

Pathways of Scientific Dissent in Agricultural Biotechnology

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Abstract

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Scientific controversies surrounding agricultural biotechnology have deep significance for the politics of food, the governance of technology, and the organization of public and private research. Debates over the ecological and human health impacts of genetically-modified crops not only reveal the high stakes of particular contested ‘facts,’ but also expose questions about the reliability of regimes of regulation and the public’s trust in the scientific community. Powerful economic, intellectual, and political institutions struggle to guide patterns of scientific inquiry that influence policies and practices for developing and deploying agricultural biotechnologies.

Scholars in the social studies of science have long recognized the role of scientific dissent in the production of knowledge, but few have explored its heterogeneity as a social practice. The highly politicized field of agricultural biotechnology presents an ideal site to explore this complexity. In this context, the practice of scientific dissent becomes a window into the negotiation of social order. This dissertation focuses on three case studies of scientific controversy in agricultural biotechnology that occurred in the late

1990s and early 2000s: David Quist and Ignacio Chapela's announcement that native landraces of Mexican maize had incorporated transgenic DNA fragments, presumably due to cross-pollination; John Losey and colleagues' finding that *Bt* corn pollen could harm monarch butterfly larvae; and Arpad Pusztai's announcement that rats fed GM potatoes developed physiological abnormalities.

Beyond contributing to the historical record of these controversies, this dissertation makes three theoretical claims about scientific dissent. First, the diverse practices of scientific dissent emerge as part of a pathway. This pathway reflects the predominantly *promotional* context of agricultural biotechnology, the appearance of *contrarian* science as a first spark of dissent, and the myriad challenges to the credibility of contrarian science. Contrarian scientists only become *dissenters* when they actively respond to those challenges. Second, the metaphor of science as performance reveals the significance of constructing publics to serve as audiences to scientific controversy. Third, when dissenters engage this metaphor self-consciously, they enact performances of *dissident science*, a form of scientific dissent that takes on an explicitly political character and challenges conventional relationships among scientists, publics, and politics.

TABLE OF CONTENTS

Table of Contents	i
List of Figures	vi
List of Tables	vii
Acknowledgments	viii
Dedication	xi
Chapter 1 Introduction	1
1.1 Taking ‘The Pulse’ of Scientific Dissent.....	2
1.2 Scientific Dissent as a Pathway and Performance	6
1.3 The Stakes of Scientific Controversies in Agricultural Biotechnology	9
1.4 Dissent, Democracy, and Expertise.....	20
1.5 Outline of Dissertation	24
Chapter 2 Diving Off the Shoulders of Giants.....	28
2.1 Situating Scientific Dissent Theoretically	29
2.2 Taking the Dive: Research Methodology	46
2.3 My Own Pathway	54

PART ONE: SCIENTIFIC APPROACHES IN CONFLICT

Chapter 3 Promotional Science: Workshop on Biotechnology for Horticultural Crops	67
3.1 Narrative	68
3.2 Stage Management.....	72

3.3	Characters -----	77
3.4	Audience -----	95
3.5	Summary -----	99
Chapter 4	‘Neutral’ Science: Gene Flow Conference in Mexico City-----	101
4.1	Narrative -----	102
4.2	Stage Management-----	109
4.3	Characters -----	115
4.4	Audience -----	121
4.5	Summary -----	124
Chapter 5	Embedded Assumptions in Promotional Science -----	127
5.1	Global Agriculture -----	128
5.2	Safety -----	133
5.3	Benefits-----	141
5.4	Progress -----	142
5.5	Control-----	145
5.6	Summary -----	151
Chapter 6	Case Studies of Contrarian Science-----	152
6.1	Chapela Maize-----	153
6.2	Losey Monarch-----	170
6.3	Pusztai Potato-----	183
6.4	Discussion-----	202

PART TWO: RESISTANCE

Chapter 7 Resisting Chapela Maize----- 212

7.1 Preface to Resistance: Transgenes and Mexican Maize in Science ----- 212

7.2 Negotiations with Nature about publishing----- 214

7.3 Intimidation by Mexican officials ----- 219

7.4 Challenges to Credibility----- 226

7.5 Formal Scientific Critiques ----- 235

7.6 Replication as Resistance, and Resistance to Replication ----- 251

7.7 Conditional invitation to GeneFlow conference ----- 253

7.8 Denial of tenure ----- 254

7.9 Summary ----- 257

Chapter 8 Resisting Losey Monarch----- 259

8.1 Resistance to Publication----- 259

8.2 Disparaging Remarks----- 261

8.3 Attacks on Methodology ----- 264

8.4 Attacks on Significance ----- 268

8.5 Connecting with the ‘Technology as Progress’ Narrative ----- 272

8.6 Monarch Research Symposium ----- 274

8.7 Summary ----- 279

Chapter 9 Resisting Pusztai Potato----- 281

9.1 Pusztai Removed from the Stage, Misinformation Takes the Stage----- 281

9.2 Enforced Silence and Suspension----- 284

9.3 Resistance in Audit’s Clothing----- 288

9.4	Lancet Publication -----	291
9.5	Summary -----	301
9.6	Conceptualizing and Mapping Resistance -----	302

PART THREE: RESPONSES OF DISSENT

Chapter 10	Expected Responses -----	313
10.1	Dispute Facts and Provide Additional Evidence -----	313
10.2	Recruit Additional Allies-----	318
10.3	Claims of Disciplinary Territory -----	324
10.4	Discussion-----	329
Chapter 11	Performances of Dissident Science -----	331
11.1	Black Canvas -----	332
11.2	Open Office Hours -----	339
11.3	Pulse of Scientific Freedom in the Age of the Biotech Industry -----	353
11.4	Summary -----	373
Chapter 12	From Agonistic Engagement to Dissident Science -----	375
12.1	Expose the Resistance -----	376
12.2	Make Politics Explicit -----	378
12.3	Expand the Network beyond the Scientific Community -----	379
12.4	Anti-Autonomous Boundary Work (Involve the Public) -----	383
12.5	Mimic a Social Movement-----	385
12.6	Shift the Intellectual Terrain of Debate -----	388
12.7	Discussion-----	392

Chapter 13	Conclusion	396
13.1	Major Claims	399
13.2	Collision of Worldviews	404
13.3	Dissertation as Performance	409
References		415

List of Figures

Figure 1: Scientific Dissent as Pathway and Performance-----	8
Figure 2. Hurdles to horticultural biotechnology -----	70
Figure 3: Distinguishing Approaches to Risk Assessment -----	327

List of Tables

Table 1: Categories of boundary-work-----	37
Table 2: Quist and Chapela's Maize Samples -----	156
Table 3: Heterogeneity of Resistance -----	306
Table 4: Agonistic Dissent vs. Dissident Science-----	392
Table 5: Comparison of Contrarian and Promotional Worldviews-----	405

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For Tonya,
my match

Chapter 1 Introduction

Scientific controversies surrounding agricultural biotechnology have deep significance for the politics of food, the governance of technology, and the organization of public and private research. Debates over the ecological and human health impacts of genetically-modified (GM) crops not only reveal the high stakes of particular contested ‘facts,’ but also expose questions about the reliability of regimes of regulation and the public’s trust in the scientific community. Powerful economic, intellectual, and political institutions struggle to guide patterns of scientific inquiry that influence policies and practices for developing and deploying agricultural biotechnologies.

Scholars in the social studies of science have long recognized the role of scientific dissent in the production of knowledge, but few have explored its heterogeneity as a social practice. The highly politicized field of agricultural biotechnology presents an ideal site to explore this complexity. In this context, the practice of scientific dissent becomes a window into the negotiation of social order. This dissertation focuses on three case studies of scientific controversy that occurred in the late 1990s and early 2000s. While these cases differ in crucial respects, which speaks to the heterogeneity of dissent, their commonalities reveal patterns that begin to outline scientific world-views that organize controversy in agricultural biotechnology.

Beyond contributing to the historical record of these controversies, this dissertation makes three theoretical claims about scientific dissent. First, the diverse practices of scientific dissent emerge as part of a pathway. This pathway reflects the predominantly *promotional* context of agricultural biotechnology, the appearance of

contrarian science as a first spark of dissent, and the myriad challenges to the credibility of contrarian science. Contrarian scientists only become *dissenters* when they actively respond to those challenges. Second, the metaphor of science as performance reveals the significance of constructing publics to serve as audiences to scientific controversy. Third, when dissenters engage this metaphor self-consciously, they enact performances of *dissident science*, a form of scientific dissent that takes on an explicitly political character and challenges conventional relationships among scientists, publics, and politics.

1.1 Taking ‘The Pulse’ of Scientific Dissent

In the fall of 2003, Ignacio Chapela, professor of microbial ecology in the University of California, Berkeley’s Department of Environmental Science, Policy, and Management, invited John Losey, Arpad Pusztai, and Tyrone Hayes to participate in “The Pulse of Scientific Freedom in the Age of the Biotech Industry” on the UC Berkeley campus. The four participating scientists had not been colleagues – they had never worked together nor provided informal or formal peer-review to one another; they came from different sub-disciplines of biology; they represented both public and private research organizations, four ethnicities, and three countries; and they had no plans to conduct research together. What brought them to the stage was their common experience of publicizing research that challenged the safety of agricultural biotechnology.

Between 1998 and 2001 these four scientists and their research became focal points in political controversies over the ecological and human health impacts of GM crops. Arpad Pusztai, a senior scientist at the Scottish Rowett Institute, conducted rat-feeding studies with a GM potato and announced disturbing changes in organ

development that were likely due to the *process* of genetic modification (see "Experiment fuels modified food concern" 1998; Ewen and Pusztai 1999a). John Losey and colleagues from Cornell University published results suggesting that *Bt* corn pollen could be lethal to monarch butterflies (Losey, Rayor and Carter 1999). Ignacio Chapela, along with his graduate student David Quist, discovered transgenic DNA fragments in native landraces of Mexican maize in Oaxaca, Mexico, and further analyzed patterns of genetic insertion that questioned the assumptions of precision and predictability of transgenic technologies (Quist and Chapela 2001). Tyrone Hayes, also a professor at UC Berkeley, found evidence that extremely low concentrations of atrazine, a popular herbicide manufactured by Syngenta, the transnational biotechnology corporation, caused severe deformations in frog development through the process of endocrine disruption (Hayes, Haston et al. 2002; Hayes, Collins et al. 2002). In short, these four scientists produced research with ecological and policy significance during a period when agricultural biotechnology was gaining significant political and economic momentum (Charles 2001; Gottweis 1998; Kloppenburg 2004 [1988]).

Not surprisingly, controversy erupted over the social implications of these scientific findings. Should people have stopped eating GM food? Was the European Union's de facto ban on GM crops justified? Could the public trust genetic engineers to filter out harmful technologies before they left the confines of the laboratory? Had Mexico's moratorium on planting GM maize failed, and if so, did it really matter? Would biotechnology threaten crop diversity and biodiversity? Why had regulatory agencies and biotechnology corporations failed to discover (or disclose) these unintended negative effects? What other hidden harms from modern agricultural technologies remained to be

discovered? Were these technological ‘downsides’ necessary tradeoffs to ensure adequate agricultural production to feed a growing global population? Such questions spanned the range of technical, political, and institutional concerns and tapped into historical themes of questioning scientific progress, globalization, and technological governance.

At the Pulse Event, the four scientists did not gather primarily on the basis of the *implications* of their research, but because of the challenges they faced in producing and defending their research and legitimacy as scientists. More precisely, they experienced comparable patterns of resistance to the veracity of their data, the soundness of their methods, their professional credibility, and their ability to participate as scientists in the political process of governing research and policy surrounding agricultural technologies. Their stories included personal intimidation, removal of research funding, professional reprimands, coordinated smear campaigns, ostracism and isolation, specious challenges on ‘technical grounds’, and unprecedented sanctions by respected public and quasi-public scientific institutions.

From these common experiences, a related set of questions emerge surrounding these scientists as ‘controversial’ researchers. Were these scientists legitimate, or did they *deserve* to be discredited? What had they done to justify or explain the severity of the resistance to their work? Why weren’t these scientists protected under institutional umbrellas of academic freedom? Shouldn’t we expect that scientific inquiry occasionally produces errors, but that these errors become corrected over time with ‘normal’ scientific conduct? Did their stories provide indirect evidence of a pattern of scientific suppression, or did their near-celebrity status bolster arguments that the freedom of scientific inquiry was alive and well? In what ways did these scientists navigate the turbulent waters of

controversy to land with portions of their scientific credibility intact? These questions generally address the phenomenon of scientific dissent as practiced in a politicized field of research.

While neither scientific dissent nor politicized science is new, the Pulse Event showcased novel formations of dissent that challenged notions of how scientists *should* defend their credibility. Scholars of the social studies of science have explored the important areas of struggles over fact-making (Fleck 1979 [1935]; Knorr-Cetina 1981; Latour and Woolgar 1986 [1979]), resistance to novel findings (Kuhn 1970 [1962]; Shapin and Schaffer 1985; Simon 2002), political and economic alignments between interest groups and research institutions (Kleinman 2003; Krimsky 2003; Proctor 1995), and the incorporation of the public (interest) into research (Jasanoff 2003; Shapin 1994; Winickoff, Jasanoff et al. 2005). Within this literature, dissent has functioned primarily as the obligatory *position* to prove the existence of scientific controversy. In my view, however, the *practice* of dissent has remained undifferentiated and invisible. This dissertation aims to expose not only the variability of opportunities for, and forms of, scientific dissent, but also the significance of that complexity for the production of knowledge. The case studies demonstrate how actors struggle within institutional frameworks to control and manage science as a source of social power. These struggles reveal identifiable and fascinating commonalities that begin to sketch a broad and relevant account of the complexity of dissent in scientific arenas with high political and economic stakes.

1.2 Scientific Dissent as a Pathway and Performance

This dissertation identifies and analyzes scientific dissent as a *pathway* rather than as merely an intellectual or political position. The pathway characterization emphasizes that issues of history, context, and choice determine forms of dissent. In order to understand various modes of scientific dissent, we have to pay attention to the broader political and social landscape for the production of science in a given field. Dissent arises against a more dominant flow within the scientific community. The case studies show dissent as a particular process of disagreement that operates at the border of heresy – one that challenges a more powerful (or even hegemonic) set of institutions, beliefs, resources, and practices.

The content of science that challenges vested interests informs, but does not determine, *performances* of scientific dissent. I engage a dramaturgical lens to highlight the relationships among scientific actors, narratives about research and policy, subtle controls of the context for scientific communication (stage management), and audiences (publics). Applying this framework throughout the dissertation suggests surprising continuities between a peer-reviewed scientific publication and a staged discussion such as the Pulse Event, or between a published scientific critique and a seemingly capricious professional reprimand. The performance aspect further emphasizes the active quality of scientific dissent – one does not dissent in solitude in a private diary, but within public fora and in front of constructed audiences. My representation of multiple performances, which speak to one another and have significant overlaps in terms of audience, reveals patterns within the pathway of scientific dissent.

At the nexus of pathway and performance, I begin the project of charting the mostly unexplored territory of scientific dissent as a complex response by scientists. These responses include behaviors and rhetoric aimed at diverse audiences within and beyond the scientific community. In a heuristic move to explore the continuum of responses, I distinguish *agonistic dissent* from *dissident science*. The former refers to performances of dissent that respect the conventions and norms of scientific communication in the struggle over facts. In contrast, I use the term dissident science to describe a special form of dissent that challenges conventional notions of defending the legitimacy of research and researchers. Dissident science explicitly acknowledges the politics within and around scientific controversy, and advocates for new relationships among scientists, the public, interest groups, and academic institutions. As such, dissident science incorporates intellectual struggle with social action.

Figure 1 illustrates the conceptual model that embodies this dual approach of pathway and performance. As all models do, it simplifies a much more complicated and interdependent reality, but serves as a heuristic tool with which to consider the historical and performative aspects of scientific dissent.

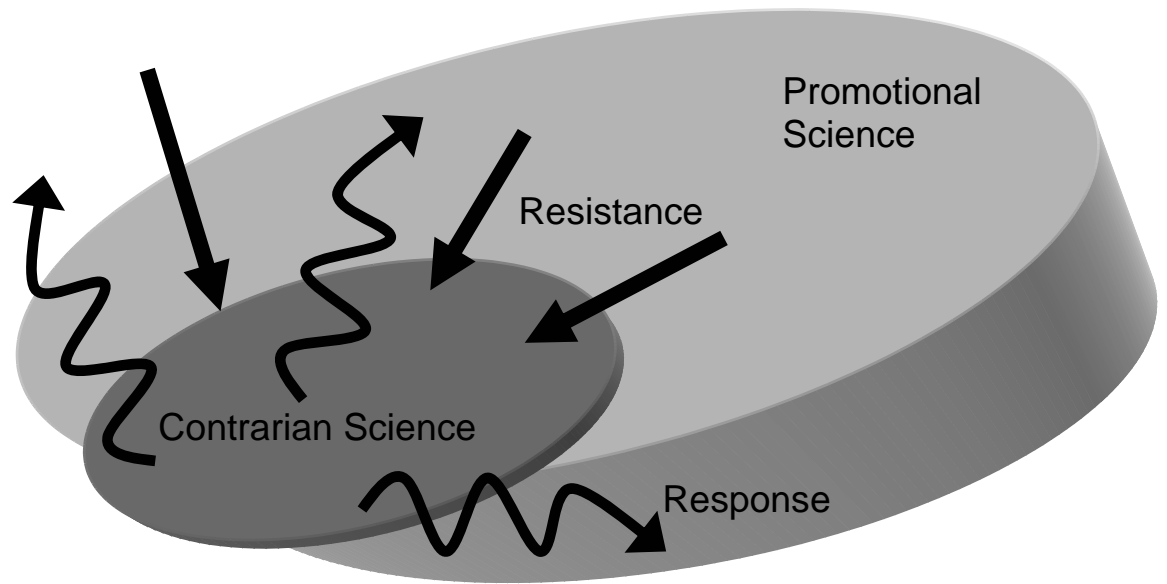


Figure 1: Scientific Dissent as Pathway and Performance

Promotional science is my term for the dominant discourse of mainstream science in agbiotech, science that promotes the research, development, and deployment of agricultural biotechnologies.¹ *Contrarian science* appears on this landscape as the first spark of dissent, in which some scientists begin to question facts, theories, and assumptions of promotional science.² Contrarian science provokes *resistance*, actions that seek to de-legitimize contrarian scientists and their research (e.g., published scientific critiques, denial of tenure). Actors and institutions resist the movement of a contrarian claim toward credible knowledge. Finally, the *responses* to this resistance represent the

¹ Promotional science would not necessarily characterize dominant flows in other domains of science. For example, in the field of human genetic engineering, the more marginal position of advocating for germline transformations in the spirit of eugenics might be called promotional, although occupying a space of dissent against the majority of scientists who oppose such research in favor of ‘gene therapy’ that does not affect cells with reproductive potential.

² In the arena of global warming, for example, scientists who currently argue that human activity has not contributed to an increase in the Earth’s mean temperature are contrarian scientists.

various modes of scientific dissent. These heterogeneous responses range from agonistic engagement to dissident science and emerge as performances for audiences – wide and narrow, restricted and open, professional and lay, private and public.

1.3 The Stakes of Scientific Controversies in Agricultural Biotechnology

Biology has taken on enormous technical status in its focus on the tools of genetics and informatics in the last thirty years, but controversies have continued to emerge that challenge the authority of biology to define bio-technological futures or to re-define the boundaries of nature and human artifice. Conflicts of interest around pharmaceutical trials have exposed how personal and financial interests can distort research approaches, interpretation of data, and publication of findings at both personal levels (research showing drug tests results are more favorable when conducted by a shareholder) and institutional levels (strategic partnerships that delay scientific publication, create private intellectual property, etc.) (Krimsky 2003). Stem cell research and cloning, which hearken back to the recombinant DNA debates of the mid-1970s, have raised the specter of science and technology out of control *a la* the Frankenstein monster. And questions about the environmental, social, and health consequences of genetically-modified (GM) food intensify the debates that arose in the aftermath of the Green Revolution – questioning the corporate control of agriculture, the role of technology in reducing poverty and hunger, and the potential for developing nations to go beyond food security and achieve food sovereignty (Kloppenborg 2004 [1988] ; Rosset 2003).

Scientific controversies in agricultural biotechnology thus have great practical and political significance. As nearly all supposedly ‘technical debates’ do, they engage diverse streams of social concern (e.g., food security, the privatization of science, biodiversity, and the autonomy of regional and national governance structures). While the resolution of the scientific controversies, per se, would not resolve these social concerns, the *discourses* of controversy make these concerns salient and clarify the ways in which the debates engage issues that transcend the domains of scientific knowledge.

Definitions

Agricultural biotechnology is the science and practice of manipulating agricultural organisms at the genetic level with the tools of modern molecular biology. This includes genetic engineering (the addition, subtraction, or modification of sequences of DNA at the molecular level), transgenic organisms (organisms with novel splices of DNA from another species), genetic screening/selection (the analysis of cells or organisms on the basis of specific sequences of DNA), DNA sequencing (the production of ordered lists of the nucleotide bases of portions or all of an organism’s genome), and cloning (the transfer of one organism’s genome into the nucleus of another cell in order to create a new organism with nearly identical DNA to the donor).

Arguments that foreground the continuity between agricultural biotechnology (as defined above) and technologies of agriculture that rely upon biological processes (e.g., conventional breeding, fermentation, cheese production) distract from the importance of

existing controversies.³ Such connections may be informative from a historical perspective – in the same way that code-breaking efforts during World War II informed the discipline of genetics (Keller 1995). Emphasizing continuities with general biological processes, however, serves rhetorically to transplant notions of safety and familiarity from traditional plant breeding to genetic transformation of plants, rather than providing insight into what agricultural biotechnology is today (see Kloppenburg 2004 [1988]). If anything, such efforts undermine the significance of modern developments in agricultural biotechnology, which deserve analysis and consideration as powerful and new technoscientific projects.

I use the term *agbiotech* to refer to the institutional matrix that promotes, develops, and deploys agricultural biotechnologies. *Agbiotech* emphasizes the linkages of science and society rather than serving as an abbreviation of the scientific sub-discipline of agricultural biotechnology. It thus includes boutique research firms, transnational corporations, public and nonprofit organizations that promote the use of agricultural biotechnologies, and individuals involved in the chain of research, product development, marketing, distribution, and management. In this framing, ‘agbiotech critics’ encompass those who base their concerns on the social, economic, political, ecological and/or philosophical aspects of agricultural biotechnologies. Although this distinction is rarely made by others, it serves the purpose of clarifying the stakes of opposition between ‘agbiotech critics’ and ‘agbiotech promoters.’ Agbiotech promoters do not categorically

³ For example, the Biotechnology Industry Organization’s website states: “Biotechnology is a refinement of breeding techniques that have been used to improve plants for thousands of years. The 20th century, in particular, saw the development and application of many new techniques to transfer genes between related and even unrelated species for crop improvement. Biotechnology is the latest in a long line of increasingly powerful tools for enhancing crops” (<http://www.bio.org/foodag/faq.asp#2>. Accessed 23 September 2005).

promote all forms of agricultural biotechnology research (e.g., long-term feeding trials to humans to assess health impacts or the development of pharming⁴); just as most agbiotech critics would not oppose research into the stability of transgenes or the nutritional differences between GM crops and their conventional counterparts.

The Political Economy of Agbiotech

Agbiotech has brought together powerful economic and political concerns. Gottweis (1998) argues that the United States, Britain, and Germany made key political decisions to pave the way for the development of domestic biotechnology industries as a means to benefit from the ‘next big’ economic engine. In the 1980s and early 1990s, these governments allocated significant public monies to research, minimized regulatory hurdles, and created incentives for corporate investment in research and development in biotechnology.

The resonance among these ‘public’ priorities, a frenzy of investment by venture capital firms in the 1990s, and a complex trajectory of acquisitions, mergers, and spin-offs in the ‘life sciences’ industry has resulted in agbiotech becoming a major force in the global economy (Clarke and Inouye 2002, 6-7; ETC Group 2005; Krimsky 1991).

According to the International Service for the Acquisition of Agri-Biotech Applications (ISAAA), in 2004 the global area of GM crops grew at an annual rate of 20%, the ninth consecutive year of double-digit growth. Approximately 8.25 million farmers in 17 countries planted 81 million hectares (200 million acres) of GM crops. These had a

⁴ ‘Pharming’ refers to the development of transgenic food crops that contain medicines or vaccines that are delivered by ingesting the crop as food (e.g. bananas that contain a hepatitis vaccine) or by extracting the compounds for conventional delivery. Some such products have been grown in field trials, but no experiments have yet included feeding the products to humans.

global market value of \$4.7 billion, representing 15% of the global crop protection market and 16% of the global commercial seed market in 2003 (ISAAA 2004).

The United States has led the world in investment and production of biotechnologies. At the end of 2003, there were 1,473 biotechnology companies in the U.S., and publicly traded biotech companies had a market capitalization of \$311 billion as of April 2005 (Biotechnology Industry Organization 2005a). According to the Pew Initiative on Food and Biotechnology (PIFB), U.S. farmers planted 105.7 million acres with GM crops in 2003, followed by Argentina with 34.4 million acres and Canada with 10.9 million acres (PIFB 2004). While the story of how this industry came to power remains beyond the scope of this dissertation, the scale and reach of the business of agbiotech informs any discussion of action that threatens the profitability of this industry (see Charles 2001; Kenney 1986; Kloppenburg 2004 [1988]; Rabinow 1996; Rifkin 1998).

The Privatization of Science

In parallel with the rapid growth of the agbiotech industry, concerns over the privatization of science are central to agbiotech controversies. Generally, critics address three areas of concern: transfer of goals from corporations to public research institutions; loss of an 'objective' voice when interested parties fund science; and enclosures of the intellectual and biological commons.

Scholars have noted the alignment of public research institutions with the goals of commerce. In *America by Design* (1977), David Noble traces the influence of strategic

needs of expanding industry with the development of university systems, especially the discipline of engineering. He writes:

The corporate reformers never required that all who pursued higher learning in America be conscious of the utility of their work, nor even that all such work be of ultimate utility. Rather, they created an institutional apparatus which would correlate the activities of academics 'behind their backs,' thereby rendering such consciousness of purpose unnecessary (p. 245).

