* (2000) Mihail Roco promises that:

> *The initiative [NNI] will support long-term nanotechnology research and development, which will lead to breakthroughs in information technology, advanced manufacturing, medicine and health, environment and energy, and national security. The impact of nanotechnology on the health, wealth, and lives of people will be at least the equivalent of the combined influences of microelectronics, medical imaging, computer-aided engineering, and man-made polymers developed in this century.*

This is an impressive exercise in public imagination. In simple terms, Roco promised Congress that nanotechnology would be, at a *minimum*, as socially transformational as the total combined influence of all of the most important technological breakthroughs of the 20th century. In effect, Roco engaged not only in science-making, but future-making for the entire country. Budget requests to Congress often contain promises about revolutionary new technologies, but Roco's claim was grandiose even by those often hyperbolic standards.

These breakthroughs were supposed to transate into unprecedented economic and social benefit. The industrial revolution would, according to Roco, lead to a trillion dollar nanotechnology industry sometime by 2015 (Nanotechnology: Societal Implications 2001:25).[28] This would come from revolutionary changes to manufacturing, electronics, healthcare, pharmaceuticals, chemical plants, transportation, and sustainability. Thomas Kalil, deputy director for the White House Office of Science and Technology Policy (OSTP), enlisted an even grander vision. In the cCngressional report on nanotechnology's societal implications he observed the following:

> *Discoveries involving nanoscience will be as dramatic and, I believe, even more important than the creation of the Internet. Let's consider the economic impact nanoscience may have our society [sic]. Bill Joy, co-founder and Chief Scientist of Sun Microsystems, has estimated that the combination of the information and physical world will create in this century a thousand trillion dollars worth of wealth. As a former lawmaker, I thought I was used to dealing in big sums. This is really big! In fact, it would be adding 100 U.S. economies in the world market* (Nanotechnology: Societal Implications *2001:25*).

Kalil took Roco's imaginative future-making work and not only made it bigger but made it global. His choice to use Bill Joy, however, for this economic promises is odd for a coupl of reasons. First, Bill Joy is no economist. He was an engineer, a businessman, and a popular futurist. Kalil strategically ignored Bill Joy's lack of

[28] It is unclear whether nanotechnology ever met this goal or even came close. The issue is complicated by the fact that nothing like a discrete nanotechnology industry ever emerged. Nanotechnology is more of an "enabling" technology, meaning that it simply changes the production of existing products while not necessarily leading to new ones. According to interviewees, industry actors also learned not to advertise the use of nanotechnology in their products, fearing negative public response. Since nanotechnology is still largely unregulated, there is no way to tell whether a product contains nanoparticles. The Project on Emerging Technologies at the Woodrow Wilson Foundation ran an open database where users could enter products that claimed to contain nanotechnology, but later found that two-thirds of the entries made claims that were either false or unverifiable.

expertise and credibility regarding global economics when he deployed Joy's estimate. Second, Bill Joy was no supporter of nanotechnology. He wrote a popular article about the potential for nanotechnology to cause major social disruptions to the very fabric of society, essentially a nanotechnology-enabled post-human dystopia. Joy called for total relinquishment of nanotechnology, making him an odd choice for Kalil here.

Nonetheless, nanotechnology's advocates saw in it an almost unimaginable degree of promise. So much so, in fact, that the nano-future came to be seen as a foregone conclusion and any hindrance to that future as essentially robbing people of a future that was already in-hand. Several scientists criticized this hype as promising more than nanotechnology could reasonably deliver, especially in the usual decades'-long time frame from technological discovery to marketable consumer products (Berube 2006). In the period from 1999 to 2005, however, these voices were drowned out by those who saw nanotechnology as the key to prosperity.

If the nanotechnology revolution was a foregone conclusion, then the only real question left was who would benefit. American scientists and administrators of funding agencies argued that it had to be the U.S. Funding nanotechnology was framed as a way to stabilize American dominance in science and commercial technologies. Failing to fund nanotechnology amounted to dooming U.S. science and commercial technological industries. They discussed nanotechnology as a race against other countries who might win for themselves the spoils of the nano-revolution. The rhetoric around this race strongly paralleled that of the Cold War, treating the nano-revolution as the spoils in a zero-sum game (McCray 2013).

In this discourse, among scientists and adminsitrators at the NSF and Congress, the U.S. was framed in a perpetually fragile position. The U.S. was imagined as always leading the race to nanotechnology at that given moment, but also always about to lose its position. Scientists and industry leaders presented a picture in which Europe, China, and Japan were always just about to catch the U.S. From a 2005 hearing devoted to the standing of the U.S. in nanotechnology research, committee chairman Bob Inglis (R-SC)

> *The U.S. is currently ahead of the nanotechnology curve, but other nations continue to invest more and more time, energy, and money in their nanotechnology efforts. If we pause even to glance over our shoulders, we*

will see them on the horizon, several of whom are already on our heels and pushing to take the lead. (*Nanotechnology Where Do We Stand* 2005:14).

Inglis' mixed metaphor gets a bit complicated if you analyze it too closley, but it clearly takes on the qualities of a horror movie. America, the hero, is running from a horde of monsters that are so close on the hero's heels that there is barely time to even take stock of the chase. Matthew Nordan, Vice President of Lux Research, Inc. a private organization that assessed the value of emergning technology markets, expressed the nanotechnology revolution as a race with no clear end.

It is just like the Red Queen in Alice in Wonderland. It is not an issue of being in one place before someone else. It is always being one step ahead, being able to run faster. There is an evolutionary arms race to think about (29).

The race for nanotechnology supremacy became enmeshed with American anxieties about the state of it superiority in science, its scientific education, brain drain, and training America's foreign competition. Some were afraid that the American nanotechnology workforce was increasingly from China, India, Japan, and South Korea. Worse than training a foreign workforce that stayed in America to work was training one that went back home. As Matthew Nordan observed, the U.S. risked becoming a "drive-through educational institution for other countries' students" (29-30). The race was not only a concern for the stated goal of realizing nanotechnology's benefits. It was also a concern about the relative state of the U.S. standing in science, science education, and the native workforce. Nanotechnology came to signify what concerned scientists most about American science. For all of these reasons, it was imperative to scientists, funding agency administrators, and many policymakers in Congress to fund the NNI. It is also worth noting that this discourse was not peculiarly American. Brice Laruent describes how the organization Nano2Life in France was supposed to come up with a solution to the problem that "European nanotechnology research was lagging behind that of other developed countries, most notably the United States" in a "global nanorace" (Laurent 2017:5, Hullmann 2006).

Edward Hall, a scientist involved with nanotechnology prior to the formation of the NNI, summed up this story well.

> *You have a situation in the 1990s where the NSF were looking for a big idea to sell to Congess to boost their budget. And it may be because the NIH was getting a lot of money under Clinton and they [the NSF] was pretty much static. And so there was a little brain storming going on, and it was Mike [Roco] that came up with this idea of nanotechnology and sold the idea. And he did it very well. He sold it in a way where the emphasis was on job creation, economic growth, and along with this the idea that this is so new and cutting edge that if the U.S. isn't there, it would be left in the cold. So that was a very clear narrative that Mike was largely responsible for. And he was also smart enough not to just focus on the science, but he brought in some of those, sort of, broader ethical considerations as well into that dialogue* (Hall Interview 5/19/16).

To summarize this section, funding for the NNI was seen as an imperative for three reasons. First, it was a funding windfall for an ailing NSF. In part, because it had such a loose definition, nearly any scientific discipline could count as "nanotechnology." Therefore, nanotechnology could provide funding for the broadest array of work under the NSF. Second, nanotechnology promised to be the most transformational technology of the 21st century. The imagination around its promise reached truly hyperbolic levels. It was, according to its advocates, more revolutionary than the combined influences of some of the 20th century's most transformational technologies. As a form of political imaginary, it framed governance around nanotechnology by making it too valuable not to pursue. In other words, it discursively precluded anything like a moratorium to give time to discover answers to questions like why gold nano-particles are surprisingly toxic. Finally, nanotechnology got rhetorically and symbolically wrapped into discourse about the state of American science. Nanotechnology was framed as a race in a zero-sum game that borrowed heavily from the rhetoric and values of the Cold War. The tolerances, then, for obstacles to nanotechnology were almost non-existent. Anything could lose America the race and with it, all of the transformational benefits nanotechnology had to offer. For the NSF, it also risked losing its best hope for an expanded budget after years of struggling for even modest funding increases.

The Public Threat to the Nanotechnology Revolution

The enormous promise of the NNI brought with it the concomitant fear among scientists who might benefit from nanotechnology funding and administrators at science funding agencies, particularly the NSF, that all of nanotechnology's immense promise might not be realized. Nanotechnology's proponents had two major concerns in this regard. The first, was that the government simply would not fund the NNI. As we just saw, scientists and supporters from industries were able to frame nanotechnology in such a way so as to win big funding for nanotechnology, thereby defusing this threat in the short term. Second, they feared that a significant public backlash might severely impact, if not outright destroy, the fledgling initiative before it brought about the revolution it promised. This concern came from several directions at once.

As discussed earlier, the controversy over genetically modified foods, particularly in Europe, gave nanotechnology's advocates reason to worry. Workshops at the NSF and witnesses before Congress repeatedly mention the need to avoid the European backlash to GMOs, which dwarfed the GMO backlash in the U.S. Anti-GMO protesters had concerns about the environmental, health and safety issues with GMO foods. They and their concerns were disregarded as unscientific and irrational. What no one wanted to admit, however, was that in terms of a pure risk-benefit calculus, their concerns were completely rational, at least in terms of the risk society. The risks to consumers were at least somewhat unknown, which made them non-zero. The benefits, however, went not to consumers but to producers (Bennett, 2010). With a couple of exceptions such as "golden rice," a nutrient-rich rice strain made with food-insecure nations in mind, GMO foods were no cheaper, did not taste better, nor were they more nutritious than their non-GMO counterparts. The benefit of GMO foods were that they were easier and cheaper to produce. Scientists and representatives from the agricultural industries in Europe responded by trying to convince people that their concerns were irrational and anti-science. That strategy was a spectacular failure of which those in the fledgling NNI were well aware. Alysha Carter, a social scientist involved in public engagement for the NNI, recalled the signficiance of the GMO controversy for nanotechnology.

> *There was the whole GMO excitement that would've been a blip on the screen before nanotechnology became a national priority. And what you saw around GMOs was enormous public backlash that wound up being pretty expensive for corporate interests, and kind of blindsided apparently, in my mind, a lot of the technology developers and technology entrepreneurs and this kind of thing. So I think that you know there was just this kind of interest in preserving profitability and sort of that tiered flow of technology development. So there was this interest in looking at nanotechnology in this broader light, paying attention to public opinion around nanotechnology* (Carter Interview 7/19/16).

Nanotechnology's advocates cited other examples of failed commercial technologies in the vein of GMOs, particularly nuclear energy and stem cell research. Many considered nuclear energy a lost opportunity for clean energy as nuclear programs in Europe flourished while it largely disappeared in the U.S. (Jasanoff and Kim, 2009). Stem cell research experienced enormous public backlash and was ultimately defunded at the federal level in 2001. Nanotechnology's advocates feared that it would join the list of these scientific lost opportunities, what has been dubbed "undone science" (Frickel et al. 2010). Representative Ralph Hall (D-TX) put the three together in his statement before Congress.

> *We know too well that negative public perceptions about the safety of a technology can have serious consequences for its acceptance and for its use. This has been the case in such technologies as nuclear power, genetically modified foods, and stem cell therapies* (*The Societal Implications of Nanotechnology* 2003:12).

Representative Hall reflects a common framing for these problems: their public acceptance. Once the scientific process produces a technique or a product, the question is how to get publics to accept it. As we will see, while much of SEIN research and public engagement took seriously the implications of nanotechnology, many took fostering public acceptance of nanotechnology as their main mission. Yet, not all fears came from the history of other technoscientific fields.

Besides the cautionary tale of GM foods in Europe, three other imagined technoscientific crises for nanotechnology emerged that made its advocates worry about a possible public backlash. The first and most significant source of these visions came not from anyone opposed to nanotechnology but from Eric Drexler, nanotechnology's founder and its most vocal and prominent advocate. Drexler gave the world the first imagination of a nanotechnology apocalypse. In his 1986 book, *The Engines of Creation*, he described a scenario he called the gray goo. As an engineer, Drexler has always imagined nanotechnology to mean nano-sized machines, or nanobots as they came to be called. He argued that it would be inefficient to construct nanobots individually with macro-scale machines, like cars in a factory. Thus, Drexler imagined that building nanobots would require nano-sized assemblers to construct them. These nano-assemblers would work by breaking down surrounding material at the atomic scale to produce not only nanbots generally but more nano-assemblers. Nano-assemblers would have to be run by artificial intelligence since it would be impossible to manually control something that small.

In the gray goo scenario, a nano-assembler breaks with its programming controls and begins to turn all matter around it into more nano-assemblers. The faulty coding is reproduced and the problem grows exponentially, very much like an engineering version of cancer. In Drexler's vision, the world would be slowly digested into more and more nano-assemblers until the planet was turned into a "gray goo" of tiny nano-assemblers. The gray goo was a thought-experiment that Drexler intended to flag that nanotechnology had societal implications worth considering. *Engines of Creation* had positive impacts for nanotechnology. It put nanotechnoloy on the proverbial map, leading to university programs, helping win important public support for research, and inspiring a generation of scientists to study nanotechnology, including the man who would become Drexler's staunchest critic and arch-nemesis in the battle to define nanotechnology, the physicist Richard Smalley. For all this, though, it also saddled nanotechnology with a powerfully frightening scenario ripe for science fiction and the public imagination. It presented to nanotechnology's advocates a problem that had to be solved.

In 2000, as Mihail Roco, the future Director of the NNI, successfully petitioned President Clinton and Congress to develop the NNI, Richard Smalley threw down the proverbial gauntlet, openly challenging Drexler about the feasibility of nano-assemblers. We will cover only the basic details of the debate here as there is already a great deal of quality scholarship on that debate (Bennett and Sarewitz 2006, Kaplan and Radin 2011, McCray 2013, Selin 2007). The debate was essentially a contest to define nanotechnology as either a science of nano-scale machinery, Drexler's view, in which nano-scale materials played a crucial part or, in Smalley's view, as a nano-scale materials science in which nano-scale machines had no part. The explicit subject for this definitional contest was feasibility of nano-assemblers but it was implicitly about the possibility of the gray goo. If nano-assemblers were impossible, then so was the gray goo.

Their argument boiled down to whether or not it was possible for a nano-assembler to manipulate objects at the atomc scale, which is necessary if you want to assemble those atoms into materials and machines. Drexler thought that it was theoretically possible while Smalley mantained it was not. Smalley had two reasons. First, the nano-assemblers would necessarily have "hands" larger than atoms, making them too big to grab things. He called this the "fat fingers" problem. Second, the electrical charge of the nano-machine's hands would make it impossible for them to both pick up and drop an atom. It would do one or the other but not both. Smalley called this the "sticky fingers" problem. Drexler admitted that fat and sticky fingers were limitations, but ones worth exploring through scientific research, not reasons to give up on nano-assemblers at the outset. Smalley was performing crucial boundary work here. Whereas Drexler saw his vision as consistent with the power of science to do previously unimaginable things, Smalley framed Drexler's vision as mere science fiction. Furthermore, the motivation for this boundary work was primarily social rather than scientific. If not for the apocalyptic gray goo and its potential to capture the public imagination, it is likely that no nanoscientist would have much cared about the feasibility of nano-assemblers, at least before any real research on them had been proposed.

Sticky and fat fingers would have been questions to answer in the laboratory, not discursively in battles of the imagination. That a debate raged at all is a testament to nanoscientists' fear of the gray goo to capture the public imagination and the power of that imagination to negatively impact their nascent field.

Smalley won the debate that determined the future of nanotechnology. There was no scientific closure on the problem of nano-assemblers or fat and sticky fingers. Indeed, there could be no such closure in the absence of research. Rather, Smalley's arguments, based in physics, looked more scientific than Drexler's engineering-based arguments.[29] Towards the end of the debate, Smalley was still publishing his opinions on the impossibility of nano-assemblers in journals like *Nature* while Drexler was having to publish his opinions in his own organization's newsletter or his personal blog. In a protracted exercise of socially-motivated boundary work, the most important scientific threat to the emerging field of nanotechnology was defined away as science fiction. The debate also got rid of nanotechnology's most significant heretic. By the end, Drexler was no longer taken seriously as a scientist. He was no longer invited to speak at conferences or to witness before Congress.[30] He was effectively ousted from the field he had almost single-handedly founded and the origin-story of nanotechnology was re-imagined to give the founding role to the widely-loved and already deceased physcist Richard Feynmann (McCray 2013, Selin 2007).

As the Drexler-Smalley debate was wrapping up, nanotechnology was hit by another apocalyptic vision. This time it came from physicist and Sun Microsystems CEO William (Bill) Joy. Unlike the Drexler-Smalley debate, Joy's vision was published in the popular *Wired Magazine* with followup pieces in the *New York Times* and the *Washington Post*, among others. In his article, *Why The Future Doesn't Need Us* (2000), Joy argued that advancements in robotics, artificial intelligence, and nanotechnology would render humans obsolete. Humans would either be melded with these technologies, making them effectively no longer human, or would replaced by them. Ultimately, Joy advocated for the total relinquishment of nanotechnology. Recall

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[29] David Noble (1977) gives a history of engineering in the U.S. in which he shows how it came to be seen as less scientific than fields like chemistry and physics.

[30] His wife, Christine Peterson, continued to engage in these activities on behalf of the Foresight Institute, which she co-founded with Drexler. Importantly, she never mentioned either the gray goo or the issue of sticky and fat fingers.

earlier that Thomas Kalil cited Bill Joy when discussing the thousand-trillion dollar global nanotechnology market and the positive social disruptions this would portend. Kalil's use of Bill Joy's ideas were odd, given that Bill Joy feared rather than endorsed nanotechnology. Unlike Drexler's gray goo, Bill Joy's concerns began in popular rather than scientific discourse, and so could not be exorcised with a bit of determined scientific boundary work.

Roco feared Bill Joy's vision and the media response it had received. In a 2000 email exchange, Roco enlisted Richard Smalley, still engaged in his debate with Drexler, to help set boundaries on the legitimate concerns of nanotechnology. Roco asked Smalley speak about "The Technological and Cultural (educational) Consequences of Nanoscience and Nanotechnology" at an NSF workshop on nanotechnology's societal imlications. As Kaplan and Radin have written (2011:467):

> *Roco concluded his invitation with a hint of urgency, saying to Smalley, "I hope you could come because this issue [concern with the implications of nanotechnology] is becoming a determining one." Smalley's reply expressed sympathy with Roco's concerns, "Dear Mike, I hope all is well with you, and that Bill Joy has already hit his high water mark! Cheers, Rick."*

Bill Joy had not hit his high water mark. Three years later, in 2003, nanoscientists and administrators at the NSF were still fighting Bill Joy's relinquishment. And in some sense, the spectre of relinquishment would remain always in the background of nanotechnology whenever scientists became fearful of the public and the consequences of a public backlash.