Thus, the privatization of science can occur under the radar of consciousness of those actors and institutions being co-opted. A host of political and institutional changes during the past thirty years have accelerated such trends (for excellent reviews, see Kleinman 2003; Kloppenburg 2004 [1988]; Krimsky 2003), enticing critics to transfer the language of the 'military-industry complex' to the 'university-industry complex' (Kenney 1986). The recent controversy over the partnership between Novartis (a transnational biotechnology corporation, re-named Syngenta) and UC Berkeley's Department of Plant and Microbial Biology (a five year, twenty-five million dollar contract signed in 1998) revealed the complexity of this theme playing out in agricultural biotechnology. Proponents hailed the alliance as an opportunity to leverage private assets for the public good, in partial reference to the increased access academic researchers would have to proprietary genetic databases. Critics deplored what they viewed as an infiltration by corporate interests and personnel into a public university and accused administrators of negotiating the deal under a veil of secrecy (see Washburn 2000). A comprehensive academic study of the agreement suggested that while the worst fears of the critics did not come to pass, the agreement had negative impacts on the university and represents a risky trend in efforts to increase research funding (Busch, Allison et al. 2004).

A second concern stemming from the privatization of science involves the bias of research that is paid for by vested interests rather than neutral organizations. Historical examples include research on the dangers of cigarette smoking, product safety testing, and nutritional research. The increasing focus on disclosing conflicts of interest in scientific journals testifies to the widespread belief that the funding source of research can affect the results (see Worthy, Strohman and Billings 2002). A corresponding increase in the private funding of academic research makes this concern more crucial. Kleinman reviews several studies to estimate that private funding of total academic research and development rose from less than 3 percent in 1972 to between 8 and 12½ percent in the late 1990s, with industry providing over one-third of all funding for university research in biotechnology in the 1980s (Kleinman 2003, 44).

The question of the fidelity and strength of public institutions of science comes into focus when we consider relations between the issues of biased research and the university-industry complex. How does society protect a space for research in such a context, one that fulfills the public interest – both in terms of exploring questions of relevance to public good (as distinct from corporate profit) and conducting research independent of the oversight and funding of private sources? Even laboratories headed by academic researchers committed to maintaining independence from the influence of agribusiness can still find themselves constrained by issues of dependence and culture (Kleinman 2003). Kleinman’s modern laboratory ethnography reveals “how corporate domination of a field of scientific investigation early in its development can indirectly affect the questions that are asked and the answers that are acceptable at a later time, even if the later research is not funded by industry” (p. xi). The possibility thus emerges that

science conducted within the institutional boundaries of public research organizations will not reflect the ‘proper’ public bias.

Finally, the massive emphasis on and extension of intellectual property rights with respect to biological research has raised concerns about the enclosure of the intellectual and biological commons (Kloppenborg 2004 [1988]; Krimsky 2003; Schurman and Kelso 2003; Shiva 1997). Issues range from the right to patent forms of life, to the appropriation of indigenous knowledge, to the transfer of biological material from the developing world to industrialized countries without proper remuneration, to the increasing difficulty for public researchers to gain access to proprietary materials and technologies, to the slowing of technological innovation that accompanies dense ‘patent thickets.’ According to the ETC Group (2005), “The top ten seed companies control half of the global supply of commercial seed. The market for biotech seed traits (herbicide tolerance and insect resistance) has shot up from \$280 million in 1996 to \$4,700 million in 2004 – a 17-fold increase over the past nine years.” Agbiotech thus serves as a nexus for debate about efficient, just, and ethical regimes of protecting intellectual property.

Cascading Consequences – Bt Corn

The development and commercialization of agricultural biotechnologies have potential and realized impacts that touch upon diverse issues and constituencies. Detailed work has explored this arena from a number of different perspectives, approaches, and disciplines (Ferber 1999; Haraway 1997; Hindmarsh, Lawrence and Norton 1998; Ho 1998; Kloppenborg 2004 [1988]; Krimsky 2003; Lappe and Bailey 1998; McAfee 2003; Pollan 2001a; Rifkin 1998; Rowell 2003; Schurman and Kelso 2003; Smith 2003; Wright

1994). The following example of *Bt* corn's cascading consequences hints at the degree of complexity that emerges even around a single agricultural biotechnology.

Bt corn is a family of varieties of transgenic corn, genetically modified to express a protein made by the bacteria, *Bacillus thuringiensis* (*Bt*). This protein has been used as an insecticide because it dissolves the guts of lepidopteran larvae that ingest it. Organic farmers have used *Bt* in spray form to control for pests, and corporations such as Monsanto and Syngenta have incorporated this regime of pest control into the plant itself. The many varieties of *Bt* corn express the insecticidal toxin in most, if not all, parts of the corn plant throughout the growing season, making control of pests (e.g., European corn-borer, corn earworm, and southwestern corn borer) possible (Mendelsohn, Kough et al. 2003).

Advocates for *Bt* corn describe a number of potential benefits. For farmers, higher yields due to lower losses to pest damage and a simplified pesticide regime could translate into higher profits. In addition, if more toxic pesticides are displaced, the chain of persons involved in producing and applying pesticides experience less exposure. For consumers, assuming the safety of eating the protein, which has been shown to be harmless to mammals when produced by the bacteria, *Bt* corn could reduce the amount of residual harmful pesticides on and in their food. Increased yields could also potentially lower the price of corn. For researchers, *Bt* corn represents a proof-of-concept that agricultural crops can produce proteins they otherwise would not. This opens the possibility of exploring ways to genetically engineer food crops to produce other types of proteins such as additional nutrients, medicines, or industrial products. For those concerned about environmental consequences of agriculture, *Bt* corn could reduce

harmful pesticide runoff and slow the conversion of wilderness to agricultural land by making current farms more productive.

The critics of *Bt* corn describe a number of potential and actual problems with this agenda. For farmers using the technology, *Bt* corn may breed resistance within insect populations much faster than the historical use of *Bt* sprays. The constant presence of the insecticide creates strong selective pressure that favors survival of the sub-population of insects that can better metabolize the toxin. This could have severe repercussions for organic farmers who rely on *Bt* sprays as a last line of defense against insect invasions and who cannot use other pesticides without giving up their organic status (which brings them a premium in the marketplace) (Mendelsohn, Kough et al. 2003). Farmers also face higher input costs, with seed premiums and/or technology licensing fees that accompany GM crops. Thus, economic advantages may disappear depending on the actual levels of infestation and the costs of alternative, and more flexible, regimes of pest management (Benbrook 2003). In a sense, some of the expertise of farming has been incorporated into the seed as a package of technology – reducing the value of farmers’ knowledge. For consumers, the ingestion of the GM form of the *Bt* toxin is potentially more harmful than the bacterial form. The protein differs in subtle ways – for example, the GM form does not need to be activated by an insect’s gut to become toxic – and these differences could have health consequences in terms of toxicity or allergenicity. Consumers also lose some freedom of choice, given the difficulty in keeping GM corn segregated from conventional corn over time; consumers may object to eating GM food for religious or cultural reasons. For those concerned about the environment, the decrease of pesticide use is questionable over time, especially because many farmers do not regularly spray for corn

borers (Obrycki, Losey et al. 2001, 358). The GM version of the toxin also persists for a longer time in the agricultural ecosystem, partly from degrading more slowly and partly from being expressed in all parts of the corn plant including corn pollen and the roots, which together increase the likelihood of non-target effects on other insects or soil microorganisms. Lastly, the pursuit of *Bt* technology represents significant resources applied to a marginal solution to problems emerging from the paradigm of industrial farming – forgoing alternatives that might reduce pesticide use just as significantly, but which cannot operate within monocultural farming.

From a regulatory perspective, *Bt* corn raises a number of interesting issues. First, because of a political decision issued by the U.S. Office of Science and Technology Policy in 1986, the U.S. regulates GM products under the “Coordinated Framework for Regulation of Biotechnology” (51 FR 23302). This involves the various agencies with regulatory responsibilities for food and agriculture to oversee GM products under existing institutions of oversight – rather than creating a new, stand-alone agency with a specialized regime for regulating GMOs. As Michael Pollan described in a 1998 *New York Times Magazine* article, this had the bizarre consequence of both the EPA and the FDA absolving themselves of regulating *Bt* crops. The EPA claimed it could not regulate a *Bt* potato because it was food; the FDA claimed it could not regulate the same crop because it was a pesticide (Pollan 1998). Second, the EPA has wrestled with its role in regulating ‘refuges’ for farmers planting *Bt* corn. Within this context, refuges are areas of conventional corn (with no applications of pesticides) to protect at least some of the agricultural acreage from exerting selection pressure for resistance within an insect population. EPA’s response has been to issue a guideline, but one that is not enforced and

poorly followed. Third, the Starlink™ scandal in the late 1990s brought attention to the odd EPA decision to permit a version of *Bt* corn that had not fully passed regulatory muster for human consumption to be grown as feed corn. Contamination quickly followed, implicating not just the seed companies and the farmers, but the entire infrastructure of transportation, storage, and processing of grains. An expensive recall was undertaken by the company that produced Starlink™, but traces of the particular variety have continued to appear in products and shipments of grain ("Banned as Human Food, Starlink Corn Found in Food Aid" 2005).

Bt corn thus operates in a huge network of human and non-human actors (Callon 1999 [1985]), and issues of knowledge and power permeate this network. The stakes are incredibly high, given that there are strong political and institutional commitments to GM technologies, the technologies in question alter the human system for producing food, and the biological character of GM crops make them self-replicating technologies that cannot be recalled, and will not degrade over time in the environment.

This relatively limited discussion of the consequences and controversies around a single agricultural biotechnology suggests the range and depth of scientific controversy in agbiotech. The intensity of the policy and management debates shine light on the pathway of scientific dissent in this arena, creating the opportunity for this project to have significant theoretical and practical implications.

1.4 Dissent, Democracy, and Expertise

The political and intellectual tension surrounding scientific dissent in the 1990s and early 2000s lays bare the social task of reconciling the modern ideals of democracy

and scientific expertise. In the United States, especially, the discourses of democracy and science-based policy dominate political speech across the liberal-conservative spectrum. The rhetorical message is simple: science supplies the facts while democracy provides the values; rational and representative policy will emerge as long as these realms remain separated from one another both institutionally and sequentially. Seventy years of studying science as a social practice has shown, however, that science enjoys no such insulation from the domains of governance, economics, and culture (e.g., Fleck 1979 [1935]; Haraway 1997; Noble 1977; Proctor 1995). Increasingly, mainstream discourse has begun to adopt this perspective,⁵ exposing friction in our dominant policy narrative – namely, that democracy and expertise operate in tension by appealing to incompatible sources of authority (the masses versus the elite).

Scientific dissent brings this concern to the forefront by exposing arenas in which the tools of democracy (e.g., consensus, freedom of speech, negotiation) meet the tools of expertise (e.g., specialized knowledge, integrity, and trust). Techno-scientific controversies around global warming, nuclear waste disposal, biodiversity protection, and agricultural biotechnology thus offer us crucibles in which actors struggle to compile social and scientific order. Scientific dissent pulls public concern into what might otherwise be ‘technical’ controversies, and calls attention to the politics within and around science.

⁵ Historically, the periods of controversy around tobacco science and Rachel Carson’s (1962) *Silent Spring* have primed the contemporary openness to the possibility of interest-based science. The current widespread acceptance of conflict of interest disclosures in academic journals testifies to the mainstream rejection of a functioning boundary between scientific and social practice.

Scientific dissent has always been janus-faced,⁶ a perspective reflected to some degree by early philosophers and sociologists of science (e.g., Merton 1973 [1942]; Polanyi 2000 [1962]). One face shores up the credibility of science as a path to truth that can be self-reflective and self-correcting – distinguishing science from dogmatic and inflexible ways of knowing. The trope of the scientist as a constant skeptic, questioning assumptions at every turn, implores us to trust the institutions of science to provide reliable knowledge that continues to improve over time. The other face of scientific dissent challenges the credibility of science by revealing a lack of consensus, which undermines scientific claims of objectivity. When multiple scientists arrive at opposing conclusions about the same phenomenon, science’s claim of providing an objective representation of reality becomes tenuous. Moreover, the intense activity around cases of scientific dissent – credibility contests that may begin as arguments over ‘facts’ but quickly evolve into complex struggles over reputation, method, inference, and implication – makes visible aspects of scientific practice that mimic political activity in society at large (e.g., Collins and Pinch 1982; Collins 1981; Gieryn 1999). The enactment of scientific dissent thus creates both cognitive and political tension within science and society.

In this sense, interrogating scientific dissent offers value on multiple levels. First, understanding the context for the performance of scientific dissent provides insight into the mix of forces that govern knowledge production in a given policy or technical arena.

⁶ Latour (1987) refers to science itself as “janus-faced.” One face represents current efforts to produce knowledge that are messy and contested; the second face represents knowledge that has become accepted and credible (facts), stripping away the uncertainty and materiality of the processes of producing knowledge.

Second, recognizing the diversity of forms of dissent reveals the contested nature not only of certain disciplines of knowledge, but of the rules by which we judge knowledge to be credible and knowledge-producers to be trustworthy. Third, acknowledging dissident science as a form of dissent rather than a full break from scientific practice provides a window into the social project of negotiating the tension between democracy and expertise.

In the agbiotech arena, some forms of scientific dissent embody such tension by bringing together discourses of expertise and discourses that reach out to a broader polity for legitimacy. In particular, dissent that acknowledges the political realities within the community of scientists and the related social forces flowing through that community has the potential to support a practice of science with a wider and deeper base of credibility. This dissertation begins to locate such action as *dissident science*, a form of scientific dissent that is explicitly political without drifting into scientific relativism wherein expertise fades from significance.

The case studies in this dissertation show how dissident science can involve performances of dissent that bring publics and politics onto the scientific stage – at times a stage in the literal sense. Such performances challenge traditional notions of autonomy and objectivity of scientists, but create a new vision of scientific accountability among all actors on stage (rather than having the scientific performers formally accountable to a director, a producer, or the audience). This rejects scientific institutions of credibility based on technologies of ‘witnessing’ (Shapin 1994) in favor of an improvisational practice of interaction among lay and scientific actors. Scientific credibility becomes dependent not upon the separation of scientists from the world of the audience, but

through integration and interaction with that world. Scientists become vulnerable to a level of transparency that not only demands explanation of what they discover but why they discovered it and not something else.

1.5 Outline of Dissertation

Chapter Two complements this introduction with a discussion of the literature engaged by this dissertation and a commentary on the methods used to collect the empirical data that forms the backbone of my arguments. Science and technology studies (STS) offers significant insights into understanding scientific controversies and supplies tools that have guided my approach to collecting data and organizing my thinking. Scholars also provide reassurance that the high political and economic stakes make agbiotech a favorable site for investigating the science/society dynamic, rather than a ‘merely popular’ realm better left to investigative reporters and political advocates. In terms of methodology, I explain what brought me to this research site and how I have struggled to manage the necessary tension experienced by an investigator who brings a mix of passion, commitment, and intellectual curiosity to his topic. While I have never aimed for a ‘disinterested’ analysis, I strive for transparency and have brought my utmost integrity to the project from its inception.

Part One describes conflicting scientific approaches in agbiotech that establish the landscape for the pathway of dissent. Chapter 3 explores the phenomenon of *promotional science* in agricultural biotechnology. While it would be relatively straightforward to demonstrate the ways in which science organized and conducted by the agbiotech industry carries a promotional tone, I take the more challenging and significant task of

revealing how a scientific meeting hosted by a public institution embodies a strong and explicit bias toward promoting agricultural biotechnologies. I engage a dramaturgical lens to analyze the “Workshop on Biotechnology for Horticultural Crops: Challenges and Opportunities,” held in Monterey, California in March 2002 and organized under the institutional umbrella of the University of California. Chapter 4 shifts attention to a conference co-hosted by the Pew Foundation’s Initiative on Food and Biotechnology and the U.S.-Mexico Foundation for Science: “Gene Flow: What Does It Mean for Biodiversity and Centers of Origin” held in Mexico City in September 2003. Pew billed the conference as an opportunity to discuss science and policy in a “nonpartisan” context, but my analysis suggests that it largely contributed to the discourse of promotional science. Chapter 5 analyzes these two events to map the powerful assumptions that underlie mainstream science in agricultural biotechnology – setting the stage for the emergence of scientific dissent.

Chapter 6 presents the initial phases of my three case studies as moments of *contrarian science*: Chapela’s claim of the introgression of transgenic DNA into native landraces of Mexican maize in Oaxaca; Losey’s evidence for the potential of *Bt* corn pollen to harm monarch butterfly larvae; and Pusztai’s announcement that GM potatoes caused serious malformations in rats. I focus on the creation of scientific claims and use the dramaturgical lens to analyze the explicit and implied narratives that emerge. Each case was contrarian because it challenged basic tenets of promotional science – offering a view contrary to that held within dominant discourse. At the most basic level, these cases – Chapela Maize, Losey Monarch, and Pusztai Potato – challenged the wisdom and safety of agbiotech. At this stage, the performances suggest only the first sparks of

scientific dissent. While the scientists certainly recognized the ways in which their claims ran contrary to dominant scientific discourse, they held some hope that their research would find acceptance within the scientific community and alter the landscape of understanding of agricultural biotechnologies.

Part Two follows the pathway of dissent into the realm of *resistance* to contrarian science. By resistance, I mean actions and discourses that challenge the potential for contrarian claims to become legitimate and the ability of contrarian scientists to achieve and maintain scientific credibility. I present the diversity of resistance, both to show its heterogeneity of form and also to suggest how patterns of resistance emerge that become greater than the sum of their parts. STS literature has tended to focus on examples of ‘normal’ scientific resistance (e.g., peer review, production of new data, generation of more comprehensive hypotheses) or examples of ‘extraordinary’ resistance (e.g., intellectual suppression, political repression). I choose to use the single term ‘resistance’ to designate all such action as having similar purpose and effect regardless of motive. My role as an analyst is not to judge whether one example of resistance is ‘appropriate’ or ‘justified’ – indeed this is the stuff of controversy itself – but to show how the totality of resistance faced in a particular case provokes the opportunity for the performance of scientific dissent.

Part Three analyzes the moment at which scientific dissent becomes manifest as a response to resistance. In this phase, scientists either must accept the resistance and withdraw themselves and their claims from scientific discourse, or respond within a context of budding controversy. In order to chart the spectrum of strategies of dissent, I introduce two heuristic concepts: agonistic engagement and dissident science. Agonistic

engagement (a term borrowed from political theory) signifies responses that follow the 'rules of the game' of scientific discourse (i.e., harmonious with such idealized norms as objectivity, insulation from politics, and expertise segregated from public involvement). By contrast, dissident science represents responses that not only break the rules of scientific discourse, but challenge those rules and related institutions that define how credible knowledge should be produced. Dissident science is explicitly political, without giving up the authority of scientific knowledge, and attempts to create alternative relationships between scientists and publics.

The concluding chapter offers some perspective on the implications of this project for policy and research around highly charged scientific controversies. I review the major claims, suggest lines of inquiry that emerge from this dissertation, and present a table comparing promotional and contrarian world-views in agbiotech. Lastly, I aim for reflexivity and analyze this dissertation as a scientific performance in itself.

Chapter 2 Diving Off the Shoulders of Giants

Scholars in science and technology studies (STS) have used controversy as a window into the practice of science, taking into account the mixture of ‘internal’ and ‘external’ politics that shape the creation of scientific consensus, otherwise known as the closure of controversy (Collins 1981; Latour 1987). Dissent has played a role in these explanations, but it has mostly remained a simple category of opposition, with perhaps a flavor of the power differentials between opposing scientific camps. In particular, STS has not come to terms with forms of scientific dissent that violate expected norms of scientific engagement (what I will call *dissident science*). Crucially, however, scholars have established the analytical benefits of examining science as a social practice, which has created numerous insights into scientific controversy: the ‘messiness’ of struggles for credibility, the role of power, and the contested nature of boundaries separating science from non-science and experts from laypersons.

One strand of controversy studies employs the metaphor of science as performance to highlight the ways in which scientific communication involves the control of information (stage management), the careful presentation of character, and the formation of narratives (Hilgartner). This methodology guides attention to strategies of credibility-building (and dismantling) that operate behind the explicit discourses of competing scientific claims. I extend this metaphor to consider aspects of audience construction – how actors and institutions construct publics that participate in contests for legitimacy. This has particular relevance in my case studies of dissident science, in which scientists explicitly and self-consciously put their science, literally, ‘on stage.’

With these insights in mind, I wade into my case studies of scientific controversy in agricultural biotechnology. I have taken seriously the challenge of conducting research as a participant-observer, which in such highly contested research sites made *my work* controversial even before my analysis was complete – due to the questions I posed and the persons to whom I asked those questions. Donna Haraway offers some encouragement for such a dilemma: “The point is to make a difference in the world, to cast our lot for some ways of life and not others. To do that, one must be in the action, be finite and dirty, not transcendent and clean” (Haraway, 1997 #95, 36).

2.1 *Situating Scientific Dissent Theoretically*

Early historians and philosophers of science developed a narrow view of scientific dissent in their quest to explain how science could produce valuable knowledge for society. Michael Polanyi’s classic essay, “The Republic of Science” (2000 [1962]), views the scientific community as a “society of explorers,” acting in virtual cooperation by following their own interests and ideas. He argues against the centralized control of scientific pursuit, articulating the intellectual efficiency attainable by a community of individuals mutually adjusting their research priorities according to the discoveries happening around them. Recognizing that science must both generate creative and original ideas and also judge those ideas as acceptable or not, Polanyi highlights the importance of selection criteria:

Both the criteria of plausibility and of scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. The professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it” (Polanyi 2000 [1962], 6)

This *essential tension* is, of course, problematic within the practice of science. Polanyi notes that the action of scientific authority may “sometimes be mistaken, and as a result unorthodox work of high originality and merit may be discouraged or altogether suppressed for a time. But these risks have to be taken” (p. 8). He thus joins Kuhn, Merton and Popper in great optimism of science’s ability to manage the essential tension: “The capacity to renew itself by evoking and assimilating opposition to itself appears to be logically inherent in the sources of the authority wielded by scientific orthodoxy” (p. 6). In other words, dissent is an asset to science, although its value may sometimes be obscured by the resistance to challengers. The case studies explored in this dissertation, however, offer a more complex view as the momentum of “orthodoxy” appears to overwhelm potential contributions of dissenters.

For scientists to maintain epistemic authority, they must manage dissent internally.⁷ Merton’s fourth norm, *organized skepticism*, is “both a methodological and an institutional mandate” (Merton 1973 [1942], 277). It resonates with the Popperian notion of falsification as a methodological requirement of science, but goes further to prescribe a sociological context for promoting challenges to scientific claims: “The scientific investigator does not preserve the cleavage between the sacred and the profane, between that which requires uncritical respect and that which can be objectively analyzed” (p. 278). In other words, doubt is fair game, no fact or theory is off limits, and thus dissenters are welcome and encouraged. But Merton’s norm is not “free-for-all

⁷ A further irony is that the most celebrated scientists are those who fought the hardest from the margins and overturned dominant theoretical paradigms (e.g., Darwin, Einstein, Mendel). Perhaps these ‘heroes’ and the mythical narratives around them help maintain the presentation of science as open to dissent when the evidence becomes powerful enough.

skepticism” or skepticism that brings scientific inquiry to a halt. Instead, skepticism is *organized*.

The adjective “organized,” as it modifies skepticism, holds a number of potential meanings. First, as opposed to “disorganized,” it suggests an orderliness to skepticism – perhaps some formality or agreed upon norms of how skepticism is communicated and then resolved. In Krimsky’s analysis of Merton, “Organized skepticism is not idiosyncratic but is bound by norms of empirical inquiry” (Krimsky 2003, 79). These sub-norms or procedures would dictate how and when dissent overlaps with skepticism, and when dissent might lie outside of desirable scientific behavior. Second, as opposed to “unorganized,” it assigns some agency to the ordering of skepticism, which raises the question of who decides the order and under what criteria. This reading approaches the question of how dissent should be managed – how skepticism *should be* organized. In Merton’s world, perhaps the norms of disinterestedness, communism, and universalism keep the organization of skepticism a rational and unproblematic procedure, but in scientific arenas where these norms falter or nearly disappear, the *organizing* of skeptics and skepticism becomes a critical site for understanding the production of knowledge. Merton himself invoked the phrase, “Standing on the shoulders of giants.” But which giants get stood upon? When standing on shoulders, can you only look in the direction the giant was looking, or is there some potential for using your high vantage point to look behind you as well? When does the stack of giants become so tall that falling off or coming down is simply too risky? When do you get to be a giant yourself?

These questions have consequences for practicing scientific dissent and point to the need to understand science, broadly, as a social practice. Nearly thirty years before

the social studies of science began to coalesce institutionally (Hess 1997), Ludwik Fleck laid this foundation in *Genesis and Development of a Scientific Fact*, first published in German in 1935 and translated into English in 1979. Although lesser known than Thomas Kuhn's *The Structure of Scientific Revolutions* (1970 [1962]),⁸ Fleck provides a nuanced set of insights about the production of facts.

Fleck challenges the notion of a fact existing apart from social practice. This insight into science, so taken for granted in modern STS, opened the door to perceiving scientific controversy as more complicated than a momentary lapse in science's ability to reveal absolute truth. *Controversy is not the camouflage of fact, but its ordinary precursor*. Fleck's interrogation of the connection between syphilis and the Wasserman reaction suggests that controversy (of some degree and quality) underlies every 'truth' science takes for granted. He historicizes science by uncovering the uncertainty, luck, and materiality that went along with the discovery of the causative agent of syphilis. My case studies of scientific dissent in agbiotech display just how forming fact from controversy remains a social project.

Written fifty years later, Bruno Latour's (1987) *Science in Action* argues forcefully for controversy as the key research site for understanding science. He views science as Janus-faced – with faces of “science in the making” and “ready made science” (p. 4). While each side is real, they are inconsistent in their explanations of scientific legitimacy. Only by studying controversy can we see how science works and facts are

⁸ Kuhn himself praised Fleck in a forward to the English translation of *Genesis and Development of a Scientific Fact*. He mentions citing Fleck as a source that spurred his sociological understanding of science, but remains vague about what he borrowed on a conceptual front: “I have more than once been asked what I took from Fleck and can only respond that I am almost totally uncertain” (Fleck 1979 [1935]: viii).

created: “We do not try to undermine the solidity of the accepted parts of science. We are realists as much as the people we travel with...But as soon as a controversy starts we become as relativist as our informants” (p. 100). This approach admits the utility of facts generated by science (what use is there in challenging accepted facts that are not actually challenged?), but sees controversy as the window on the social process of creating acceptable facts. Latour’s call to “become as relativist as our informants” takes special significance in the study of dissent, as the choice of informants largely determines whether the STS researcher understands a controversy as ‘closed’ or ‘open’. If we are to understand dissent, and take the perspective of dissenters seriously, we may find ourselves holding open a controversy that mainstream science considers non-controversial and closed. A number of promotional scientists accused me of making this error, with one UC Berkeley professor declining my request for an interview because he could not imagine what I would hope to learn by asking questions about such an illegitimate piece of science.

Latour (1987) offers methodological advice for studying scientific controversy. He suggests that we take our bearings by examining the stage of the claim, find people pushing on the claim (toward fact or artifact), and check which direction the claim is pushed (p. 59).⁹ In the spirit of symmetrical investigation, we should not attempt to explain why some people believe a particular statement or to judge the rationality of a position, but instead, “ask who are the accusers, what are the proofs, who are their witnesses, how is the jury chosen, what sort of evidence is legitimate, and so on, setting

⁹ Latour envisions scientific claims as contested – pushed either toward fact or toward artifact (irrelevance/falsity). At closure, the controversy evaporates from view and the accepted fact is available for consumption and evidence to bolster further claims.

up the complete frame of the tribunal in which the accusation of irrationality takes place” (p. 185). In other words, *treat scientific controversy as a court case*. This method helps the researcher remain open to sensitive analysis in spite of dismissive claims of irrationality by opposing participants, but difficult questions emerge in extending this framework beyond the laboratory context – the locus of Latour’s original study (Latour and Woolgar 1986 [1979]) – to a politicized scientific context such as agbiotech. What happens if the tribunal (as an exchange of arguments in front of an audience) doesn’t happen because of the insurmountable hostility between camps? Can multiple tribunals exist within different thought collectives, with multiple juries and standards of evidence? If so, which tribunal matters – one embedded in a traditional scientific context, one with direct political force, or one dominated by media that influences public opinion? As Latour notes, “It is hard to popularize science because it is designed to force out most people in the first place” (p. 52). This may be true, but the process of forcing ‘the people’ back in actually can illuminate the character of scientific controversy in which resolution (closure) requires political backing. Such is the context of agricultural biotechnology, as witnessed by the significant role that public opposition to GMOs has played in guiding policy in the European Union.

Latour’s rules of rhetoric to bolster one’s claims predict how dissenters will be treated:

[W]eaken your enemies, paralyse those you cannot weaken, help your allies if they are attacked, ensure safe communications with those who supply you with indisputable instruments, oblige your enemies to fight one another, [and] if you are not sure of winning be humble and understated (Latour 1987, 37).

In an odd twist, Latour’s focus on process leads back to a structural view of allies and enemies and the social position of the dissenter. “The power of rhetoric lies in making the

dissenter feel lonely” (p. 44). As Latour states more clearly when invoking actor-network theory directly, the network of connections determines the persuasive power of a scientific argument (Latour 1999 [1983]). An odd consequence of this perspective is that the most successful means for suppressing dissent is not overwhelming evidence and high volume discourse, but deafening silence: “There is something still worse, however, than being either criticised or dismantled by careless readers: it is being ignored..No matter what a paper did to the former literature, if no one else does anything with it, then it is as if it never existed at all” (p. 40). We must take care, however, in separating scientific silence from an absence of discourse. Latour’s rules may not extend to the rhetoric of corporate public relations or government spin. In a controversy rife with private and public interests, rhetoric may take on new shapes and forms.

In *Cultural Boundaries of Science* (1999), Thomas Gieryn extends the Latourian inquiry of controversy from *science in the making* to *science as consumed* (p. ix), which informs the consideration of the ‘public’ quality of dissent and controversy. This move broadens STS to examine how science depends upon active representation to maintain its credibility as the provider of truth. His key concept, *boundary-work*, acts to encourage a cartographic understanding of science’s representation of itself in society, following the use of the map metaphor in *Leviathan and the Air-Pump* (Shapin and Schaffer 1985). Gieryn writes, “The spaces in and around the edges of science are perpetually contested terrain: cultural maps are the interpretative means through which struggles for powerful ends are fought out—the right to declare a certain rendition of nature as ‘true and reliable’” (p. 15). The most obvious boundary demarcates what is science and what is non-science, since maintaining this distinction signals what knowledge and associated

spokespersons have the authority of science behind them. Thus, we can expect scientific dissenters and their opponents to argue not just over facts, but over epistemic position – who stands within the boundary of credible science and whether that boundary should be shifted or completely re-drawn. Gieryn points out, as well, that each struggle over placing a particular claim within or outside the science boundary also serves to reify the existence of the boundary itself, regardless of the outcome of the struggle (p. 14). We can therefore view scientific controversy as an ongoing process of legitimating science rather than as an occasional and dangerous threat to scientific authority. From this perspective, the practice of scientific dissent shores up the strength of science as a voice of authority.

Gieryn's (1999) boundary-work includes several genres, all of which apply to the study of diverse scientific controversies (p. 15-17).

Table 1: Categories of boundary-work

<i>Genres of boundary-work</i>	<i>What does the boundary separate?</i>	<i>What is sought by scientists?</i>	<i>Broader social effects</i>
Expulsion	“Real science” FROM “pseudoscience, amateur science, deviant or fraudulent science, bad science, junk science, [and] popular science”	The umbrella of scientific legitimacy	1) Scientists learn the boundaries of legitimate inquiry and explanation 2) The public witnesses science’s ability to police its own conduct and content
Expansion	Proper domains of scientific authority FROM areas of concern better informed by other ways of knowing (e.g., religion, folk knowledge)	To extend science’s jurisdictional control	Non-scientific ways of knowing become collectively less credible, increasing science’s jurisdictional momentum.
Protection of autonomy	Scientists FROM “outside powers” that seek to exploit scientific authority	a) Freedom to determine research direction b) Authority to police the boundaries of science c) Immunity from responsibility for downstream (technological) consequences of science	1) Scientific freedom is preserved, protecting from ‘improper corruption’ 2) Public accountability through media and/or government is more difficult 3) Internal conflicts of interest of scientists become harder to police

These genres of boundary-work call attention to the contested legitimacy of science and scientists to exert power in the world. Struggles over dissent most obviously involve expulsion boundary-work, but opportunities also arise for other types of boundary-work. In particular, scientific dissenters could attempt to *lower* boundaries of scientific

autonomy if non-scientific allies provide resources in a struggle with mainstream scientists. My case studies demonstrate the deployment of this strategy, and its strengths and weaknesses. Such scenarios speak to contests over the location of expertise, the role of the public in science-making, and the protection of scientific freedom.

The recognition of the processes and consequences of boundary work reflects one of the premier insights of STS: viewing science as embedded within society. This perspective stands in opposition to perceiving science as an institution that sits outside of society in order to provide objective input for a value-laden policy process. Fleck (1979 [1935]) exposes the complexity of the science/society relationship by arguing that empirical observations alone do not construct hypotheses and stabilize facts (p. 3). “Sixteenth-century physicians were by no means at liberty to replace the mythical-ethical concept of syphilis with one based upon natural science and pathogenesis. A stylistic bond exists between many, if not all, concepts of a period, based on their mutual influence” (p. 9). Thus, scientists work within a social milieu that limits the power of science to displace taken-for-granted knowledge. What Fleck notes explicitly is that the momentum necessary to pursue and fund research is dependent on public priorities: when compared to syphilis, “tuberculosis had done far more damage, but it never received comparable attention because, unfortunately, it was not considered the ‘accursed, disgraceful disease’” (p. 77).¹⁰ Science has little independence from society at large, which implies that scientific controversies must be understood as reflections, or at least

¹⁰ Fleck’s orientation is more psychological than political; scientists operate within a particular ‘thought style’ and thus do not even perceive possibilities well-beyond the confines of the thought style. Interestingly, even though the publication of *Genesis and Development* was delayed a year and forced out of Germany into Switzerland because Fleck was a Jew (p. 150), he does not discuss the impact of socio-political censorship.

shadows, of social controversies. Controversies over agricultural biotechnologies may appear technical, but they connect with contests over what are primarily social futures. Debates about whether transgenic DNA has introgressed into Mexican landraces of maize *only matter* within a value system that includes notions of purity, biodiversity, and local control of maize as a cultural/natural resource.

Fleck elaborates this interdependence with attention to the democratic process. *Thought collectives*, groups of scientists who communicate and work within a particular *thought style* (see footnote 10), include esoteric circles of individuals testing new ideas as a kind of knowledge vanguard and exoteric circles of individuals with a more general and popular understanding. The esoteric circles provide knowledge and have great freedom, but are also held in check by exoteric circles that connect directly with the polity. Fleck views science as democratic because the masses occupy a stronger position than the knowledge-creating esoteric scientists (Fleck 1979 [1935], 105). He thus engages with the key political issue of science in his time – how science operates and thrives under various political regimes. What he could not foresee is the complex corporate involvement in today’s biotechnology science, in which the mix of accountability, public perception, and funding of research are spread across quite a different terrain. These realities may very well undermine the power of the masses to hold accountable the esoteric circles of scientists.

Sheila Jasanoff, writing nearly seventy years after Fleck, wrestles with this very issue. In her article, “Technologies of Humility: Citizen Participation in Governing Science” (2003), she describes how scientific oversight has become as much an issue of accountability as quality control. In her analysis, the model of “speaking truth to

power”¹¹ no longer suffices, as “[t]here is growing awareness that even technical policy-making needs to get more political – or, more accurately, to be seen more explicitly in terms of its political foundations” (p. 225). At stake are new visions for the relationship between scientists and publics as they negotiate governance implicated by, and controlling of technoscience. Jasanoff responds to the resulting “participatory turn” with a proposal for “technologies of humility,” which are social technologies that embrace uncertainty, foster political discussion, and maintain awareness of the power of framing assumptions to undermine knowledge production. These aims correspond well with the practices of scientific dissent within my case studies, locating scientific dissent as a site for negotiating changing understandings of authority between science and society.

Scientific dissent thus connects strongly to questions of power relations in science. Recognizing the role of power, Fleck upends the view of scientific knowledge as categorically open to challenge and revision.

Once a structurally complete and closed system of opinions consisting of many details and relations has been formed, it offers enduring resistance to anything that contradicts it...(1) A contradiction to the system appears unthinkable. (2) What does not fit into the system remains unseen; (3) alternatively, if it is noticed, either it is kept secret, or (4) laborious efforts are made to explain an exception in terms that do not contradict the system. (5) Despite the legitimate claims of contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them substance (Fleck 1979 [1935], 27).

Fleck thus holds quite a conservative view of science – knowledge with the potential to disrupt a thought style has very little chance of actually doing so. If we define scientific dissent as not simply a challenge to an existing fact, but an implicit or explicit challenge

¹¹ “According to this template, technical input to policy problems has to be developed independently of political influences; the ‘truth’ so generated acts as a constraint, perhaps the most important one, on subsequent exercises of political power. The accidents and troubles of the late twentieth century, however, have called into question the validity of this model – either as a descriptively accurate rendition of the ways in which experts relate to policy-makers, or as a normatively acceptable formula for deploying specialized knowledge within democratic political systems” (Jasanoff 2003, 225).

to a system of scientific thinking (thought style), then dissent will be met with (1) surprise, (2) indifference to the point of dismissal, (3) efforts to reduce the exposure of the dissenters, (4) inappropriate and inefficient explanations that attempt to subsume the dissent, and (5) a field of evidence and practitioners well equipped with evidence to defend their own claims. Fleck minces no words when he comments on the personal experience of dissenters: “Heretics who do not share this collective mood and are rated as criminals by the collective will be burned at the stake until a different mood creates a different thought style and different valuation” (p. 99). The details of the resistance faced by contrarian scientists in agbiotech reflect this conclusion.

Brian Martin’s (1991) layered analysis of the controversy surrounding the fluoridation of public drinking water provides a model for analyzing science as a form of power. Martin provides a “power picture of science,” viewing science as “something which people do which serves some interests in society more than others, especially the interests of scientists themselves and of other groups with money and power enough to fund research and apply results” (p. 8). This perspective goes beyond Fleck’s thought styles and collectives to bring broader social and political-economic concerns into the analysis of controversy. Martin thus tackles his topic of controversy with attention to the impact of social power on scientific dissent: “In the English-speaking countries at least, fluoridation has long been virtually untouchable for ‘serious scientists.’ Opponents of fluoridation have been categorized as cranks, usually right-wing, and akin to those who think the earth is flat” (p. 2). In the spirit of Latour’s technoscience, the subject of fluoridation demands consideration of ethical, political, toxicological, dental, and corporate discourse to make sense of why fluoridation as a practice has become

absolutely dominant in the U.S. and elsewhere, but has not completely closed as a black box. Understanding controversies around agbiotech require similarly diverse investigations.

An important component of Martin's research program involves continuing attention to behaviors opposing and promoting scientific dissent. He explicitly acknowledges a power gap between mainstream scientists and dissenters – it is not a level playing field upon which arguments compete for credibility. A Latourian/ANT (Actor Network Theory) framework would perceive these power differentials as a *dependent variable* explained by differences in the density of assembled networks. Instead, Martin foregrounds it as an *independent variable*. The power differential exists, and a diversity of behaviors to resist dissent emerges in that context.

Silence is one mode of resistance. When the “overwhelming weight of professional credibility and endorsements is on one side,” the mainstream can ignore dissent (Martin 1991, 71). A savvy mainstream partisan will even reject the opportunity to debate a dissenter since “the mere fact that the debate even took place conveys to the public that a legitimate scientific controversy exists” (p. 61). This strategy carries certain risks – silence by the mainstream means free reign by dissenters to dominate public discourse, and refusal to engage may make mainstream partisans appear arrogant (p. 62).

Other modes of resistance reach beyond the confines of scientific discourse. Even if mainstream partisans avoid engaging dissent as a scientific issue, they may resist dissent in explicitly non-scientific fora. Martin explains how pro-fluoridationists adopt this strategy:

The scientific part, they believe, consists of scientific findings which contain no basis for opposing fluoridation. This is the foundation for the claim that there is no scientific

debate. The political part of the issue arises from the existence of opponents who are motivated for nonscientific reasons. This political opposition must be countered, and thus many of the proponents counsel the waging of a political struggle for fluoridation (Martin 1991, 62).

Specific tactics include attacking in general terms (e.g., “the opposition rely on innuendo, half-truths, and deliberate untruths...They know the answer with religious fervor” [p. 71]), describing dissenting arguments in lay terms without providing credibility-enhancing references or names of experts (p. 73), and circulating unpublished critique which both “denies the criticized paper the status of being taken seriously in a prestigious open forum” (p. 73) and prevents counter-critique from occurring since distribution is informal. In a commentary published as part of Martin’s book, Edward Groth III writes:

Ironically, the 'antis,' who are usually portrayed as unscientific, often act more scientifically in the debate, probably because it is politically useful to do so...By contrast, the political profluoridation stance has evolved into a dogmatic, authoritarian, essentially antiscientific posture, one that discourages open debate of scientific issues (p. 189-90).

Groth thus notes that fidelity to norms of scientific engagement may stem less from ideological commitment and more from political expediency.

By themselves, strategies of silence and extra-scientific attack fall short when dissenters emerge with credentials of expertise. In such a situation, opponents may resort to personal attack to destroy the personal credibility and authority of dissenters who otherwise would have expert standing in the technoscientific controversy (Martin 1991, 76). Tactics include accusing dissenters of mixing “emotion with reason” (p. 78), name-calling and disparaging comments (p. 77), criticizing dissenters because their position is also held by extremist groups (“guilt by association”) (p. 79), accusations of intellectual dishonesty and professional incompetence (p. 82), framing dissenters’ behavior as revengeful attempts to “get back at the scientific community” (p. 89), and communicating threats through superiors or professional societies (p. 93-7).

Martin explores a shockingly institutionalized format of the personal attack strategy in the fluoridation controversy: “a dossier on opponents compiled by the Bureau of Public Information of the ADA [American Dental Association] since the mid 1950s...[entitled,] ‘Comments on the Opponents of Fluoridation’” (Martin 1991, 79). The dossier includes entries for the Ku Klux Klan, various ‘health quacks,’ convicted criminals, and mental patients, which further stigmatizes the dissenting scientists who are attacked at length in the document (p. 79-81). Martin quotes Dr. George Walbott, an internationally respected allergist and leading scientist in the U.S. who opposed fluoridation: “This dossier accused me of intellectual dishonesty and incompetence. I was grouped with lay opponents, one of whom was alleged to have escaped from a mental institution, the other was claimed to be an imposter. Subsequently, wherever I raised my voice against fluoridation, this dossier always showed up like a steady companion...wherever and whenever there was a need for countering my data” (p. 82). After describing a consistent public campaign to discredit Walbott, Martin comments, “Perhaps it is not surprising that few scientists have made serious attempts to find and study cases of fluoride toxicity” (p. 86). Thus, Martin infers that personal attacks perform professional discipline on a broad scale by showing the consequences of pursuing dissenting lines of inquiry.

These strategies of resisting dissent are descriptions by Martin, not prescriptions. While his data from this one case do not permit him to make broader claims, he suggests that “the massive early push for fluoridation, which brushed skeptics aside, laid the seeds for its own lack of complete victory” (p. 143). Aggressive promotion and harsh resistance to dissent thus may have stimulated open opposition. Proponents could have attempted to

co-opt dissenters or at least respond to early critics (although there was some danger that the critique would be verified), but hubris ruled in the case of fluoridation (p. 144). On the other hand, Martin reminds us that fluoride toothpaste has been virtually unchallenged except for use by very young children – suggesting that water fluoridation may have absorbed the ‘heat.’ We might ask the same question of agricultural biotechnology as a zone of controversy that protects a related technoscientific domain (e.g., medical biotechnology).

Turning from strategies of resisting dissent to possible responses by the targets of resistance, we find a near-void in the STS literature. To my knowledge, Martin is the only scholar who has attempted to elucidate the range of options available to scientists under attack. In “Strategies for Dissenting Scientists” (1998) Martin describes and prescribes a set of responses. First, dissenting scientists can mimic orthodox science by emphasizing credible affiliations, engaging well-respected methods, or establishing new scientific journals. Second, they can seek alternative audiences for their science (e.g. outside their discipline, in less prestigious journals) in order to avoid the most hostile interests. Third, they can expose the suppression publicly or build a social movement, provided they have connections with activist organizations or constituencies. But Martin generally counsels that when they are attacked, dissenting scientists should not “become preoccupied by the injustice of attacks, for example by suing. Rather, the focus should always be returned to the work in question and the need for fair evaluation.” Martin thus sees great risk in abandoning the norms of scientific conduct.

This framework, developed out of a career of studying cases of intellectual suppression, exposes much of the diversity of possible responses by scientific dissenters

(Martin 1981, 1991, 1998, 1999; Martin, Baker et al. 1986). Few authors have taken the complexity of scientific dissent as seriously. Nevertheless, Martin falls short of showing how some dissenting strategies represent epistemic arguments that disrupt conventional understandings of science and society. Performances of dissent hold significance beyond their strategic value in a particular credibility contest – cases of dissident science, especially, embody efforts to reconfigure relationships among scientists, publics, and politics.

2.2 *Taking the Dive: Research Methodology*

While scholars may rely upon intuitive notions of how to enter controversy as outright participants, entering controversy for the purpose of analysis presents a formidable challenge. Questions emerge regarding one’s identity as a researcher, expectations of participants, and tools to guide observation and data collection.

STS researchers cannot simply compare evidence when evaluating controversies – they must also analyze the actions of participants. Fleck notes that the “formulation of a problem already contains half its solution” (Fleck 1979 [1935], 37), suggesting that the power to frame a question is itself part of scientific controversy. Second, experience is more complex and deeper than experiment (p. 10). This implies that resolving controversy requires a much broader view of the participants than the data they choose to showcase. Third, Fleck borrows from evolutionary language as he describes the frequent occurrence of “mutations” in thought styles (p. 26). But taking the metaphor further, what selective pressure determines which mutations will survive? Is studying controversy to be

reduced to studying the winners? And if so, what does the survival of some mutations over others teach us about the practice of science in a social context?

A disturbing outcome of Fleck's model is that thought collectives – groups of scientists who operate within a thought style – have trouble communicating with other thought collectives. “If A and B belong to the same thought collective, the thought will be either true or false for both. But if they belong to different thought collectives, it will just not be the same thought! It must either be unclear to, or be understood differently by, one of them” (p. 100). Controversy thus becomes a symptom of the existence of different thought collectives – resolvable, perhaps, only by the acquisition or obliteration of one thought collective by another, or by a transformation in thought style that accommodates both groups. At a fundamental level, Fleck prepares those who study scientific controversy to encounter frustrated participants who can barely communicate with one another:

The greater the difference between thought styles, the more inhibited will be the communication of ideas...The alien way of thought seems like mysticism. The questions it rejects will often be regarded as the most important ones, its explanations as proving nothing or as missing the point, its problems as often unimportant or meaningless trivialities (p. 109).

It is helpful to imagine agbiotech as a field of overlapping thought collectives and thought styles, with struggles that lay bare fundamental differences in orientations toward the perception of questions and evidence.

In such a context, rife with political and intellectual controversy, the investigator cannot hope to adhere to traditional notions of the non-partisan observer. Martin acknowledges that by seeking symmetry (not just explaining the beliefs and behavior of the dissenters, which is a more common intellectual project), his ‘neutrality’ vanishes. In

his book on the fluoridation controversy, he writes, “Since proponents generally maintain that there is no credible scientific opposition to fluoridation, my analysis appeared to give the opponents far too much credibility” (Martin 1991, 165). Adele Clarke and Theresa Montini (1993) make the point more broadly: “By the very scholarly act of representing most or all of the actors in print, we are turning up the volume on the less powerful actors, empowering them in the arena. The research is consequential. By following a current controversy, we are feeding it” (p. 69). I have received similar messages from my informants, some of whom suggested that I had nothing to study or recommended that I expand my cases so as not to dwell on an uninteresting case. Martin states:

Separation of the researcher and the researched may work in some cases, but practical experiences show that it often cannot be sustained in dealing with contemporary controversies with a strong public involvement. To some extent, the social researcher is inevitably involved in the controversy being studied (p. 161).

Thus, in a field such as agbiotech with high public involvement, the researcher must expect to become part of the story. I take this to mean that my analytical integrity stays intact not through denial of my biases, but through a reflexive and participatory analysis.

Entering the action, however, does not imply that we should judge the controversy on its own terms. For example, one interpretation of the Pulse Event introduced in Chapter 1 is that the four scientists gathered out of desperation. We might see their performance as an attempt to distract attention from their shoddy and politically-motivated science with the ‘smoke and mirrors’ of claims of suppression and calls for ‘scientific freedom’ – as if scientific freedom allowed the complete suspension of standards of quality in the practice and interpretation of scientific research. Indeed, none of the four spent significant time at the event shoring up his substantive claims with further evidence or careful arguments about why his science was of the highest quality.

Instead, each of them framed the resistance to their science as corrupt, tragic, misguided, and essentially baseless. Were they doing nothing more than ‘attacking their attackers’ and defending their science with political (i.e. non-scientific) appeals?

It is a tempting question, and one that represents a trap in conducting research on scientific controversies as a social scientist. While a significant part of our interest as researchers may be the curiosity of getting close enough to a controversy to glean the hidden details, our research shifts our gaze from product (truth) to process (science) and enables us to ask questions that transcend the particulars of the controversy at hand. The intellectual value of our efforts is not ultimately demonstrated by our ability to untangle the discord into neat stories of winners and losers, heroes and villains, lies and truths, and fact and fiction. Thus, on one level, this dissertation aims to be ‘above’ the controversy, as a set of observations and interpretations that are independent of the quality of the science-making under analysis. At the same time, this dissertation is partially embedded in the controversies at hand – the analysis cannot help but participate in the controversies by entering the discourse, framing the stories, and highlighting some actors, facts, events, and histories more than others.

The alternative to the trap of judgment is analysis. While my model of scientific dissent as a pathway (promotional science, contrarian science, resistance, and response) provides a conceptual framework, my analytic framework stems from understanding science as performance, an insight garnered from Stephen Hilgartner’s *Science on Stage: Expert Advice as Public Drama* (2000). Hilgartner analyzes written materials surrounding controversial National Academy of Science reports with the explicit lens of dramaturgy. Invoking concepts such as stage management and character presentation, he

provides a language to describe scientific writing designed to persuade a public audience. Departing from traditional styles of academic writing, Hilgartner even presents one exchange during the controversy as a Greek play, complete with a chorus and stage directions.

While I appreciate Hilgartner's move to connect scientific communication with drama, I believe he misses an opportunity by glossing over the materiality of theater. A script may be read in almost any context, but theater is material and contextual. The stage exists prior to the play (although it may be modified within the constraints of the building and its seating arrangement); set designers fashion scenery and backdrops that anticipate actors' performances (but cannot adjust to performance-time changes or mishaps); and props managers collect and manufacture the objects that may be put 'into play' by the actors. Furthermore, set designers and props managers often *adapt* these materials from previous productions; theaters keep old backdrops, scenery, and props, which are then retrofitted or simply re-used with no modification at all. In short, the materiality of the theater, a production in its own right, constrains the viewed performance in part because many theatrical elements are primarily produced *ahead of time*.

If we include the material aspects of theater as part of performance, then importing the dramaturgical metaphor into studies of science controversy forces us to consider the historical, structural, and contextual elements that circumscribe and permeate the unfolding of controversy. Theater attendees experience a performance as a seamless web of constructions—some produced in the moment (an improvised comment), some developed during rehearsal (the blocking of scenes), some imagined at the production's outset (scenery), and some constructed years before the playwright was

born (the architecture of the venue). Translated to the performance of scientific controversy, the public(s) likewise may discern little difference between context and rhetoric, structure and boundary-work, or institutions and power-moves. But we as analysts can make such distinctions – either when evidence suggests that the actors themselves are aware of such constraints, or when through our ability to transcend the spatial and temporal boundaries experienced by actors and audiences, we see behavior that can best be explained by attention to structure (even if participants themselves do not ‘see’ that structure¹²).

I engage the dramaturgical lens as an analytical method to explore scientific communication along the pathway of scientific dissent. I extend Hilgartner’s method to diverse examples of scientific performance, from conference meetings, to journal articles, to explicitly theatrical events that focus on scientific controversies. In particular, I use the concepts of *stage management, character, narrative, and audience*.

In theater, the stage manager coordinates the behind-the-scenes work to support the action that occurs on stage. Examples include set design, props, lighting, sound, and set transformations that happen between scenes. Hilgartner focuses especially on how techniques of stage management in scientific controversy affect what the audience (public) is permitted to see – controlling what stays backstage and what appears as part of the performance. I broaden this perspective to include all the decisions that control the

¹² Kleinman (2003) describes how researchers have come to accept industry standards for fruit appearance and quality as the proper metric to evaluate a biological control agent. “This is not to suggest that the rules of the game—in this case, the cosmetic standards for fruit—are in any way ahistorical or trans-historical. To the contrary, they were clearly established at a particular point in time but once they were firmly entrenched, these guidelines fundamentally shaped the practices of growers and scientists alike” (p. 78). No awareness on the part of the researchers is required; they simply design their methodology according to standard practice, with little likelihood or incentive to question what has become a ‘natural’ way to test for pesticide efficacy.

setting for a scientific performance (e.g. organizing the program for a scientific meeting, choosing the venue for a public event, assigning a title to a printed letter in a scientific journal). Considering such techniques of stage management deepens my analysis of the often invisible, but highly influential, actions of those who have power over scientific performances.