The nanotechnology apocalypse had one more source, which seemed to frighten nanoscientists and NSF administrators even more than Drexler's gray goo and Joy's call for relinquishment. In 1997 Twentieth Century Fox had purchased the film rights to a then-unwritten book by Michael Crichton he called *Prey*. The book was a dramatic, science fiction treatment of the nanobot apocalypse. In it, nanobots break from their programmed safe-guards, much like Drexler's gray goo. Instead of digesting the planet, however, the nanobots attack humans in massive swarms. Twentieth Century Fox film executive Hutch Parker said, upon buying he rights, that “we are really fired up over this acquistition. Our instinct is to do this quickly” (Boehm, 1997). When the book was finally published in 2002, scientists panicked. As Vicki Colvin, a biochemist from Rice University and Director of its Center for Biological and Environemental Nanotechnology, testified to Congress at the 2003 Societal Implications hearing, “the public relations nightmare it [*Prey*] could spawn is just as frightening to me, a nanotechnology researcher, as nanobots might be to some lay people.” Crichton’s novel changed the issue of public backlash from a vague possibility into an impending certainty. Edward Hall, a scientist involved with the NNI from its beginning, recalled the impact of *Prey*.

> *When I joined, in early 2001, there was already a lot of internal discussion amongst the twenty or so agencies that formed the NNI about “how do we make sure this thing doesn’t go wrong?” And very much the feeling was: this was a massive commitment by the government, a massive investment. It would be an absolute disaster if there was an anti-nano movement amongst the public and amongst the NGOs. So the question was: “how do we prevent this from happening?” And a smaller question was: “Well if there are any health and environmental [implications] then actually we should do something about them anyway.”*
>
> *But the turning point was, I don’t know if you remember Michael Crichton’s book Prey. You have never seen people so panicked in the government. It so astounds me to think back to that. People were sitting around the table and saying “Oh my gosh, what are we going to do about this? Everybody is going to read this book, everybody is going to become scared of nanotechnology. We’re not going to be able to realize the benefits of this!” And that’s when people began thinking seriously about what they needed to do to shore up public trust, how they need to engage with publics* (Hall Interview 5/19/18).

To sum up, in the face of growing threats to nanotechnology research, nanoscientists and funding agency administrators, particularly at the NSF, were looking for a solution that would protect the National Nanotechnology Initiative. Although these actors had always expressed a commitment to SEIN research and public engagement, none was immediately funded. Only once scientists and administrators with the NSF, in particular, began to perceive the public as a threat to the NNI were the rhetorical commitments to a SEIN program involving social scientists and to upstream public engagement realized. The consequences of public perception on the initiative were not something that they could address through technocratic means – by researching appropriate possible risks, for instance, and giving the public the numbers (Fischhoff 1995, Porter 1996). Indeed, nanotechnology posed a level of uncertainty that meant that no such numbers existed. In the time it would take to get them, something like Crichton's *Prey* might capture the public imagination. Furthermore, the disciplinary work that excised the gray goo from nanotechnology discourse would not work with the public. That disciplinary work had been a credibility contest, in Gieryn's terms, between two of the most prominent scientists in nanotechnology. Such boundary work does not apply to the public imagination.

Acceptable Concerns and New Experts

The 21st Century Act secured funding for SEIN research and public engagement. Initially, the questions about what kinds of issues they would pursue and who would do the pursuing were open ones. The NNI underwent an explicit period of reorientation that went along with these debates in the early 2000s. Specifically, two types of boundaries were eventually remade: (1) between legitimate (scientifically credible) and illegitimate (scientifically non-credible) concerns and (2) between legitimate and illegitimate actors, particularly social scientists and publics. We will discuss each of these in turn.

The battle around ideas of a moratorium on nanotechnology, a battle of political imaginaries of the technology, illustrates the effort to discipline discourse around nanotechnology by defining acceptable concerns for science policy. Unlike the Human Genome Project and rDNA research, proposed moratoria on nanotechnology came from several directions. Recall that Bill Joy had called for the total relinquishment on nanotechnology – essentially a full moratorium and did so several years before the NNI got started. Additionally, President Clinton's speech hinted at a moratorium when he said, "some science should not be done." The moratorium gathered steam from another source as well. In 2002, the ETC Group, a science NGO based out of Canada, had publicly called for a global moratorium on nanotechnology in their publication *The Big Down*. They were dismissed by many nanoscientists, Congresspersons, and by many of the social scientists whom I interviewed. Allen Lewis, one of those social scientists, described them as nothing more than "a few people and a pretty good fax machine" (Lewis Interview 2/26/2016), they received a great deal of attention in popular media, at least for a complex scientific issue. Prince Charles, after reading *The Big Down*, met with ETC Group and eventually joined them in the call for a moratorium (Feder 2003). These moratoria differed from each other in important ways. Unlike Bill Joy, for instance, the ETC Group had not called for total relinquishment. In my interview, Patrick Mooney of ETC Group explained their position.

> *We weren't proposing a long moratorium. We were willing to discuss sort of a time-lined moratorium of, we thought it could literally be a matter of months, so that governments or industry could establish even a voluntary set of guidelines that would guarantee that they would be monitoring workers, lab workers especially, and establish standards for safety. What got us concerned was…we had folks from the South African Nanotechnology Initiative come to us and tell us that they were alarmed by nanoparticle safety. They showed us a video of how their students handled gold nanoparticles in the lab. They wear these, you know, space suits, and then told us that in France they wore those masks that you use in subways, like the Japanese [wear in] subways, which were useless, and that in Texas [Texas A&M], they were bare handed* (Mooney Interview 1/28/16)!

It is remarkable how much the ETC Group's response mirrors the Asilomar strategy. Like the scientists at Asilomar, the ETC Group proposed a temporary moratorium of nanotechnology in order to determine best practices around laboratory safety. Similarly, the ETC Group did not want to enlist publics or even social scientists. They were not concerned about the larger social implications. Theirs were technocratic concerns about health and safety. That this was, by this time, so unthinkable to scientists, shows how much ideas about acceptable science policy had changed since the rDNA research debates. Although the lack of existing safety protocols worried scientists and funding agencies as well, they were never going to support such a moratorium. Yet the numerous calls for a moratorium, already injected into popular discourse by Bill Joy's articles, made it impossible for those at the NNI to repeat what James Watson and the first ELSI working group did for the Human Genome Project, simply taking the possibility of a moratorium off the proverbial table before the initiative started. Instead, nanotechnology advocates had to argue against it actively. In so doing, they were setting boundaries between legitimate and illegitimate concerns.

Since they could not discard the idea of moratorium, they had to define concerns in a way that made a moratorium unnecessary or even ineffective for protecting public health and safety. The first effort to define away dangerous concerns was Smalley's arguments in the Drexler-Smalley debate. Scientists were not overly frightened by concerns like that of the ETC Group. Most seemed confident that health and safety concerns, including laboratory safety, would be addressed once the initiative and its funding had been secured. Rather, they feared the public's fear of runaway nano-assemblers and the moratorium to which that might lead.[31] Thus, the Drexler-Smalley debate may have hinged on a narrow technical issue, but it represented the most important issue in the question of a moratorium. To be clear, despite his concerns, Drexler never wanted any kind of moratorium and his argument against one was brought to Congress in the 2003 *Societal Implications of Nanotechnology* hearing even in his notable absence. Christine Peterson, his wife at the time and the co-founder of their technology NGO, the Foresight Institute, argued from his book, *Engines of Creation*, that:

> *Individuals and organizations with legitimate concerns regarding advanced nanotechnology have suggested delays in development, even moratoria*

[31] In his interview, Patrick Mooney distanced their moratorium from of the nanobot scare. He said "Our concern is…the social and economic implications of a new technology that can be disruptive and damaging to marginalized peoples. And secondarily...it's sort of the health and environmental considerations." He then described *Prey* as a "silly thing" and a "terrible book" that was "all scare about nanorobots coming together to make shapes [that] do weird things."

or bans. While these reactions are understandable, this approach was examined over a decade ago [in Engines of Creation] and rejected as infeasible (*The Societal Implications of Nanotechnology* 2003:66).

Peterson appealed to diplomatic pragmatism, arguing that a moratorium did not address the key questions and anyway, was unenforceable. Philip J. Bond, the Undersecretary for Technology, U.S. Department of Commerce, stated the point more bluntly in a 2003 workshop about nanotechnology's societal implications.[32] Under the sub-heading "First Message: Nanotechnology Is Coming and It Won't Be Stopped" (Roco and Bainridge 2005:17-21) he stated:

Some around the world are calling for a slowdown or even an outright moratorium on nanotechnology research and development. To those calling for a slowdown or a halt on nanotechnology… I say instead: Prepare for the inevitability of a world blessed with nanotechnology and nano-enabled products and services. … These are forces that cannot be held back.

One of the elements of this technocratic approach was that the development of technology was inevitable. Again, this contrasts with the technocratic approach for rDNA in which the questions answers were technical ones, but where the technology itself was not considered inevitable. Bond here frames nanotechnology development as a "force of nature," and therefore outside the control of human agency. Interestingly, in Beck's view of the risk society, forces of nature are also without blame for the risks they pose (1992). This frame also contrasts with the concerns about a public backlash around nanotechnology. But if nanotechnology development is a force of nature that cannot be stopped then why was public opinion so frightening? It should have been unable to impact it if it was a force of nature. This goes to show the disciplining work Bond was engaging in and which was reflected elsewhere. Nanotechnology was fragile when it came to securing both funding and public support. It was invulnerable, however, when it came to the question of moratoria and relinquishment.

[32] Nanotechnology: Societal Implications – Maximizing Benefits for Humanity. December 2-3, 2003

Further, into his statement, however, Bond employed a softer tactic, one more often used by scientists and those from funding agencies fighting against a moratorium. Under the sub-heading “We must identify legitimate ethical and societal issues and address them as soon as possible” he proposes a division between legitimate and illegitimate concerns.

> *The first thing we need to do is to sort legitimate concerns from imaginary ones, those that are based on science from those based in science fiction. Then we must debunk the latter and devote time, attention, and resources to seriously address the former* (Roco and Bainridge 2005:17-21).

Concerns such as Bill Joy’s post-human dystopia, Drexler’s ambivalent mix of techno-optimism and apocalyptic gray goo, and of course Crichton’s story in *Prey* were discarded by agency administrators like Roco as mere science fictions. Science fiction had an odd double-existence in the discourse around the NNI. Scientists and administrators, like Roco above, often employed science fiction to help set the boundaries between legitimate and illegitimate technical concerns for nanotechnology. They used it as a discursive resource to set the boundaries on acceptable science policy. Yet they also employed science fiction as a resource of imagination, to instill wonder at the possibilities of nanotechnology and thereby justify its ongoing funding. When asked, “What are the principle justifications for such an initiative,” for instance, Dr. Eugene Wong, the Assistant Director of the Engineering Directorate for the NSF, answered of nanotechnology that “Science fiction has become science reality. The potential to transform nearly every aspect of human existence is almost without parallel” (*Nanotechnology: State of the Science*, 1999). Science fiction is a particularly valuable resource for scientists because they alone claim the authority to determine what is science fiction, with a reasonable chance of one day becoming science fact, against what is and will always remain purely science fiction. The discourse of science fiction gave scientists and funding administrators a tool both for future-making and for discarding obstacles to that future-making. Drexler found himself on the wrong side of this definitional battle. Smalley effectively framed him as one who could not discern science fiction from science fact. The boundary work distinguishing science from science fiction created an internal sense of coherence for the emerging field of nanotechnology. The discourse of science fiction was not always applied consistently. Bill Joy's prediction of a trillion-dollar global nanotechnology economy, repeated by deputy director for the White House Office of Science and Technology Policy Thomas Kalil, would be considered science fiction by any serious-minded economist, if only for the uncertainty involved in a nascent scientific field. In another example, more directly related to the question of scientific facts, NSF Director Rita Colwell told a Congressional budget committee that:

> *Nanoscale science and engineering will allow the development of a machine smaller than the head of a pin that could be placed in a person's bloodstream to monitor the health of the heart and blood vessels, thereby obviating strokes and heart attacks* (*Proposed R&D Budget for Fiscal Year 2002* 2001:128).

Director Colwell trumpeted the wonder of medical nanobots in 2001, at the same time that Smalley was trouncing Drexler over the impossibility of such nanobots. To be fair, a machine smaller than the head of a pin does not necessarily qualify as a nano-sized machine. Still, Smalley and his supporters were waging a war against nano-machines as a whole. When Drexler had tried to use viruses to argue about the possibility of nano-machines, which would not run into Smalley's fat and sticky fingers problem and which were essentially the same thing that Director Colwell promised above, he had been shot down. As an emerging field, nanotechnology was able to benefit from the positive imagination of nanobots for funding purposes. Nanoscientists then discarded the idea as epistemically bereft to avoid its implications for regulatory purposes. When it came to nanobots, nanoscientists and NSF administrators got to eat their cake and have it too.

Bond seemed to recognize the limits of a purely technocratic approach and the fact that something like a civic epistemology had consequences for nanotechnology that necessitated a more concerted effort in public reasoning than simply discounting science fictional concerns. Sorting the real from the imaginary risks, as Bond described above, was insufficient to get rid of the public threat to the emerging field of nanotechnology. If Drexler's gray goo, Crichton's *Prey*, and Joy's post-human dystopia were mere science fiction, then why would scientists have had to do so much work to discount them in the first place? As Mihail Roco and William Bainbridge seemed to understand, those scenarios had power quite apart from their feasibility. They had the power to capture the public imagination. This was something that the lawyer Alex Capron had told the bioscientists assembled at the Asilomar conference, seemingly to their surprise. Awareness about the risk of public perception had increase since that time but was nothing close to universal among nanoscientists. As Matthew Nordan, VP of Lux Research, warned policymakers in a 2005 Congressional hearing about America's standing in nanotechnology:

> *The first cut that you have to make is between real risks and perceptual risks, which are equally important. So, on one hand, there are real risks of manufactured nanoparticles, nanotubes, metal oxide nanoparticles, fullerenes, dendrimers.*
>
> *On the other hand, there are perceptual threats, which are often written off by participants in the field of nanotechnology as not terribly important. But if you look at the genetically modified food experience in Western Europe, you can see that the belief that there might be a threat, even when none actually exists, can choke commercialization just as real risks do. In fact, in many cases, those threats can appear earlier, and have a broader magnitude of impact, than real risks* (*Nanotechnology: Where Does the U.S. Stand?* 2005).

The experience with GMOs in Western Europe set the stage for nanoscientists, funding agency administrators, and those with commercial interest in nanotechnology to heed the power of public perception. Crichton's *Prey*, Drexler's gray goo, and Joy's call for a permanent moratorium gave teeth to the fear of public perception. For purposes of this dissertation, this marks the first big difference between the NNI and its programs in SEIN research and public engagement on the one hand and the Asilomar conference and the Human Genome Project's ELSI research on the other. The latter two focused on what Nordham calls the real risks. Even the ELSI program, which took into account ethical concerns instead of focusing solely on issues of health and safety, focused on what they thought were real risks. The NNI, responding to lessons learned in Western Europe about perceptual risks, came to focus on this new category of risks.

Although administrators like Roco and Bainbridge were aware of perceptual risks, nanoscientists still often discounted them. Nordham above makes that point when issuing his warning about perception. Chadwick Hall, a social scientist involved in public engagement in nanotechnology whom I interviewed for this dissertation, said that nanoscientists never seemed to appreciate the perception problem, especially once their fears of *Prey*, the gray goo, and Bill Joy's moratorium had subsided.

> *The perception problem is a ticking time bomb. There's a lot at stake. If something [like a scandal] gets exposed about nano, and let's say it has to do with nano-silver particles in your plastic baggies for lunch, and environmental groups or advocates take off with this, then it's going to slow down all the stuff that can save peoples' lives, nano-sized medicine for cancer and all of that. That's a serious reason to care about the vulnerability [of perception] to the whole program. And I just wish that the recognition of that was, well, a little bit deeper than it seemed to be* (Hall Interview 2/29/16).

They could not deal with the risks from public perception in the same way as risks associated with health and safety, or real risks as they called them. Just as the Human Genome Project's ELSI program had expanded the scope of relevant expertise in the initiative to include social scientists and hybrid-scientists with expertise in ethical issues and social implications, nanotechnology had to expand relevant expertise to include that on the public and its collective mind. Some, like Philip J. Bond, again the Undersecretary for Technology, U.S. Department of Commerce, argued that scientists possessed the relevant expertise to connect with the public and create trust.

> *Scientists and engineers are in the best position to contribute to sound policy development, addressing legitimate concerns and allaying irrational*

public fear. Scientists and engineers alone have the scientific and technical knowledge necessary to sort the wheat from the chaff. In addition, while not historically great communicators, scientists and engineers have unique credibility with the public in speaking to these issues (*Nanotechnology: Societal Implications – Maximizing Benefits for Humanity* 2005:21)

Bond was not alone in this, but the idea that scientists would do this work never went far among funding agency administrators. Even Bond admitted above that scientists were "not historically great communicators." One wonders why he thought it would be different with nanotechnology. Instead, Mihail Roco began arguing for an expanded role for social scientists to serve this function. Social scientists were not a natural fit. Although they had an official, if carefully managed role in the Human Genome Project's ELSI program, some scientists were still bitter that funding was spent on social science that could have gone to biological science (Andrews 1999, Cook-Deegan 1994). Others were upset that the ELSI program had not led to any permanent solutions to bioethical issues (Beckwith 2002). Thus, Roco had to make the case for enrolling social scientists into the nanotechnology program. In the opening statement in a report for a workshop on the societal implications of nanotechnology he presented his reasoning.

Sober, technically competent [social science] research on the interactions between nanotechnology and society will help mute speculative hype and dispel some of the unfounded fears that sometimes accompany dramatic advances in scientific understanding…

The inclusion of social scientists and humanistic scholars … is an important step for the NNI. As scientists in their own right … they are professionally trained representatives of the public interest and capable of functioning as communicators between nanotechnologists and the public or government officials. Their input may help maximize the societal benefits of the technology while reducing the possibility of debilitating

> *public controversies* (*Societal Implications of Nanoscience and Nanotechnology* 2001:iii).

Roco extends the concept of acceptable science policy to make room for social scientists for a few reasons. First, he establishes social scientists as another kind of expert. Social scientists are experts on the public. Second, they are boundary-spanning actors. They can represent the public back to scientists and scientists to the public. Finally, social scientists are actually scientists, though of a different type. Social scientists have their own credibility mechanisms that give them legitimacy just as natural scientists do. Roco also makes it clear why they need social scientists: to protect the NNI against the threat of public perceptions of nanotechnology so that the immense promise of nanotechnology could be realized. Roco frames social scientists as crucial allies in the development of nanotechnology.

The fact that there was not a larger discussion or contentious credibility contest needed to open a space for social scientists is a testament to Roco's power at that time. Roco exercised an unchallenged influence on the early development of the NNI, consistent with his commonly used moniker as the "Czar of Nanotechnology." Edward Hall, an interviewee cited earlier, described it this way.

> *There was a transition point, which must have been around 2004, where Mike was internally ousted from his leadership role. Before this, though, Mike was the head of the NNI. Everything that was done was basically down to his personal visions. If he thought something was important, it was supported. If it wasn't important to him, it wasn't supported* (Hall Interview 5/19/16).

Mihail Roco wanted a SEIN program led by social scientists and so he made it happen. This did not get rid of the animosity towards the social scientists or the bitter feelings towards the Human Genome Project's ELSI program. Dana Rohrbacher (R-CA) said of giving a role to social scientists, "It sounds like to me, you are putting all of the sociology and literature majors in charge of defining the goals of the engineering and science majors" (*The Societal Implications of Nanotechnology* 2003:56). Even Langdon

Winner, a political scientist from the Rensselaer Polytechnic Institute and the only social scientist invited to testify before Congress about the development of the NNI, warned against letting the SEIN program become the “Nanoethicist Full Employment Act” (76). This was a criticism of the ELSI program and its focus on bioethics research which produced a lot of research papers but, as Winner went on to explain, managed to “exclude the participation of those who are the ultimate stakeholders, the general public” (76-81).[33] Social scientists would have to continue to prove both their own relevance to the NNI and the publics whom they studied and engaged.