From a dramaturgical perspective, character differs from identity in that character is temporally and contextually circumscribed. This perspective thus frees the analyst to ask questions about the construction of character within a particular performance (in contrast to other aspects of identity that might be emphasized in a different time or place). It is especially helpful to acknowledge how such character formation emerges from the actions of both the individual (playing the character) and other actors (on and off stage). Especially in the field of scientific controversy, where issues of credibility and trust are paramount, the achievement, attack, and defense of character carry great significance for how a scientific performance plays out.

Character and stage management contribute to the concept of narrative. A scientific performance tells a story – the power of that story to convince its audience and motivate action (e.g., further research, political behavior, funding) defines its effectiveness as a performance. A particular piece of scientific research describes relationships among things and ideas, often in the name of fact-making. Narrative analysis promotes careful consideration not of the ‘facts’ of an argument, but of the logical, rhetorical, and aesthetic coming together of a story that has persuasive power. The narrative draws together the elements of research – historical, theoretical, material, and political – to persuade the audience to accept an argument.

Paying attention to the concept of audience in scientific performance serves a number of methodological purposes. First, the existence of an audience or multiple audiences undermines the tired trope of ‘the public’ as the receiver of scientific knowledge; members of the public do pay attention to scientific performances, but they do so in organized ways and with greater or lesser degrees of passivity. Second, the reality that audiences are constructed draws attention to the work that includes and excludes people from experiencing and participating in scientific performances; even ‘public’ events hold unspoken expectations for who can show up to serve in their audiences. Third, in the tradition of Steven Shapin (1994), who recognized the role of gentlemen witnesses as audiences constructed to provide legitimacy for scientific results among broader publics, audiences operate at various scales; the attendees of a conference may serve as a kind of participatory audience, but the political impact of the conference as a performance may focus upon the audience created by the media coverage of the conference (and the behavior/reaction of its immediate audience).

Together, these four dramaturgical concepts play an organizing role in presenting my arguments about scientific dissent in agricultural biotechnology. They provide insight into the complexity of highly contested terrain, posing important questions. What structural factors impact how a scientific debate appears for consumption and interpretation? What explains the consistencies and discrepancies between the personae of scientists during controversy? How are ‘facts’ connected to values in the communication of science? Where does the power reside to settle scientific controversy and make the ‘technical’ results socially relevant? The analysis of the pathway of scientific dissent as a series of performances enables access to these questions.

2.3 *My Own Pathway*

Karen Barad's (1999 [1998]) concept of "agential realism" offers a mode to understand my role in this research project. She brings together the work of Michel Foucault and Neils Bohr to suggest that while realism has always been an illusion (constructed, of course), social constructivism ignores the power of physical bodies. Agential realism shifts our attention to the *phenomena* under consideration, which encompass power, knowledge, and materiality. As a social scientist, I have agency – the conceptual model of scientific dissent, for example, does not emerge magically from the data. My work assembles a mix of actors, institutions, and bodies, forming a whole of some sort and creating new patterns of meaning. Together, these constitute a phenomenon that can provide insight into the contexts of agbiotech, scientific dissent, and the politics of knowledge. This suggests that the validity of my research depends not only on the vague notion of representational fidelity (letting my subjects appear without overly distorting them for my own purposes), but also on *me* being transparent and accountable to *you* (the reader; my audience). As Donna Haraway states succinctly, "feminist objectivity means quite simply situated knowledge" (Haraway 1999 [1988], 176).¹³ It is my duty, then, to situate myself as researcher and author.

I began a doctoral program at UC Berkeley's Department of Environmental Science, Policy, and Management (ESPM) within the College of Natural Resources (CNR) in the fall of 1998, aiming to learn about the political and technical terrain

¹³ She goes on argue "for politics and epistemologies of location, positioning, and situating, where partiality and not universality is the condition of being heard to make rational knowledge claims" (Haraway 1999 [1988], 181).

between policies and publics. I had never heard of agricultural biotechnology, but soon became fascinated with the implications of GM crops. More specifically, I arrived on campus just as the UC Berkeley-Novartis agreement was presented to the faculty and students of CNR as a *fait accompli*. This agreement provided \$5 million per year in research funds and access to proprietary genetic databases from Novartis to a substantial majority of researchers in UC Berkeley's Plant and Microbial Biology (PMB) department. In return, Novartis received the first right to negotiate licenses on a percentage of the intellectual property patented by participating researchers, representation on the committee that distributed research funds, and intangibles such as access to university researchers and association with the respected name of the university. A brief presentation by Professor Miguel Altieri to the incoming cohort of graduate students in ESPM described some of the potential implications of GM crops and also raised the question of whether UC Berkeley should ally itself with a transnational biotechnology corporation (Novartis, now Syngenta).

As a former grassroots organizer, I quickly put my skills to work and helped to form Students for Responsible Research (SRR). We began as a "fact-finding" organization and quickly moved to oppose the agreement as it had been written. Our concerns included academic integrity, the drifting of the public university toward corporate goals, the privatization of scientific knowledge, and the institutional consequences of investing resources in pursuing some disciplines of knowledge (biotech) over others (e.g., bio-control). Concerns about environmental and social consequences of GM crops played a substantive role in motivating many of us to speak out during these

debates.¹⁴ Despite our efforts, the agreement was signed at the end of November 1998, but our group maintained involvement in campus politics around the deal. For example, I testified on behalf of SRR at the California Senate's hearings on the UC Berkeley-Novartis agreement in May 2000, and was invited to speak at the "Berkeley Chancellor's Forum on University-Industry Partnerships" in 2001 because of my involvement with SRR.

It was in this context that I came to know Ignacio Chapela, Assistant Professor of Microbial Ecology in ESPM. Chapela had been appointed to chair CNR's faculty executive committee, and despite his junior status, refused to rubber-stamp the UC Berkeley-Novartis agreement with the faculty's approval. He became one of only a few faculty members to speak out publicly against the deal, and used his position as executive committee chair for the CNR faculty to conduct a college-wide survey of faculty opinion about university-industry partnerships. I came to respect his willingness to take a stand against administrators, found his critical arguments persuasive, and valued his knowledge of and experience with agbiotech (Chapela had worked with Sandoz, a precursor to Novartis, before joining the ESPM faculty). I conducted historical research about the emergence of the biotech industry under the guidance of Chapela in the summer of 1999, and we both participated in an informal discussion group about agbiotech in 2000-2001. Chapela chaired my oral exam committee in December 2000, and at the time I imagined that he would serve on my dissertation committee as well.

¹⁴ The history of the UC Berkeley-Novartis agreement, including opposition by SRR, is described in a report commissioned by the UC Berkeley Academic Senate and carried out by a team of researchers from Michigan State University (Busch, Allison et al. 2004).

Beginning in the fall of 2001, and with the support of the MacArthur Foundation, I co-coordinated a graduate working group called “Promoting and Resisting Biotechnology in Transnational Contexts” (later re-named the Berkeley Biotech Working Group) with Carol Manahan of the Graduate Theological Union. This group invited Chapela and Quist to discuss their controversial findings soon after *Nature* published the manuscript announcing their discovery of transgenic DNA in native landraces of Mexican maize (Quist and Chapela 2001). Shortly thereafter, I decided that my dissertation research would focus on this controversy. I met with Chapela to formally sever our academic relationship and to establish professional expectations in the transition to a “researcher/subject” relationship. Since that day I have received no guidance from Chapela about my project, nor have I provided him access to any drafts of my writing in advance of filing this dissertation.

Over the next four years I conducted extensive interviews with Quist and Chapela, including partial oral histories, and continued my involvement in the Berkeley Biotech Working Group and to a lesser extent SRR. I collected media and archival materials surrounding the Chapela Maize controversy, interviewed authors of published critiques, and conducted participant-observation of a great many relevant events. I held in constant tension my desire on one hand to actively engage in projects that reflected my beliefs and on the other hand, my commitment to maintaining distance from the action in order to garner a critical perspective. My theoretical focus on scientific dissent as a performance helped me in this regard by making it clear that my professional duty as a scholar was relatively independent of my personal judgments of the legitimacy of Chapela’s scientific or political claims. In other words, STS allowed me to locate myself

within an arena (agbiotech) where I had great passion, but still create opportunities for deep learning. My strategy of engaging comparative cases of scientific dissent in agricultural biotechnology (Losey Monarch, Pusztai Potato), although with less empirical depth, reflects an additional effort to experience a greater variety of analytical distances to inform my thinking and guide my interpretation of data.

Three tools informed my analysis and supported the writing of this manuscript. First, as mentioned above, the dramaturgical lens created a discipline of attention that organized my thinking within the metaphor of performance. This tool is easily visible to readers in the structure of my arguments in the remainder of the dissertation. Second, I used Atlas.ti, a qualitative data analysis software, to organize and code my data. Although I do not claim to have followed the methodology of grounded theory (Glaser and Strauss 1967), the coding process enabled me to look for patterns and connections throughout my data, which became the structure of the chapters that follow. I discovered a great deal about my own thinking in the context of ‘playful work’ within the context of this software. Third, the process of collaboration and sharing ideas has occupied a central place in my methodology. The members of the Berkeley Biotechnology Working Group,¹⁵ attendees of the annual meetings of the Society for the Social Studies of Science, several key colleagues, and the faculty on my dissertation committee have created countless opportunities for me to do the work of bringing my data to life in a dissertation. This iterative, social method of idea generation and testing was, in reference to this chapter’s title, what kept me afloat after the dive into research and during the long swim of writing.

¹⁵ Especially Daniel Latham, Carol Manahan, Leah Nichols, Mark Philbrick, and Ken Worthy.

I have often experienced the pangs of failure. As an historian of the Chapala Maize controversy, I have not always been in the right place at the right time to record key events, and I regret that much of what I have recorded has not found its way into this dissertation. As an activist, I have suffered quietly while watching tasks go undone or done poorly, knowing that taking on a leadership role would undermine my purpose as a researcher. As an STS scholar, I have wished for tighter methodology and a sharper ability to critique those with whom I empathize. And as a follower of controversies in agbiotech, I have often wished for deep knowledge of genetics, molecular biology, plant physiology, agronomy, toxicology, and ecology – deep enough to judge for myself the quality of scientific claims. But I sleep at night, at least when my daughters don't wake me up, because these uncomfortable shortcomings remind me that my method as a researcher is one of self-critique and awareness. It is this value that has permitted me – no, required me – to tell this story here rather than in a preface.

PART ONE

SCIENTIFIC APPROACHES IN CONFLICT

Introduction

Agricultural biotechnology operates not as a unified scientific field nor as a single Fleckian thought collective, but rather as a tangle of diverse scientific approaches that conflict with one another. Controversies emerge not just out of disagreement over ‘facts’, but through the incompatibility of particular practices and worldviews that together form the science, or ‘sciences’, of agricultural biotechnology. Part One explores this diversity in order to demonstrate the foundations of scientific controversy in agbiotech.

In the context of this dissertation, ‘promotional science’ refers to science that promotes the research, development, and deployment of agricultural biotechnologies. One might argue that the science of agricultural biotechnology differs from genetics or molecular biology because it is inherently an applied field, and therefore promotional of some technology. Indeed, it would be hard to imagine the science conducted within Monsanto or Syngenta as anything but promotional in this sense. What I aim to accomplish, however, is not to reveal the technological enthusiasm bubbling within the private sphere, but *the promotional orientation that appears to dominate the science practiced in public and quasi-public spheres*. This promotional orientation was evident in two events that convened university scientists, industry employees, government personnel, and NGO representatives to discuss both agbiotech research and policy – events which I analyze through the dramaturgical lens. Chapter 3 presents the “Workshop

on Biotechnology for Horticultural Crops: Challenges and Opportunities” (Monterey, California, March 2002), an explicitly promotional event in its framing as a workshop to overcome barriers to commercializing horticultural biotechnologies. Chapter 4 presents the conference: “Gene Flow: What Does It Mean for Biodiversity and Centers of Origin” (Mexico City, September 2003), sponsored by the Pew Initiative on Food and Biotechnology (PIFB). PIFB rhetorically located this conference on ‘neutral’ ground – framing it as part of its mission to bring objective scientific results to the public and to decision makers – but for complex reasons of procedural control and agenda setting, the event functioned as promotional science.

Although they addressed disparate issues in the broad arena of agricultural biotechnology, the Workshop on Biotechnology for Horticultural Crops and the Gene Flow Conference shared four characteristics. First, while neither was a ‘public’ event, in the sense of being open and advertised widely to the general public, there were no overt restrictions on participation. Registration was open and reasonably priced, although the locations may have hindered participation. Second, quasi-public organizations sponsored each event, situating them in a category hovering between industry, universities, NGOs, and government. This lent legitimacy to the events and encouraged a presentation of goals in terms of independent, scientific objectives. Such a framing aligned the events broadly with the public interest rather than, for example, agribusiness interests which were also well-represented among participants. Third, both focused explicitly on issues that required attention to scientific issues *and* policy issues, including sessions and presenters in both realms. Fourth, despite some diversity of involvement, both events

primarily provided a voice to the promotion of agbiotech. Contrarian science and values were either marginalized or absent.

As a participant-observer at these two events, I attended all formal sessions and engaged with other participants informally during breaks and meals. I tended to avoid lengthy discussions of my own work and introduced myself as a “UC Berkeley graduate student studying controversies in agbiotech.” My data included personal notes, conference handouts, copies of slides from presentations, audio recordings, and published material covering the events.

Methodologically, I recognize the problem of characterizing diverse discourses as homogeneous. At both events, speakers brought their own biases and I would be naïve to imagine that the audiences accepted all claims without question. Indeed, I gleaned important insights from participants who entrusted me privately with their disagreements and dissatisfactions with other speakers. However, I defend my choice to characterize these two events as cases of promotional science for three reasons. First, as a participant-observer, I experienced the emotional and interpersonal dynamics of each event. The timing of laughter, the character of applause, the tone of voice of presenters, facilitators, and question-askers, the grumblings and seat-shifting after some comments and not others were all obvious in the moment, but difficult to document satisfactorily in retrospect. As a qualitative researcher, I must trust those more intuitive moments of experience and reflection. Second, whenever possible, I cite multiple sources and voices to substantiate my claims and reduce the possibility of me mistaking a vocal minority for

the gestalt of the group.¹⁶ Third, invoking the theoretical lens of ‘science as performance’ leads to a provocative empirical stance. Even if the vast majority of the audience disagreed or found fault with a statement made during a presentation, the statement was an integral part of the performance. Unless challenged overtly, the statement characterized the performance as a whole, regardless of the actual level of support in the room. In this vein, an extracted quote need not represent the collective attitude of conference participants or organizers; rather it serves to represent the performance. A performance is less the aggregation of individual acts or preferences than the action and discourse available to the audience during the event.

Chapter 3 and Chapter 4 begin with discussions of the events as ‘performances’. As discussed above, the public or quasi-public character of the events helps the metaphor of ‘science as performance’ make the leap from National Academy of Science reports (Hilgartner 2000) to workshops and conferences. While the degree of control wielded by authors of publications and speeches certainly outshines that of conference organizers, the enactment of an event implies a variety of dramaturgical work: choosing the venue (conference location and later media communications about the event), casting the characters (speakers and facilitators), assembling and organizing the audience (participants and speakers), creating a narrative (conference announcement and program), and stage management (controlling opportunities for discussion, setting limits or fostering audience participation).

For both cases, the most difficult aspect involves the understanding of audience. The boundary between speaker/actor and audience constantly shifts during a conference,

¹⁶ Whenever possible, the first time I cite a speaker, I will provide a footnote indicating their affiliation.

not only because many attendees take turns in giving presentations, but also because the audience participates by asking questions, offering comments, and remaining silent. Furthermore, efforts to publicize the proceedings of such events create secondary audiences who lack the opportunity to participate in a dialectical fashion. For clarification, I will use the term “audience” to designate those persons physically attending the conference (and therefore with the ability to take part in the performance), and “media-audience” to refer to persons reading or hearing about the conference (an important ‘public’ that can react sequentially after the performance when presented through media).

Invoking the dramaturgical lens not only encourages attention to diverse aspects of an event, but also prepares the reader for a discussion of embedded assumptions (aspects of performance that often remain hidden from both actors and audience). In Chapter 5, I extract a set of scientific and political assumptions that underlie the discourse of promotional science, as represented by the two events. I avoid confrontation over factual matters, instead focusing on how approaches to problems and taken-for-granted ‘truths’ reveal the character of promotional science in agbiotech.

Chapter 6 shifts the focus to three cases of contrarian science – examples of scientific approaches that diverge sharply from those described in Chapter 3 and Chapter 4 and that challenge the assumptions elaborated in Chapter 5. Analyzing these first ‘moments’ of contrarian science both answers and asks a critical question. The cases described answer the empirical question of what research provoked the resistance explored in Part Two. More importantly, the cases urge the question, “Why all the fuss?” Answering this question is a fundamental task of this dissertation. My thesis, that *the*

institutional, ideological, and commercial strength of promotional science depends upon the strict policing of expertise that could undermine the foundational assumptions of promotional science, has implications for how we understand and envision resolution to controversies over agbiotech, and even how we understand the limits of major contributions of STS. Chapter 6 introduces the stimuli for that strict policing of expertise (resistance).

Presenting contrarian science as a stimulus to resistance does not imply a linear vision of how various forms of scientific dissent emerge. I present promotional science before contrarian science for conceptual reasons, not because contrarian science always appears sequentially after, and on a stable landscape dominated by, promotional science. In fact, one could view the development of promotional science as constantly reacting to forms of contrarian science and scientific dissent. These phenomena overlap, inform one another, and continue to develop dynamically.

Chapter 6 presents three case studies of contrarian science in agbiotech. The first case emerges from research by two UC Berkeley researchers, Ignacio Chapela and David Quist, who announced the presence of transgenic DNA in native landraces of Mexican maize. Their *Nature* publication not only provided evidence for this unsuspected ‘contamination’, but also presented data that suggested unforeseen genetic instability occurring in transgenes within an ecological, rather than agronomic, context. This case elevated the scientific and political significance of gene flow from GM crops to conventional varieties and wild relatives. The second case analyzes research by John Losey and colleagues at Cornell University suggesting that *Bt* corn might be lethal to monarch butterflies. Their laboratory experiment was the first evidence of the potential

for unintended non-target effects of GM crops expressing an insecticidal protein. Monarch butterflies have since become the ‘charismatic mini-fauna’¹⁷ of activism against agbiotech – protests have included children, dressed as monarch butterflies, collapsing in the presence of a mutant ear of corn. The third case stems from research coordinated by Arpad Pusztai at the Rowett Research Institute in Aberdeen, Scotland. His team conducted a study to measure physiological differences between rats fed GM potatoes and conventional potatoes. Their results showed stunted growth and immune deficiencies among the experimental (GM-fed) rats, and their methodology suggested that the technical *process* of genetically modifying a food crop might introduce a health risk to animal or human consumers. This study served as a critical inspiration to the strong movement in the European Union against GM food.

These three cases of contrarian science anticipated controversy because of the incompatibility between their major claims and the mainstream discourse of promotional science. In isolation, the research approach, methodology, and interpretive claims of the contrarian scientists could not have predicted the resistance they engendered. Crucially, however, these performances of science did not appear in a ‘neutral’ scientific context, but rather within the politicized context of promotional science with its associated assumptions.

¹⁷ Chris Henke, Assistant Professor at Colgate University, and I both began using this term and consider it a mutual invention of sorts. Neither of us is seeking a copyright.

Chapter 3 Promotional Science: Workshop on Biotechnology for Horticultural Crops

In March 2002 a collection of organizations under the University of California umbrella sponsored a workshop in Monterey, California entitled, “Workshop on Biotechnology for Horticultural Crops: Challenges and Opportunities” (Hort-Biotech Workshop). Sponsors included: the Agricultural Issues Center (AIC), the BioSTAR Project, the College of Agricultural and Environmental Sciences, the Division of Agriculture and Natural Resources (DANR), the Giannini Foundation, and the Seed Biotechnology Center.¹⁸ The seventy-two official participants included industry scientists, academic researchers, government employees, biotech entrepreneurs, representatives from trade associations, graduate students (including myself¹⁹), and one representative from an NGO (Center for Science in the Public Interest). The three day program covered scientific, economic, regulatory, and commercial aspects of horticultural biotechnology, focusing on the broad question of *how to overcome the hurdles preventing biotechnology from becoming a tool used in horticultural crops*, especially in California. As a follow-up, the AIC published an article in its quarterly newsletter that described the workshop and its findings. In addition, the April-June 2004 edition of *California Agriculture* (Volume 58, Number 2), published by DANR, was

¹⁸ While many of these organizations within the UC system have a ‘promotional’ character and mission (Jeff Romm, personal communication, 7 June 2005), the broad mandate of the land grant colleges remains one of furthering the public interest. It is precisely this construction of promoting agbiotech as *connected to* the public interest that creates the particular landscape upon which dissent must perform.

¹⁹ I acknowledge the UC BioSTAR project for providing a travel award which covered my transportation and lodging expenses during the conference.

titled “Fruits of Biotechnology Struggle to Emerge,” and consisted mostly of articles that were revisions or compilations of presentations at the Hort-Biotech Workshop.

This chapter explores the Hort-Biotech Workshop and the associated issue of *California Agriculture* as a performance of promotional science. Below I discuss aspects of narrative, stage management, character development, and audience construction.

3.1 Narrative

As a performance, the Hort-Biotech Workshop told a compelling story about biotechnology for horticultural crops (e.g., vegetables, fruits, nuts, ornamentals). At the most general level, the narrative could be summarized as follows:

While modern biotechnology has become an important technology in agronomic crops²⁰ in the United States, horticultural biotechnology has lagged in terms of research, product development, and commercialization. This situation is *tragic* because agbiotech offers environmental benefits, nutritional benefits, and economic benefits to all parties in the supply chain of horticultural crops. Barriers include:

- reduced incentive for investment in research and development due to smaller acreages of horticultural crops compared to agronomic crops
- difficulty in obtaining ‘freedom to operate’ in the midst of a complicated patent thicket of enabling technologies (knowing who owns what patents and getting approval for licensing prior to commercialization)
- high costs for regulatory approval (time, uncertainty, and cost of providing required data)
- national policies that prevent the importation of GMOs (e.g. Japan, European Union), which, in a globalized marketplace, require segregation of GM crops from non-GM crops to protect access to non-GMO markets
- lack of consumer acceptance, or at least the perception by corporate entities of the potential for consumer rejection due to activist campaigns that target companies that market GMOs.

Scientists, public organizations, and industry will need to cooperate to overcome these barriers. Promising strategies include:

- increasing public investment in research and development of horticultural biotechnology
- creating new institutional forms that improve access to intellectual property and freedom to operate (e.g., patent pooling, patent databases)
- reducing regulatory burdens (e.g., creating a GRAS [generally recognized as safe] category for transgenic *traits* rather than approval for each transformation *event*)
- developing output-oriented technologies that offer consumer benefits that outweigh perceived risks (e.g., lycopene enriched tomatoes)

²⁰ Agronomic crops are also known as field crops, major crops, or row crops (e.g., corn, soy, and cotton).

developing technologies that might achieve commercialization ‘under the radar’ of regulatory or consumer concerns.

A notable aspect of the narrative was the sense of impending linear progress toward a commercial goal. For example, a graphic below the table of contents in the *California Agriculture* issue (p. 66) depicted a sprinter facing five hurdles (Figure 2). Rhetorically and figuratively, this graphic communicated a number of striking propositions. First, the barriers to commercialization were meant to be leapt over (or perhaps crawled under) – none were so formidable or worthy of deep consideration so as to derail the project of bringing horticultural biotechnology to market. Second, the goal was “market success,” rather than a more clearly defined social benefit such as more environmentally sustainable agriculture, increased access to nutritious food for domestic or foreign consumers, or improved stability of agricultural economies (all mentioned at various times during the workshop). This communicated either the displacement of public goals by a private goal, or more likely, the conflation of these two goals as equivalent (see Chapter 5). Third, the location of the river of “consumer acceptance” as the *last obstacle* in the series suggested that consumers (or citizens) have little role in this journey, except as targets of market research (second hurdle), until everything else has been overcome.²¹ Could a reader (media-audience) of this graphic imagine this runner leaping over all of these hurdles, gaining momentum, and then failing to cross the river? No, this little piggy was going to market – consumers might cause delays or difficulties, but they would not derail the project of horticultural biotechnology.

²¹ The creator of the graphic ordered the hurdles and river with obvious intention: each hurdle bears a page number to reference a research article associated with the topic, and the pages do not occur in perfect order.

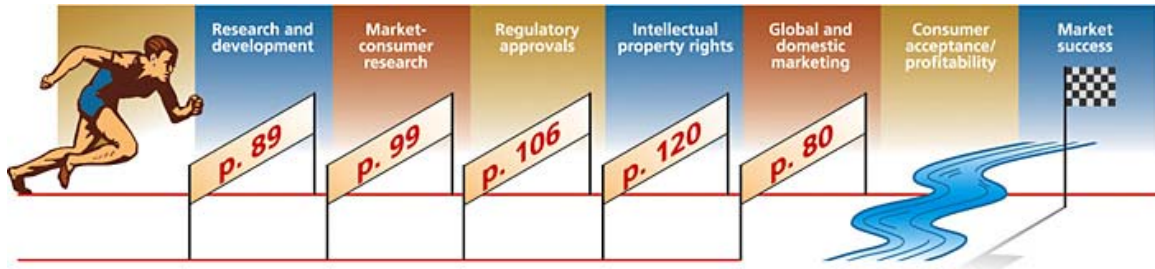


Figure 2. Hurdles to horticultural biotechnology

Similarly, the opening keynote presentation of the Hort-Biotech Workshop characterized the issues of the workshop as “how we get these products to marketplace” and to “enable the technology to move forward” (Cook 2002).²² The next morning began with a presentation entitled “Status of biotechnology in vegetable and ornamental crops” that had a depressing tone and sounded like a lament about the pitiful market state of horticultural biotechnology compared to its exciting technical potential [Klee 2002].²³ During the final session, “Working session to summarize workshop and identify objectives for research and policy development,” one participant enthusiastically proclaimed to the room that we needed to “Just Do It! [This] technology is too good to let it be ruined by activists or multi-nationals.” The editors of the *California Agriculture* issue made the narrative stance of the workshop and its associated publication quite clear:

While recognizing that there are alternative viewpoints, we do not question the potential value that biotechnology can bring to horticulture...We believe that the responsible application of biotechnology is compatible with and has much to contribute to agricultural and environmental sustainability while helping to maintain the competitiveness of U.S. horticultural products in the global marketplace (Bradford, Alston et al. 2004).²⁴

²² Dr. Roberta Cook is Cooperative Extension Specialist, Department of Agricultural and Resource Economics, UC Davis.

²³ Dr. Harry Klee is Professor, Department of Horticultural Sciences, University of Florida, Gainesville.

²⁴ Dr. Kent Bradford directs the UC Davis Seed Biotechnology Center and is Professor, Department of Vegetable Crops, UC Davis; Dr. Julian Alston is Associate Director for Science and Technology Policy, UC Agricultural Issues Center and Professor, Department of Agricultural and Resource Economics, UC

What I would like to emphasize is that none of the quotations or descriptions in the preceding paragraphs came from the industry representatives participating in the workshop (aside, perhaps, from the unattributed quote, “Just Do It...”). All sources held appointed positions at public universities. This is significant because public employees at a quasi-public workshop on agriculture have constructed a narrative that pushes forward a controversial technology without taking pause to evaluate the logic or substance of concern among the broader public.

A related aspect of the narrative built upon this technological enthusiasm by connecting to the discourse of biotechnology as an economic engine. In his presentation on “Regulatory Challenges for Horticultural Biotechnology,” Keith Redenbaugh²⁵ (2002) included five slides on China as a “major competitor.” His corresponding article in *California Agriculture* stated:

China is taking full advantage of uncertainty caused by the European Union’s stance on biotech approvals. Beijing University vice president Chen (1999) stated, “I expect that in ten years between 30% and 80% of the rice, wheat, maize, soya, cotton and oilseed crops in China will be transgenic crops. We can take advantage of this four year halt [EU moratorium] to turn China into a world power in genetically modified organisms”... While the United States falters over biotech fruits and vegetables, China is positioning itself to be the world leader in coming years. For the American horticultural industry, the results could be devastating if the United States loses its current competitive edge and more agricultural production moves overseas (Redenbaugh and McHughen 2004).²⁶

The editors referred to this trend in their overview (Bradford, Alston et al. 2004, 69), and a separate article detailed China’s commitment to biotechnology research, expansion of area planted to fruit and vegetables, ongoing commercial releases of transgenic technologies, emergence as an agricultural trading nation, and improved education of

Davis; Dr. Daniel Sumner directs the UC Agricultural Issues Center and is Professor, Department of Agricultural and Resource Economics, UC Davis; and Dr. Peggy Lemaux is Cooperative Extension Specialist in Agriculture and Biotechnology, Department of Plant and Microbial Biology, UC Berkeley.

²⁵ Keith Redenbaugh is Associate Director, Seminis Vegetable Seeds, Woodland, CA.

²⁶ Alan McHughen is Plant Biotechnologist, Department of Botany and Plant Sciences, UC Riverside.

scientists involved in plant biotechnology (Huang and Rozelle 2004).²⁷ The narrative impact is clear; the message to the audience and media-audience was that speeding up research, development, and commercialization of horticultural biotechnology was an issue related to national security. Such a message carried significant weight in the shadow of post-9/11 fears of American vulnerability.

3.2 Stage Management

Issues of stage management contributed to the narrative of the Hort-Biotech Workshop as a performance in a more subtle manner than the content of the presentations or the journal articles. In this section I will address three aspects of stage management: the location choice, the program design, and the management of the science/policy boundary.

Location

The location of the workshop sent several messages to participants. Despite being an event sponsored by organizations within the University of California, the workshop was not held on or near a UC campus, but at the Plaza Hotel in Monterey, California. The Plaza Hotel is extremely upscale and expensive²⁸ – presumably chosen to attract corporate participants more accustomed to luxury accommodations. We enjoyed a relatively high class of service at meals (provided without additional charge) and during

²⁷ Jikun Huang is Director, Center for Chinese Agricultural Policy, Institute of Geographical Sciences and Natural Resource Research, Chinese Academy of Sciences, Beijing; Scott Rozelle is Associate Professor, Department of Agricultural and Resource Economics, UC Davis, and Associate Director of the UC Agricultural Issues Center.

²⁸ Standard room rates in February 2005 varied from \$190 to \$550 (see http://www.woodsidehotels.com/monterey/monte_rates.htm).

breaks. Despite conversations about improving nutrition and reducing world hunger, the reality at the workshop was that the agbiotech project, broadly, was providing significant resources to conference participants in the form of salaries, corporate equity, research dollars, and occasional luxurious accommodations. It is also worth mentioning that the Monterey region was both a major producer of horticultural crops and the site of the first sanctioned release of GMOs into the environment (“ice-minus” spray in 1987).²⁹ These backdrops, while not overtly mentioned during the workshop, served as reminders of a) the tremendous economic implications of introducing biotechnology into the horticultural industry, b) the potential for technology to overcome public opposition, and c) the apparent irrationality (in retrospect) of anti-GMO critics given that their doomsday scenarios about ice-minus failed to materialize.

Program design and agenda

Workshop organizers framed the event in a way that attracted some persons and not others. Workshop speakers also had discretion in choosing the content and tone of their presentations, which collectively helped shape the audience as a dynamic entity that formed during the event. Editors of the *California Agriculture* issue likewise made framing choices that attracted a media-audience with certain interests. Finally, publishers of *California Agriculture* and other media that covered the workshop have worked to attract readers over time – who thus became a part of the Hort-Biotech Workshop’s media-audience.

²⁹ For information on the “ice-minus” controversy, see Weiner (2001).

The process of designing the program remained hidden from the audience but had significant impacts on the performance. First, the use of the word “workshop” emphasized an atmosphere of cooperation – as opposed to a ‘conference’, at which one might expect greater diversity of goals and some conflict between participants. The inclusion of meals, receptions, and social breaks also contributed to the ambience of a group working together. For methodological purposes, I did not publicly challenge many of the points and comments I found objectionable, but anyone with alternative views faced a cultural space whose highly collegial character discouraged outright opposition. Indeed, my notes from the conference included scant examples of contrarian comments or combative questions.

At a general level, the program design validated the oft-repeated criticism of biotechnology as a ‘solution looking for a problem.’ The ‘problem’ presented by the workshop and reflected in its agenda was the paucity of research, development, and commercialization of horticultural biotechnology in the United States. This was perhaps a problem for scientists and corporations committed to the technology, but did not reflect a social or public problem statement (e.g., how to reduce harmful pesticide use, how to improve nutrition, or how to reduce food costs). The agenda reinforced the notion that the responsible strategy was to overcome barriers to horticultural biotechnology, not question its utility, appropriateness, or safety; not a single panel or presentation addressed these latter issues. It is worth noting that the workshop occurred only three months after Quist and Chapela’s (2001) *Nature* article announcing the introgression of transgenic DNA into Mexican landraces of maize. Controversy had just begun to heat up, yet no presentation addressed the issue of unwanted gene flow from GM crops. The one speaker who

addressed the *Nature* publication directly dismissed it as “flawed” and “irresponsible.” Participants surely had knowledge of and opinions about the controversy (especially those coming from UC Berkeley), but the agenda created no space for such an issue to enter formal discussion.

In contrast, the agenda provided ample space for fostering enthusiasm for technological strategies that overcame or sidestepped regulatory or consumer concerns. After the first day of sessions mostly dedicated to discussing the barriers to horticultural biotechnology, the second day began with the facilitator’s call for “No Whining!” as the session “Opportunities for future development of horticultural products” began. Presenters then proceeded to introduce up-and-coming applications of horticultural biotechnology in light of how they broke through the barriers outlined the day before.

The manner in which the workshop ended demonstrated powerful stage management. Gary Hudson, an industry consultant, facilitated the final session, “Working session to summarize workshop and identify objectives for research and policy development.” He began by breaking up all participants into small groups, assigning the task of discussing “next steps” – which products, policy objectives, and IPR issues deserved immediate attention. Although not translated into specific action items with assigned responsibility, ideas with broad consensus emerged as the small groups reported back and a plenary discussion ensued. The energy in the room was high and an optimistic tone prevailed. Organizing the final session in this manner served to reinforce the message of “we’re all in this together” (common goals) and to focus attention on the project of overcoming barriers to the progress of horticultural biotechnology. What had been excluded were a whole set of issues and questions around ecological concerns,

health concerns, and alternative paths to the social benefits presumed of the development of horticultural biotechnology. These areas of silence reflected both the range of participants' priorities as well as the program design and agenda, combining to create a powerful, but mostly hidden, norm of discourse.

Managing the science/politics boundary

This final session at the workshop resulted in the publication of a chart in the *California Agriculture* issue that demonstrated serious attention to managing the science/policy boundary. The sidebar chart, "Objectives for horticultural biotechnology" (p. 70), appeared in the editorial review article with the label, "A set of key research and policy objectives were developed out of discussions at the Workshop on Biotechnology for Horticultural Crops in Monterey." In contrast to the consistent integration of science and policy at the workshop (even within individual presentations), the chart divided the objectives neatly into two columns: "Research" and "Policy." This bit of stage management reified the socially constructed boundary between scientific activity and political activity, rhetorically reassuring readers of *California Agriculture* that science (objective, unbiased, pure from the pollution of politics) knew its place. It also called attention to the difference between 'practicing' scientists and those who operated in more managerial roles, riding the political boundary. From a strategic standpoint, this tied into the narrative of science occupying a social space above public criticism, which served to protect agbiotech (as a product of science) from political or ideological challenge. At the same time, it reminded the media-audience that policy work is necessary to make the world 'safe' for science to share its fruits.

3.3 Characters

In the tradition of Actor-Network Theory (ANT) (Callon 1999 [1985]; Latour 1999 [1983]), I discuss character formation for a wide variety of actors (living and non-living) including technologies, concepts, persons, and institutions. These characters support and extend the narrative described above, enhancing the persuasive power of the performance. I divide my discussion according to theatrical logic: presenting the protagonist, the supporting cast, the villains, and finally the heroes.

Protagonist – Horticultural Crops

The protagonist of the Hort-Biotech Workshop as performance was the category of horticultural crops. They included “fruits, vegetables, sweet corn, nuts, ornamental and landscape plants that are generally grown on smaller acreages than agronomic/field crops” (“Survey: *California Agriculture* Readers Diverse, Well-Educated” 2003). Speakers often referred to them as “minor crops” (in comparison to major field crops such as corn or soybeans), and distinguished a sub-category of “orphan crops” (e.g., radishes, artichokes – in reference to specialty crops with too small a market even to attract enough attention from agricultural chemical companies to develop and secure regulatory approval for associated pesticides).³⁰ The term “orphan,” in particular, encouraged sympathy for this character – connecting to the narrative that these crops

³⁰ “The IR-4 Project is a unique partnership of researchers, producers, the crop-protection industry and federal agencies designed to increase pest-management options for specialty crops... With funding from the U.S. Department of Agriculture, state agencies, commodity groups and other industry sources, IR-4 researchers and cooperators generate field and laboratory residue data, which are submitted to the U.S. Environmental Protection Agency (EPA) to secure regulatory clearances for using safer pest-control techniques on specialty crops” (Holm and Kunkel 2004). One prominent idea raised at the conference was to secure additional funding for IR-4 to expand its work in achieving regulatory clearances for transgenic horticultural crops (Holm 2002).

(more than the problems they purport to solve) deserved our attention. It was noteworthy that only one presentation paid significant attention to the plight of the growers, specifically papaya farmers in Hawaii (Gonsalvez 2002). While the ‘small-farmer’ imaginary may have supported the casting of horticultural crops as protagonist, it operated with relative invisibility.

As characters, horticultural crops were vulnerable and problematic, but full of potential. Presentations emphasized their vulnerability to insect pests, weeds, viruses, and other diseases and the unenviable journey from farm to fork that included opportunities for bruising and over-ripening; on their own (without pesticides, special handling, or a transgenic trait), they were weak.³¹ To bring a beautiful, good-tasting, marketable crop to market required extensive human intervention. Rhetorically, this characterization reduced the ‘natural-ness’ of horticultural crops, making them more a site for technical intervention than a life form.³²

As a category, horticultural crops posed special challenges to the seed industry because of the incredible diversity of varieties required, even within one product category:

[T]he diversity of crops utilized in horticulture slows the adoption of new technologies. For any given crop, there may be several different species and dozens of cultivars that are currently marketed, and the turnover of new cultivars from year to year is tremendous. For example, as many as 60 distinct cultivars of iceberg lettuce alone may be grown throughout the year as production locations shift seasonally (Clark, Klee and Dandekar 2004, 95).

³¹ Although the range of horticultural crops discussed at the workshop included nuts, some of these characteristics would not apply (e.g. vulnerability to bruising).

³² One could argue that major field crops are even further ‘de-naturalized’ by the transformative processing that occurs before consumers encounter them (e.g. milling, bleaching, distilling).

While biodiversity usually carries connotations of strength and resilience, this degree of crop diversity further characterized horticultural crops as vulnerable technologies with attendant complex management regimes. Of course, one could argue about the degree to which any given crop acts as ‘nature’ or ‘technology,’ but I am satisfied with Haraway’s (1997) perspective that favors a hybrid view, intermingling nature and culture. My point, therefore, is not to enter the debate about the biological or evolutionary dependence of horticultural crops on human action, but to highlight how the performance of this workshop emphasized the quality of horticultural crops as a site for technological intervention. Furthermore, the combination of small markets and multiple varieties reduced the economic incentive for introducing biotechnology into horticultural crops:

Their limited acreage makes it more difficult to recover the research and development costs of any new technology specific to these crops. Because of the limited size of the individual markets, the costs of gaining access to patented genetic-engineering methods and meeting the regulatory requirements for testing and registration of biotech crops represent substantial economic hurdles for horticultural products (Bradford, Alston et al. 2004, 69).

Interestingly, these collective observations did not shift attention *away* from horticultural crops, but instead rallied great emotional sympathy. The performance made the protagonist vulnerable and problematic, not to dump by the wayside in favor of a focus on biotechnology in *major* crops or low intensity/investment technologies and practices to grow horticultural crops; rather, the protagonist was positioned to be saved.

Part of the rationale for this narrative move stemmed from the emphasis on the nutritional and economic potential of horticultural crops. In her presentation at the Hort-Biotech Workshop, Roberta Cook (2002) estimated the U.S. fresh fruit and vegetable value chain as culminating in \$75.8 billion for consumers in the year 2000. She reminded participants that the U.S. was both the largest importer and exporter of horticultural

crops. A brochure provided in the workshop binder focused special attention on California's strong role in the industry:

California is recognized around the world for the quality and variety of seeds produced in the state...Most international seed companies have research and production facilities in California, growing and processing seeds of a wide array of vegetables, flowers and agronomic crops. More than half the seeds produced in California are marketed nationally or internationally, highlighting the importance of California's seed industry in the global agricultural economy (University of California 2000).

So while horticultural crops may have been biologically vulnerable and a problematic site for technological development, as a category (and character) they were worth significant attention because of their cumulative value.

The most fascinating aspect of character development involved the rhetorical linking of domestic horticultural crops and subsistence crops in developing countries. At the workshop, Brian Wright³³ (2002) described minor crop markets as having "common cause with staple crops in LDCs (Less Developed Countries)." The keynote evening speaker, Richard Jefferson³⁴, linked the two categories in the title of his presentation, "Biotechnology for minor crops and developing countries," which centered on the need for a new institutional model to hold and license intellectual property for agricultural biotechnology. In the penultimate session, "Policy issues for the commercialization of horticultural biotechnology," C.S. Prakash's³⁵ (2002) talk on international policy issues focused on the importance of bringing agbiotech to developing countries: "There is a need outside the industrial world." He discussed the tremendous potential for agbiotech

³³ Brian Wright is Professor, Department of Agricultural and Resource Economics, UC Berkeley.

³⁴ Richard Jefferson is President, Center for the Application of Molecular Biology to International Agriculture (CAMBIA), an NGO committed to developing and licensing new tools for agricultural biotechnology to improve research and development.

³⁵ C.S. Prakash is Professor, Tuskegee University, Alabama. He also edits the website, "AgBioWorld" (www.agbioworld.org).

to reduce pesticide use and improve nutrition, even suggesting that we should view “biosafety” as a “trivial issue” compared to other problems like cholera, and that “biosafety...is just an excuse to slow down biotechnology.”

The *California Agriculture* issue followed the workshop’s lead. The introductory article by the editors stated, “New licensing structures for enabling technologies developed in universities and public research institutions may be particularly helpful for small-revenue crops as well as for developing countries” (Bradford, Alston et al. 2004, 70). Four photos in the journal reminded readers of the stated connection: a produce market in Vietnam (p. 77)³⁶; a produce market in Ethiopia³⁷ (p. 88); a food market in Benin (p. 125); a food market in Ethiopia³⁸ (p. 125). Again, it was curious that the images deployed focused on the *transaction* of produced food rather than the process of food *production*, which would have included small farmers explicitly.

This rhetorical link between horticulture in the U.S. and agriculture in LDCs serves a potent purpose by connecting efforts to overcome barriers to domestic horticultural biotechnology with altruistic action to improve the quantity and quality of food for the global poor. In the pages of *California Agriculture*, the faces of (presumably poor and malnourished) Vietnamese and Africans came forward to embody the sibling relationship between minor crops and subsistence crops. Yet, none of the scientific talks addressed subsistence crops – all focused on minor crops in the U.S.. Second, the

³⁶ The caption stated, “A major promise of biotechnology is reducing the cost of delivering higher quality fruits and vegetables to malnourished or hungry people.”

³⁷ The caption read, “Supporters of agricultural biotechnology believe it can help to reduce pesticide use and provide more abundant food for an ever-increasing global population. Government can play a role in guaranteeing safety while ensuring that unreasonable hurdles are not preventing its broader distribution.”

³⁸ The caption for the two photographs stated, “New biotech crops must meet the intellectual-property and regulatory requirements of importing countries, and there are no firm rules as to which technologies will be protected or regulated in which countries. This situation can create serious difficulties for exporters.”

participant list did not include anyone who might credibly speak or advocate for subsistence farmers, hungry populations, or policies to improve global nutrition and reduce global hunger. Third, no speaker mentioned the unique institutional, cultural, ecological, and political challenges to marketing and deploying agricultural biotechnologies in a developing country context (e.g., increased likelihood of gene flow to weedy relatives, lack of capital to purchase premium agricultural inputs, dominance of seed-saving as a farming practice, pressure by international aid agencies to produce cash-crops rather than subsistence crops, etc.). In the language of performance, subsistence crops failed to achieve ‘character’ status, serving only to enhance the character of horticultural biotechnology through a rhetorical move stated simply as, “whatever we do to benefit horticultural biotechnology will certainly benefit the global poor.”

Supporting Cast – Supply Chain

The characters of the supporting cast included the people and organizations of the supply chain for horticultural biotechnologies. Below I briefly discuss the characters of scientists, corporations, growers, and distributors. These are, of course, not the only actors involved in supplying GM food, but some roles remained invisible at the workshop (i.e., farmworkers, transport workers, and others were not ‘cast’ in this particular performance).

Workshop participants, many with scientific affiliations, predictably characterized scientists as objective voices with altruistic tendencies. In his presentation about developing transgenic papaya to ‘save’ the Hawaiian papaya industry, Dennis

Gonsalves³⁹ (2002) stated, “[We were] just a bunch of scientists, not backed by industry.” After reminding the audience that eighty percent of papaya growers were first or second generation Filipinos, he said, “We are trying to help the people which is often forgotten in biotech.” Gonsalves thus implicitly acknowledged the critical discourse around agbiotech that accused scientists of operating out of self-interest (often because of ties to the agbiotech companies). That the altruism and objectivity of science *needed to be spoken* revealed that these qualities were contested in broader discourses. Similarly, a research update provided in *California Agriculture* reported:

UC Cooperative Extension farm (UCCE) advisors and researchers are growing genetically engineered (GE) alfalfa in small experimental plots to determine whether the technology will be beneficial to California farmers. “We would like to be ready with *research-based answers* when this technology is introduced,” says Steve Orloff, Siskiyou County farm advisor. “It’s somewhat controversial, but providing *unbiased research results* will enable growers to make intelligent decisions about it for themselves” [emphasis mine] (Warnert 2004).

Thus, in contrast to the clear advocacy-orientation of the workshop (with the goal of overcoming barriers to the commercialization of horticultural biotechnology) and the significant participation of industry, the traditional character of unbiased scientists working for the public good dominated the performance.

The characterization of the distribution chain (from farm to consumer) demonstrated a willingness of workshop participants to reflect thoughtfully on institutional and economic barriers to the commercialization of horticultural biotechnologies.

Even if the new technology is more cost-effective than the traditional alternative, monopolistic pricing could mean that the technology supplier retains a large share of the benefits. The cost savings passed on to processors and consumers may be a small fraction of the total benefits, rendering incentives for processors, retailers and consumers to

³⁹ Dennis Gonsalves is Professor, Department of Plant Pathology, Cornell University.

accept the technology comparatively small. Processors and retailers can effectively block a new technology if it does not clearly benefit them, even if there would be net benefits to the general public (Alston 2004, 86).

In other words, distributors (and buyers, wholesalers, and retailers) had their own calculus of benefits and risks, which might prevent the adoption of a technology with broad social benefit. Cook (2002) also emphasized how the consolidation of distributors on a global level translated into fewer opportunities for marketing horticultural biotechnology products – firms buying and selling in multiple national markets faced large costs of segregating GM food from conventional food in order to satisfy markets that excluded GMOs (e.g., Europe, Japan). “Large distribution firms can dictate standards independent of any regulatory system, so whether they agree to market a particular product can mean the difference between success and failure” (Bradford and Alston 2004, 85). Lastly, these firms had the same concerns about ‘identity protection’ as seed companies: “Processors are wary of jeopardizing their overall market position by risking pickets or protests from anti-biotech activists” (Bradford and Alston 2004, 84).

Chorus - Consumers

In the performance of the Hort-Biotech Workshop, consumers took on the character of a chorus – an intermittent voice that must be heeded by all actors who wish to successfully commercialize horticultural biotechnologies. One article in *California Agriculture* contrasted the “optimism generated by a long list of breakthroughs” with the “pessimism caused by a consumer backlash in some places” (Huang and Rozelle 2004, 112). Participants consistently characterized consumers as wielding great power and influence, which work directly through purchasing behavior or indirectly through other actors anticipating consumer rejection of agbiotech:

Currently, the largest impediment to adoption of at least some biotech horticultural products is the lack of market acceptance. Biotech products having documented agronomic, economic and environmental advantages have been removed from the market due to the concerns of processors and distributors about potential consumer rejection (Clark, Klee et al. 2004, 97).

In other words, the chorus of consumers had the potential to disrupt what could otherwise have been a fairly neat and scientific cost-benefit analysis to guide commercialization of horticultural biotechnologies.

The most critical aspect of consumer character development was the subtle, but consistent choice by workshop participants to limit discussion of public input to the category of consumers, rather than citizens (although a number of speakers referred to what might be considered a sub-group of citizens, activists, whom I discuss below). Jennifer James⁴⁰ began her presentation by stating that “market behavior is what we’re really interested in...not attitudes.” She reviewed the survey literature on consumer preferences, but emphasized that experimental auctions and market experiments offered much more important information (James 2002). In her research article James commented, “[T]here is a big difference between asking people if they think biotech products should be labeled and asking them how much more they would be willing to pay for those labels” (James 2004, 104). Again, this implied that what people thought should not affect strategic decisions about commercialization – what mattered was what would sell. She mentioned the discrepancy between survey results that showed “sizable opposition” to the use of recombinant bovine somatotropin (a transgenic hormone to increase milk production in cows), and consumer behavior which showed no change in

⁴⁰ Jennifer James is Assistant Professor, Department of Agricultural Economics and Rural Sociology, Pennsylvania State University.

the demand for milk.⁴¹ She concluded, “Consumers may say one thing but do another. Further, it is possible that consumer issues will fade once researchers stop asking consumers for their opinions about biotech products” (James 2004, 105). Her message seemed to imply that speech was of no value (because it was a bad predictor) and that the barrier of consumer rejection might be reduced to an artifact of misguided attitudinal studies. *If we stop asking the chorus what they think, they will likely be much more cooperative.*

This characterization carried significant implications for the performance as a whole. If citizenry becomes invisible, democracy would falter, as policy must conform to a ‘technical’ logic (which, of course would carry its own values despite claims of objectivity and neutrality). The Director of the UC Agricultural Issues Center wrote:

Global controversy over agricultural biotechnology has led to a bifurcated market for new technologies. Trade restrictions have reduced adoption and slowed the pace of scientific investment. It is unclear if this bifurcated market will continue or if governments will gradually *allow farmers and consumers to make their own purchasing decisions* [emphasis mine] (Sumner 2004, 78).

Sumner thus juxtaposed consumer freedom of choice with public policy, implying that the latter distorted the former improperly. James made it plain: “In the extreme, consumer concerns may drive policy decisions (as some argue has occurred in the European Union), with the resulting policies imposing costs on producers as well as consumers” (James 2004, 99). Reducing populations to consumers and then denying those consumers a voice in public policymaking rhetorically closed off major avenues of citizen participation in the governance of agbiotech..

⁴¹ James did not discuss the relative inelastic demand of a product like milk.

A related set of characterizations, which in some way justified the exclusion of consumers from policymaking, described consumers as ill-informed, untrustworthy, and malleable with respect to their opinions of agbiotech. A sidebar article entitled, “Words matter,” described the pitfalls of survey research. A 1994 national survey conducted at Pennsylvania State University asked respondents about their level of concern about “IMS” in seafood. Even after using two “filters [to] help minimize the tendency for survey respondents to overstate their concerns,” 18% of respondents said they were “somewhat or very concerned about IMS, a *food safety issue that does not exist*” [emphasis original] (Herrmann, Warland and Sterngold 2004).⁴² This finding, also mentioned at the workshop, not only challenged the validity of survey research, but also indirectly undermined the trustworthiness of consumers – suggesting that fear could too easily distort rational opinion. In particular, the ignorance and malleability of consumer opinion made the public vulnerable to anti-biotech activism: “Publicity stunts and negative information campaigns would have little effect on those who know about and understand the technology. The lack of consumer knowledge gives negative publicity campaigns their power” (James 2004, 105). In her review of consumer surveys, James (2004) noted:

[T]he most important and fairly consistent finding is that the majority of consumers are uninformed about biotechnology and, more generally, about how food is produced. Given these consumer characteristics, is biotechnology an aspect of the food system that *should* be consumer-driven? [emphasis original] (p. 105).

⁴² Robert Herrmann and Rex Warland are Professors Emeriti, Department of Agricultural Economics and Rural Sociology, Pennsylvania State University; Arthur Sterngold is Professor, Department of Business Administration, Lycoming College, Williamsport, Pennsylvania.