Conclusion

To understand why the National Nanotechnology Initiative funded a program in the Societal and Ethical Implications of Nanotechnology and eventually upstream public engagement, we must see nanotechnology as the topic of contested political imaginaries and public reasoning. The imaginary around nanotechnology that its advocates promoted was one of enormous promise for American science, the economy, and society as a whole. These imaginaries set out a zero-sum game of future-making in which, to prosper as a society, it was imperative to fund and develop nanotechnology. Failing to develop nanotechnology, in this imaginary, was tantamount to giving up not only a particular technoscientific field, but economic development, America’s global standing in science, and the the only future worth having. These imaginaries were backed up with some facts and figures, like Joy’s estimate of a trillion dollar nanotechnology economy, that were dubious at best. If technocratic governance involves, as Theodore Porter (996) has said, the performance of numbers to foster trust in particular modes of governance then many of the technocratic displays in the early years of nanotechnology resemble something closer to pure theater (Ezrahi 2012). Ezrahi discusses this as technocratic displays of public witnessing that have lost any basis in technical fact, retaining only the performative elements. Other aspects of the discourse for this imaginary of nanotechnology employed shifting referents. Science fiction was used to both encourage policymakers to wonder at the marvels that

[33] Winner never explicitly criticized bioethics in his published works as far as I could find, but Susan Kelly used Winner’s perspective to criticize the faith that bioethics, given its enrollment in the biomedical enterprise, could ever seriously challenge biomedicine (2006).

nanotechnology promised and later used to discipline the discourse around nanotechnology by ridding it of concerns that nanotechnology's supporters found threatening. Neither Drexler's gray goo nor Crichton's swarms of nanobots were more or less scientifically accurate at that time than NSF Director Rita Colwell's promises about nanobots swimming around in people's bodies fighting cancer. The only difference came in how these different scenarios might affect the nascent National Nanotechnology Initiative.

Nanotechnology's advocates seemed to realize that something like a civic epistemology would emerge and come to affect this emerging technoscience. This marks an important way in which the National Nanotechnology Initiative and its SEIN program differs from the ELSI program of the Human Genome Project and the Asilomar Conference for rDNA research. Administrators like the NSF's Mihail Roco and industry representatives like Phillip J. Bond were aware that the biggest threat to nanotechnology came not from foreign competitors but from fearful citizens. Their response was to engage in practices of public reasoning to discipline the discourse around nanotechnology to exclude competing imaginaries of nanotechnology that framed it in apocalpytic terms. Drexler's and Crichton's visions were framed as science fiction while a protracted discussion centered on how to diminish Joy's calls for the relinquishment of nanotechnology. Scientists, policymakers, and science agency administrators admitted that nanotechnology posed important societal concerns. They engaged in important rhetorical work to define the scope of those concerns and determine their relevance. This is at odds with the realization that public perceptions, what I have been calling civic epistemology from Jasanoff, might impact the initiative. If a large enough public is concerned about gray goo, then the feasibility of that scenario hardly matters. Still, they felt that they had to not only engage potential societal concerns but define them as well.

In the end, what was considered acceptable science policy had expanded to include SEIN research and public engagement, but only once it became clear that public perceptions posed a threat to the National Nanotechnology Initiative. These efforts were not funded when it simply seemed like the right thing to do, despite a lot of discourse among policymakers in Congress and administrators and scientists at the relevant science agencies to that effect. Like the other cases for this research, this expansion was met with a simultaneous effort to discipline the new discourse around societal implications, and to thereby contain the degree of expansion that was taking place. In the next chapter we will see, in greater detail, how this discipline continued with the actualization of upstream public engagement for the National Nanotechnology Initiative.

CHAPTER IV

Upstream Public Engagement in the NNI

Introduction

This final substantive chapter builds upon the prior two chapters by showing what happened once public engagement became a core component of the National Nanotechnology Initiative. I focus particularly on public engagement because it was the biggest departure from technocratic modes of science policy and therefore the most significant expansion in what was considered acceptable science policy for the NNI. This chapter follows five public engagement events for nanotechnology.[34] Full descriptions of the events can be found in Appendix A. Although this chapter will turn to public documents at times, the bulk of the data for this chapter come from documents related to the five deliberative public engagement events and 33 interviews with individuals connected to these events. Those interviews were mostly with the social scientists who organized and conducted these engagement events, but also include one toxicologist, whom we have already seen in Chapter III, two former NSF program directors, and three representatives from civil society organizations involved in the discourse about nanotechnology's societal implications.

This chapter explores, in practice, how public engagement was both an expansion and disciplining of what was considered acceptable science policy. The last chapter covered the public reasoning of science agencies, particularly the NSF, and in Congress around nanotechnology and its governance. This chapter follows the social scientists who did this work, to understand what it was possible or encouraged for them

[34] In the interest of giving my subject's the anonymity I promised them, the names of these events, the universities, and the names of all of the scholars whom I interviewed are pseudonyms. I only use real names for people I did not interview or in cases where the interviewee explicitly said they did not want a pseudonym and where using their name would not identify anyone else in this dissertation.

to have publics deliberate and what seemed to remain off the proverbial table. We will also look at the outcomes of public engagement as this presents a final opportunity to discipline discourse around societal implications. The question of what agency administrators and policymakers did with the outputs of public engagement, particularly reports by or about the citizens who took the time to discuss something as unfamiliar as nanotechnology, reveals much about the degree to which public engagement was a substantive expansion of acceptable science policy or the performance of such. The answer is that it depends on how you define success for public engagement. In some ways, it was a vast substantive expansion in what was considered acceptable science policy but, in some ways, it was conditioned to limit the scope and impact of that expansion.

To begin, this chapter will recount the formation of the Science and Society Centers for nanotechnology to understand how the discourse around nanotechnology was bureaucratically disciplined at the outset to be primarily academically driven. We will then turn to the engagement events themselves to understand what the NSF wanted them to accomplish and what the social scientists themselves wanted them to accomplish. Although not quite in tension, the goals of the NSF and social scientists did not fully overlap either. Finally, I will turn to the outcomes of public engagement for the reasons just described. As a kind of epilogue, I give a brief account of how the National Nanotechnology Initiative seemed to drop its focus on upstream public engagement.

Enrolling Scholars

The way that upstream public engagement and SEIN research was implemented for the NNI helped determine the shape of the discourse to follow. Like the Human Genome Project's ELSI working group, this work for the NNI went to a select group of scholars. Most of the NSF funding was split between two Society and Science Centers. These were at universities that I am referring to here as Institution A and Institution B.[35] In some ways, this marked an innovation in the NSF's approach to SEIN research and upstream public engagement, since the NSF did not retain any direct control over what scholars did at these two universities. Additionally, what these universities proposed

[35] To protect the anonymity of the scholars whom I interviewed, I am referring to these institutions by these letters.

constituted a major expansion in acceptable science policy. In another sense, though, by enrolling well-established scholars who were, recalling the earlier reluctance to fund SEIN research from Chapter III, “meritorious enough to warrant funding,” administrators at the NSF could be reasonably sure that the work of these two universities would fall within the parameters of acceptable discourse.

Almost immediately after funding for SEIN research was secured, the NSF sent out a request for proposals to fund two Society and Science Centers. The effect was to narrow the set of actors who would conduct SEIN research and public engagement. Though Margaret White, one of the former NSF program directors whom I interviewed, said that their reasoning was that they wanted to “start with a smaller effort” and get larger later on (White Interview 9/13/17). The Science and Society Center model had come from an existing administrative structure in the NNI for the Nanoscale Science and Engineering Centers (NSEC). For the most part, he NSECs were tasked with doing laboratory research on nanoscale innovations, but this did involve a measure of attention to societal issues and implications. Societal implications were, at best however, an adjunct activity to the core mission of laboratory research at the NSECs. Furthermore, as the National Academy of Science Committee Chair Samuel Stupp had expressed to the NSF and in his first triennial review of the NNI, the societal implications work at the NSECs tended to focus only on laboratory safety instead of larger societal issues with implications outside of the lab. Lowell Evans, the other former NSF program director whom I interviewed for this research, said that there were many reasons why the NSF opted for a centralized model, but that “the idea of creating a center was probably just an easy way of addressing societal and ethical and legal dimensions…it was essentially the easiest way to go” (Evans Interview 8/17/2017). He added that the SSC model allowed for greater bureaucratic access and oversight than the de-centralized grant programs that normally went to fund research and which briefly preceded the SSC model for the National Nanotechnology Initiative. The Science and Society Center model allowed the NSF greater oversight and to see what it had purchased by funding upstream public engagement.

Although the NSF funding announcement referred to a single Science and Society Center, it wound up funding two such centers. However, the NSF did not double the money it had set aside for this work, so it could not fully fund both proposals. Instead, it split the pot of money between them. This forced both centers to scale back their efforts significantly. The difference between $15 million and $7 million over five years meant that they had to scrap what had been core areas of each of their proposals. The program directors I interviewed said that the NSF was aware of the burden this created for the two centers. Why, then, did the NSF fund two half-centers instead of one fully? As former NSF program director Lowell Evans explains, Institution B was more academically-oriented and thus safer while Institution A was more innovative but riskier.

> *At [Institution B] there was an effort focused primarily on studying public engagement, studying the public, the public's perspective on nanotechnology, studying issues of risk, studying media and these sorts of things. It's very much a scientific orientation.*
>
> *But at [Institution A], you know, the culture of the place was different, in part because … they saw it as a more holistic approach to science and society where, you know, you are engaging [the public]. You're not just doing research, but you're also getting the public engaged. There are expectations at [Institution A] that if you're working on something that is publicly relevant, well then you should be, you know, holding meetings in the public, where the public is engaged in your work. And yes, it was research-focused, but also it was focused on kind of developing more on, you know reflexive governance. They were experimenting with different ways of doing that and conducting research on that, but part of their agenda was actually just doing it* (Evans Interview 8/17/17).

In his reading, Institution A as a whole had a culture of public engagement that seemed to transcend the instrumental needs of its constituent scholars. In his view, both were studying public engagement while also doing it, but Institution B was focused slightly more on the former while Institution A was focused slightly more on the latter. Margaret White, another former NSF program director, described each center as proposing innovative research, but did frame Institution B as more focused on studying risk and public perception with Institution A focused more on public engagement.

Evans later added that funding two centers allowed the NSF to hedge its bets in case one of the centers should have a "major crisis" and fail. It seemed that the concerns about failure pertained to Institution A's center.

> *To some extent early on, [Institution A's experiments] looked weird to people at NSF. Some questioned 'what the hell are they doing,' you know, '[with] these sorts of things?' And [there was] a potential embarrassment there, so to speak, 'oh gosh,' you know, 'NSF is funding this kind of weird stuff.' And then over-time, it made sense and a lot of their work was highly successful, you know, it just took [time]. Some of that stuff looked weird and raised some questions, but you know they just went about it in a different way. And so, that might've been one of the reasons why a decision was made to fund two centers as opposed to one* (Evans Interview 8/7/17).

Institution A had lots of interesting "weird stuff." One activity was a strategy card game. Players would receive a set of cards representing technological innovations based in nanotechnology. Players were also given a country card with particular needs that could be met by different technological innovations. Participants in the game would try to trade technological innovations in order to come up with the set of technologies that best served their society's needs. They also had speculative scenario development, in which participants would explore fictional but scientifically credible scenarios related to nanotechnology. These are particular activities, but the entire orientation of Institution A's center might have looked weird to decision-makers at the NSF. Institution A proposed nothing less than the potential to evaluate emerging technologies in real-time and thereby avoid the problem that science policy always seems to come too late to make a difference in emerging technologies (Institution A NSF Proposal 2005:15).

Ultimately, though, the NSF proved willing to embrace the "weird stuff" going on at Institution A. Part of the reason is that Institution A's proposal framed the center's role and its work explicitly in terms of boundary work. The Broader Impacts section of the Institution A proposal began by stating that "Institution A's Science and Society Center is designed self-consciously as a boundary organization…with a structure that will enable it to manage the oftentimes demanding tensions between inquiry and outreach, public and private, NSE [Nanoscale Science and Engineering] and society" (Institution A NSF Proposal 2005:15). The Institution A proposal framed itself in terms that reflect Mihail Roco's earlier justification for including social scientists in the NNI in the first place. Recall, from the last chapter, that NNI Director Mihail Roco framed the value of social scientists in terms of their being "professionally trained representatives of the public interest and capable of functioning as communicators between nanotechnologists and the public or government officials" (*Societal Implications of Nanoscience and Nanotechnology* 2001:iii). Institution A's proposal promised to realize exactly this vision of the social sciences and the promise to science agencies to serve this boundary spanning role. Margaret White agreed.

> *What [Elijah Anderson, Director of Institution A's SSC] was doing was saying 'okay, we see our role here as being able to talk to these different groups, maybe using a kind of boundary object,' that is something that has a vague enough meaning or an inclusive enough meaning that different groups are willing to take it on as something in which they have an interest. Then he was saying that what they're doing is to operate with all of those languages and help translate the language from one group to another's group so some of these terms can be better understood. I think people, particularly people who were evaluating these proposals, understood this a way in which to approach the need for better communication* (White Interview 9/13/17).

It was no idle promise for Institution A to serve this boundary-spanning role. Institution A was, and remains, home to some of the most prominent scholars in science policy and boundary work, especially the primary investigator of the Science and Society Center proposal. The organization of Institution A's Center itself evidenced its potential to span boundaries. The Institution A Center would not be the product of a single university but rather a network of scholars at eight universities across the country. The SSC proposal from Institution A drew on a wide range of experts in the social and physical sciences at these universities. It appealed to the growing value of interdisciplinary collaborations at the NSF. This focus on interdisciplinarity was already evident in another organizational model in the National Nanotechnology Initiative, the various Nanoscale Interdisciplinary Research Teams (NIRT). The NIRT groups were "small collaborative groups of three or more investigators…to address education and research goals where a synergistic blend of expertise is needed" (Nanoscale Science and Engineering Research Proposal Solicitation 2002:9). Perhaps because Institution A framed itself as just what the NSF had wanted or because of the caliber of scholars involved in the initiative, the NSF was willing to gamble on the "weird stuff" that Institution A had proposed.

The effort that wound up centralizing public engagement, then, had several effects for the future of this work. Most obviously, it meant that the NSF would no longer have to worry about reviewing proposals. This had at least two benefits to the NSF. First, as long as the two SSCs were doing their job, the NSF only had to check in with them annually, which it did. Second, it also meant that they no longer had to worry about funding projects at centers "meritorious enough to warrant funding." Institution B and Institution A had established their credibility with the NSF.

Another effect of this mode of organization was that non-scholars, even professionals who engaged publics for a living, were not enrolled to conduct any of the public engagement. In 2006, the NSF had invited several professional public engagement practitioners to a workshop on the societal implications of nanotechnology. Kristyn Lee was one such professional whom I interviewed. She said that, having been invited to participate in the workshop, that she thought there was at least a possibility that her public engagement non-profit might be hired to do some of the public engagement work (Lee Interview 2/2/2016). She was disappointed to discover that that was never the NSF's intention. She also saw two potential problems with a university-based program. First, she thought that a university-based program would prove less experimental. While she had high praise for the academics at the two SSCs for their commitment to public engagement, she thought that, as scholars, they would ultimately be beholden to the norms of their discipline and the need to forward their academic careers. It is not that the work of the SSC would not experiment – indeed that was its whole motivation – but that the experiments would be social experiments first – in sociology or anthropology for example – and experiments in better engagement practices second. Recall that Evans also identified this as an issue with Institution B although oddly not with Institution A. Second, she predicted that a program based out of universities and led by scholars would run into trouble once they tried to use the outcomes of public engagement to influence science agencies or policymakers.

> *I served as chief of staff to the Governor of Ohio for the last four years of the '80s. And then I spent some time working in the Clinton/Gore White House in Washington, so I actually have a pretty deep understanding of the* real politick *world that you are connecting the citizen engagement to. In my experience, people who are dominantly or completely university-based, and have not had some life experience that puts them in the rough and tumble of public policy development, are pretty naïve about politics.* (Krysten Interview 2/2/216).

When the 21st Century Nano R&D Act had earmarked SEIN funding, however, the funding umbrella for public engagement was described as a "university-based" set of activities (§(6)(a)(2)(B)). This shaped upstream public engagement and its outcomes in key ways. Most importantly, upstream public engagement, like SEIN research as a whole, became primarily a research endeavor. It was led by primary investigators who had to report their findings back to a funding agency that assessed the value of their work in terms of its value as research. This also meant that they could not violate the key values of funders by engaging people or ideas that the latter found particularly threatening. This is one of the consequences of being enrolled into research (Callon 1986, Callon et al. 1986, Cozzens 1990). Others have called this process "ELSI-ification," referring to the ways that scholars lose their critical edge once enrolled into a major federally funded initiative (Lopez and Lunau 2002, Rabinow and Bennett 2009). Engagement practitioners were beholden to their own discipline as well. Public engagement was to be a vehicle for producing publications and forwarding academic careers. This should not be read as a criticism. Rather, the point is merely that the particular arrangement of goals and commitments by the network of actors enrolled to do this work shaped what it was possible to discuss for purposes of upstream public engagement.

The Events

This research follows five deliberative public engagement events either funded directly by the NNI in the form of an NSF grant, or indirectly through a university-based nanotechnology research center funded by the NSF. The five events are: the Midwestern Nanotechnology Deliberation, the National Emerging Technologies Conference, the Eastern Environmental Risk Forum, the Western Citizen Risk Forum, the Southern Technology Conference, and the Southwest Nano-Dialogue. Two of these did not have a direct affiliation with the SSCs. The Southwest Nano-Dialogue pre-dated SEIN funding for public engagement. It was funded through the host university but, as the organizer explained, was important to the scientists at the university, in part, because they wanted to satisfy their "broader implications" requirements of their NSF grant funding. The Eastern Environmental Risk Forum was funded through one of the NSF's Nanoscale Interdisciplinary Research Team (NIRT) grants, the model on which the Science and Society Center model was based. The Midwestern Nanotechnology Deliberation was done at a university that was part of Institution A's Science and Society Center network and the National Emerging Technologies Conference was a large-scale deliberative exercise that involved six of the eight universities comprising Institution A's network. Finally, the Western Citizen Risk Forum was the major deliberative component of Institution B's Science and Society Center.

Although there are fine-grain differences in the deliberative formats of these events, all share some essential elements. In this model, deliberations begin with some informational session to provide a framework for the discussion and to ensure that all deliberators operate with a similar foundation of knowledge. Sometimes this is sent out to participants ahead of the event, in the form of reading materials, videos, and surveys, and sometimes the event begins with lectures that provide this foundation. Deliberations usually follow afterward. Some are highly structured, with set questions that participants

Table IV.1 Nanotechnology Deliberative Public Engagement Events

Event Title	Years	Types of Engagement	Stated Goals
Southwest Nano-Dialogue	2002	One deliberative dialogue with over 200 people.	Creating “excitement and wonder” about nanotechnology.
Midwestern Nanotechnology Deliberation	2005	Thirteen people met three consecutive Sundays.	“Allow area citizens to consider the promises and perils of the many possible future nanotechnologies before they reach the market.”
Eastern Environmental Risk Forum	2007-2011 (mostly 2009)	Delphi method discussions with local civil society groups.	“(1) Identify factors in public risk perception of nanotechnology. (2) Test effects of deliberative method on public risk perception.”
National Emerging Technologies Conference	2008	Danish Consensus Conference, both online and in-person with six universities.	“To cultivate our collective ability to govern the implications of our technological ingenuity” (Hamlett, Cobb and Guston, 2008: p. 15)
Western Citizen Risk Forum	2009	Six four-hour deliberations. No repeat participants	“To examine the ways that gender operates as a factor to enable or inhibit full participation in such public forums, and how specific workshop design features such as group size, gender and race/ethnicity composition interact.”

discuss and set issues to vote on. Others are more open-ended, allowing participants to come up with their own concerns and issues to vote on. Finally, participants are often asked to vote on a set of prescribed issues or to produce a final statement or report of the group’s deliberations. Usually the participants discuss and vote until they achieve a consensus. Each of these stages – information-provision, deliberation, voting or reporting – may be repeated several times for any given event.