Again, reducing the public to ill-informed, untrustworthy, malleable consumers (a maintenance of ignorance) supported the exclusion of the public from governance (for the protection of agbiotech).

Not all workshop participants followed this line of thinking. Responding to calls for public education, Richard Jefferson (2002) claimed that educating the public about science would not, by itself, change public opinion. He suggested that the backlash against biotech derived from a “legitimate emotional response to consolidation of power” in agricultural and biotechnology industries. In the context of the workshop, this was a rare critique of the political economy of agbiotech. Perhaps because of his high status within the workshop as a key speaker, the audience did not react negatively to this claim despite its contradiction to so many other arguments throughout the workshop. This moment of contrarian discourse was effectively dismantled through silence.

Villains – barriers to commercializing horticultural biotechnology

Apart from the questionable role of consumers and the inherent economic difficulties in commercializing horticultural biotechnologies (discussed above), the performance created a number of ‘villains’ who bear direct responsibility for slow rates of innovation and adoption: activists, the regulatory system, and the intellectual property regime.

Participants argued that activists bore the blame for creating the appearance of consumer rejection of agbiotech. In the plenary session, one individual reminded the

group that “consumers are not worried,” only a small but vocal minority⁴³ opposed agbiotech. Another declared, “The real fight is with activists.” James (2004) wrote:

The small group that strongly opposes agricultural biotechnology is quite vocal. Anti-biotech activist groups such as Greenpeace and the GE Food Alert are adept at communicating with the public, and willing to use inflammatory language and theatrics, as seen in their Web sites (www.greenpeaceusa.org and www.gefoodalert.org) and public demonstrations. They may oppose agricultural biotechnology as a whole, but they often target individual companies (such as with mock company Web sites depicting products and brands as dangerous). Specific companies targeted may shift their focus from satisfying customers to avoiding negative publicity (p. 105).

According to this view, activists have not *represented* public opinion; they have shaped it. Furthermore, corporate avoidance of agbiotech could best be understood as the result of activist campaigns rather than corporations having taken the pulse of consumer or public opinion. Critically, the characterization of activists contrasted very sharply with scientific modes of discourse (e.g., “inflammatory language and theatrics”).

Participants questioned the validity of activists’ motives. “Agricultural biotechnology and globalization seem to go hand-in-hand in the popular press, and protesters condemn both in the same breath. This perceived bond is puzzling to those involved in the international agricultural trade” (Sumner 2004, 77). Sumner suggested that activist discourse confused and thus failed to understand biotechnology and globalization. A photo caption included in the *California Agriculture* introductory article (Meadows 2004) described the 2004 ballot initiative passed in Mendocino County, California, to ban the growing of GM animals and plants: “Proponents [the activists, in this case] were concerned about cross-contamination of organic crops by biotech seeds

⁴³ James (2002) emphasized the problem of a small but vocal minority, although her more comprehensive presentation of survey results show that the percentage of respondents who “strongly support” agbiotech are even *smaller* than the percentage who “strongly oppose” (James 2004). Are these active proponents perhaps a smaller and equally vocal minority?

and crops” (p. 73). This caption reduced the activist motivation to one of industrial protectionism, despite the fact that the photo includes a “YES on Measure H” pamphlet that listed five separate goals including health and environmental protection. During the discussion following the session, “Opportunities for future development of horticultural products”, a workshop participant offered a more spiteful analysis: “Activists are looking after their own job, not doing what’s best.” No presentation, discussion, or journal article included reference to arguments about health, environmental, and social impacts that have organized activist critiques of agbiotech (for example, see www.etcgroup.org). The narrow presentation of activist motives served the rhetorical purpose of vilifying them as characters in the performance who had nothing positive to contribute.

The Hort-Biotech Workshop presented the U.S. regulatory system as a qualified villain. In one sense, the perceived costs and uncertainties of the regulatory regime frustrated the commercialization of valuable horticultural products. Harry Klee (2002) listed the myriad regulatory agencies involved in GM crop approval and described the process as “not clean.” Irvin Mettler⁴⁴ (2002) reported that the regulatory costs associated with approving virus resistant squash were less than \$500 thousand, but that a new crop would cost \$1.5 million. He described “major uncertainties related to increasing regulatory costs” in the U.S. and the necessity of seeking approval under many national regulatory regimes for a globalized product. In their article entitled, “Regulatory challenges reduce opportunities for horticultural biotechnology,” Redenbaugh and McHughen (2004) discussed the “extensive safety data” required for each transformation event (p. 107), and they bemoaned the possibility that “the bar may continue to be raised

⁴⁴ Irvin Mettler is Director of Biotechnology, Seminis Vegetable Seeds, Woodland, CA.

as new testing technologies are developed” despite the “absence of evidence of harm” (p. 108).

The characterization of the regulatory system included strong defense of the concept and practice of regulation. Terry Stone⁴⁵ (2002) described the U.S. regulatory system as a “tea party” compared to other countries’ requirements and affirmed that U.S. regulatory oversight has increased product credibility and therefore has promoted global marketing. Participants in the plenary strategy session agreed that attempting to weaken or reduce regulation was “not advisable” because the public could interpret such efforts as lack of confidence in the safety of the products. In short, the cost and complexity of the regulatory system gave it a Dr. Jekyll character, but the existence of a regulatory regime that held the public’s confidence communicated that the promoters of the technology had nothing to ‘Hyde’.

Although public discourse around intellectual property rights (IPR) and biotechnology often has pigeon-holed biotech promoters as staunch defenders of IPR, the workshop created a space for participants to offer a sharp critique of IPR without undermining the goal of commercialization. Alston (2004) described IPR as “a double-edged sword: to the extent that they provide a greater incentive for investing in research they are also likely to result in lower adoption rates” (p. 81). Although no one suggested eliminating IPR for biotechnology processes or products, much discussion centered on

⁴⁵ Terry Stone is Regulatory Affairs Manager, Specialty/Horticultural and Ornamental Crops, Monsanto.

the difficulty of licensing enabling technologies to conduct research and commercialize new products. Graff et al.⁴⁶ (2004) wrote:

The costs and headaches involved in working out “who owns what” and “who owes what to whom” can balloon into what economists call the “tragedy of the anticommons” and render the development process unfeasible. The “tragedy” is arguably worse in horticultural crops than in row [field] crops. Given the smaller markets involved, there is less incentive in industry to consolidate IP portfolios around horticultural crops (p. 124).

The evening keynote suggested a new institutional model to manage intellectual property in such a way to make basic tools and foundational technologies more accessible to innovators worldwide (Jefferson 2002). With a similar approach to critiquing the regulatory system (without calling for its demise), workshop participants advocated for IPR reform without questioning the foundational principles of the institution of IPR.

Heroes – overcoming barriers to commercialization of horticultural biotechnology

The heroic characters within the Hort-Biotech Workshop ranged from technological, to institutional, to political-economic. While participants discussed many ideas, I highlight the characters that emerged as prominent heroes in the final plenary session focusing on scientific and political strategies to encourage horticultural biotechnology commercialization.

Given the risk-benefit framing of the issue of consumer acceptance, the most obvious class of heroes were technologies with increased benefits and/or reduced risks. Reducing the perceived risk might include engaging biotechnology for non-transgenic

⁴⁶ Gregory Graff is Researcher, and Brian Wright and David Zilberman are Professors, Department of Agricultural and Resource Economics, UC Berkeley. Alan Bennet is Professor, Department of Vegetable Crop Science, UC Davis and Executive Director, Office of Technology Transfer, UC Office of the President.

applications such as marker-assisted selection (Roose 2002)⁴⁷ or gene-silencing to delay fruit ripening (Dandekar 2002)⁴⁸. Improving the benefits required moving from first generation GM crops with so-called “input traits” (with direct benefits to growers such as herbicide tolerance or pest resistance) to second generation GM crops with “output traits” such as improved nutrition (Zischke 2002)⁴⁹, new flower colors, improved floral fragrance, or slower-growing turf grass to reduce mowing frequency (Clark 2002).

A related category of technology heroes possessed a more ‘stealth’ characterization. John Driver⁵⁰ (2002) introduced two examples:

1. Plant *Bt* apple trees as a “trap crop” within walnut orchards. The transgenic apple trees attract and kill the codling moth, a walnut pest, because of the moth’s tremendous preference for apple trees. The consumable product, walnuts, remains conventional (non-transgenic).
2. Create transgenic rootstocks to protect against diseases (e.g., crown gall) and graft a conventional scion onto the rootstock. The fruit and pollen, produced by the scion, presumably remains conventional, carrying no transgenic trace.

The room was buzzing (the chorus stirring) as Driver described these possibilities, and they emerged as favorites during the plenary session. Taking a slightly different tack, Kathy Means⁵¹ (2002) argued for the “need to bring biotech home to consumers...[it] may not provide a social benefit, but engender familiarity...[which is the] key to initial success.” Technological examples focused on transgenic ornamental flowers (non-food,

⁴⁷ Mikeal Roose is Professor, Department of Botany and Plant Sciences, UC Riverside.

⁴⁸ Abhaya Dandekar is Professor, Department of Pomology (fruit and nut crops), UC Davis.

⁴⁹ Mary Zischke is Vice-President, Mixed Vegetables, Dole Fresh Vegetables.

⁵⁰ John Driver is President, Dry Creek Labs, Modesto, CA.

⁵¹ Kathy Means is Vice-President, Produce Marketing Association, Newark, DE.

but in one's home) and transgenic turf grass (literally, "in your backyard," as one participant quipped in the plenary session). Collectively, these heroes represented a dual emphasis on side-stepping regulatory and consumer barriers and overcoming the negative, anti-biotech emotional attitude held by a significant number of consumers.

The third group of heroes reflected an institutional character by focusing on public-private partnerships. The executive director of the IR-4 program (see footnote 30) described a cooperative effort involving IR-4, Monsanto, and Seminis Vegetable Seeds to gain approval for Roundup-Ready™ lettuce. Although grower and distributor concerns slowed the process to a halt, the partnership overcame the significant barrier of regulatory approval following the model of gaining pesticide approvals for orphan crops (Holm 2002). A second example involved the promotion of public-private partnerships at earlier stages of research and development:

Horticultural research is conducted primarily in the public sector, with research at private institutions playing a relatively minor role. As a result, research gaps naturally emerge between the basic research generated by public institutions and the research needs of industry. One approach for reducing this gap is to form public-private research partnerships that harness the complementary research and academic expertise of universities with the commercialization and marketing expertise found in industry (Rausser and Ameden 2004).

At the workshop, Rausser⁵² (2002) showcased the UC Berkeley-Novartis agreement as a shining example of such a partnership. Both his workshop presentation and associated journal article discussed aspects of institutional design and strategy to make these relationships most productive, while protecting the integrity of public institutions.

⁵² Gordon Rausser is Professor, Department of Agricultural and Resource Economics, UC Berkeley; during the negotiation and signing of the UC Berkeley-Novartis agreement, he served as Dean of the College of Natural Resources, UC Berkeley.

The final heroic character emerging from the performance revealed a form of ruthlessness within promotional science. Specifically, workshop participants looked to agricultural crises (whether instigated by biological or political processes) to create openings for agbiotech in horticulture. As a historical example, Gonsalvez (2002) acknowledged that the crash of the Hawaiian papaya industry altered the context for regulatory approval and public acceptance of transgenic papaya. Klee (2002) pointed to the likely phase-out of methyl bromide in the U.S. as a crisis ripe with opportunity for commercializing transgenic tomatoes, peppers, and strawberries: “Lack of effective replacement presents a unique opportunity. Growers will pay a large premium or be forced out of business.” Other participants mentioned Pierce’s disease (which affects grapes and thus far lacks an effective management option) as an opportunity for transgenic research and development. Together, these examples carried the message that a crisis would dull the opposition to horticultural biotechnology, perhaps at both the level of regulatory approval and market acceptance. The emphasis on, and indeed the excitement about, these crises fueled the narrative of a ‘solution looking for a problem’.

3.4 Audience

Although a public institution (the University of California) sponsored the Hort-Biotech Workshop and no overt restrictions on registration existed, the audience lacked representation from a range of public interests in horticultural biotechnology. One announcement for the workshop, appearing in *Agricultural Issues Center Quarterly* (“Biotechnology workshop set for March” 2001), published by the University of California, stated:

A principal objective of the workshop is to foster open communication. Invited participants will include scientists and economists from UC and other Land Grant universities, grower/shipper representatives and commodity board leaders from a range of horticultural crops, representatives of regulatory agencies (USDA, EPA, FDA), seed and nursery companies, food processors and marketers, food and nutrition groups, and consumer and marketing experts (p. 4).

Although the list appeared extensive, “open communication” still excluded the perspectives of those who were not invited or ignored invitations, as detailed below.

A separate dimension of audience construction related to the media-audiences. These audiences lacked the ability to actively engage with the actors during the performance, but they spread wider than the workshop participants, both in number and in diversity of readership. The Agricultural Issues Center, one of the primary sponsors of the workshop, produced a quarterly newsletter that both announced the workshop (see above) and ran a summary of the event ("Horticultural biotechnology issues aired at Monterey workshop" 2002). In 2005, their newsletter had a circulation of nearly 3000 contacts, roughly representing 25% university/cooperative-extension/education, 25% government, 45% agricultural business/industry, and 5% other.⁵³ *California Agriculture*, one of the oldest continuously published land-grant university research publications in the U.S. (first published in 1946), had approximately 14,000 domestic and 1,700 foreign subscribers in 2005. According to a survey they conducted in 2002, 31% of readers identified themselves as university faculty, 22% as corporate officers/managers, 22% as professionals (e.g., doctors, attorneys, lab technicians), and 5% as elected officials ("Survey: *California Agriculture* Readers Diverse, Well-Educated" 2003).

⁵³ I thank Laurie Treacher of the Agricultural Issues Center for providing me with details of their January 2005 subscription list (personal communication by author, 4 March 2005).

In contrast to the stability of readership of ongoing publications, the construction of the workshop's (immediate) audience depended largely on the list of invited speakers. Nearly half of the attendees listed in the workshop binder gave presentations (34 out of 72), and it seems likely that many other audience members registered with some association with or interest in hearing those speakers. Who was missing? Without suggesting a change in the focus of the workshop (challenges and opportunities for commercializing horticultural crops), I suggest a number of voices absent from the agenda:

1. *No scientist presented research about potential negative ecological or health impacts of GM crops.* Such speakers could have a) provided information about the technical rationale behind some of the opposition to agbiotech, which would have assisted in discussions on how to deal with the 'activist problem', and b) suggested safety data or technological changes that might make horticultural biotechnology more palatable to consumers and regulatory institutions.
2. *No NGO representative (or researcher of such groups) presented the history, philosophy, or politics behind opposition to agbiotech.*⁵⁴ Such a presentation could have a) offered a thicker description of activism, which was identified in the context of the conference as a major barrier, and b) created the opportunity to identify (if not explore) points of contention. As it was, many potentially contentious points were glossed over because of the lack of diversity of

⁵⁴ Dr. Doug Gurian-Sherman from the Center for Science in the Public Interest is listed on the participant list, but he did not serve as a speaker. Nor did I note a comment by him, although it is possible that he spoke during the meeting.

viewpoints in the room. This could have undermined strategies if agbiotech proponents had failed to anticipate arguments against horticultural biotechnology.

3. While marketing association representatives spoke on behalf of consumers, *no consumer group representative spoke directly*. This might have provided deeper understanding about consumer attitudes and behaviors and fostered opportunities to educate consumers or involve them in developing attractive horticultural biotechnologies.
4. *No NGO representative, researcher, or international expert spoke about the connections among horticultural crops, biotechnology, subsistence agriculture, and hunger in LDCs*. Without such information, the narrative linking progress in horticultural biotechnology to improved nutrition in LDCs remained a tenuous assumption (see Chapter 5).
5. Although speakers included many representatives of the supply chain (small scale biotech firms, transnational biotech/seed companies, major agricultural commodity firms, distributors, and marketers), *no grower or grower's association provided information about the context for technology adoption at the farm level*. This might have strengthened the argument that growers truly appreciated the benefits of agbiotech and wanted more technology, and/or offered a richer view of the kinds of technologies that would appeal to growers enough to make them take the financial risk of growing GM crops and exercise their political muscle to encourage public support of such technologies.

Not surprisingly, the full participant list also lacked these same voices. Such groups and persons chose not to come, could not attend for logistical or financial reasons, or were outside the network of professionals to whom organizers advertised the workshop.

Partly due to ‘stage management’ techniques of creating the assumption of common goals, and partly due to the absence of any expertise that could offer meaningful challenge to perspectives presented at the workshop, conflict was mostly absent or invisible. For example, one workshop participant (who made no formal presentation), a government employee who worked with growers directly, explained during casual conversation between sessions that growers “won’t touch Roundup-Ready lettuce...They are conservative...They want to know what’s in the twenty-two page licensing agreement [that growers of herbicide-tolerant crops must sign at the time of purchase].” This informal, but incisive observation about the cultural barrier between growers and the practice of technology-licensing was invisible at the level of the workshop as performance. In that sense, the audience was not only constructed⁵⁵ to minimize opposition, but controlled to minimize the expression of doubt or concern.

3.5 Summary

The Hort-Biotech Workshop embodied a performance of promotional science. Its dominant narratives, supported by character development and stage management, emphasized the promising scientific, political, economic, and ecological benefits of GM crops. The audience, constructed both formally and informally, engaged in strategic

⁵⁵ I use the term ‘constructed’ not to imply heavy-handed control from above (e.g., conference organizers deliberately choosing speakers after ensuring their allegiance to horticultural biotechnology), but in the sense of socially constructed by the myriad actors and forces in the context of planning and executing the workshop as an event.

discourse to overcome barriers to bringing biotechnology to horticultural crops. This discourse reflected an internally coherent world-view that included assumptions about technical promise, consumer/citizen behavior, and global agricultural networks.

The workshop participants took their task seriously, and at least one of the hoped-for technologies has been pursued for commercialization (herbicide-tolerant turf-grass, see Pollack 2004). Aside from this strategic achievement, however, the impact of the workshop extends powerfully into the arena of scientific dissent in agricultural biotechnology. Assuming that the discourse at this workshop was somewhat typical of meetings involving a cross-section of university faculty, corporate managers and researchers, and regulatory personnel, the intellectual landscape provides little space or opportunity for the emergence of scientific dissent within mainstream public and quasi-public institutional settings. Furthermore, the dissemination of the proceedings of the workshop through newsletters (AIC) and more formal journals (*California Agriculture*) extend the performed narratives to broader audiences, affecting the perceptions of agbiotech through international political and professional networks.

Performances such as the Hort-Biotech Workshop create the specific terrain through which scientific dissent must travel. As the following chapter demonstrates, even meetings held in the name of an environmentally-related concern (unintentional gene flow) can reproduce the promotional narrative in a more subtle but powerful manner.

Chapter 4 ‘Neutral’ Science: Gene Flow Conference in Mexico City

In September 2003, the Pew Initiative on Food and Biotechnology (PIFB) and the Fundación Mexicano-Estados Unidos para la Ciencia (FUMEC: U.S.-Mexico Foundation for Science) sponsored a conference in Mexico City, entitled, “Gene Flow: What Does It Mean for Biodiversity and Centers of Origin” (hereafter, Gene Flow Conference). The purpose of the conference was “to explore the current state of knowledge about the potential ecological and socioeconomic effects of gene flow from genetically modified maize on the native varieties of Mexico” (PIFB 2003, 2). Over 200 persons participated, including “scientists, government officials, representatives of non-governmental organizations (NGOs), industry representatives, and reporters” (p. 3).⁵⁶ Framed and attended as such, this conference had the potential to represent a much more neutral scientific approach than the explicitly promotional mission of the Hort-Biotech Workshop.

This describes the Gene Flow Conference as a dramaturgical event, in parallel with the previous description of the Hort-Biotech Workshop. While the Gene Flow Conference differed in many important ways (stated purpose, diversity of audience, scope of concern), it still resulted in an enactment of promotional science. Drawing on my notes from the conference, informal conversations with attendees, the official conference proceedings (PIFB 2003), the PIFB website, and other formal interviews with informants,

⁵⁶ I acknowledge the Institute for International Studies’ Environmental Politics Summer Fellowship program and a small grant from the Society and Environment division of ESPM, which enabled me to attend the Gene Flow Conference.

I analyze the dramaturgical elements of the Gene Flow Conference: narrative, stage management, character development, and audience construction.

4.1 Narrative

Unlike the Hort-Biotech Workshop which had a strong and cohesive narrative thrust, the Gene Flow Conference embodied more of an exploration of interrelated topics. The dominant themes included gene flow, biodiversity, sustainable agriculture, GM crops, maize, and Mexico. Both speakers and audience members represented a greater diversity of backgrounds and perspectives than those present at the Hort-Biotech Workshop. Nevertheless, the Gene Flow Conference, as a performance, made two significant narrative claims. The first, that the issue of gene flow in centers of origin should be defined within the domain of science, was predictable by institutional commitments. PIFB and FUMEC focus on science, they emphasize biodiversity (a category of scientific measurement, albeit with social, cultural, and political implications), and scientists dominated the slate of presenters. The second, however, that transgenes and GM crops were receiving too much attention, was not implied by the official purpose of the event as quoted above. *How does a conference explicitly aimed at exploring the impacts of a technology deploy a narrative that backgrounds the very technology in question?*

The Gene Flow Conference defined gene flow from GM maize to Mexican maize landraces⁵⁷ as a scientific issue. Gregory Jaffe, an audience member and Director of the

⁵⁷ Landraces are “[p]lant varieties developed in centers of origin which are the progenitors of modern varieties grown around the world” (PIFB 2003). In Mexico, the center of origin for maize, small-scale and indigenous farmers continue to grow landraces of maize, also called ‘criollo’ varieties. These varieties

Center for Science in the Public Interest's Biotechnology Project, asked a revealing question during a panel discussion. He began by acknowledging that transgenes would certainly flow into Mexican maize (if they had not already done so), and that the proper question was how to make sure the transgenes would not remain. He directed his question to Robert Horsch⁵⁸, a panelist from Monsanto, asking if the company knew the "fitness value"⁵⁹ of their transgenes, and if not, if they would conduct that research. He further wondered whether Monsanto might engineer only transgenes with negative fitness. Horsch responded that negative fitness transgenes "would be against the point," but that the Terminator technology⁶⁰ would theoretically perform as Jaffe suggested. Horsch explained that Monsanto had agreed not to pursue Terminator technology, but that hybrids served a similar function.⁶¹ This exchange placed the management of gene flow squarely within science, as a technical/ecological project rather than a socio-political issue. The only notable exception involved the recognition by several speakers that gene flow from GM maize to conventional maize or landraces could result in economic harm to Mexican growers as they would be excluded from GM-free national markets.

exhibit much more phenotypic and genetic diversity than industrial varieties, and they often have important cultural and spiritual properties. Conventional modern breeders rely upon landraces as 'source material' for developing new varieties for large-scale production.

⁵⁸ Robert Horsch is Vice President of Product and Technology Cooperation, Monsanto.

⁵⁹ The higher the fitness of a trait, the greater evolutionary advantage conferred upon the individual organism. Thus, a low-fitness transgene might disappear from a population over time because it confers an evolutionary disadvantage.

⁶⁰ Terminator technology, also known as Genetic-Use-Restriction-Technology (GURT), engineers sterility in the offspring of GM crops. The use of the technology would prevent seed saving (an advantage to seed companies, but decried as a diabolical threat to small-scale agriculture) and would also theoretically prevent gene flow from GM crops to closely related species (e.g. weedy relatives, other varieties).

⁶¹ Hybrids lose their vigor in successive generations. Horsch's analogy holds for the question of discouraging the practice of informally breeding an improved crop, but does not actually address Jaffe's request that the gene itself have negative fitness value. A hybrid variety could still allow a particular gene to 'escape' into landraces or weedy relatives and increase its population frequency. Several participants commented that gene flow from hybrids to landraces of maize had certainly occurred many times.

Speakers who addressed the regulatory framework emphasized the scientific research to date on the extent of gene flow more than the political and social choices about policy that could affect gene flow (Carpentier 2003; Gálvez 2003).⁶² The first day of the conference, which focused on scientific issues, ended with a panel dedicated to the detection and monitoring of transgene flow. While these invocations of science might situate research as a tool, the broader narrative never addressed *who* might use this tool to decide the kinds and degrees of gene flow allowable and/or desirable.⁶³ By sidestepping the *question of prevention*, the Gene Flow Conference began and ended the narrative of gene flow in the domain of science. This implied either that no value judgment was needed (science makes the ‘right’ policy obvious) or that the same experts conducting the research should have authority over the policy. In response to a question about Greenpeace activists attempting to block a shipment of GM maize from entering Mexico, Horsch replied that those people can “express their opinion,” but that managing gene flow required “objectivity.” In the context of the conference, objectivity meant science, and the thrust of the conference seemed in line with Horsch’s perspective. In sum, the Gene Flow Conference narrative claimed, “The evaluation, discussion, regulation, and control of gene flow in Mexico are the responsibility of scientists.”⁶⁴

On a superficial level, the second narrative aspect appeared to undermine the purpose of the conference by shifting attention *away* from GM crops, transgenes, and

⁶² Amanda Gálvez is Professor of Chemistry, Universidad Nacional Autónoma de México (UNAM) and Coordinator, Comisión Intersecretarial de Bioseguridad y Organismos Genéticamente Modificados (CIBIOGEM). Chantal Line Carpentier is head of Economy, Environment and Trade Program, Commission for Environmental Cooperation of North America (CEC).

⁶³ An entry in my journal during the conference stated, “Who gets to decide whether gene flow is OK?”

⁶⁴ This perspective on the Gene Flow Conference resonates with historical analyses of the Asilomar conference on recombinant DNA, during which scientists defined the safety issue as a technical problem that only they could understand and manage (Wright 1994).

gene flow. An overwhelming number of speakers, including those who framed the entire event, contributed to this narrative, which I parse into three rhetorical moves, progressively eliminating the narrative focus upon transgenes, gene flow, and biodiversity.

The first rhetorical move de-emphasized the unique contribution of transgenes and GM crops with respect to gene flow as a threat to biodiversity. In her slide listing “Concerns,” Dulce María Arias⁶⁵ (2003) states simply that “Transgenic maize might be considered only a marginal threat.” Allison Snow⁶⁶ noted that conventional crops include desirable genetic traits that can pass to landraces or weedy relatives and that “whether the challenges to biodiversity presented by transgenic crops differ substantially from those presented by conventional crops” remains to be evaluated (PIFB 2003, 11). Peter Raven⁶⁷ went further, suggesting that GM crops should theoretically pose less of a risk than conventional crops. During the closing remarks of the conference, he presented a slide stating:

- Every new strain of maize brought to Mexico poses problems for native maize diversity.
- Strains produced by traditional breeding methods introduce hundreds of different genes, mostly of unknown impact.
- Strains produced by GM techniques bring only a few new genes with precisely understood effects. ...
- Effects of GM maize on land races and teosinte less than those of conventional strains.
- No reason proposed so far that suggests GM maize could pose a threat to other native biodiversity (Raven 2003a).