Competing, Coexisting and Complimentary Goals

The goals of the NNI and the social scientists who engaged the public also help us understand how the science policy domain was simultaneously expanded and discourse disciplined. Advocates of deliberative public engagement have argued that it can produce substantively better decisions, obtain a broader set of viewpoints, give democratic legitimacy to decision-making, improve public trust, and is normatively appropriate for decision-making in a liberal democracy (Blok 2007, Bohman and Rehg 1997, Guston 2004, John Rawls 2002, Sclove 1995). Goals are a symbolic expression of an actor's values and, in the case of the stated goals of a bureaucratic organization, the ordering logic for the practical actions it supports. In this section, two key points stand out. First, the main goal for both funding agencies and social scientists was that public engagement should happen but did not always accompany a statement about what it should accomplish or how it would be assessed. Second, funding agencies and social scientists each had a goal that was not always shared by the other. Funding agencies wanted to foster trust in nanotechnology. Many social scientists, but not all, wanted engagement to affect policy- and decision-making around nanotechnology.

The science agencies that comprised the NNI and policymakers in Congress had largely overlapping goals for public engagement. The NNI's first Strategic Plan in 2004 mirrored concerns from previous Congressional hearings about the "responsible development of nanotechnology," the title for the fourth goal of the NNI. The importance of this work was that it "builds trust among all stakeholders" because "perceptions and acceptance of new technology are critical in the realization of economic and other social benefits" (NNI Strategic Plan 2004:10-12). Part of the effort to realize that goal was to "Support efforts to create … broadly inclusive interdisciplinary dialogue. Assess and analyze public understanding of, and attitudes towards, nanotechnology, [Identify] effective means to raise awareness...and obtain input from the general public" (NNI Strategic Plan 2004:10-12). The President's Council of Advisers on Science and Technology (PCAST) report on nanotechnology of the following year supports this in its section describing the importance of public engagement, adding that "The public is generally very supportive of the Federal Government's investment in scientific research" (PCAST Report 2005:38).[36] "To sustain this support," it began, "the scientific community and the Federal agencies that fund scientific research must communicate more directly with the public, not through surrogates such as the entertainment industry" (43). It goes on to warn, "the NNI…should vigorously communicate with various stakeholders and

[36] Report to the President and Congress on the First Assessment of the National Nanotechnology Initiative (2005)

the public about the Government's efforts to address societal concerns. Without such communication, public trust may dissipate, and concerns based on information from other sources…may become dominant" (43).

From the perspective of those at the NNI and the President's Council, then, we can discern three goals: (1) to understand what the public is thinking with respect to nanotechnology, (2) to protect or foster public trust in- and acceptance of nanotechnology, and (3) to exert some measure of control over the narrative about nanotechnology in the public. The first goal evidences an expansion of the ELSI program of the Human Genome Project. Whereas the ELSI program treated ethical issues as objective and scientifically discoverable problem, the SEIN program does the same for public perception. It is, as Brice Laurent says, the scientific understanding of the public (2017). The second goal explains why they were willing to engage in this expansion: they expected it to result in tangible benefits in the public perception not only of nanotechnology but, potentially, for science as a whole. The third goal evidences the disciplining of discourse that went along with this expansion in science policy. Part of the purpose in communicating with the public was to manage the information the public received and ensure its scientific credibility.

The social scientists who were enrolled to conduct this ethical work and public engagement were not necessarily opposed to these goals but had their own as well. One of the most common goals for the social scientists doing this work was to help democratize technoscience and science policy (Brown and Guston 2009, Guston 2004, Irwin 2001, Sclove 1995, Stilgoe 2007). Democratization was, as one social scientists put it, "the name of the game" (Thompson Interview 1/12/16). Louis Campbell, a social scientist and one of the organizers for the Eastern Environmental Risk Forum, explained this more fully.

> *Well the way I put it is this: you know, we live in a democracy and we make decisions through democratic processes you know legislation, sometimes litigation, sometimes civic involvement, you know, protests or whatever. You know, we're a democracy and we don't set aside science and science policy to be something that's going to be made only by scientists* (Campbell Interview 11/10/2015).

Most of the social scientists enrolled to do SEIN research and upstream public engagement wanted their work to lead to some measurable impact on policy or decision-making about nanotechnology. This had been a key point from the inception of public engagement in nanotechnology. Two key social scientists affiliated with Institution A had criticized the ELSI program of the Human Genome Project for having “no policy relevance,” saying that it failed “to link ELSI research to policy decision processes” (Laurent 2017:129). This goal was consistent with the rhetoric funding agencies espoused as well, about the value of public engagement. Many of the social scientists I interviewed seemed to understand that policy impacts might be difficult. David Guston, an STS scholar with expertise in science policy, has written about this problem. He has written that people often expect the results of ELSI research or public engagement in science to have policy impacts that “occur directly on decisions…affecting some regulatory, legislative, budgetary, or other decision” the “magic bullet approach” (1999:459). He has generally advocated for a broader view of the impacts of ethical research and public engagement that resonated with many of the scholars who did this work. Nonetheless, many still wanted policy impacts in the “magic bullet” model.

Institution A’s Science and Society Center elaborated a clear set of goals which it divided among four sub-programs. These were: (1) to define the scope of nanoscale science and engineering and identify possible linkages with public values, (2) to identify and track changing public perceptions and values of nanoscale science and engineering, (3) to develop “plausible visions of nanotechnology-enabled futures” and use them in deliberative public engagement events to “help refine future visions and enhance contextual awareness,” and (4) to reflexively investigate how the knowledge produced by Institution A impacts nanoscale scientists and engineers and assess the value of the center more generally (Institution A Project Proposal 2005:3-12). The Center at Institution A also endeavored to use its public engagement work as a way to train citizens to participate effectively in civic life and to train a “cadre of interdisciplinary researchers to engage the complex societal implications of NSE [nanoscale science and engineering]” (Institution A Project Proposal 2005:3-12). Theirs was a kind of double boot-strapping effort. The work at the center would train a new group of scholars with the skills to engage the societal implications of nanotechnology into the future.

Similarly, upstream public engagement around nanotechnology would produce a public more informed and more effectively involved around the policy issues of science and technology.

Most of these goals were consistent with NSF/NNI goals or at least did not conflict with them. For instance, both administrators for the various agencies of the NNI and relevant policymakers in Congress had a strong interest in discovering public opinions. In these goals we see overlap among social scientists, policymakers, and funding agencies. Training citizens in civic life, however, was never a stated goal among policymakers or funding agencies. Still, such training did not seem to threaten the NNI or the goals of educating citizens and discerning public opinions and concerns.

There were goals that social scientists and funding agency administrators did not share. Some social scientists resisted the idea that their public engagement exercises should be oriented towards fostering public trust. Most were aware, however, that fostering trust was an expected outcome of their work by funders.

> *The challenge [of public engagement] is to try to present that information in as balanced a way as possible. You know, so as not to predispose them to thinking in certain ways. That's my goal. I have that goal. There are others that do quote, unquote "public engagement," and they do not have that goal and they want to bias the public in certain directions. You'll have scientists go out and they say, "you know we got to talk to the public because we need to convince them that this is safe." And then so they go in and they engage with the public with that in mind. For me, that's not my personal goal. My personal goal is to try to present the information in as balanced ways as possible.* (Thompson Interview 1/12/16).

None of the people whom I interviewed wanted to foster public *dis*trust; they simply did not want to serve as public relations representatives of the National Nanotechnology Initiative. Still, this did not seem to lead to any overt conflict. Social scientists saw an opportunity to do the work to which they were committed and the opinion at the NSF seemed to be that as long as public engagement involved some measure of public education in nanotechnology, then public trust would undoubtedly follow. The assumption by those at funding agencies, that education leads to trust, was a holdover from deficit models of science communication (Wynne 2006, Thorpe and Gregory 2010). The public engagement work for nanotechnology and research on public opinion would upend this assumption.

The myriad and sometimes contradictory goals of those involved in this public engagement work show that it could simultaneously mean different things to different actors while getting broad support from all of them. Through public engagement, social scientists were able to create venues for publics to be critical of an emerging technoscience. Yet public engagement also helped to enroll a set of social scientists into the issues and goals of science funding agencies, like fostering public trust, who might not otherwise have been enrolled, or enrolled to serve those goals in particular. One respondent described his own somewhat grudging enrollment.

> *I probably shouldn't admit this but, ironically, I told a colleague that I was really hating [how] all these STS scholars were jumping on the Nano bandwagon just because there was money there (laughter). And then I ended up being one of the largest recipients! I shouldn't say recipient. I never actually got a huge amount of funding for it, but it did sort of, uh…it was the reason why [I received], not just my postdoc, but my ultimate faculty position.*

Public engagement and its umbrella, Societal and Ethical Implications of Nanotechnology research, could do the work of bringing together a disparate set of actors and containing, simultaneously, many different goals. The absence of a clear and concrete definition for public engagement or the modes of evaluating it probably helped these actors to see in it what they wanted. Everyone involved thought that they might get something positive from upstream public engagement and SEIN research. The values underlying these goals, however, are not entirely complimentary. The goal of scientists and agency administrators, to foster public support for nanotechnology and trust in science, does not necessarily sit well with the goal of democratizing science and training a critical citizenry. Ultimately, though, the social scientists enrolled for this project had to avoid violating the goals of the NNI even if they did not actively try to realize them. Social scientists could engage in practices to democratize science, but they could not be seen to foment public distrust in nanotechnology. This might be especially true for Institution A, which wanted to train a new cadre of scholars savvy in the societal implications of science and science policy. To do so effectively would require long-term relationships with science funding agencies. In practice, this meant that the social scientists enrolled to do public engagement had to show that deliberative public engagement did not foster public distrust. They did so in part by trying to construct the rational public for engagement. This mirrors the Rawlsian perfect deliberator who engages in “reasonable pluralism,” meaning that they can discuss differences civilly according to shared norms of discourse (Rawls 2002). As Hurlbut argues, a focus on Rawlsian public reasoning delimits the types of concerns that can emerge in discourse (2017:22).

Constructing Publics, Constructing Issues

With public engagement events, the imaginaries around science, science policy, democracy, an engaged citizenry were put into practice. If we view imaginaries, as Ezrahi does, as a kind of world-making, then the construction of the deliberative public for nanotechnology, from imaginaries about the potential of nanotechnology and ideal deliberative discourse, brought the nanotechnology public into existence. If public engagement in nanotechnology was a process of expansion in terms of science policy, then the construction of specific publics for deliberation was an exercise in disciplining discourse. By itself, that is neither surprising nor problematic. The nature of representation, whether in politics or research, means that parts must stand in for wholes. Any subset of the public will constitute a narrowing of discourse. The question here is how the construction process wound up disciplining discourse around nanotechnology.

The first way in which construction of the public disciplined discourse was unintentional. Most of the events studied here could not get the publics that they had first imagined. The problem was that people simply were not interested in nanotechnology. As Hugh Davis, a participant in the Midwestern Nanotechnology Deliberation, explained:

> *We were idealistic, or at least I was, and I believed people would jump at the opportunity to engage in a democratic process. And it turned out that, you know, people are busy and have lots of commitments and most of them didn't know anything about nanotechnology. So, I mean, if we had held a consensus conference over the possible citing of a toxic dump five miles from peoples' homes, we would have gotten probably a ton of people to participate. But we were dealing with this issue that was pretty removed from peoples' lives, and so it was hard to find people* (Davis Interview 9/24/2015).

The transcripts for the National Emerging Technologies Conference show another way in which people's uninterest wound up narrowing discourse. The NETC involved an online component across the six participating universities. People logged in to ask questions of experts and write comments to one another in real-time. Transcripts show event organizers admonishing people, however, for logging on but not participating. People got paid whether they typed anything or not. Organizers suspected that people were logging on and then minimizing the chat window to watch television. The organization of the event likely exacerbated this in another way. Not everyone could speak at once, so participants were given windows of time in which they could participate. A few people had to be reminded that it was not their time to deliberate. Some people later admitted that since they were not allowed to deliberate, that they did not feel like they needed to pay attention. They said they would read the transcript the next day (NETC Transcript Day 6 2008).

Publications for these events indicated that the publics they were able to recruit wound up sharing certain characteristics. For three of the four events, all but the Eastern Environmental Risk Forum, this resulted in the selection of publics that were more highly educated than the general populace.[37] In two of the three events, the National Emerging Technologies Conference and the Midwestern Nanotechnology Deliberation, participants were also skewed towards the political left. In the National Emerging Technologies Conference, for instance, 29% of the participants had some

[37] The EERF engaged what one interviewee involved with it described as "vernacular communities." These were pre-existing civil society groups with which they helped to plan a nanotechnology engagement event for themselves.

college, 31% had a college degree, and 33% had graduate degrees. Additionally, 44% were Democrats, 9% were Republicans, and 36% were Independents. These percentages tracked almost perfectly with the categories Political Liberal, Conservative, and Moderate, respectively (Wickson et al. 2013). The Western Citizen Risk Forum selected for a more or less even split among self-identifying liberals and conservatives. Having a higher education and being more politically liberal both correlate with more positive attitudes towards science (Brossard and Nisbet 2006, Fischer et al. 2013, Gauchat 2012). The selection bias did not always emerge, however, from accidentally attracting more science-positive publics from an otherwise open pool of people willing to deliberate about nanotechnology. For instance, when organizers of the Midwestern Nanotechnology Deliberation discovered that they could not attract their intended fifteen people, they were forced to recruit from the university itself. They even recruited several graduate students. This might explain, in their particular case, why participants were more politically liberal and more highly educated. Even still, they only wound up with thirteen of the fifteen people they hoped to recruit.

Organizers for the Western Citizen Risk Forum events were able to avoid these problems by using a third-party organization that specialized in recruitment for survey research and focus groups. Their motivation, however, was to avoid another problem. As Raquel Jackson, the chief organizer of the event, explained:

> *We did not want to use "nano" in recruitment because nano is a pretty big turnoff. Meaning that, for this, many people would self-exclude if they thought it was about nanotechnology. They would assume that they don't…that that's not their thing, and they don't know anything about it. And since our institutional location has it ["nanotechnology" in their name], if you're trying to do it without priming the nanotechnology pump, [then] we needed to have a third party do it, so that it wasn't coming from us.*

Organizers of the Western Citizen Risk Forum event consciously worked to avoid the self-selection of people out of engagement for nanotechnology and thereby were able to recruit a more diverse group, in terms of education and political ideology. Their event was a notable exception, however.

The self-selection problem manifested not only in terms of publics opting out of such engagement events but also opting into them as well. Johnathan Parker, an organizer for both the Midwestern Nanotechnology Deliberation engagement event and the National Emerging Technologies Conference, illustrated the problem with some of the later public engagement work he had done for NASA.

> *So, there are a group of people, who, for decades, have known exactly what NASA should do and they will take every opportunity to tell NASA what they should do. And they speak, you know, for one percent of the population and NASA didn't think it was possible to turn those people off, to actually hear what, you know, non-NASA fanboys might be interested in. And so, in recruitment for that, there was specific questions like "Have you ever witnessed a rocket launch live?" And we took significant steps to … minimize the number of people that would fit that category, to try and weed out and make sure that the regular groups that would always attend NASA public outreach meetings were represented in a very small number* (Parker Interview 10/10/17).

This was one of only two explicit exclusion criteria discussed by engagement organizers and it illustrates an important principle about the construction of publics. Organizers wanted to represent the public writ large, the public "out there" in all its enormity. To the degree to which they were willing to represent already-interested publics or the oppositional publics, it was only in proportion to the numerical representation of those publics among the larger populace.[38] This model of representation operated at the level of the group, but it also manifested, synecdochally, in the individuals selected as well. The Midwestern Nanotechnology Deliberation and the National Emerging Technologies Conference both wanted to recruit "average" or "ordinary" people. The NETC did this, in part, with a questionnaire that asked people if they had participated in any activism around nanotechnology to select people who had not engaged in such activism (Wickson et al. 2013). Average and ordinary people were

[38] Mark Brown (2009) gives an insightful critique of the idea that political representation refers back to a pre-existing populace. He proposes, instead, a model of political representation that recognizes the ways in which it actually constitutes publics.

supposed to represent the ideal every-person, instead of a person with biases or opinions either in favor of- or opposed to nanotechnology. They were supposed to serve as an indicator of how the public might reason and what issues they might find concerning. Furthermore, since they are not particularly special in terms of expertise, knowledge, or intellect, they help prove something that was important to the majority of the social scientists enrolled to do this work: that publics can meaningfully participate in science policy and decision-making. The final report for the National Emerging Technologies Conference made the case for the value of their event and for public participation more broadly in terms of their participants' averageness (NETC Final Report 2008:2, 11).

> *Average citizens very much want to be involved in the decisions that shape technologies that, in turn, shape their lives. Given good information, access to experts, and the time to discuss their concerns with other citizens, average people are able to learn the important details of even very complex issues, and to generate thoughtful, informed, deliberative recommendations.*

Later in the report, it also said that:

> *These were thoughtful, committed, and well-informed panelists, not misinformed, hysterical, individuals being manipulated by outside groups.*

This quote from the final report of the National Emerging Technologies Conference was intended to justify the expansion of science policy to include deliberative democratic exercises. It did so, in part, with the promise that average individuals could be given a measure of technical expertise. Essentially, this was a promise that the discourse could be disciplined by disciplining participants. This idea that average people could learn and were not the result of hysterical groups influencing them assured agency administrators that citizens can have relevant concerns if properly trained. In a larger sense, it is a promise to funders that deliberative public engagement in technoscience will not threaten the scientific endeavor itself.

Hugh Davis, who participated in the National Emerging Technologies Conference and the Midwestern Nanotechnology Deliberation, as well as some of the World Wide Views conferences in Europe, explained how they tried to ensure that they got average or ordinary people.

> *There were supposed to be mechanisms in place that would exclude people who weren't ordinary, in the sense of, if you know they were too expert, if they had expertise, or if they had affiliations with organizations that were active on Nanotechnology, those you know, excluding mechanisms were supposed to be in place.* (Davis Interview 9/24/2015).

There could be real consequences for engagement events that do not recruit average citizens. Davis went on to explain this as well.

> *The reason why something like this is credible is because you're recruiting ordinary people. You're not recruiting interested stakeholders. You're not recruiting experts. You're not recruiting people with prior opinions about the issue. And I think that the narrative is that, you know, we get these 'blank-slate' people, kind of like a jury for a trial, where you would exclude the people who are likely to be biased. Also, if they are already interested or if they're biased, then suddenly, you know, then what are you actually demonstrating? That was one of the critiques of the World Wide Views exercise, was that we were criticized for basically bringing a bunch of Greens [members of the Green Party] in the room, and that critique meant that we had no credibility to say anything about the power of the collective opinion of our participants, going beyond that little group of people.* (Davis Interview 9/24/2015).

The consequence here was that their research was considered meaningless by scientists and funders. He took this lesson with him when designing Midwestern Nanotechnology Deliberation. We also see another meaning to "average" here as well.

Average also means lack of bias. This emerges, in part, from the average person's lack of expertise. Non-average are potentially biased and biased individuals cannot be disciplined through proper training as discussed in the National Emerging Technologies Conference final report. To engage non-average people, then, is to risk the credibility of the event. We can also see this in terms of democratic representation – the group they recruited in World Wide Views was not seen by funders to reflect accurately the thoughts and opinions of the larger public. Average returns, again, to an individualized potential to assess technologies, like the "reasonable person" in American jurisprudence. Because average people, in the sense of the larger citizenry, had no awareness of nanotechnology to have an opinion (Cobb and Macoubrie 2004, Fischer et al. 2013).

Interestingly, average people, defined in these ways, are not that useful to funders for understanding public opinion and funding agencies' goals of avoiding public backlash. Public awareness about nanotechnology has remained steadily low (Burri and Belucci 2007). If the goal was to understand the current public's attitude to nanotechnology the answer is easy and requires little research: the public, as a whole, has no opinion. If the goal of engagement is to change opinion or even simply understand it, then the only people or publics that would matter would be those with existing opinions. These might be individuals with positive opinions, even in the form of "fanboys," or negative ones, in the form of the civic groups and science dystopians discussed in the last chapter. Robert Wilson, one of the organizers for the Eastern Environmental Risk Forum, gave this as a reason why their event encountered so much difficulty.

> *It continues to be a tricky issue with stuff like nanotechnology. If you've got something where there's a clear kind of public concern or like you've already got citizens that are organized and interested, like say you got a disease like breast cancer, you've got this engaged group of ready citizens and I think there you can have these really fruitful interactions with them and the scientific community. But something like nanotechnology, you mainly just have like these really like technophilic people who love new technology or technophobic [people]. You just get a few sort of really*

contrasting perspectives and I feel like it's hard to get really interesting public engagement going (Wilson Interview 11/16/2015)

In the terms of acceptable public engagement, the bias of technophobes or technophiles rendered them irrelevant. In terms of theories of public engagement, this is interesting. Dewey argued that publics emerge wherever there are issues of concern, especially when existing structures of governance do not address ([1927]1990). Where publics do not spontaneously form around an issue, and the issue is on for which the state has no strategy for governance, the state has a duty to help publics form themselves. In the case of nanotechnology, something quite the opposite of this happened. New and uninterested publics were formed despite the existence of publics who actually were interested in the issues, as small and unrepresentative as they might have been.[39] They were considered too messy by both funders and social scientists for purposes of engagement because their discourse as not amenable to bureaucratic and epistemic discipline by experts in either nanoscience or deliberative democracy. I think this is interesting for another reason, beyond the question of expansion of science policy and the disciplining of discourse. Focusing on unbiased or otherwise average people may not help meet the NNI's goals of understanding public perceptions and avoiding public backlash. As we saw in the last chapter, people involved in the NNI complained that the ETC Group was little more than a few people and a good fax machine. Even so, they effectively got the attention of Prince Charles, who then became a high-status voice critical of nanotechnology development. Public discourse about emerging technoscience is more likely to be driven by a vocal, if unrepresentative, minority than millions of uninterested average citizens.

The discourse at these engagement events was also disciplined by virtue of what participants were recruited to discuss. To some degree, determining the content of the deliberation is inevitable as there is no such thing as an unframed message (Benford

[39] Felt and Fochler (2008, 2010) have studied the construction of deliberative publics in Austria and argue that publics push against the manner of their construction, forming their own identity as a public in the course of deliberative exercise. The point here is not so much that the publics so engaged were dupes hemmed in by the modes of their construction but rather the implications of the values behind the construction of the publics to agency administrators and scientists.

and Snow 2000). To varying degrees, however, these public engagement events framed nanotechnology positively.

The Southwestern Nano Dialogue presented nanotechnology in the most favorable light of all the events. In the scenario for deliberation, participants were told that a new and deadly virus, the Pandora Virus, was on the verge of becoming a pandemic. Participants were to take on the role of either a scientist, a politician, or someone from the “general public” to discuss how a nanotechnology-enabled vaccine should be used to fight Pandora Virus. The materials described the nano-vaccine in the following way.

> *After 5 years of work, last year, a famous interdisciplinary team of university nano-scientists, collaborating with other research institutes, successfully engineered a prototype of a kind of “nano-vaccine” to both treat and prevent the attack of some viruses.*
>
> *To help, the Food and Drug Administration fast-tracked approval of the university nano-scientists’ hybrid nano-vaccine that will help protect the country from a potential pandemic. As with the bird flu virus, where governments have already ordered that 150 million chickens be destroyed, millions of people could die if Pandora’s virus spreads into a pandemic.*

Victoria Harris, the main organizer of the Southwestern Dialogue and director of the STS program at that time, recognized that they framed nanotechnology positively.

> *I think we had a fairly positive spin, because we wanted to get people involved in the excitement of learning about the science and becoming knowledgeable for active citizens, that was one of our…one of my main goals. I didn’t want it to become a polarized situation where the scientists were kind of called to account. Rather, I wanted to draw on their good skills and enthusiasm for science.* (Harris Interview 10/22/2015)

The National Emerging Technologies Conference also engaged in imagination work. For that event, participants were given background materials numbering sixty-one pages. It presented participants with six possible future possibilities to imagine and deliberate. These were: engineered human tissues, bionic eyes, a personal disease detector (“Doc in a Box”), a “healthy chip” that could monitor health in real-time and dispense medication as needed, a cognition enhancing “brain chip,” and the “barless prison,” a drug that is “injected into prisoners that becomes activated by radio control” if prisoners go beyond certain boundaries. The background materials noted that these “fictional scenes are extrapolations from current nanoscale research; they have been vetted for their technical plausibility by real scientists currently working in nanoscale research” (NETC Background Materials 2008:4). Organizers of the NETC were careful not to have citizens engage with concerns that scientists and funding agencies would discard as mere science fiction.

In the face-to-face meetings for the National Emerging Technologies Conference, participants were asked to discuss their concerns and their “excitements” regarding the NanoScenarios presented in the background materials. They were asked to rate their excitement for the technologies based on several categories, including “Medical Enhancements,” “Cognitive,” “Military,” and “Humanity/Global” (NETC Georgia Notes 2008:10). The final report for the group at one of the participating universities added a note in their report complaining that the background materials for the “Humanity/Global” category were “idealistic and utopian,” and they asked for “more skepticism” from future materials (NETC Georgia Notes 2008:10). None of the other groups explicitly complained that the background materials seemed overly positive, but people were clearly convinced that Nano-Bio-Info-Cognitive technologies held a great deal of promise. In another instance of imagination work, for instance, those background materials reprinted an NSF list of the most important promises of nanotechnology (see Appendix B).

I do not have the full materials for the other events I studied here so it is difficult to know how positive the issues were framed.[40] The Eastern Environmental Risk Forum

[40] There is sort of a Spanish Flu story behind my use of these materials and my discussion of the National Emerging Technologies Conference. What became known as the Spanish Flu was actually a disease ravaging most of Europe. Only Spain, however, publicly acknowledged the problem. For their honesty, they were saddled with the name of that

directed participants to design their own events, including finding their own nanotechnology experts, so it is difficult to know the degree to which they framed nanotechnology positively. The Western Risk Forum and the Midwestern Nanotechnology Deliberation both described efforts to achieve balance between risks and benefits. Still, as Jasanoff has explained, a focus on risks and benefits already frames the discussion in ways that certain public concerns cannot emerge (2012). By asking people about their concerns for how nanotechnology would be applied, for instance, organizers for these events may have framed the development of nanotechnology as inevitable. Inevitability is a core component of the modern technocratic approach to science policymaking (Porter 1996). Either way, the framing of nanotechnology and its possible issues constituted imagination work about possible futures and their desirability. In none of these, however, was anything like a moratorium presented or entertained. Furthermore, there is good reason to think that it never would. With the exception of the Southwestern Dialogue, all of these events had to report their findings and justify their grant funding to the NSF. The two Science and Society Centers had to conduct annual meetings with reviewers from the NSF. If they had had participants discuss anything truly heretical to the development of nanotechnology, it very well could have affected future funding. Again, though, there is no way not to frame discourse in a deliberation. It is a way in which discourse can be disciplined, however, especially if that discourse is funded upstream by federal science agencies and if scientists are enrolled as participants in the event, as they were for each of the events studied here.

Discourse around nanotechnology was likely disciplined by the selection of publics to participate and the selection of issues. The publics engaged for the deliberation disciplined the discourse around nanotechnology in two ways. First, discourse was disciplined by the fact that organizers had trouble recruiting people. The people they were able to recruit were often more highly educated and political liberal, both of which are characteristics associated with more optimism about science and technology (Gauchat 2012). Second, discourse was disciplined by the need to recruit

disease. Organizers of the NETC were the only ones kind enough to share all of their materials. Therefore, I want to be careful not to make it seem like that event was uniquely problematic in these ways. I simply know more about it than any of the others.

unbiased, “average” citizens who could both speak for the larger public and prove to science funding agencies that lay publics could play a valuable role in science policy. By virtue of engaging average and unbiased citizens, however, discourse did not include highly critical ideas about nanotechnology. It would have been nearly impossible for participants to bring up any of the apocalyptic scenarios around nanotechnology or ideas about a moratorium or relinquishment. Furthermore, the selection of issues made doubly sure that issues like these would not be discussed. At the micro-institutional scale, they were exercises in public reasoning. These events put issues on and off the table that drove discussion. There is no way to avoid this for any upstream engagement event since organizers inevitably guide discussion. Yet both of these are ways in which the expansion of acceptable science policy was met with a disciplining of discourse, whether or not this was intentional on the part of event organizers.

The Outcomes of Engagement

The question of outcomes for public engagement helps us to see to what degree it was an expansion of science policy and to what degree this expansion was disciplined, particularly by agency administrators. The most obvious measure for any policy mechanism is whether it influenced policy. While this section outlines the difficulties that organizers of these engagement events encountered in trying to affect policy, I do not want this section to suggest that engagement was useless or a failure. The first reason it was not a failure is that there are no standardized criteria for evaluating public engagement exercises (Guston 1999:457). Second, it is difficult to assess the full value and long-term impacts of public engagement on science policy processes as well as on the citizens who participate. Rather, the importance of policy impacts for this dissertation derives from the fact that those impacts, or lack thereof, demonstrate the degree to which public engagement was an expansion of science policy and to what degree it was constrained in order to minimize the expansion.

Not all of these events were intended to impact policy, though documentation for all of them cite the importance of public dialogue for the responsible development of nanotechnology. The purpose of the Southwest Nano Dialogue was simply to “create a sense of wonder” in attendees about nanotechnology. In Ezrahi’s terms, their outcome was simply the imagination work itself (2012). The Western Citizens' Risk Forum was mainly a research project in deliberative exercises, intended to inform future deliberative efforts. The Eastern Environmental Risk Forums was also intended to test a deliberative format. Specifically, the EERF endeavored to foment the capacity for future, self-directed public engagement in issues of science policy among existing civil society groups, such as the Elks Club or the Rotary Club. One of the major outcomes for each of these events was knowledge-creation around deliberative engagement.

The primary evidence that knowledge had been created around deliberative engagement was publications. This is another outcome that all of these activities shared or were supposed to share. The NSF lists all of the publications for each grant it funds. I read all of the publications listed for the grants that funded each of these events. When reading the publications for the Eastern Environmental Risk Forums, however, I noticed that none of them mentioned the public engagement work of the grant. The EERF was funded through a larger Nanoscale Interdisciplinary Research Grant that funded a number of activities of which the public engagement aspect was only one. I asked Robert Wilson, one of the organizers, about this and he did not seem surprised.

> *It's not an accident that you didn't find publications [laughter]. I mean, I think the goals were fantastic and I think that, you know, we totally should have yeah, gotten a lot more out of this.*

He went on to discuss the logistical difficulties of doing public engagement as well as the lack of a pre-existing scholarly relationship among the event's organizers which made it difficult for them to work together. As he continued to talk, however, he became more ambivalent about the goals of the project.

> *I guess…one needs to be clear in the particular context; what actually are we trying to achieve here? Are we trying to get a better decision? Are we trying to get people to get on board? Do we have political reasons or, you know, is it just the right thing to do to bring people together? So, tying all this back to our project, yeah, I'm not sure that we had a super clear goal.*

Foster, another organizer for the Eastern Environmental Risk Forum, was more sanguine about the goals of their public engagement exercise. When talking about outcomes, I asked everyone about policy impacts, since they are so commonly cited. He said, "That's not my main goal." When asked what his main goal was, he responded:

> *My main goal is to work in the university, teach my students, funnel as much money [to them] as humanly possible, and at the end of the day hopefully, you know, educate them.*

Unsurprisingly, outcomes were tied directly to the goals and organization of the activity, or lack thereof in the case of the EERF. It seemed that, for the EERF, the scholars involved did not all understand what their goals should be.

The other outcome that every event shared was a positive participant experience. All of them, in different ways, measured the participants' experiences of the deliberation. For the Western Citizens' Risk Forum this was also data for the larger research project, since that event intended to understand some of the factors that made people comfortable and effective deliberators. For the others, it was largely a measure of whether or not participants enjoyed the activity. This may be seen as part of a longer-term impact on citizen engagement and maybe even policy impacts since individuals who enjoyed deliberative engagement are probably more likely to do it in the future. Caroline Lee, however, has critiqued deliberative activities that do little more than provide participants with an emotional experience, however positive (2014). Again, though, it is difficult to gauge whether participant experience may have long-term impacts on the development of technoscience and future civic engagement (Guston 1999).

Not all of the events wanted to have policy impacts, but at least two did. The Midwestern Nanotechnology Deliberation and the National Emerging Technologies Conference both listed policy impacts as one of the goals of the project. Both met with difficulties in realizing that goal.

The Midwestern Nanotechnology Deliberation was thwarted in its attempt to influence state policy by a change in state leadership. They had intended to bring the final document from the Midwestern Nanotechnology Deliberation to introduce into the state legislature as a way to advocate for creating a nano-registry. The nano-registry was supposed to serve as a database for all existing products containing nano-materials. One common concern among participants' in the Midwestern Nanotechnology Deliberation was the fact that nano-enabled products, about which nothing was known in terms of toxicity or long-term effects on people's health or the environment, were already in the marketplace.[41] According to Nigel Martin, one of the organizers, this started off well.

> *We had networks with the press, we had networks with people in the state legislature –I give [Jessica King] most of the credit in this regard – and we pushed hard on those. We felt we got a fair bit of press; some of our citizen participants were on TV, on the radio, etc. We held a press conference at the State Capitol. Legislatures came and some of them continued to be interested* (Martin Interview 10/2/2015).

As Jessica King, whom Martin credited with doing the bulk of this work explained, it crumbled with a change in the political climate.

> *We were working with a couple of legislators who were really into this. And the very simplified version of what happened was, we basically gave up. Because once [a new Republican Governor] was elected, the legislators we were working with just said "It's hopeless, we can't do anything."* (King Interview 11/11/15).

A changing political context seemed to limit the ability of these social scientists to translate the outputs of their engagement event into a policy influence, despite incredible efforts. The Midwestern Nanotechnology Deliberation did motivate one concerned participant, retired school Principal Larry Miller, to fly to Washington D.C. at his own expense to give his testimony before the Nanoscale Science, Engineering, and Technology subcommittee in its

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[41] It may be worth noting that the Woodrow Wilson Center's Project for Emerging Nanotechnologies has something like a nano-registry, called the Consumer Products Inventory (CPI). Its value was limited, however, by the fact that it was consumer-generated. As documentation for the CPI states, the facts in the listings are unreliable at best and many products listed may not even exist. It can be found at: http://www.nanotechproject.org/cpi/

first ever public meeting. The meeting was to discuss the *Environmental Health and Safety Aspects of Engineered Nanoscale Materials* (2007). The nanotechnology newsletter *Nanowerk* notes that, without Larry in attendance, "this meeting would have been just one more ritual gathering of scientific and government experts, debating what to do next with little public input" (Nanowerk 2007).[42] Mr. Miller used the opportunity to submit a copy of the Midwestern Nanotechnology Deliberation final report and, according to *Nanowerk*, to complain about the event's lack of policy impact, saying that in the two years following their report, he did not feel that the engagement event had had any impact on nanotechnology policy or decision-making (Nanowerk 2007).

The National Emerging Technologies Conference was not met with a shift in the political climate like the Midwestern Nanotechnology Deliberation, but also seemed to struggle to impact policy. Penelope Brown, one of the social scientists who had participated, said that it was because the event had no mechanism in place for affecting policy.

> *There was no task built in of 'take this to your state legislature, let's take this to Congress,' whatever, and the main thrust of the report I would guess would be for the research community* (Brown Interview 12/2/2015).

She seemed disappointed that the main focus of the exercise was research-oriented. The main organizer of the project did attempt to bring public opinions from the National Emerging Technologies Conference to the attention of administrators at the NSF in a presentation about the societal implications about nanotechnology.

> *[Elijah] coordinated a presentation to the Nanotechnology caucus in Washington D.C., I went to that meeting, there were presentations, and I sat on a panel and answered some questions and things like that. But I'm just not sure, it's really hard for me to say with any confidence that we had*

[42] In its self-description, *NanoWerk* establishes its credibility, in part, by distancing itself from Drexler's and Crichton's visions that we saw in the last chapter. It begins "Why a newsletter about nanotechnology risks? Are we scaremongers? No; and we are not covering killer nanobots and grey goo either." (http://www.nanowerk.com/nanorisk/nanorisk.php Accessed 2/15/16).

> *any sort of impact. I mean I think we were part of the discussion, uh but, yeah, I don't think that we had much impact there.* (Davis Interview 9/24/2015).

For various reasons, including simple bad luck, a lack of planning, and optimism about citizens' level of interest, engagement organizers proved unable to influence science policy and decision-making, at least according to the "magic bullet" model of policy influence (Guston 1999). There are two reasons why this might still be relevant, however. According to several people whom I interviewed, the promise of such policy impacts was used to enroll both social scientists and participants in the projects. Jacqueline Nelson, who worked on the National Emerging Technologies Conference and the Midwestern Nanotechnology Deliberation, argued that it was okay not to have policy impacts, as long as you do not promise such impacts to participants in order to recruit them.

> *It also became clear pretty quickly that there wasn't going to be a sort of feedback into any real policy process, which was a critique that…there were sort of promises made to recruit people, like "come and do this neat thing and affect the future of this new technology" when, in fact, these were really academics experiments more than anything. I think academics were overly optimistic about what would happen with the result of this* (Nelson Interview 9/3/2015).

Participants had good reason to think that they might affect decision-making around nanotechnology. The first sentence of the National Emerging Technologies Conference background materials given to participants stated: "In this project, you will be asked to help develop a set of guidelines that you, and the other panelists in your group, believe should steer the development of some very powerful, new technologies" (National Emerging Technologies Conference Background Materials 2008:1). Read closely, this statement does not promise participants that their opinions would have policy impacts, but it seems reasonable for them to have thought that it did.

Nelson, who joined the projects, in part, to affect policy, realized that her goal might not have coincided with the goals of the main organizers. Discussing both the Midwestern Deliberation and the National Emerging Technologies Conference, she expressed qualified frustration about the promise of impact.

> *I was really attracted to some of the more democratic promises made by the organizers about what contributions the event could make to democratic decision making around science and technology. But I think if you probably asked like [Elijah] at the time, "Well is this going to have any impact on policy?" He probably would have said, "That's beside the point. Just getting people in a room to deliberate is democratic in and of itself." But I think, from my perspective, I sort of wanted, I wanted more from both of those exercises. There's a disconnect between the sort of hope of what effect it would have in a concrete way and then it being more of an academic thing* (Nelson Interview 9/3/2015).

Many engagement organizers and practitioners fell back on this idea when describing the disappointing lack of impact these events had on science policy and decision-making. At least, as far as the logic went, engagement had happened and perhaps even had important outcomes they would never know. For some, like Elijah Anderson, this really was the main goal of engagement and not merely some secondary outcome.