If we accept Raven’s technical claims and logic, the conference was misdirected entirely by focusing on the relatively innocuous GM maize. His arguments carried special

⁶⁵ Dulce María Arias is Director, Center of Environmental Education, Sierra of Huautla, Autonomous University of Morelos (CEAMISH-UAEM).

⁶⁶ Allison Snow is Professor of Biology, Ohio State University.

⁶⁷ Peter Raven is Director, Missouri Botanical Garden.

significance because of his status and role in the conference. Klaus Ammann⁶⁸ (2003), another presenter with high status, also argued that the ecological impact of GM crops was minimal, simply instructing the audience, “Let’s not focus on the transgene alone.”

The second rhetorical move broadened the discussion of threats to biodiversity to factors other than gene flow. Ammann began by “reminding the audience that gene flow is a natural event, and that all agricultural techniques, not just the propagation of transgenic crops, reduce biodiversity” (PIFB 2003, 13). Major Goodman⁶⁹ (2003) argued that the

continued existence of maize diversity in Mexico (outside of underfunded or nonfunded germplasm banks) depends upon economic viability of small-scale Mexican maize farmers who cultivate, consume, and conserve the native maize varieties. Lack of economic viability of small farmers is far more certain to erode Mexico's maize diversity than is improved or transgenic maize.

Other speakers mentioned a host of other factors that could harm biodiversity or crop diversity: a “general shift in agricultural practices” (Hernández 2003); migration of farmers, changes in consumption patterns, urbanization (Ortega 2003); ranching, deforestation (Arias 2003); increasing age of small farmers, emigration of younger men to cities (Aragón 2003); habitat loss for wild varieties, replacement of traditional varieties by new higher-yielding varieties, deterioration of seed banks (Horsch 2003); the drop in the market price for maize, especially due to NAFTA (Carpentier 2003); agriculture (intrinsically) (Raven 2003b); abandonment of cultures, invasive plants and pests, insufficient attention to indigenous peoples (Raven 2003a); intensive livestock grazing,

⁶⁸ Klaus Ammann is Director, Botanical Garden, University of Bern.

⁶⁹ Major Goodman is Professor of Crop Science, Statistics, Genetics and Botany, North Carolina State University.

deforestation, and use of improved technology and seeds (Arias 2003).⁷⁰ Thus, as the narrative expanded to include the many factors influencing biodiversity, the salience of gene flow (and by extension, transgenic maize) faded.

The third rhetorical move questioned the focus on biodiversity itself, suggesting that other issues were at least or more important to consider. Manuel Mendez Nonell⁷¹, who made an early presentation on the “Conceptual Framework” of the conference, spoke about the importance of considering population growth and nutritional needs, advocating for a technology strategy to maintain food sovereignty in Mexico. Immediately following Mendez, Raven delivered the “Opening Remarks,” in which he reminded the audience that globally, one in eight persons was starving, one in two was malnourished, and “the situation is not getting any better.” In addition to hunger issues, Raven also lamented the tragedies of soil erosion, population growth, salinization, and pesticide poisonings and deaths. He presented the infamous $I=PxAxT$ equation (Environmental Impact = Population x Affluence (consumption) x Technology), a framing of the problem that arguably included biodiversity and biotechnology, but overshadowed them to an extreme degree (Raven 2003b). José Sarukhán⁷² opened the second day of the conference with the bold statement, “We are asking the wrong question.” He followed by speaking of the near inevitability of food shortage, the need to increase food production, and the problems of increased greenhouse gas emissions,

⁷⁰ Juan Manuel Hernández and Flavio Aragón are from the Instituto Nacional de Investigaciones Forestales, Agrícolas y Pecuarias de México (INIFAP). Rafael Ortega Paczka is from the Universidad Autónoma de Chapingo.

⁷¹ Manuel Mendez Nonell is Deputy Director, National Council for Science and Technology – Mexico (CONACYT).

⁷² José Sarukhán is from the Institute of Ecology-UNAM (Universidad Nacional Autónoma de México) and is the National Coordinator of CONABIO (Comisión Nacional para el Conocimiento y Uso de la Biodiversidad).

nitrogen use, and pesticide runoff. He proposed that the proper question was how to feed the Mexican population in 2050 with sustainable agriculture (Sarukhán 2003).

Reviewing the specific history of the monarch butterfly – *Bt* corn controversy, Horsch (2003) described how follow-up research proved that GM corn posed no significant threat to monarch butterflies but that “level-headed people stepped in” to protect the monarch butterflies’ winter habitat in Mexico. This analogy played upon the background controversy to the Gene Flow Conference (discovery of transgenic DNA in Mexican landraces of maize), suggesting that attention to scientifically-confirmed aspects of conservation, such as habitat protection, would do more than wasting resources to investigate and prevent gene flow between GM crops and other varieties.

Raven closed the conference in a manner entirely consistent with the rhetorical move to broaden the focus beyond gene flow, GM crops, and biodiversity.

A number of problems associated with population growth and migration to cities impact agricultural productivity. Peter Raven began his closing remarks by outlining some of these problems, including the reductions in agricultural lands, deforestation, soil erosion, grazing, and pollution. Raven presented a list of the requirements for global sustainability including national and international goals for population levels, sustainable levels of consumption, and development of sustainable technologies. He stated that this must occur in a context where certain rights are respected globally, including the right to health, freedom of choice, education, and adequate quantities of food, water, and energy (PIFB 2003, 31).

Raven gave a colorful presentation of slides illustrating the I=PAT equation, global and Mexican population growth projections, the diversity of environmental threats, and the need for a universal declaration of human rights.

On one hand, this broadening of considerations (parsed into three rhetorical moves) represented an incredibly thoughtful and appropriate response to what could have become an esoteric, technical discussion. Indeed, this global perspective situated the

conference as a whole beyond the domain of classically scientific discourse, extending into realms of ethics, visions for future development, and philosophy.

On the other hand, and from a more critical perspective, the broadening of focus as a narrative strategy caused paralysis of action and confusion about expert status and issues of representation. This was a drama of tension between the powers of scientific explanation and authority. The jump from GM crops' impact on biodiversity to the agricultural demands of a growing global population arguably would require an equally massive jump in expertise – presumed but not demonstrated by many of the speakers. Most significantly, downplaying the significance of the focal question (transgenes and biodiversity) severely damaged the opportunity for discussion to lead to action. Shifting the question from “What do we know about the flow of transgenes to maize landraces and what should we do about it?” to “How will we feed the Mexican population in the year 2050?” transformed the narrative into a general (albeit emotional) story with little to say about the management of GM crops or maize landraces. Rhetorically, the potential costs of GM technology were hidden (beneath global environmental concerns) and their benefits were showcased (as part of the solution to developing a sustainable agriculture to feed a growing human population). In this sense, the narrative of the event shifted the conference toward the domain of promotional science.

4.2 Stage Management

Although an absolute line does not separate narrative aspects from stage management in a scientific performance, this section explores qualities of the event that stemmed from more ‘backstage’ work or pre-event planning. Unlike the rhetorical moves

described above, these actions remained less visible to the conference participants, but had an equally powerful impact on the messages and experience of the Gene Flow Conference. Below, I discuss two aspects of stage management: science/policy boundary-work and the diversity of voices among conference speakers.

The Gene Flow Conference agenda reified the science/policy boundary. Most obvious, the organizers arranged the ‘scientific’ presentations on day one, and the ‘social/policy’ presentations on day two. Pew’s executive summary stated:

The *first day* of the conference focused on issues surrounding maize evolution, genetic engineering, gene flow, biodiversity, monitoring maize landraces and their wild relatives - the teosintes, and the close relationship of maize to cultural development in Mexico. Scientists discussed studies aimed at understanding gene flow in maize and the teosintes, the effects of transgenes in various genetic backgrounds including their effects on the environment, and the fitness of various traits. Finally, methods to detect gene flow from transgenic varieties to local landraces and possible monitoring systems were presented. The *second day* included talks on the implications of introducing transgenes to centers of origin, and on the organizations that address policy questions raised by this issue [emphasis mine] (PIFB 2003, 3).

Not only did the agenda claim to separate the two types of presentations, but the primacy of the science day performed significant rhetorical work and had enormous implications for how participants experienced the conference. Rhetorically, dealing with the science first and then moving on to policy discussions constructed science as a technical matter that could be insulated from political and social concerns. Raven’s (2003a) closing remarks epitomized this attitude. He presented a slide, “Facts about Genetic Modification,” which presented four claims (all of which have been challenged by other scientists) and ended with a final bullet point, “We need to assimilate or further text these points, facilitate public confidence, and move on.” This notion of ‘moving on’ once the science settles disentangled the practice of science from its social environment, its political context, its motivations, and its affiliations with centers of power in society.

Ironically, Raven's political and scientific expertise and authority were completely entangled in this plea for purity.

A striking consequence of this aspect of stage management resulted from the placement of Andrew Light⁷³ as the penultimate speaker in the final panel of the second day. The conference agenda described his topic as "Ethical/sociological concerns." Light introduced the notion of "deep disagreement" to characterize controversies that could not be resolved by "more science" (Light 2003). He compared concerns over GM crops to the U.S. abortion debate, explaining that advocates on both sides invoked science, rendering additional technical information impotent to reduce conflict. Instead, he argued for the need to appeal "to pluralism and tolerance or to a fundamental right that might override the issue of contention" (PIFB 2003, 28). His framework, which denied the separation of scientific questions from social or political questions and invited moral arguments as a crucial component to seeking solutions to deep disagreement, could have framed the conference and provided a platform for discussion by diverse participants. Instead, placed nearly at the end of the conference, Light's framework of deep disagreement came across to the audience as a kind of afterthought. Managed this way, the Gene Flow Conference as performance maintained credibility by including "ethical/sociological concerns," but relegated them to a peripheral role in terms of organizing the approach to the problem and the formation of potential solutions.

The second area of stage management also involves agenda-setting, in the sense of controlling the diversity of speakers. Pew has claimed to create a nonpartisan space that remains open to a diversity of viewpoints. Their mission statement declares:

⁷³ Andrew Light is Assistant Professor of Environmental Philosophy, New York University.

The Pew Initiative on Food and Biotechnology was established in 2001 to be an independent and objective source of credible information on agricultural biotechnology for the public, media and policymakers. Funded through a grant from The Pew Charitable Trusts to the University of Richmond, the Initiative advocates neither for, nor against, agricultural biotechnology. Instead, the Initiative is committed to providing information and encouraging debate and dialogue so that consumers and policymakers can make their own informed decisions...

The Initiative produces reports and sponsors workshops and conferences to showcase the diverse points of view that recognized experts have on the broad array of topics relevant to the debate about agricultural biotechnology (PIFB 2005b).

In his welcoming remarks, Michael Fernandez⁷⁴ introduced the conference as a chance to present “unbiased information on biotech.” Indeed, unlike the Hort-Biotech Workshop, the Gene Flow Conference did include scientific discourse that raised questions about the ecological, agricultural, economic, and social impacts of GM crops. In addition, representatives from NGOs critical of agbiotech (e.g., Greenpeace, ETC Group, Centro de Estudios para el Cambio en el Campo Mexicano [Center of Studies for Rural Change in México]) attended the conference and challenged some of the speakers.

In terms of stage management, however, the dissenting voices remained off the stage. Harsh criticism of agbiotech, generally, or transgenic ‘contamination’ of Mexican maize, specifically, surfaced occasionally in an audience member’s question, but never as part of a prepared presentation. Although at least four NGOs were represented in the audience, all formal speakers hailed from academia, government, or industry.⁷⁵ This designed formation of hierarchy, which incorporated an assumed social structure from the start, marginalized dissenting viewpoints. One woman who spoke from the audience delivered an emotional speech calling attention to the lack of certainty around the health impacts of biotech. She criticized the pattern of excluding NGO voices and cited several

⁷⁴ Michael Fernandez is Director of Science, Pew Initiative on Food and Biotechnology.

⁷⁵ A fellow participant (not affiliated with an NGO) confided to me that FUMEC would have rather restricted the participation of some NGOs, but felt they “*had* to keep it open,” fearing criticism.

scientific studies supporting her point of view. No one responded to her; the panelists moved on immediately to hearing the next question. More poignantly, the moderator of the final panel discussion interrupted and cut off a Greenpeace representative during her question about alternatives to agbiotech and the transgenic status of grain from the blockaded ship in Veracruz. In the most emotionally charged moment of the conference, Silvia Ribeiro, representing the ETC Group, challenged Ammann during the discussion after his presentation. She attacked his claims that GM crops used less herbicide and caused no environmental damage and argued that technology could not solve global hunger, especially when in the hands of “five companies, one of which we’ll hear tomorrow [Monsanto].” Ammann responded to her in a calm voice,

“You just chose and filtered my statement in a way that I cannot accept...I did not expect such a polemic here at Pew.” I understand your anger, but there are “powers that overrule your vision of the world.” I hope you find a way to debate more fairly. “I would like to speak to you, but not on this level.”⁷⁶

He dismissed and deflected her challenge from his position of authority (invited speaker, white European male, natural scientist) and effectively marginalized her arguments (in accord with her rank as reluctantly-accepted audience member, Latina, activist). Another conference participant commented to me informally that Ammann made her look “foolish.” While Ribeiro’s style of attack certainly contributed to her dismissal, the conference did not provide an opportunity for her viewpoint to emerge on a level playing field. At the end of the conference, a journalist who attended confided in me that he considered the Gene Flow Conference a “biotech road show” and PIFB’s claim of nonpartisanship absurd.

⁷⁶ Because of a brief failure in my recording equipment, I am unable to provide the full quotation, word for word. The text in quotes are direct quotations from my notes; the remainder are paraphrases from segments of my notes during which I did not scribe each word.

A final example of the marginalization of dissenting voices during the conference involved the treatment of Ignacio Chapela and his research. The combination of the stated purpose of the conference, “to explore the current state of knowledge about the potential ecological and socioeconomic effects of gene flow from genetically modified maize on the native varieties of Mexico” (PIFB 2003, 2), and the frequency with which speakers referenced Chapela or his research (seven separate times) suggested that Quist and Chapela’s (2001) publication provided significant motivation for the conference. Yet, neither Quist nor Chapela attended the Gene Flow Conference, a fact noted during a question from an audience member who criticized the “strong bias” in the way speakers were chosen – the “lack of NGO representation” and persons who played a “crucial role” in the initial Oaxacan research. Anticipating the conference, I was surprised to learn that neither Quist nor Chapela would be attending. Chapela named two factors for declining the invitation by PIFB: 1) the list of other speakers suggested to him that the conference was organized to further the political purpose of agbiotech, and he did not wish to lend his reputation to improve the credibility of the event, and 2) although his research was primary in defining and motivating the conference, he received a belated invitation and learned that the head organizers of the event had not wanted to include him as a participant (Chapela 9/11/2003) .

In sum, aspects of stage management reified the science/policy boundary and marginalized dissenting voices. The agenda and key speakers separated science from policy in a manner that decontextualized science and marginalized ethical and sociological concerns, which furthered the agenda of promotional science. In addition, although rhetorically claiming a nonpartisan discursive space that reflected a diversity of

expertise, conference organizers relegated dissenting voices to the audience and even failed to include the person most responsible for raising the issue of gene flow in Mexican maize.

4.3 Characters

As in the Hort-Biotech Workshop, the Gene Flow Conference demonstrated the power of character development in performing promotional science. Below I discuss the details and significance of the character formation of maize, scientists, campesinos/indigenous farmers, and activists.

Maize – the protagonist

Presenters devoted considerable time and attention to the biological, cultural, economic, and historical importance of maize in Mexico. The booklet produced by PIFB as a summary of the conference included an incredible number of illustrations from pre-hispanic and colonial artists that portrayed the role of maize in Mesoamerican culture.

The booklet's text began:

Maize is a crop of significant nutritional, economic, environmental, historical and social importance in Mexico. Mexico's people, culture and landscape have been intrinsically linked to its development and cultivation. And because it is the center of origin for maize, maintaining and protecting the biodiversity of maize in Mexico is important for the rest of the world as well (PIFB 2005b, 2).

The first conference panel began with extensive reviews of the evolution of maize and its modern relationship with its wild ancestor, teosinte (Ezcurra 2003; Hernández 2003; Tiffin 2003).⁷⁷ Bruce Benz⁷⁸ (2003) and Arias (2003) next reported significant gene flow

⁷⁷ Exequiel Ezcurra is chairman, National Institute of Ecology (INE), Mexico; Peter Tiffin is Assistant Professor, Plant Biology, University of Minnesota.

between teosintes and maize, resulting from ecological factors and agricultural practices. One consequence of this characterization of maize was that gene flow in maize was ‘naturalized’ to a great extent. Maize varieties, even the landraces only grown by small farmers, existed in a dynamic genetic environment – communicating that maize was perhaps an impossible site for ‘preservation.’ In his conceptual framing, Ezcurra declared that the “genetic patrimony of a nation” was at issue (deploying the trope of purity at the level of population genetics). Defining significance at that scale (national rather than local or regional) solidified the discussion of maize as a *valuable collection* of varieties (genetic reservoir) rather than a collection of *valuable varieties* (prized and distinct phenotypes). The conference thus acknowledged the spiritual and cultural value of criollo varieties, but the thorough scientific characterization of maize overshadowed the representation of *particular* maize varieties as important to protect and minimized the significance of gene flow as a threat. One can imagine a very different framing if the indigenous farmers and campesinos who grew and valued particular criollo varieties (described by Soleri 2003) had been invited to speak at the conference – perhaps defining conservation within an indigenous rather than nationalistic discourse.

Scientists – objective voices for those who cannot (or should not) speak

In his opening remarks on the second day, Sarukhán explicitly characterized the role of scientists in relation to the public’s need to make policy decisions:

The second problem Sarukhán described is the tendency for society to see things in terms of absolutes - either all good or all bad. Such interpretations differ fundamentally from the way science works. *Science is neither dogmatic nor absolutist, but rather an objective study of how nature works.* Consequently, it always includes an element of uncertainty.

⁷⁸ Bruce Benz is Assistant Professor of Biology, Texas Wesleyan University.

Scientists describe results and offer opinions in terms of probabilities of occurrence of certain phenomena. Obviously, Sarukhán pointed out, this presents challenges for a society seeking clear black and white answers. He stated that uncertainty is not a reason to avoid making decisions, but emphasized that decisions need to be made in light of all available evidence. He continued by saying that societies must deal with the consequences of their choices and *this includes measuring the consequences of any decision with monitoring*. Finally, he reminded the audience that *communicating information to the public in an informed and thorough fashion* is critical since the decisions made impact people [emphasis mine] (PIFB 2003, 19).

Sarukhán thus made three related claims: 1) that scientists were capable of objective study of nature 2) that scientists should provide this information to the public so that it could influence policy formation, and 3) that scientists were crucial in evaluating the success of policy choices. These claims, consistent with other speakers' attitudes about the role of science, positioned scientists in a hybrid role – disinterested researchers who somehow remained protected from the messiness of politics and authority figures interwoven into the policy process.

As discussed earlier, scientists dominated the list of speakers at the Gene Flow Conference. They not only framed the narrative and defined concepts such as gene flow, sustainability, and biodiversity, but also spoke on behalf of groups not represented among the list of presenters. Daniela Soleri⁷⁹ (2003) and Aragón (2003) gave thoughtful and insightful presentations about Mexican small farmers' knowledge of transgenes, interest in experimenting with GM crops, economic challenges, and practices of seed saving and selection. Soleri, in particular, brought farmers' voices to her presentation by quoting some of their responses to questions about genetic engineering:

“...that is strange, bad, not natural...”
“...Ayiii!!!.... but,can you get me some seed?”
“...OK, but I would watch other people eat it first....”
“...sure, I eat parts of animals and parts of trees, so why not eat sorghum with parts of animals or trees in it?” (Mali)

⁷⁹ Daniela Soleri is a research scientist in the Environmental Studies Program, UC Santa Barbara.

“...scientists wouldn’t be working on it if it could cause any harm, would they?”
“...I would assume that it is good/safe to eat and grow based on my confidence in who gives it to me...”

These quotations, presumably selected for their content and ability to represent many hours of conversations between researchers and informants, partially transported the small farmers to the conference, but only as “immutable and combinable mobiles” (Latour 1987, 227). Soleri collected the quotations, organized them for effect, and transported them to the conference. Once at the conference, they became elements within scientific discourse, disconnected from the bodies and minds that produced them. To some degree I applaud Soleri for using her scientific tools, resources, and position to ‘bring the farmer perspective’ to the Gene Flow Conference, but bringing their perspective differed enormously from bringing a million small farmers. In part, Soleri was complicit in preserving the legitimacy of the conference by *representing diversity* without actually performing *diverse representation*. When Allison Snow said, “Once we know if [transgenic] gene flow is occurring...we need to focus on what differences it makes”(PIFB 2003, 11), no farmer was present to potentially say, “But wait! Shouldn’t we know what difference it makes *before* we let gene flow occur?”⁸⁰ Soleri thus contributed to the characterization of scientists as fit to represent (adequately and accurately) the world, human and non-human. Combining this characterization with Sarukhán’s description yielded scientists as extremely powerful in their position *vis a vis* the ‘natural world’, the public, and the political process.

⁸⁰ In making this argument, I do not presume that all or a majority of small farmers would raise such an objection. The point is that their lack of physical presence prevented them from doing so. Likewise, no small farmer could contradict José Sarukhán’s emphasis on feeding the 2050 Mexican population, saying, “Yes, but what about feeding us now!”

Campesinos / Indigenous farmers – the off-stage heroes

Although no campesinos or indigenous farmers attended the Gene Flow Conference, they emerged as heroic, or at least potentially heroic, characters. Multiple speakers described the key role that small farmers played in creating and maintaining the diversity of maize varieties (Aragón 2003; Benz 2003; Fernández 2003; Hernández 2003; Light 2003; Ortega 2003).⁸¹ Goodman (2003) made the argument most strongly:

Goodman suggested that the primary steward of maize diversity in Mexico is currently the small farmer - a group whose viability is also in question. Consequently, the economic situation of the small farmer presents the largest threat to the Mexican landraces. Goodman stated that the impact of transgenic crops, seed companies, or research institutions on maize biodiversity pales in comparison to the impact that small farmers and their threatened welfare have on maintaining this resource (PIFB 2003, 20).

Given the widespread consensus at the Gene Flow Conference on the important role of small farmers in maintaining maize diversity and biodiversity, the failure to include small farmers or campesino/indigenous organizations as speakers stood out. In terms of character formation, this paradox suggested that while small farmers might have acted as stewards, their agency was limited (and thus not needed as a conference participant). These off-stage heroes were cast as important players in preserving diverse landraces of maize, but their successful stewardship depended primarily on their immediate context (e.g. economic incentives, policy requirements, educational outreach). As Ortega said, “Farmers need motivation to continue using native materials and their traditional seed selection processes” (PIFB 2003, 9) The responsibility for creating that motivation remained in the hands of scientists, policymakers, and other experts – those with authority and agency.

⁸¹ José Carlos Fernández is General Director of Environmental Economy, INE-SEMARNAT.

Activists – the interlopers

Unlike the Hort-Biotech Workshop, the Gene Flow Conference audience included activists. Their behavior and their treatment combined to construct activists as an interloping character in the performance. The PIFB conference proceedings (2003) described several questions asked by representatives of activist organizations (Greenpeace, ETC group, and the Center of Studies of Rural Change) and identified the speakers as such. The PIFB text, however, completely glossed over the intense conflict in the room whenever an activist spoke during a panel discussion. A scientist with some ideological sympathies with the activists confided to me that he felt “frustrated” with Ribeiro’s behavior, commenting that she had spoken too fast, too loudly, and too carelessly in mentioning a barrage of studies to make her point. Likewise, another activist, who wore a bright green t-shirt with political slogans, asked her lengthy question with fierce emotion. In both cases the activists appeared to want to make a longer statement but seemed constrained by time during the question/answer panel discussion. At other times, activists stayed more within the discursive norms of the conference, but their questions always struck a dissonant chord – bringing up a great deal of external information that formal speakers had not mentioned and calling for action or reflection on topics slightly outside the conference panels as defined by speakers and moderators. These behaviors served to alienate the activist character from the conference, characterizing them as unwelcomed guests who must be tolerated.⁸²

⁸² My own experience as an activist suggests that ‘inappropriate’ and ‘rude’ behaviors often emerge by participants when they feel disempowered in a given context. A tension always exists between ‘playing by the rules’ to maintain legitimacy, and ‘breaking the rules’ to call attention to the institutional barriers to full

The treatment of activists by high status participants furthered this characterization. Ammann (a panelist) dismissed and patronized Ribeiro (ETC Group) after her question, and the moderator cut off a Greenpeace representative in the middle of her question, citing the lack of time for lengthy comments from audience members during panel discussions. Significantly, no other audience member or panelist was ever cut off. In fact, in the discussion portion of a conference panel that did not include Peter Raven as a formal speaker, he was permitted to ask a six minute question from the audience, without interruption or other comment by the moderator. This disparity of treatment, as part of the performance, positioned activists clearly as annoying, inappropriate, and unhelpful interlopers.

4.4 Audience

The audience of the Gene Flow Conference emerged as a result of a loosely coordinated set of actions. At the organizational level, PIFB and FUMEC had responsibility for inviting the speakers and publicizing the event. The lack of campesinos, indigenous people, and activists as formal participants with panel responsibilities had a tremendous impact on the formation of the audience – their lack of inclusion both discouraged broader participation by those groups and established a steep hierarchy between scientists/government personnel and the activists/NGO representatives who did attend. At the discursive level, speakers included a significant degree of technical discussion in their presentations and slides (economics, evolutionary biology, genetics,

participation. Given the exclusion of activist organizations from all panels, it is not surprising that activist audience members fell into such a pattern.

regulatory policy), inscribing the audience with a certain degree of expertise and technical capacity. This carried significance by emphasizing to attendees what kind of person had a proper place in discussions about gene flow and biodiversity. Finally, at the personal level, speakers and moderators constructed the audience as elite, representing *but not representative of* the public. As quoted earlier, Raven's closing remarks (2003a) included a slide of "Facts about Genetic Modification" with the final instruction: "We need to assimilate or further text these points, facilitate public confidence, and move on." The boundary rhetorically imposed between "we" (the audience of the Gene Flow Conference) and the "public" (whose confidence must be facilitated) constructed the audience as powerful and the wider public as dependent.