Another common goal, from the perspective of these engagement exercises was to train participants to act as engaged citizens. To what degree was this an outcome? We have already seen one success. Larry Miller from the Midwestern Nanotechnology Deliberation flew to Washington D.C. to represent the public about nanotechnology. The Midwestern Nanotechnology Deliberation was successful in another respect. It led to the formation of a citizens' group called the Nanotechnology Citizens' Engagement Organization or "Nano CEO" that met regularly about the issues surrounding nanotechnology. They met with one another, organized public events, and wrote opinion pieces. They were forced to stop meeting, however, once they began plans to test nano-silver imbued socks. They wanted to determine whether the nano-silver particles came off in the washing machine, winding up in the water supply. Up until that point, the university had given them a small amount of funding to hold their meetings and plan events. They had plans in place to collaborate with a toxicology laboratory on campus to conduct the research. This should have been a stellar example of citizen science and just the kind of outcome public engagement was supposed to foster. It was not viewed this way by local nanoscience industries, however.

Jessica King, the university liaison to the NanoCEO group and an organizer for the Midwestern Nanotechnology Deliberation, explained that the director of the university's Nanoscale Science and Engineering Center that funded her post-doctoral research sent her a letter telling her that Nano CEO had to stop its research.

> *Participant: Well my boss was an engineer who does Nano and science stuff. One day, I got a letter just completely and totally out of the blue. I specified to NSF about the work [of NanoCEO] and everyone [there] was raving, all this great engagement and citizens and scientists! So, I got a letter out of the blue, "You need to stop this work immediately. I think this work is illegal."*
>
> *Interviewer: Why illegal?*
>
> *Participant: Well, there was nothing illegal about it. No, it's not illegal. In fact, a bunch of other professors and researchers were doing this work and have since. There was more to it than that. "You must stop this project and all of your projects immediately. Do not do any work." That's crazy, okay? It was crazy. I was completely flabbergasted, of course, and a lot of things happened. I left academia and never looked back. But the Citizen component made them [industries] really nervous. And my boss admitted to me that the companies that support their work…basically told him "You have to shut this person down."* (King Interview 10/15/15).

The Nano CEO group disbanded shortly thereafter. I reached out to the former boss she mentioned above but did not receive a response to my inquiry. I then asked four other people with direct knowledge her story if they could confirm what King had said. Three of them clearly did not want to talk about it, one saying that I should just talk to her. Hall, quoted earlier, however, confirmed her story.

> *So now, she and her group actually had a really tough time. I didn't see any evidence of them actually drumming up concerns that were invalid. But I did see a lot of criticism about that. And, by and large, that criticism came from threatened groups within academia and business. It was a very clear case of a privileged few, in research labs in particular, being allowed to do what they wanted to do. And all of a sudden, somebody being in a position of questioning them. They didn't like it, at all.* (Hall Interview 5/19/2016)

What should have been a clear victory in terms of public engagement was shut down in order to protect the commercial possibilities of an emerging technology. As both King and Hall indicate, these were not the irrational and vocal publics that scientists and funding agencies feared. Rather, by working with university scientists and collaboration on laboratory research, these lay people worked within the realms of acceptable scientific credibility and expertise.[43] Whether they realized it or not, the Nano CEO group inserted itself into an ongoing regulatory debate about the safety of nanosilver. In 2007, the Korean technology company Samsung released an "antibiotic" washing machine that released nanosilver into its washing basin to sterilize clothing. The National Resource Defense Council had asked the Environmental Protection Agency to investigate the impacts of nano-silver, making it an issue of public concern (Laurent 2017:102-103). King indicated her awareness of this when she said that research into the toxicity of nanoparticles was being done elsewhere. Had it not been for that ongoing issue then perhaps the NanoCEO group would have been seen positively. King stressed, multiple times in our interview, that the NSF regarded the citizen group very highly.

[43] As Jessica King asserted, this research was later done by toxicologists at ASU (Benn et al. 2010). They found that all but one of the products they tested did, indeed, leak nano-silver particles into wash-water.

Another surprising outcome related to the NNI's most important goal for upstream public engagement: fostering public trust. In a published paper, Christian Jones and Nathan Reed, two organizers for the National Emerging Technologies Conference and the Eastern Environmental Risk Forum, found that another set of smaller engagement exercises they had organized around nanotechnology increased not only participants' hopes but also participants' fears. From the perspective of funding agency administrators, this is exactly what public engagement was not supposed to do. Lowell Evans, one of the former NSF program directors, recounted a discussion he had in a planning meeting in the early 2000s with the Department of Energy in which they discussed the worry that engagement would foster public fear. The purpose of that meeting was to determine how public engagement for the NNI should be organized.

> *In my group, the discussion was about, to some extent, about how we can do this with the least amount of hassle. We need to spend this money, but how can we do this without…well, not without not "the least amount of hassle," I shouldn't put it that way. But without it getting out of control. You know? Without it getting wild.*

Upstream public engagement for nanotechnology was fine so long as it was disciplined. Concern about Jones and Reed's findings came up in a 2008 workshop *Communicating Health and Safety of Emergent Technologies* at North Carolina State University. Dietram Scheufele, a SEIN researcher funded through the NNI, gave a presentation about the public's capacity to learn about complex scientific issues. His work was part of the same NSF grant that funded the Eastern Environmental Risk Forums, among many other activities, in which Jones and Reed had both participated. Someone in the audience asked him about the finding that engagement can also increase public fear, saying "[Reed] found that in these consensus type programs…the more people have learned, the more likely they are to raise these vexing ethical kinds of questions about distributive justice, privacy, and other…non-technical kinds of issues" (2008)[44]." Scheufele responded:

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[44] Communicating Health and Safety of Emergent Technologies Workshop

Learning more [about science and technology], for a community activist going out to one of these meetings and [who] already has a problem with government regulations, maybe with GMOs and so on, and then goes into one of these groups, and then discusses this, well there very well [may] be a more critical view that comes out of that.

That's an important fact to keep in mind for a lot of these deliberative meetings, is that they're self-selective. … It's sometimes tempting, when I look at these issues, to say "Oh, it's interesting that people think this" but it's, no, it's interesting to know that the people who actually took the X amount of dollars that they got and went and went through the effort to do three rounds over three weekends, that that particular group of people thought in a certain way.

In effect, what Scheufele was saying here is that these were not average, ordinary citizens. They were interested and already biased individuals who came to participate or people who were only in it for the money. He discounts the increased level of concern among deliberative participants as a product of the bias with which they entered that deliberation.[45] He also seems to discount upstream public deliberation as a whole, since it will always require some small sample of individuals who will always require an incentive to participate and who will always evidence some degree of selection bias. In any case, it seemed the Scheufele wanted to distance his own work from that of his colleagues and the potential of the latter to foment public fear. Furthermore, the person who first asked Scheufele the question about "vexing concerns" sounded like those were not necessarily his concerns but were rather common concerns about upstream public engagement. I asked Edward Hall, the scientist involved in the NNI since 2001, about whether administrators in the NSF were worried about public engagement increasing public concern.

[45] Incidentally, this contradicts Jones and Reed's descriptions of their event participants, who they framed not only as average but as more often unrepresentatively knowledgeable and supportive of science.

Interviewer: [Jones' and Reed's] work shows that sometimes public engagement work actually makes people more concerned as they become informed. Did you hear that concern, you know, bounced around the [NSF] hallways at all?

Respondent: Oh, yes. Oh, oh, absolutely, yeah. So that was definitely the control element, again, especially from the advocates of nanotechnology, investors in nanotechnology. So, they wanted engagement as long as they retained the power and they retained control of the narrative. If there was any indication that they were going to lose control of the narrative, they became very, very uncomfortable about it, and very uncomfortable about the fact that engagement might lead to consequences that they didn't want, including people becoming more concerned about the technology.

Clearly retaining control of the discourse around nanotechnology's ethical and societal implications is a form of disciplining discourse around nanotechnology but so too was the lack of influence on policy. Policymakers and science agency administrators have the power to use the outputs of SEIN research and upstream public engagement to influence policy or to choose not to do so. There is no way for citizens or engagement organizers to compel them to listen or act. I do not mean to suggest that ultimate power should lie with citizens, necessarily, but rather simply point out that this is another way of ensuring that the expansion of acceptable science policy does not expand too widely. This is the power that administrators and policymakers have with *upstream* public engagement, as opposed to more adversarial forms, such as grassroots social movements. Since agency administrators had enrolled the participants in the first place, it was their prerogative as to whether or not they would act on participants' concerns. As long as upstream public engagement did not foster too much ongoing civic engagement, like NanoCEO, or too much public concern, then agency administrators could control how upstream public engagement was incorporated into science policy domains. Passively listening to engagement participants' concerns, without acting on them, was one way to discipline discourse around nanotechnology.

The End of Public Engagement in Nanotechnology

Upstream public engagement seemingly disappeared from the National Nanotechnology Initiative. In part, it seems that it was difficult for the agencies of the NNI to figure out what they had purchased with SEIN research and public engagement. Documents for the NNI show that administrators struggled to figure out how to assess what public engagement had achieved. In its report about assessment for the NNI, the President's Council Assessment of Science and Technology 2008 report on nanotechnology admitted that measuring progress "remains challenging" for all aspects of the NNI (PCAST Report 2008:11).[46]

> *Nonetheless, appropriate metrics are essential… Commonly used measures include bibliometrics such as publications, patents, and citations; knowledge mapping; counts of research centers, networks, user facilities, principal investigators, new trainees, start-ups, new products, initial public offerings, and acquisitions; and amounts of funding support from public and private sectors (corporate R&D as well as venture capital).*

The problem is that few of these work for public engagement and SEIN research. The only thing that might apply would be publications, but even that does not capture the purported democratic value of SEIN research and public engagement or the instrumental value to funding agencies and policymakers in helping inform science policy. Margaret White, one of the former NSF program directors I interviewed, elaborated on the difficulties of assessment.

> *Well, I think it's an issue broadly with research that, you know, the justification for research generally in the United States is that it is [a] wealth-creating or wealth-building endeavor. You know, "did it give us new transistors," for example. It's more difficult when you're talking about social and behavioral sciences.*

[46] Report to the President and Congress on the Second Assessment of the National Nanotechnology Initiative (2008)

> *If you look at the results of educational and ethics research, it can be assessed as creating a good deal of value. But it doesn't prevent [people from] saying "Well how do we know that it creates value?" So, assessment can become used for not supporting some realm of research that does not fit into the major paradigm [of wealth creation]* (White Interview 9/13/17).

Jackson, one of the organizers for the Western Citizen's Risk Forum, said something similar when explaining why she though that there was no longer deliberative public engagement for nanotechnology.

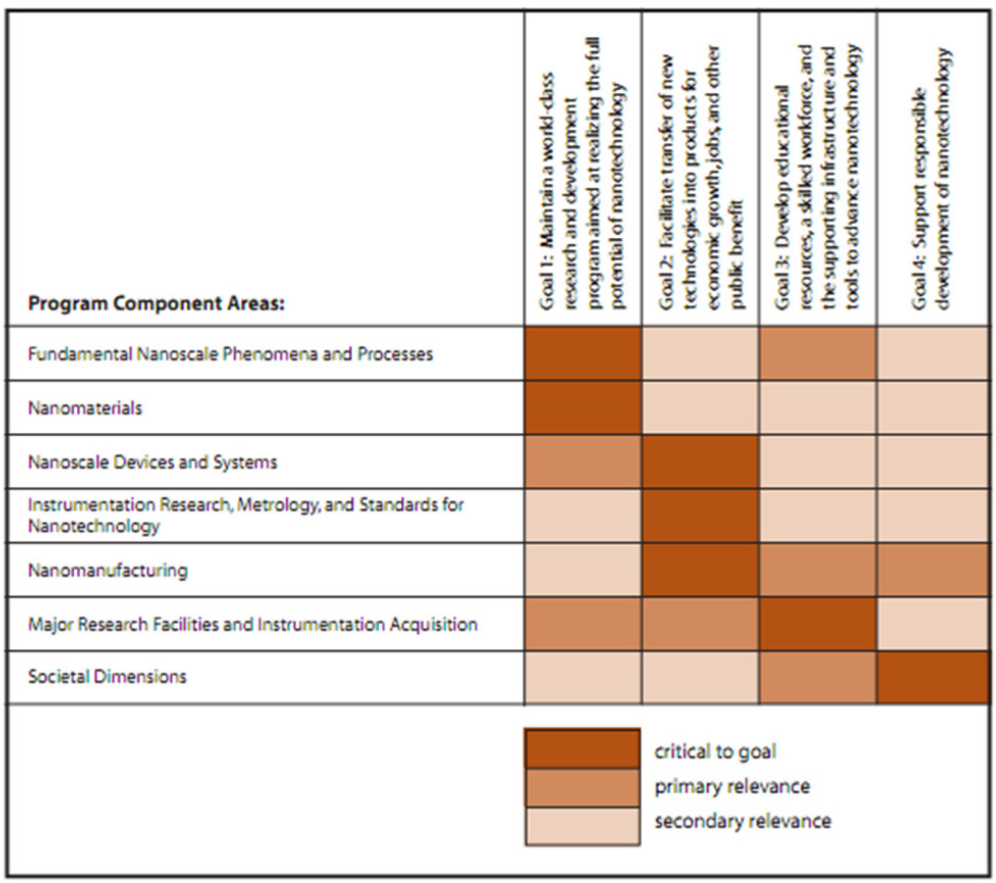
> *I'm not even sure where we might go to fund more focused work on deliberative engagement. I'm not sure that it's even a fundable…I mean it's fundable when it touches down on another politically-sensitive risk object like synthetic bio[ology] or, you know, some other emerging tech that people are investing heavily in and are worried about public backlash. Then maybe.* (Jackson Interview 4/13/16)

The fact that publics seemed entirely uninterested in nanotechnology, to say nothing of a massive public backlash, did not help to justify continued upstream public engagement. A report reflecting on the Western Citizens' Risk Forum recalled that they ran the event to "capture upstream views because we (wrongly) anticipated rapid social amplification" of concerns around nanotechnology. Without that pressure, support for public engagement seemed to dwindle, at least for nanotechnology.

Societal and ethical implications of nanotechnology research, including public engagement, now manifests only as informal science education and environmental health and safety (EHS) research. Informal science education had always been a popular method of public engagement for scientists and administrators at the NSF and it was another site of innovation for the NNI. The Nanoscale Informal Science Education Network (NISEnet) endeavored to do "actual engagement around the social issues rather than just promotion of the technology" (Hall Interview 5/19/2016). I do not discuss the work of NISEnet in this dissertation only because no one involved in that work returned my request for an interview.

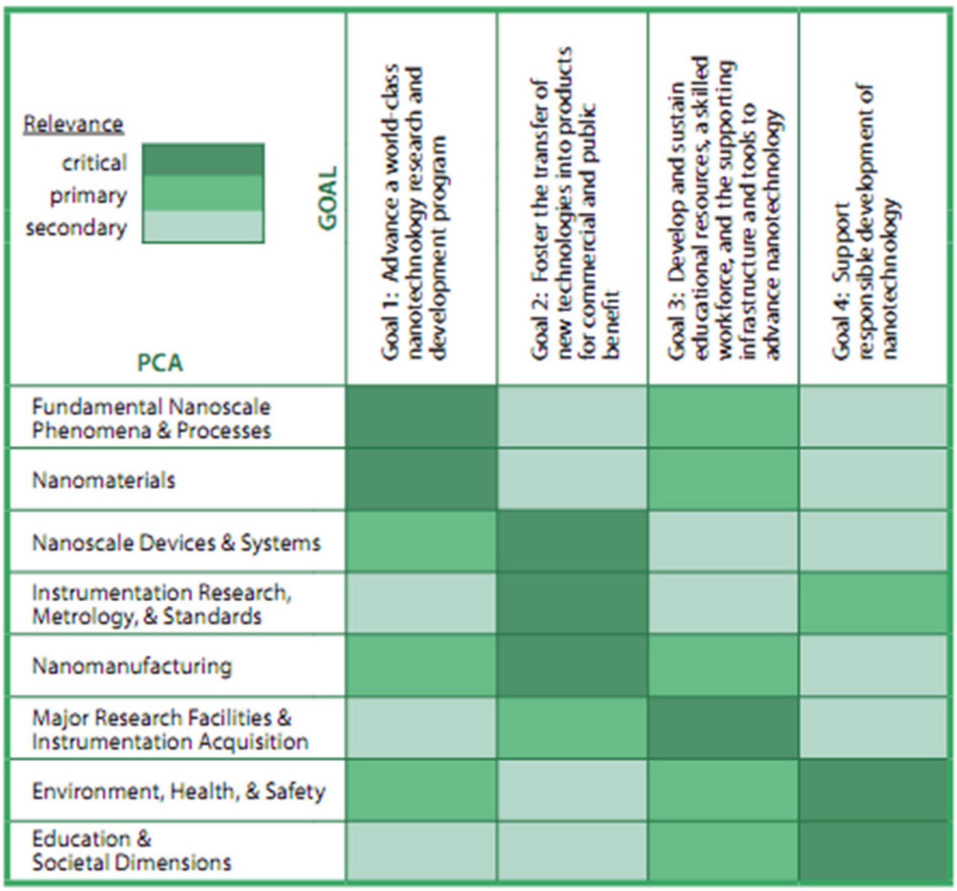
In a return to a more technocratic mode of governance, Environmental Health and Safety research seemed to absorb societal implications research, including public engagement, in the years following these public engagement events. We can see this reflected in the Program Component Areas for the National Nanotechnology Initiative Strategic Plans of 2004, 2007, and 2015. Program Component Areas, again, were areas of targeted funding and goal setting for the NNI. In the 2004 Strategic Plan, Societal Implications was the last Program Component Area. It was listed as the most important area for the goal of “supporting responsible development of nanotechnology” (NNI Strategic Plan 2004). See Figure IV.1 below.

Figure IV.1 Program Component Areas from 2004 NNI Strategic Plan



In 2007 *Environment, Health, and Safety* was added as the seventh Program Component Area while *Societal Dimensions* was moved down to eighth and became *Education & Societal Dimensions*. Both were listed as supporting the responsible development of nanotechnology. See Figure IV.2 below.

Figure IV.2 Program Component Areas from 2007 NNI Strategic Plan



In the 2015 Strategic Plan (Figure IV.3), societal dimensions disappeared entirely, leaving only *Environment, Health, and Safety*. The format of the Strategic Plans had changed as well, focusing more on Nanotechnology Signature Initiatives rather than the Program Component Areas for setting goals and directing funding and research. The Nanotechnology Signature Initiatives cover all of the most important technical goals for nanotechnology research and development.

Figure IV.3 Program Component Areas from 2015 NNI Strategic Plan

1. Nanotechnology Signature Initiatives (NSIs)

- Nanotechnology for Solar Energy Collection and Conversion
- Sustainable Nanomanufacturing
- Nanoelectronics for 2020 and Beyond
- Nanotechnology Knowledge Infrastructure (NKI)
- Nanotechnology for Sensors and Sensors for Nanotechnology

2. Foundational Research

3. Nanotechnology-Enabled Applications, Devices, and Systems

4. Research Infrastructure and Instrumentation

5. Environment, Health, and Safety

It appears that nanotechnology returned to a more technocratic model of dealing with issues related to nanotechnology. No vexing issues like distributive justice or ethical concerns were likely to emerge under these headings, nor would any of these categories necessarily fund anything like deliberative public engagement.

Although societal and ethical implications research and deliberative public engagement did diminish for nanotechnology, one of the Science and Society Centers has shifted gears, becoming much larger and more broadly active in science policy. In fact, the growth of that center might be one of the most enduring impacts of the NNI and its societal implications program. When I asked the director of that Center what would have been different had public engagement work not happened for the NNI, a question I asked every engagement practitioner, he answered:

> *So, we created a network of, in essence, of skilled professionals through the National Emerging Technologies Conference and that network of skilled professionals was in place when the solicitations for groups to participate in the first World Wide Views on Global Warming came out. We had a group of folks who then participated in World Wide Views on Global*

Warming, then World Wide Views Biodiversity, and then World Wide Views Climate and Energy that we're already there. And, you know, quite frankly if we had not done the National Emerging Technologies Conference, I don't know who if anybody would have participated in or invest in World Wide Views. That capacity and that experience became what was drawn on for the NASA awards and now for further awards directly related to the charges from agencies that we're starting to work on. So yes, that's us, but it's the agencies as well. It's a demonstration, a refinement of the technique [of public engagement] as a research tool and as a policy information tool. (Anderson Interview 11/18/15).

The real value of the deliberative public engagement work, then, might come in the future. It may come from this cadre of social scientists trained not only in public engagement but the *real politick* of science policy that Kristyn Lee saw them lacking. The National Emerging Technologies Conference in particular has led to long-term, ongoing work through its Science and Society Center work. Its work has created a certain momentum around societal implications research and public engagement. Additionally, there have been and continue to be many smaller events, particularly around other sciences like synthetic biology. One interviewee said that I really had to look "beyond nanotechnology" to really see what happened with public engagement. It is beyond the scope of this project to look deeply into public engagement in these other scientific arenas. A cursory inquiry, however, does not indicate anything like the large deliberative events that happened around nanotechnology, nor the associated hope that such engagement would democratize science. Furthermore, Elijah Anderson above was something of an outlier in his answer to that question. Most of those whom I interviewed did not have the sense that public engagement might significantly inform the course of scientific development or science policy. In fact, most seemed quite disillusioned about the influence of their work on the NNI.

Conclusion

Upstream public engagement in science was touted by its supporters as the answer to problems of public trust in science and more democratic science. Given the history of science and science policy, it also represented one of the largest expansions in ideas about acceptable science policy. Nanotechnology was seen as a unique opportunity to apply the theoretical tenets of public engagement to an emerging technology (MacNaghten et al. 2005). This chapter is not intended to take away from the value of public engagement to improving the relationship among scientists, science agencies, and publics. This is not the same as fostering public trust, which puts the onus on the public to trust technoscientific development seemingly irrespective of what it does (Wynne 2006). Nor is this chapter intended to question the value of upstream public engagement for democratizing science and science policy.

Rather, this chapter intends to show some of the limits of upstream public engagement in practice and the ways that discourse can be disciplined, even passively, by the institutions upstream. Discourse was disciplined first by the selection of academic social scientists to run deliberations. In a different way than the Human Genome Project's ELSI program, scholars were enrolled to reflect critically on nanotechnology but to do so within certain limits. Nanotechnology was framed by these events in either a positive light or a critical one, but never would an idea like deferral been made a matter of deliberation. The publics enrolled into these events were constructed to reflect the rationality of publics writ large for democratic engagement in issues of technoscience and science policy (Burgess and Longstaff 2011). To do this, organizers of these events described these publics in ways similar to the Rawlsian perfect deliberator: rational, willing to listen and learn, and ultimately arguing based in scientifically established facts. While not necessarily a bad thing in itself, this approach does leave out many real, existing publics who could potentially want to be engaged with issues related to nanotechnology.

Finally, public engagement, and the discourse that emerged from it, was constrained by the fact that science agency administrators and policymakers had the ultimate power to decide whether or not to make use of its products. Dietram Scheufele, quoted earlier, has argued that upstream public engagement has not fulfilled its promises, and perhaps promised too much (2011). David Guston, as mentioned earlier, argues that it is impossible to tell how much of an effect public engagement might have had, especially if we look narrowly at science policy as the only acceptable outcome (Guston 1999, 2014). Regardless of whether or not upstream public engagement has little effect or an important, long-term effect that it is difficult to identify, it will suffer from the constraints imposed by the very fact that it is organized by upstream actors. If too highly constrained, upstream public engagement risks reinforcing the deficit model and putting a veneer of democracy over otherwise technocratic modes of decision-making (Owen et al. 2012). With fewer constraints, public engagement may indeed realize much of its promise for democratizing science and science policy, but would risk fomenting public fear, giving voice to people from their echo chambers, and threatening the very endeavor that funds the exercise. It is hard to imagine science agencies, or any upstream funder, willing to tolerate those dangers.

CHAPTER V

Conclusion

This dissertation shows three cases in which a combination of internal and external pressures led scientists and science agencies to imagine and include new non-technical concerns, non-scientific actors, and new forms of expertise to existing modes of science policy and governance. These changing ideas about the relevant concerns of technoscientific research, the role of publics in science policy, and forms of expertise that scientists and science agency administrators deemed relevant aggregated into new imaginaries about how science acquires its social legitimacy. I argue that scientists and administrators at science agencies were motivated to imagine new ideas about concerns, publics, and legitimacy because of a tension between their desire to maintain scientific autonomy and the imposition of civic epistemological pressures to do science in a manner consistent with public needs and values. Scientists and science agencies wanted to retain as much autonomy as they thought possible. This took different forms for the different cases studied here. This autonomy came into tension, however, with the public expectation of not only safe science but science for the public good. This expectation by the public came from many sources, including past scientific crises, contemporary scientific activism in Europe, contemporary non-scientific issues and activism, and the work of scholars in various social science disciplines. Scientists and science agency administrators were forced, by this public pressure, to reimagine relevant concerns, the role of publics, the limits to their own autonomy, and the nature of scientific legitimacy given the power of the civic epistemology. In each of these cases, scientists and science agency administrators learned from the past and from what was going on elsewhere to renegotiate the tension between their autonomy and the responsibilities of science to the larger society. In each case, the result was for scientists and science agencies to recognize new kinds of expertise and to share authority with a new set of experts. In so doing, scientists voluntarily gave up a degree of autonomy to protect the autonomy they thought still possible to retain. These changes are summarized in Table V.1.

Table V.1 revisits Table I.2 set out in the first chapter, which set the analytic categories of this dissertation research. In Chapter I, we were only setting the terms of comparison. Now we can see how each of these cases played out according to these categories. Since we now have the stories of each case, I have two columns, one in the imaginary, showing the imagined basis of the social legitimacy of science, and another showing the outcomes of these events in terms of autonomy and the public response to the upstream efforts of scientists and science agencies. Note first that outside pressures and the outcomes of previous endeavors have forced scientists and science agencies to negotiate their scientific autonomy and authority with non-scientific actors, particularly publics. The public backlash against Asilomar, for instance, led the Human Genome Project to make changes in their approach. Second, this process has been towards greater inclusion, at least nominally. The imaginary around relevant concerns, the relevance of publics, and the relevant forms of expertise has steadily expanded. All of these changes were motivated, in part, by the perception among scientists and science agencies of outside pressure in the form of possible public backlash. It was only for rDNA research that scientists perceived no significant public backlash, which led them to underestimate the publics' response. A useful follow up to the research in this dissertation would compare the ELSI style programs and upstream public engagement work for research initiatives with no expectation of public concern or backlash. Finally, we see in these imaginaries a changing idea about the social basis of legitimacy for science in society. Scientists doing rDNA research reflected the idea that doing safe science was the extent of their responsibility to the public. This was consistent with ideas about the social contract of science, in which scientists were supposed to produce innovative new technologies but were otherwise left alone (Guston and Keniston 1994). This imaginary about the social legitimacy of science expanded to include scholarly non-scientists and eventually lay publics. The real and imagined consequences of their efforts to evidence the social legitimacy of scientific research have led scientists and science agency administrators to reimagine the role of science in society. This is

Table V.1 Cases and Analytical Categories Revisited

		Dominant Imaginaries					
	Pressures	**Relevant Concerns**	**Relevance of Publics**	**Relevant Expertise**	**Basis of Social Legitimacy**	**Autonomy**	**Outcome**
rDNA / Asilomar	Concern about science and safety but no direct pressure	Laboratory safety	Uninvited. Deemed irrelevant.	Scientific expertise	Doing safe science	Full scientific autonomy	Public backlash.
HGP/ELSI	Asilomar's public backlash. Internal disagreement. Broader anxiety about big and expensive science	Societal and ethical. Techno-cratically defined	Uninvited but deemed a relevant stake-holder.	(1) Scientific and (2) controlled social and ethical expertise	Identifying and minimizing negative impact	Politically-negotiated scientific autonomy	Science unimpeded but scholarly criticism from social scientists.
NNI/SEIN	Public backlash against GMOs in Europe and scholarly criticism of HGP's ELSI program	Societal and ethical. Broadly defined	Invited but controlled. Only unbiased or average publics.	(1) Scientific (2) Free social and ethical, and (3) Public's expertise	Incorporating public concerns into scientific development	Politically and publicly-negotiated scientific autonomy	Science unimpeded. Public uninterested. Scholars supportive.

important to note, since their imagination work around emerging technologies shapes, to a significant degree, what is researched, how it is researched, and the role of publics in the process of scientific development (Frickel et al. 2010).

This imagination work was also important for emerging areas of technoscience for what they indicate about power. Scientists, by virtue of their temporal priority to these areas, get the first chance to perform this imagination work. If this imagination work is an exercise in future-making, consistent with Jasanoff's observations about the co-production of "is and ought," then scientists get the first chance to make the world in which these new technoscientific areas emerge (Ezrahi 20120, Jasanoff 2012:16). That is the power associated with vanguard visions (Hilgartner 2015). As we saw in the example of the Asilomar conference, this is not a monopoly power. Bioscientists working in rDNA research tried to retain full control and authority over the world of rDNA research, full autonomy in short, by addressing all possible concerns before the public got interested in rDNA research, but they failed to do so. Furthermore, in so doing, rDNA researchers inadvertently made themselves responsible for the future of rDNA research and became the focus of people's concerns. The inclusion of journalists, supposed to satisfy the requirements of transparency and public witnessing demanded of scientific development in a democratic society, did not satisfy publics' concerns (Ezrahi 1990). Publics wanted more than just transparency. They wanted research that was both safe and, on the whole, oriented towards the public good. Scientists realized too late that they were not in a position of power relative to these concerns. Public concerns about rDNA research turned into bureaucratic, political action in nine towns in the U.S. and in Congress, who took the power of the imaginary around rDNA research out of the hands of scientists. I will not recount each of the cases, but Asilomar set the stage for both the Human Genome Project and the National Nanotechnology to engage in re-imagining the role of concerns about science, the role of publics, and especially the degree of scientific autonomy that was possible.

The failure of rDNA researchers to retain authority over their field raises questions about who gets to participate in the imagination work, the vanguard visions, around emerging technologies. Since these imaginaries can become contested and adversarial, it often boils down to questions of power. To what degree do those in an emerging technoscientific area have the monopoly power to imagine their field and the modes of governance to which it subject? As we saw with these cases, they do not enjoy monopoly power over these visions. The question then becomes, who can they enroll to legitimize their imagination? This question comes second because of the temporal primacy afforded to scientific insiders to an emerging technoscience. This affords them the opportunity to enroll allies before most outsiders would know that a new technoscientific area was even emerging. In effect, enrolling sympathetic outsiders to participate in imagining relevant issues and risks is a way of retaining authority and autonomy. Through this enrollment, insiders get to determine, to an extent anyway, the actors who will scrutinize them. Finally, who can insert themselves uninvited? This last question refers, most obviously, to social movements and civil society actors but might also refer to the NASA fanboys that Johnathan Parker described in Chapter IV (Epstein 1996, 2000, Hess 2010a, 2010b, Kinchy 2012).

The bioscientists in the rDNA controversy learned from their experience when it came time to develop the ELSI program of the Human Genome Project. They could not effectively claim the legitimacy to define and deal with rDNA's uncertainties. As the attorneys present at Asilomar had tried to warn them, the public did not trust them to serve as a one-stop shop for scientific development and governance. In the end, publics had inserted themselves uninvited. Thus, when it came time to start the Human Genome Project, James Watson expanded ideas of acceptable science policy to include ethical and societal concerns and new actors, seemingly to avoid the problem of publics inserting themselves. These were new actors enrolled, in part, to do the imagination work around the implications of knowing the human genome. Having learned from Asilomar, Watson understood that they did not have monopoly power to imagine their own regulatory world into existence free from outside influences. Put another way, he understood that a practice of public reasoning, through a state-sanctioned science agency, was the new normal for large, federally funded projects like the Human Genome Project. In his capacity as Director of the Human Genome Project, he was both a scientific insider and one of the most important bureaucratic actors for this new effort of public reasoning around genomic research. He had the power to enforce his own imaginary but seemed to understand that outsiders had to be included.

For this dissertation, the inclusion of outsiders was the most significant way in which the ideas about acceptable science policy had been expanded. Watson made sure to enroll outsiders who accepted the limits he set on the possible imaginaries for regulating genomic research. For him, the only real limit was to avoid repeating the discourse about a deferral of research, which he saw as the key mistake of Asilomar. When Francis Collins took over the Directorship from Watson, however, the limits to the ELSI working group and its power to direct its own imagination work about the ethical and societal concerns of mapping the human genome became more evident. Collins used his power to set budgets and his authority to attend meetings to micro-manage the working group. Collins made sure that it only studied the things he found important. Collins' imaginary about the ethical and societal issues of the human genome trumped the new experts in ethics and social science that had been enrolled into the project. Recall, too, that the working group was supposed to direct the course of rest of the ELSI program, in effect, through its imagination work. Controlling that group went a long way to controlling the ELSI program as a whole. In the National Nanotechnology Initiative, this process of selection was subtler but more streamlined. Years of scholarship in science policy and the ethical concerns of science and technology gave credibility to the scholars whose Science and Society Centers were eventually funded. Those scholars had a great deal more freedom than the social scientists enrolled for the Human Genome Project, but it was clear that they would not be discussing the gray goo or a nanobot terror with the publics they engaged.

This intersection of power and imagination work points to a larger point of this dissertation about the tension between expanding science policy and containing that expansion. In each of these cases, we see an expansion. Scientists in the Asilomar conference came, often reluctantly, to accept the idea that they had a moral and ethical commitment to public health and safety even in doing their laboratory research. They adopted the idea, new to them, that deferring otherwise sound scientific research was an acceptable way to approach these concerns. They accepted these new ideas, in part, to retain their own autonomy and their authority to determine the course of rDNA research. In other words, it was an expansion motivated by a desire to constrain a potentially larger expansion, with the inclusion of non-scientists and non-allies. The Human Genome Project's ELSI program expanded to include new actors, social scientists and ethically minded scientists, and institutionalized the idea that technoscientific projects should deal with ethical concerns and societal implications in addition to health and safety concerns. Here, again, we have new ideas and new actors. The discourse around these new ideas, particularly ethical concerns and societal implications, was disciplined by Watson when he enrolled people to serve on the ELSI working group and by Collins when he began directing their work. They also took a technocratic approach to these quite non-technical concerns. Watson and Collins, particularly, limited what was possible discursively, what I have been calling "disciplining discourse." In terms of public reasoning, they got to enjoy the legitimacy that came from having such a group while not necessarily having to do anything different in terms of the larger project, other than spending money on an ELSI program (Fisher 2005, Lopez and Lunau 2012, Rabinow and Bennett 2009). Many scholars have criticized that ELSI program for being ineffective. Finally, with the National Nanotechnology Initiative, we see another expansion. For the NNI we see ethical and societal concerns institutionalized and a more established role given to the actors that had been new for the Human Genome Project. The real expansion, however, came with the inclusion of lay publics in processes of policy- and decision-making. The ethical and societal concerns discourse under the NNI was disciplined in far subtler ways than for the Human Genome Project. These ways were not altogether intentional. The realm of possible discourse around societal concerns was shaped early on by discarding ideas deemed by scientists to be science fiction. It was later disciplined by the selection of social scientists to participate, the selection of publics to engage, and by the power of science agency administrators to listen, or not, to what publics had to say. With every expansion came a concomitant constraint on what it was possible to imagine and discuss in terms ethical concerns and societal implications.

Finally, I want to point out why scientists and science agencies embraced these expansions in the first place. In every case, they were motivated by the threat of potential public backlash and even state action following such a backlash. For rDNA, bioscientists feared that they might create a tumor-causing virus. Later, they were concerned that the public might discover the possibility that they would create a tumor-causing virus and shut down their research. For the Human Genome Project this is less clear. Watson was clearly motivated by a desire to avoid what had happened for rDNA research. This was likely made more important, however, by the fact that the project emerged under internal conflict among bioscientists who did not support the project. Already on unstable ground internally, any amount of serious public opposition early on would have constituted a serious threat to the Human Genome Project. Finally, people in the National Nanotechnology Initiative talked a great deal about the importance of its Societal and Ethical Issues in Nanotechnology program. Nothing beyond laboratory safety was funded or approved, however, until administrators in the NSF began to fear a serious public backlash. When that fear was shown to be overblown, public engagement in nanotechnology seemed to disappear. As people interviewed for this project seemed to suggest, it is only when scientists or science agencies perceive a threat to a scientific endeavor that they will expand what they take to be acceptable science policy to include new ideas like ethical concerns and new actors like publics. That depends, however, either on the imminent threat of public concern or at least the perception among scientists and those in science agencies that such a threat is imminent. I think this is why public engagement has moved from nanotechnology to fields like synthetic biology and geoengineering (Buck 2009, Rabinow and Bennett 2009). These emerging fields seem strange and threatening. Do they pose the most important pending ethical issues, however? Geoengineering is purely an imagined field at present. You do not see public engagement work around artificial intelligence and robotics or cyber-terrorism, technological issues with far more immediate consequences than either synthetic biology or geoengineering. I think the reason is that even though people have a lot of concerns around artificial intelligence and robotics, for instance, calls for a boycott on research or commercial applications of these technologies have not been socially amplified (Pidgeon et al. 2003).

All of this points to the fact that an upstream focus on ethical and societal concerns and public engagement in science will likely have serious limitations. Every expansion in ideas about acceptable science policy was met with efforts to constrain and discipline the discourse that comes out of these efforts, containing the changes that might otherwise result from those newly expanded boundaries. From an institutional perspective, the primary motivation is not more democratic science but the protection of the research project in question and scientific autonomy more generally. This is not to say that individual scientists are not themselves committed to doing responsible research and, potentially, even more democratic science. The initial motivation for rDNA researchers was to do the right thing ethically. Rather, I think that institutional logics and values will ultimately drive and constrain these endeavors. With respect to public engagement, Jurgen Habermas envisioned a more communitarian model for creating social change (1991). It was intended to replace, in an ideal world anyway, the conflictual models of social change evidenced in the anti-Vietnam war movement, the Women's Liberation Movement, and the Civil Rights Movement. Habermas' model has been critiqued in a number of ways (Bohman 1996). I will only enlist that of Chantal Mouffe, however, who uses Carl Schmitt's friend-enemy distinction to say, in part, that people should not expect their enemies to create appropriate conditions for deliberation such that their views will be heard and respected (Mouffe 2000). We should not trust upstream ethical bodies or public engagement exercises to serve as a one-stop shop for dealing with issues of scientific ethics or even imagining what issues and what areas of science might be important for doing science democratically.

What would all of this indicate for future large, federally funded research projects in emerging technologies like the Human Genome Project and the National Nanotechnology Initiative? Whether or not we will see continued upstream attention to ethical and societal concerns and upstream public engagement or the development of new modes of dealing with issues and publics depends on three elements: (1) the degree of learning among such initiatives, (2) public pressure on the initiative, and (3) the vanguard visions that scientists and science agencies have about public concerns, the role of publics, and the autonomy that scientists can reasonably expect and hope to maintain.

As each of the cases of this dissertation illustrate, federally funded science borrows from past projects in refining their approaches to both issues of concern and to publics. These projects need not share substantive similarities. The Human Genome

Project and the National Nanotechnology Initiative had very little in common in terms of science. They had a lot in common, however, in terms of being large-scale projects that courted potential controversy. Furthermore, none of the projects studied in this dissertation emerged in a context of strong public controversy. Only the Human Genome Project might be said to have had any real public knowledge of the research initiative. Rather, the administrators of each were responding to the controversies of past projects and an imagined future controversy. I would argue that the culture around science and science policy has shifted in the U.S. such that it is now incumbent upon large, federally funded science projects to give attention to ethical and societal implications. The 2014 Strategic Plan for the Brain Research through Advancing Innovative Neurotechnologies (BRAIN) Initiative, for example, has a section dedicated to “neuroethics” which is explicitly described as an ELSI program (NSTC 2014:13). This is true even for research that does not have a single umbrella initiative like BRAIN or the National Nanotechnology Initiative. Research into synthetic biology and geoengineering, for instance, have borrowed from the National Nanotechnology Initiative in their approaches to ELSI issues and publics (Buck 2009, Parkhill and Pidgeon 2011, Rabinow and Bennett 2009). Attention to ELSI concerns have become, as a scientist once observed to Human Genome Project ELSI working group member Jonathan Beckwith, a “political tax,” if nothing else (Beckwith 2002:116). Smaller federally funded research projects might not have to organize a full-fledged ELSI program. Nonetheless, they will likely have to show some attention to ethical issues and public concerns. Even individual grant recipients must attend to the “broader implications” of their research. The Nanologue public engagement event described in this research, for instance, was motivated by scientists who had to fulfill the broader implications requirements of their NSF grants (Harris Interview 10/22/2015). Although, as we have seen, scientists can meet the broader implications requirements in a number of ways that do not require public engagement or attention to ethical issues. Often simply making data and findings widely available is enough to satisfy broader implications. For the near future, some kind of ELSI program will almost certainly accompany any major federally funded research.

The imagination work of the scientists and science agencies involved in an area of research shapes whatever ELSI style programs or public engagement that follows. While something like an ELSI program has become the norm, such a program may not engage publics or give authority to the new experts (e.g. social scientists and ethicists) that the program enrolls. Scientists and science agency administrators will likely base their ELSI programs on what they perceive as the likelihood for publics to become concerned and their dependence on the initiative for funding their research or their field as a whole. If scientists and science agencies imagine the public as very likely to become fearful about their research and if they see themselves as more dependent on a particular initiative for their funding, the more likely scientists and science agencies are to engage both issues and publics in an upstream manner. They would take this view because they would already perceive the possible public response as a threat to their autonomy and the future of their research. While an upstream ELSI program that engages publics and enrolls non-scientific experts diminishes a certain aspect of scientific autonomy, particularly scientists' role as the only relevant voices in the initiative, it protects scientific autonomy in a larger sense, by allowing potentially controversial research to move forward.

Table V.2 Expected Approaches to Ethical Concerns and Publics

	Low Dependence on Initiative by Scientists	**High Dependence on Initiative by Scientists**
Imaginary of High Public Concern	• ELSI • Researchers move to other initiatives or areas of research • Broader Implications grant requirements	• ELSI • Upstream Public Engagement (Consensus Conferences, Deliberative Exercises, Town Halls) • Broader Implications grant requirements
Imaginary of Low Public Concern	• Technocratic approaches • Broader Implications grant requirements	• ELSI • Technocratic approaches • Broader Implications grant requirements

Remove or diminish any one of these elements, however, and I argue that it will be less likely that scientists and science agency administrators will voluntarily engage publics and empower non-scientific experts. If they imagine public concerns to be low or non-existent, then there would be little reason to diminish, voluntarily, their autonomy to make space for non-science experts and publics. Larger research projects would still likely have an ELSI style program, but non-scientific experts would likely have little authority and autonomy, as we saw with the Human Genome Project. Decision-making about what issues to pursue and how to pursue them would likely take a more technocratic form. By this, I mean that the approach would resemble that of rDNA research in which scientists would deal with technical issues of health and safety but give little to no attention to larger issues like ethical concerns, societal implications, and public response. Scientists would keep to a technocratic approach because it allows them to retain greater autonomy and authority over their work compared to programs that include ethicists, social scientists, and publics. If scientists are not dependent on any particular research initiative for funding or if any particular initiative is only one among many, then scientists might decide to reframe their research and look elsewhere for funding. This would be the opposite of what we saw for nanotechnology, in which scientists reframed their research as nanotechnology to pursue funding. The initiative may still have an ELSI style program attached to it and scientists would still have to fulfill their broader implications requirements, but they would have little reason to reduce their autonomy to incorporate non-scientific experts and publics. Finally, if scientists imagine public concerns to be low and they also do not depend on that particular initiative for their funding, then I think we can expect to see little in the way of an ELSI program or upstream public engagement. To whatever degree scientists choose to deal with the non-technical issues of an initiative, they will likely take a technocratic approach. Otherwise, they will rely on the broader implications requirements of their grant to evidence their commitment to doing responsible research.

Regardless of whether a future research initiative takes a more technocratic approach or gives a role to non-scientific experts and publics, the initiative will constrain the latter to fit with its primary goals. Upstream public engagement and attention to societal and ethical concerns can only ever be an addendum to the main goal of the research endeavor: to produce scientific knowledge. Individual research projects have often been postponed or relinquished, particularly in the medical, social, and behavioral sciences because they work directly with people and their bodies. This is usually the result of institutional review board (IRB) determinations but sometimes due to the decisions of researchers themselves (Fox and Swazey 2002). Large-scale research initiatives are less likely to get postponed or relinquished, at least due to upstream ethical research and public engagement. One reason is that by the time they reach the point of becoming a large-scale research initiative they have usually undergone a lengthy review process that includes the initiative's political viability. Both the Human Genome Project and the National Nanotechnology Initiative took years to reach the point of a major initiative. Second, and more in line with the data from this dissertation, administrators for these initiatives will ensure that the ethicists, social scientists, and publics whom they enlist will be favorably disposed to the research initiative. If they are not so disposed, then administrators will find ways to contain their views or their impact.

While upstream research into ethical issues and societal implications and upstream public engagement are moves in the direction of more responsible and more democratic science policy in the United States, they should not be seen as a replacement for civil society organizations, citizens' groups, and other kinds of social activism around science and science policy. Indeed, these upstream modes of engaging publics and ethical and societal concerns only emerged because of the threat of more adversarial, grassroots forms of public engagement in science. Scientists and science agencies are likely to enlist publics and non-scientific experts only if they continue to feel pressure by the latter group to do so.

APPENDICES

Appendix A

Descriptions of Nanotechnology Public Engagement Events

There were four deliberative public engagement events funded by the NNI, either directly, in the form of an NSF grant, or indirectly, through one of the Centers for Nanotechnology in Society or through one of the Nanoscale Science and Engineering Centers. A fifth, called the Southwestern Nano-Dialogue, was funded through its host university, not the NNI, but was initially motivated by scientists doing research on nanoscale technologies who needed something to fulfill their “broader impacts” obligations for NSF grant funding.

How do deliberations usually take place? Deliberations usually begin with some informational session to provide a framework for the discussion and to ensure that all deliberators operate with a similar foundation of knowledge. Sometimes this is sent out to participants ahead of the event, in the form of reading materials, videos, and surveys, and sometimes the event begins with lectures that provide this foundation. Deliberations usually follow afterward with varying levels of structure to them. Some have structured questions that deliberators are supposed to discuss. Others have a more open format, allowing deliberators to discuss whatever comes to their minds. Finally, participants are often asked to vote on a set of prescribed issues or to produce a final statement or report of the group’s deliberations. Usually the participants discuss and vote until they achieve a consensus. In fact, these events often go by the name “consensus conferences.” Each of these stages – information-provision, deliberation, voting or reporting – may be repeated several times for any given event.

Southwestern Nano-Dialogue

Although Southwestern Nano-Dialogue does not perfectly fit the other cases here, because it was not funded through the NNI, it provides an interesting contrast to what came later. Since it was not funded through a science funding agency, it did not have to make justifications back to funders or provide any deliverables. Southwestern Nano-Dialogue was the only deliberative event that was not organized primarily as a research endeavor but for engagement and interdisciplinary collaboration. Its host university was satisfied that the event would serve as an opportunity for collaboration between social scientists and scientists. The university had an explicit commitment to such collaboration and Southwestern Nano-Dialogue was a valuable opportunity to evidence that commitment publicly. In terms of publicity, it was an incredible success. Victoria Harris, a co-organizer of the event, estimated that roughly four hundred people attended, including the Mayor, who gave the formal introduction to the event. This makes Southwestern Nano-Dialogue the largest single deliberative event around nanotechnology. Southwestern Nano-Dialogue stands out in several other respects as well. First, instead of asking participants their own hopes and fears about nanotechnology, they were asked to role-play as another kind of social actor. Participants took on roles as academics, business-people, government actors, or general public. The only rule was that a participant was not supposed to play a role that they already had[47]. Second, the event broke from standard deliberative practice in that it was not supposed to produce a final consensus. The goal was the deliberation itself and to foster empathy among participants for those acting in other social roles. Finally, Southwestern Nano-Dialogue was probably the most overtly positive event about nanotechnology. Participants were asked to make decisions around a fictional viral outbreak, called the Pandora Virus, in which fictional "nano-vaccines" were presented as the best solution. The deliberations centered on how to best deploy the vaccine and the ethical concerns in doing so. The materials described the nano-vaccine in the following way.

> *After 5 years of work, last year, a famous interdisciplinary team of university nano-scientists collaborating with other research institutes successfully engineered a prototype of a kind of "nano-vaccine" to both treat and prevent the attack of some viruses. Their research was backed*

[47] Of course everyone could count as "the general public," whether they are also a scientist or policymaker. Harris acknowledged this fact as an aside when describing Southwestern Nano-Dialogue's format.

by grants from the National Science Foundation, the National Institutes of Health, and an increasing percentage of private sector funding.

To help, the Food and Drug Administration fast-tracked approval of the university nano-scientists' hybrid nano-vaccine that will help protect the country from a potential pandemic. As with the bird flu virus, where governments have already ordered that 150 million chickens be destroyed, millions of people could die if Pandora's virus spreads into a pandemic (Harris Interview 10/22/2015).

The positive focus was purposeful and was likely influenced by the relative newness of the collaboration between scientists and social scientists and, thereby, the role of the STS program as a boundary organization. Harris was the director of the STS program at that time and this was the first collaboration of its size at the university between scientists and social scientists. Although the scientists involved in the project initially joined because they had to fulfill the grant requirement of broader implications, she said that, in terms of the larger importance of public engagement, they were an "already sold group" who were "interested in a much better relationship between scientists and the public." This included framing nanoscience and the work of nanoscientists positively.

I think we had a fairly positive spin, because we wanted to get people involved in the excitement of learning about the science and becoming knowledgeable for active citizens, that was one of our…one of my main goals. I didn't want it to become a polarized situation where the scientists were kind of called to account. Rather, I wanted to draw on their good skills and enthusiasm for science. (Harris Interview 10/22/2015)

The social scientists involved in the Southwestern Nano-Dialogue event were just the kind of boundary actors that Roco had described in the *Societal Implications of Nanotechnology* hearing (2001). That is, they were mediators between science and the public and enrolled to foster public enthusiasm and trust. Interestingly, the event itself also mirrored this mediating role by asking participants to role-play as the very same set of actors that social scientists were supposed to mediate among and represent: scientists, business, government, and the public. In the end, Harris identified this boundary spanning role as ultimately detrimental to their STS program.

> *I probably should tell you too that the STS program got defunded in front of the budget cuts with the university and so we weren't really able to continue with what we had started and all the good work that we had done.*
>
> *We had gotten ourselves into the position in between social sciences and the sciences,*
>
> *kind of mediating, and I think that made us a bit vulnerable in hindsight. So, the liberal arts people thought "Well why should we be funding something that's helping the sciences?" And the scientists, they thought "Well in order [to do this work, social scientists need] to get trained in science or engineering or any of those fields, you know, and we don't have time to do that." You know [have time] to do much with social impact [work]* (Harris Interview 10/22/2015).

Doing boundary work can be costly. The Southwestern Nano-Dialogue event itself was a success, according to Harris, as indicated from participants' exit surveys and a few subsequent events that followed its format. Furthermore, the scholars who did the work framed themselves as just the kind of mediators for which Roco had advocated and did work that reviewers at the NNI would have likely supported. Ultimately, though, the program seemingly could not satisfy parties on all sides of the boundary they were spanning.

Midwestern Nanotechnology Deliberation

The 2005 Midwestern Nanotechnology Deliberation was the second major test on U.S. soil of the Danish Consensus Conference model of deliberation. The

Midwestern Nanotechnology Deliberation followed the standard model for deliberation. Participants had readings and presentations by scientific experts and then discussed, over the course of three weekends, the hopes and concerns around emerging nanotechnologies. Several of the organizers of Midwestern Nanotechnology Deliberation had previously participated in a European technology deliberation World Wide Views which brought lessons learned through that deliberation into Midwestern Nanotechnology Deliberation. The Midwestern Nanotechnology Deliberation was funded through its host university's Nanotechnology Science and Engineering Center. It had been planned and approved just prior to the establishment of the two Science and Society Centers and the host university for this event was already in the network of Institution A's SSC.

National Emerging Technologies Conference

The 2008 National Emerging Technologies Conference (NETC) was the first major deliberative event that came out of a Science and Society Center and had been the keynote public engagement offering in Institution A's original application for SSC funding. The National Emerging Technologies Conference was not strictly concerned with nanotechnology, but rather the "nano-bio-info-computational" (NBIC) interface. In other words, participants did not only learn about and discuss developments in nanotechnology. Rather, they discussed nanotechnology in terms of its intersection with these other technological innovations.

As an experiment in deliberation, the National Emerging Technologies Conference tested a modern update of the Danish Consensus Conference Model. It was conducted at eight participating universities using alternating face-to-face (F2F) meetings at each university and online, keyboard-to-keyboard (K2K) meetings where everyone came together at once. It addressed a criticism of the Danish model, that it worked in a country as small and homogenous as Denmark but would not translate to a country as large and heterogenous as the U.S.

Western Citizens' Risk Forum

The Institution B event was the most straightforward research project of the five. Through three consecutive deliberations, this project endeavored to test how certain demographic factors influence the act of deliberation itself. Organizers mainly focused on gender as their explanatory variable. Unlike the other events, organizers made a special effort to control for political leaning and education. It was also unlike the others in that it never framed itself to participants as anything more than an academic research experiment. This project promised no policy influence to its participants from the public and made no grand claims about democratizing science, despite the fact that the three organizers involved all described a deep personal commitment to this.

Eastern Environmental Risk Forums

This project was funded through one of the Nanotechnology Interdisciplinary Research Teams (NIRT). The NIRT structure was intended to fund interdisciplinary collaborations and pre-dates the establishment of the Science and Society Centers. This grant funded two different but interacting projects. One focused on laboratory scientists and their understanding of toxicology in the lab and beyond. The other involved public engagement work.

The engagement work endeavored to move beyond the practical and theoretical issues involved with constructing publics for the purpose of engagement. Instead of recruiting people to participate in an engagement event held at a university, the NIRT grantees took engagement to existing publics, or what one organizer dubbed "vernacular communities." Furthermore, instead of planning the event for these communities, they helped the communities plan their own events. One respondent explained that they went to the Elks Lodge and talked to them about having a nanotechnology deliberation. They then helped the group find speakers and acquire relevant materials to prepare themselves. From there, they let the deliberations unfold as they may.

Although I was able to interview five people who helped organize the EERF's engagement work, I found neither any documents from the NIRT project nor any publications. For each funded grant, the NSF hosts a web-page that gives brief details about the grant and lists all publications associated with the grant. I read all of these for each of the four NSF-funded engagement events. All of the articles listed on the NSF's grant page for this project, though, concerned only the toxicology research with scientists. When I asked about this, interviewees commonly gave two answers. One was that the research did not have the results they had expected. There was little to no actual deliberation. The organizations that organized nanotechnology events, with the help of the NIRT team, listened attentively but passively. The events were little more than exercises in informal science education. They had hoped for more. The other reason was that the collaborative group doing the engagement did not work effectively with one another. This is not to say that they disliked one another. As Robert Wilson, one of my interviewees, explained:

> *When one develops interdisciplinary work, you know, I think that sometimes you can get these really fruitful synergies and other times …the links don't form super well and so it's interesting; sometimes I think you get the best stuff when you already have people who have been collaborating and then they pursue a project like this. In this case, we just kind of sort of found each other and tried to do this. I'm just not sure that, you know, we just totally jelled, and we were all busy doing other things and so on. So anyway, no need to belabor it. But it's not an accident that you didn't find publications.*

Unlike the other events, then, what I know of the Eastern Environmental Risk Forums and their work with vernacular communities comes solely from my interviews with participant-organizers and the grant description given to the NSF.

Appendix B
List of Future Nanotechnology Applications

1. Direct broadband interfaces between the human brain and machines, transforming work in factories, controlling automobiles, ensuring military superiority, and enabling new sports, art forms and modes of interaction between people.

2. Wearable sensors and computers enhancing individuals' awareness of health condition, environment, chemical pollutants, potential hazards, etc.

3. Robots and software agents useful for human beings, operating on principles compatible with human goals, awareness, and personality.

4. Individual learning more reliable and quickly.

5. Communication and co-operation possible across traditional barriers of culture, language, distance, and professional specialization, increasing effectiveness of groups, organizations, and multinational partnerships.

6. A human body more durable, healthy, energetic, easier to repair, and resistant to many kinds of stress, biological threats, and ageing processes.

7. Machines and structures constructed of materials with desired properties, with ability to adapt to changing situations, high-energy efficiency, and environmental friendliness.

8. Combinations of technologies and treatments compensating for physical and mental disabilities.

9. National security strengthened by lightweight, information-rich war fighting systems, uninhabited combat vehicles, adaptable smart materials, invulnerable data networks, superior intelligence-gathering systems, and effective measures against biological, chemical, radiological, and nuclear attacks, as well as instantaneous access to tailored information.

10. Expanded creative abilities for engineers, artists, and architects by a variety of tools and improved understanding of the wellsprings of human creativity.

11. Human welfare benefiting from the ability to control the genetics of humans, animals, and agricultural plants; widespread consensus about ethical, legal, and moral issues will be build in the process.

12. Outer space and the Moon, Mars, and near-Earth asteroids will exploited by means of efficient launch vehicles, robotic construction of extraterrestrial bases.

13. New organizational structures and management principles based on fast, reliable and relevant communication and information increasing the effectiveness of administrations.

14. Individuals will have improved awareness of the cognitive, social, and biological forces operating their lives.

15. Factories will be organized around CT and increased human-machine capabilities (intelligent environments) achieving maximum benefits of both mass production and custom design.

16. Increased yields and reduce spoilage through networks of cheap, smart sensors monitoring the condition and needs of plants, animals, and farm products.

17. Safe, cheap, and fast transportation thanks to ubiquitous real-time information systems, high-efficiency vehicle designs, and use of synthetic materials and machines fabricated from the nanoscale.

18. The work of scientists will be revolutionized by importing approaches pioneered in other sciences.

19. A transformation of formal education by a unified but diverse curriculum based on a comprehensive, hierarchical intellectual paradigm for understanding the architecture of the physical world from the nanoscale through the cosmic scale (Roco & Bainbridge, 2002)

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