Another fascinating aspect of audience construction involved the management of the discursive space as free of conflict. The PIFB web page entitled "The Debate About Agricultural Biotechnology" declares:

Because the proponents and opponents of agricultural biotechnology have such contradicting perspectives, it is not surprising public debate on this issue has become contentious, polarized, and confusing. For the debate to evolve *beyond conflict* [emphasis mine], the many parties concerned with this technology need a forum where they can foster consensus rather than further conflict. The Pew Initiative on Food and Biotechnology was created to provide such a forum (PIFB 2005a).

What could it mean to move beyond conflict? If the parties were in conflict, where could the conflict go? The PIFB mission statement ends with the following paragraph:

Through these efforts, the Pew Initiative on Food and Biotechnology aims to help move the discussion about this technology [biotechnology] beyond conflict and toward a sustained process of constructive engagement about the regulation and use of this important tool (PIFB 2005b).

These quotations have a number of implications: 1) conflict leads to confusion, while consensus leads to clarity; 2) constructive engagement requires moving beyond, transcending, compartmentalizing, or backgrounding conflict; 3) the scope of successful

outcomes is limited to the regulation and use of biotechnology (because it is an “important tool”) not the rejection of biotechnology in favor of other tools. Hence, any audience constructed by PIFB faces these implications as cognitive expectations and codes of conduct.

An early move to minimize conflict demonstrated the commitment to the principles above. Two of the first three speakers involved in the introduction of the conference made explicit pleas for orderly and polite discussion. Fernandez repeated this request at the very beginning of the second day. With such emphasis from key figures involved in conference organization, these requests became ground rules to which participants could later appeal. Ammann’s rebuke of Ribeiro included him saying, “I did not expect such a polemic here at Pew.” Ribeiro’s challenge, obviously charged with conflict, was essentially ruled out of bounds. Likewise, cutting off the Greenpeace speaker during the panel discussion (in a procedural, rather than oppositional manner) was appropriate in a context that defined constructive engagement as devoid of the signs of conflict.

Prohibiting audience members from exposing and acting out conflict during a performance already replete with power imbalances (e.g. lack of representation of NGOs or campesinos on panels) had severe consequences. Audience members who disagreed with the apparent consensus had to choose between sitting quietly to maintain the appearance and comfort of consensus, or acting out in a manner that provoked official censure and general discomfort among other audience members who had heard the rules loudly and clearly. An informal discussion I had with three other participants gave evidence of the power of this dynamic. At the end of the first day I noted my surprise at

the level of apparent consensus during the conference. Only obviously marginalized voices had contradicted the agenda put forth by high status speakers such as Raven and Ammann. As trust built within our group of four over dinner (and a little wine was consumed) all of us revealed our discomfort with the framing of the conference and our healthy distrust of industry and government representatives who preached the safety and benefits of agbiotech. We were a relatively moderate group (all researchers affiliated with public institutions) – none of us rejecting the technology in principle, but all of us entertaining structural, institutional, and scientific questions that were absent from any of the panel discussions. One participant expressed disdain at how Ammann and Raven had stepped well beyond their expertise (biology) in making social and political claims. Although the scale of the conversation certainly contributed to a degree of honesty and openness not attainable during a conference, I would suggest that the ‘ground rules’ of politeness, order, and all that these imply drove these participants to keep their doubts and concerns to themselves. Hence, moving ‘beyond conflict’ restricted the field of perspectives in position to enter ‘constructive engagement.’

4.5 Summary

Despite the rhetorical commitment to explore ecological and social impacts of gene flow from transgenic maize (a ‘neutral’ scientific framing), the Gene Flow Conference embodied a performance of promotional science. Similar to the Hort-Biotech Workshop, scientists retained the highest degrees of authority and agency, rhetorically protected from the ‘polluting’ forces of politics and non-experts. Likewise, the pervasive effects of stage management limited opportunities for participation by more marginal

actors (e.g. farmers, activists). A key difference, however, was the prominent impact of the ‘rules’ of engagement that disciplined whatever voices of dissent had found their way into the rooms. These rules were hierarchical, elitist, exclusive, and perhaps even colonial in their character.

The Gene Flow Conference also differed from the Hort-Biotech Workshop in its dramaturgical treatment of agbiotech. The Hort-Biotech Workshop celebrated all things GM, creating a ‘heroic quest’ narrative of barriers and ways to overcome those barriers. Part pep-rally, part strategy session, the workshop looked forward to the implementation (by participants and their colleagues) of a particular technoscientific future replete with transgenics on your plate and in your backyard. In contrast, the Gene Flow Conference performed political jujutsu by redirecting environmental and social concerns about transgenic crops to the ‘real’ threats (e.g., environmental degradation broadly conceived, small farmer dislocation), which posed far greater dangers than agbiotech. Perhaps in part because the conference occurred eighteen months further into the failure of agbiotech to win demonstrated public confidence, the promotional strategy had shifted to one of emphasizing the continuities of transgenic crops with conventional agriculture and the need to see them as one (necessary) component in the global effort to end hunger and save our environment.

These two performances help define the landscape upon which scientific dissent appears (Chapter 5) and struggles for legitimacy (Parts Two and Three). Emerging from a public institution (a land-grant university) and a quasi-public institution (a foundation dedicated to collecting and disseminating unbiased information to educate the public and guide policy), these events could well describe what we might imagine as a moderate

position between agbiotech corporations and activist NGOs. While their ‘positions’ might have been moderate with respect to the extremes, their discourses promoted agbiotech. With varying degrees of legitimacy across a very broad network of actors and institutions, these performances provide insight into the mainstream flow of science around agbiotech. The assumptions embedded in this mainstream flow permit a mapping of the shadow of promotional science: contrarian science. The following chapter takes up the task of analyzing these embedded assumptions.

Chapter 5 Embedded Assumptions in Promotional Science

Having analyzed two case studies of promotional science, I turn to the task of extracting the scientific and political assumptions that support the narratives, character development, stage management, and audience construction. Specifying these assumptions serves a number of analytical purposes:

1. Understanding the assumptions within and behind promotional science creates opportunities for making sense of corresponding actions and discourses as logical and internally consistent.
2. Listing the assumptions allows comparison with assumptions within and behind contrarian science, as a way to explore the struggle to define some claims as truths and others as opinions.⁸³
3. Exploring the field of assumptions demonstrates the range of beliefs and perspectives that come together to support promotional science.

Although the following list is not exhaustive, and my categorization is only one of many possibilities, I organize the assumptions into five groups: global agriculture, safety, benefits, progress, and control. I illustrate each assumption with evidence from one or

⁸³ At their core, controversies are boundary struggles between fact and opinion, assumption and truth, perspective and reality. As an analyst, I recognize that organizing my discussion around ‘assumptions’ rather than ‘claims’ may cause discomfort in readers whose perspective aligns with promotional science (or contrarian science in the next chapter). In fact, I purposefully avoid a discussion of claims because such a treatment would require presenting the evidence to support the claims – an important project, perhaps, for resolving controversy but less so for *understanding* controversy. Focusing on assumptions allows me to present the implicit, and sometimes explicit, beliefs about the world that provide a foundation for claims, arguments, and action.

both case studies and comment on how they relate to one another and their broader implications for organizing thought and action in powerful ways.

5.1 Global Agriculture

The first set of assumptions involves predictions about the future of agriculture. Together, they provide an ethical foundation for the pursuit of agbiotech as socially desirable at the global scale.

Industrial agriculture represents the future of global food production

Promotional science partially aligns itself with the Green Revolution in assuming a future of large-scale, industrial agriculture. At the Gene Flow Conference, “Horsch reminded the audience that breeding strategies that have caused vast improvements in seed productivity over the past fifty years have relied upon large and segregated populations - an approach which doesn’t transfer to small single farmer systems” (PIFB 2003, 23). In other words, abandoning the economies of scale in agricultural production would have tremendous (presumably insurmountable) costs.

At the same time, promotional science recognizes some negative aspects of industrial agriculture, and presents agbiotech as an ideal tool to eliminate the drawbacks while maintaining the paradigm of Green Revolution farming. In his article in *California Agriculture*, Alston (2004) wrote: “The relevant comparison then is between the environmental risks associated with these biotech crops and those associated with the annual burden on the environment of 163 million pounds of chemical pesticides that could be avoided by growing biotech crops” (p. 86). Likewise, at the Gene Flow Conference, Ammann stated that “he would ‘prefer to go to the *Bt* maize fields’ to live

rather than in fields treated with conventional pesticides if he were a beneficial or non-target insect” (PIFB 2003, 14). While Ammann also mentioned organic farming as a method of avoiding pesticides, his enthusiasm was not for exploring how to expand organic production, but for developing a new category of agricultural production that integrated biotechnology into organic farming – in his words: “Organo - Transgenic Crops and Organic Precision Biotechnology” (Ammann 2003).

The implication of this assumption is reluctance, if not refusal, to explore more radical reform of industrial agriculture. One participant from the Hort-Biotech Workshop spoke to me about his excitement for GM crops that would focus on output traits. He mentioned transgenic feed with reduced phosphorous to reduce the pollution emanating from industrial hog and chicken farms. This epitomized the inability to escape from a factory model of agriculture – rather than question the system of factory farming, reduced phosphorous feed takes the system for granted and looks to solve an effluent problem by looking narrowly at inputs. In a similar vein, Dennis Gonsalves (2002) described the drop in price for papaya that followed the return of Hawaiian production made possible by transgenic varieties. He referred to “overproduction” as an inevitable “part of the cycle.” What was striking was that he had minutes earlier reminded the audience that their purpose as scientists was “to help people,” yet he seemed resigned to situate his technology in a system of production that would not help growers over the medium to long term. Staying within the paradigm of industrial agriculture thus severely limits promotional science’s ability to overcome negative consequences of the current system.

Although the development and deployment of agricultural biotechnologies has thus far occurred primarily in industrialized countries, the technologies will transfer successfully to less developed countries (LDCs).

Promotional science recognizes that the majority of current GM crop varieties, developed for Northern agricultural systems, could not thrive in tropical regions. At the Gene Flow Conference, Goodman (2003) reported that most transgenic maize would not suit the Mexican climate because it was developed for the Midwestern U.S., but he also assumed that this was a temporary barrier. The moderator of Goodman's panel, Sarukhán, listed some of the potentially engineered traits that would benefit Mexican agriculture (higher yields and nutritional value, ability to grow in marginal soils) and declared that Mexican resources would have to support the research to make such developments a reality. Others showed even greater optimism:

Horsch underlined that current biotech products, designed for U.S. farmers, are showing a surprising, relevant benefit in countries for which the product was not originally designed. He described the cases of *Bt* corn in the Philippines and *Bt* cotton in South Africa and China. On this same subject Juan Manuel de la Fuente of Monsanto stated that before the moratorium on transgenic maize in Mexico, several field trails of GM corn showed biologic efficiency, adding that some new products designed to fight pests that commonly damage crops in Latin America are just now being evaluated in tropical regions (PIFB 2003, 24).

None of these speakers addressed the question of whether factors other than biology and financial commitment to research might play a role in the success of agbiotech in LDCs. The challenge by Rivera and Ribeiro, that "most peasants practice subsistence agriculture and do not have access to new technologies such as transgenic seeds" (PIFB 2003, 15) received no response or comment. Promotional science appeared to minimize such concerns – why worry whether small-scale, subsistence agriculturists could benefit, when the future of agriculture would not depend upon their success or survival as farmers.

Alternatively, at the Hort-Biotech Workshop, Prakash (2002) argued that agbiotech represented an ideal fit for improving agriculture in LDCs for a host of reasons, including its ability to work in agricultural environments characterized by small holdings, subsistence farming, limited water and land, and impoverished infrastructure. He described agbiotech as “integrated technology delivery,” scale-neutral, environmentally-friendly, portable across crops, versatile, capable of rapid response to ecological or pest problems, and able to work with rather than displace traditional methods. But whether or not a promotional scientist viewed agbiotech as a panacea, the consistent assumption was that GM crops would become important in LDCs. As Raven (2003a) put it in his closing remarks at the Gene Flow Conference, answering the challenge to increase production and move toward sustainable agriculture at a global scale would require “various strategies, doubtless including GM technology.” This lack of ‘doubt’ is the essence of the assumption that GM crops will transfer to LDCs.

Increasing agricultural productivity with the tools of agbiotech represents the most promising strategy for reducing world hunger.

As discussed earlier, both events included significant references to biotechnology as a tool to reduce world hunger. Unlike assumptions about the continuation of industrialized agriculture which remained in the background, promotional science brought to the foreground assumptions about feeding the world. At the Hort-Biotech Workshop, Ted Batkin (2002) included a slide that simply stated “Biotechnology can be used to enhance the availability of food on a worldwide basis.” The issue of *California Agriculture* declared, “A major promise of horticultural biotechnology is reducing the

cost of delivering higher quality fruits and vegetables to malnourished and hungry people” (Sumner 2004, 77). And as described in detail above, Raven and Sarukhán framed the Gene Flow Conference largely in terms of the challenge of feeding a growing population, and both they and other speakers lauded agbiotech as a crucial strategic component in responding to that challenge.

Promotional scientists invoked this assumption in a manner that rhetorically trumped other concerns. For example:

In response to Light’s talk, Ammann from the University of Bern suggested that while we must respect the unique character of various cultures, we must understand that “cultural identity can only be realized with full human rights and that includes the eradication of famine” (PIFB 2003, 30).

Here, Ammann foregrounded hunger in a manner that displaced concerns about cultural identity – implying that they were sequential (famine must be considered first) and disconnected (working to protect cultural identity would have no impact on reducing hunger). At the Hort-Biotech Workshop, Redenbaugh (2002) included a slide that communicated a similar message: “‘The stuffed people never understand the hungry people’ - Old Chinese saying.” This was a thinly-veiled attack on the entire range of ecological, health, social, and ethical concerns that might block or slow the deployment of GM crops in LDCs. Although presented in a pithy format, even this example demonstrated the power of the ‘biotech feeds the world’ assumption to undermine contrarian discourse before it even began.

5.2 Safety

A number of related assumptions fortify the general view of GM crops as a safe technology. They range from assumptions about the history of technology development, genetics, ecology, medicine, and the relationship between science and technology.

Biotechnology is the most recent development in a continuum of technological innovations.

The first paragraph of the editorial overview in *California Agriculture* began by defining biotechnology in a broad historical sense:

The term “biotechnology” encompasses a wide array of techniques through which humans employ biological processes to provide useful products. In the broadest sense, it includes the use of yeast in brewing and baking, and the breeding of plants and animals. More recently, the term has come to mean the collection of techniques that allow the direct manipulation of specific pieces of genetic material within and between organisms (Bradford, Alston et al. 2004, 68).

Although the editors admitted that modern biotechnology rarely referred to the use of yeast, the rhetorical effect was to connect genetic modification to a lengthy history of technology, much of which we now consider completely innocuous. Likewise, Raven (Raven 2003b) began the Gene Flow Conference by reminding the audience that biotechnology and genetic modification were “universally used” for medicine, insulin, cheeses, and beer. He went on to demonstrate a second aspect of this assumption – that the precision of genetic modification made it *more safe* than conventional methods of plant breeding. Commenting on the controversy over GM crops, Raven asked wryly, Why has so much “imprecise genetic modification” been accepted thus far [in reference to conventional breeding]? In the same vein, the *California Agriculture* editorial

overview contained an image of a broccoli photo and a cauliflower photo spliced together with the caption:

Cauliflower and broccoli are derived from the same genetic ancestor, *Brassica oleracea*, but were developed over many years into individual and very different vegetables through selection and breeding. Biotechnology can make this process more precise and less time-consuming (Alston 2004, 70).

Again, the text emphasized both the historical continuity of plant breeding technologies and the precision of biotechnology.

These assumptions have enormous implications for evaluating the safety of GM crops. A regulatory framework that honors the continuity of biotechnology techniques would likely have much lower hurdles for approval than one that assumed a more discontinuous development. The assumption of precision furthers this tendency by suggesting that anyone who demands a much more thorough safety evaluation of GM crops should *rationaly* demand at least as rigorous evaluation of all the other crops ever produced by conventional breeding. In effect, it sets up contrarian science to appear either inconsistent (concerned about some modifications and not others) or ridiculous (concerned about the safety of food we have been eating for thousands of years).

A linear model of genetics (the central dogma) is sufficient to understand biotechnologies and predict their behavior in the environment.

While promotional science may recognize explanatory and empirical limitations of a linear model of genetics, the central dogma⁸⁴ offers the starting place and maintains

⁸⁴ Biology's 'central dogma' is a model of the relationship between genetics and phenotype. It views DNA as a master molecule holding all of the information for producing life (i.e. DNA as code). DNA's information is transferred to RNA in specific sequences (genes). RNA then codes for a string of amino acids that fold into a protein required by the organism. The key insights of the central dogma are that information is stored hierarchically and information flows one way (DNA-RNA-proteins). For a fascinating

its role as the central model for understanding biotechnology. A handout provided at the Hort-Biotech Workshop, produced by the Seed Biotechnology Center (UC Davis) and labeled as “UC peer reviewed,” begins a discussion of DNA cloning techniques by stating, “Plant traits are encoded in the DNA of their genes” (Suslow, Thomas and Bradford 2002, 9). The following pages contain a description of recombinant DNA techniques as analogous to videotape editing:

The instructions organisms need to grow and function are encoded on linear pieces of DNA, just as images are encoded magnetically in the linear pieces of videotape. When the videotape is played, the encoded information is converted electronically into the scenes displayed on the TV screen. Similarly, cellular machinery reads the DNA and converts the genetic information into proteins, which then carry out the functions necessary for life. Because DNA is chemically similar among all organisms, the instructions on these cloned pieces of DNA can be readily exchanged and “understood” between organisms as dissimilar as yeast and tomatoes. Just as technicians can cut, copy, and splice sections of videotape to create new scenes, scientists can now copy and exchange genes among organisms to introduce new characteristics (p. 10-12).

This ‘gene-cassette’ model of recombinant DNA techniques implies a high level of precision and predictability that together present genetic modification as a safe technology under the control of the editors/engineers/scientists.

At the Hort-Biotech Workshop, this assumption laid the groundwork for one of the policy objectives advocated for by many participants: “Replace regulation based on a single gene-insertion ‘event’ with a more general approval of species-trait combinations” (Bradford, Alston et al. 2004, 70). In the videotape analogy, a producer inserting a movie preview before the feature presentation on a collection of videotapes need not review every single movie to make sure the preview works properly. The technology is predictable enough, and effects are localized enough, to guarantee a high degree of safety

discussion of the development and implications of this model, see Lily Kay’s (2000) *Who Wrote the Book of Life?*.

and functionality (e.g., it is unlikely that the wrong preview would have been inserted or that the preview would occur in the middle of the movie, or that the preview would cause certain scenes of the movie not to play properly). Transporting this model to genetics yields a clear rationale for reducing or simplifying regulation – once you’ve seen one insertion (of a particular trait), you’ve seen them all.

Discourse at the Gene Flow Conference raised a number of anomalies that challenged the central dogma, but the featured speakers remained true to the assumption. Luis Herrera Estrella⁸⁵ opened the panel on genetically modified maize with a set of questions that did not fit within the central dogma:

[He asked] “whether the presence of transgenes can alter the structure and function of the maize genome.” Herrera reviewed the various elements that can modify the genome of any organism, including induced mutations, abnormalities arising from recombination events, insertion of genes from the organelle genome into the nuclear genome, and the movement and amplification of mobile DNA elements known as transposons. Transposons, which are particularly abundant in the maize genome, cause a number of changes in genomic DNA including the translocation, inversion, and deletion of genes. Promoting the movement of large segments of DNA can result in the activation or suppression of other genes. (PIFB 2003, 10).

In the same panel, Julien Berthaud⁸⁶ emphasized the importance of understanding the genetic background of the recipient plant, referring to interactions between the transgene and other genes. But he assured the audience “that transgene insertion is very precise. Researchers evaluate hundreds of events to ensure that the insertion of the transgene has the desired effect and then further develop the most successful insertion” (PIFB 2003, 10). In other words, even if the linear model of genetics was problematic, the surrounding technology (evaluation and selection of events) was robust enough to maintain the

⁸⁵ Luis Herrera Estrella is Director, Centro de Investigación y Estudios Avanzados del Instituto Politécnico Nacional de México (CINVESTAV-Irapuato).

⁸⁶ Julien Berthaud is from the Institute of Research for Development (IRD), France.

assumption of precision, so basic to the central dogma assumption. Raven, however, who opened and closed the conference, emphatically defended the safety of GM technology:

[In his opening remarks, he assured the audience that] GM crops have presented no food safety problems to date. He explained that this safety record is not surprising - Raven points out that no scientific evidence exists indicating that the insertion of genes from one species to another is inherently dangerous. He stated that all mammals “have about 100 percent of their genes in common ... and that about 33 percent or a third of our genes are shared with the maize [plant]” (PIFB 2003, 4).

Raven’s analysis and logical progression from gene-sharing to the safety of gene insertion across species belied assumptions about the gene as a functional unit, insertion as a precise and knowable event, and predictable phenotypic consequences of changes in the genome. Together these assumptions of linearity, rather than complexity (see Strohmman 1997), combine with the view of biotechnology as a continuum to assume a high degree of safety of GM crops.

GM crops cause less ecological damage than conventional crops.

The ecological value of GM crops rests on a number of interrelated assumptions. First, as discussed above, promotional science minimizes the boundary between transgenic technology and conventional breeding, implying that there are no novel safety issues to consider with GM technology. Second, herbicide-tolerant crops will both discourage the use of more harmful herbicides (by allowing the use of ‘ecologically friendly’ broad spectrum herbicides) (Holm 2002) and encourage the ecologically-friendly practice of no-till agriculture (Prakash 2002). Third, pest-resistant crops (e.g., *Bt* crops) reduce the use of harmful pesticides by producing their own ecologically-friendly

pesticide⁸⁷ (Gianessi 2002; Raven 2003b),⁸⁸ and they have less impact on non-target organisms (Ammann 2003; Raven 2003a).

Gene flow between transgenic crops and conventional crops or wild relatives is of some concern, but requires monitoring and evaluation rather than drastic measures to eliminate any potential for the flow of transgenes.

In line with the Gene Flow Conference theme, many speakers addressed the potential negative ecological and agricultural effects of gene flow from transgenic crops to conventional crops or wild relatives (Ezcurra 2003; Ortega 2003; Snow 2003; Soleri 2003). The tone was one of concern, however, not alarm. Speakers emphasized practices of monitoring (described earlier) and evaluating problems on a ‘case by case’ basis. A policy objective developed at the Hort-Biotech Workshop illustrated this approach: “Establish practical thresholds for adventitious (accidental) presence of approved biotech products to facilitate international trade” (Bradford, Alston et al. 2004, 70). Practical thresholds make sense if gene flow from biotech products is unwanted, but not disastrous. Gonsalves (2002), the scientist who led the program to develop the transgenic papaya for Hawaiian production, acknowledged that he knew seeds would be stolen and grown in Mexico and Brazil, but that “If I felt it was dangerous, I never would have released it

⁸⁷ *Bt* crops, for example, express a variant of an insecticidal toxin derived from *Bacillus thuringiensis*, a bacteria that manufactures a toxin so environmentally benign that it is allowed in organic production.

⁸⁸ Leonard Gianessi is from the National Center for Food and Agricultural Policy, Washington, D.C. “The Potential for Biotechnology to Improve Pest Management of Fruit and Vegetable Crops”

[transgenic papaya].” This quote clearly demonstrates the assumption that the potential harm from transgenic flow is minimal, manageable, and worth the risk.⁸⁹

GM crops represent no threat to animals or humans who ingest them as food.

The assumptions about biotechnology as a continuum and the adequacy of a linear genetic model contribute to the assumption that GM crops pose no special health concerns. In *California Agriculture*, Redenbaugh and McHughen (2004) reported, “No case has been documented to date of harm to humans or the environment from the biotech crops currently being marketed” (p. 108), and they cited conclusions from a 2002 Royal Society report:⁹⁰

- “There is at present no evidence that GM foods cause allergic reactions.”
- “There is no evidence to suggest that those GM foods that have been approved for use are harmful.”
- “Risks to human health associated with the use of specific viral DNA sequences in GM plants are negligible.”
- “It is unlikely that the ingestion of well-characterized transgenes in normal food and their possible transfer to mammalian cells would have any significant deleterious biological effects” (p. 108).

Given the lack of widespread labeling, which might allow epidemiological studies to correlate health effects and ingestion of GM food, references to the absence of evidence of harm demonstrate the operation of a web of assumptions that combine to see agbiotech as safe.

⁸⁹ An important exception to this attitude emerged at the Gene Flow Conference. Many speakers and audience members decried the developing technology of ‘biopharming,’ the practice of engineering crops to produce pharmaceutical or industrial proteins. Although Horsch defended Monsanto’s choice of maize as the host plant (unfortunate, but biologically the best alternative), the absolute ban on using food crops for biopharming enjoyed great support. As Sarukhán said with great conviction, “This is one of the cases where ethical and moral responsibilities should [outweigh] the economic benefits” (PIFB 2003, 24).

⁹⁰ Royal Society (2002) Genetically Modified Plants for Food Use and Human Health – An Update. Royal Society, London, UK. www.royalsoc.ac.uk/gmplants.

Science can be depended upon to prove the safety of GM crops, both in terms of research already completed and future investigations.

Promotional science assumes that science generally supports agbiotech. At the Hort-Biotech Workshop, Batkin (2002) presented a slide on industry priorities to overcoming barriers to horticultural biotechnology, including: “Coalitions must be formed between the scientific community and the production agriculture community to execute a plan.” Batkin thus assumed that the scientific community sided with the production agriculture community (rather than imagining that involving scientists might increase scrutiny over health, environmental, or social consequences of agbiotech). David Schmidt⁹¹ (2002) made the point graphically, showing a slide titled, “The Weight of the Evidence.” The graphic featured a balance with books on one side (labeled OECD, IFT, ADA, FDA, NAS, House Science Subcommittee) and little pictures of potatoes and an upside down butterfly on the other.⁹² Not surprisingly, the balance tilted favorably toward the stack of books. This communicated both a belief about the historical weight of scientific evidence for agbiotech safety, but also that science is rightly judged by the accumulation of studies – leaving scant room for contrarian science to produce a ‘breakthrough’ study that could legitimately question the safety of GM crops.

⁹¹ Dave Schmidt is Vice-President of Food Safety, International Food Information Council (IFIC).

⁹² The potatoes undoubtedly refer to Ewen and Pusztai’s (1999a) study and the butterflies to the *Bt* corn-monarch butterfly research (Losey, Rayor et al. 1999).

5.3 Benefits

GM crops benefit growers by decreasing the cost of production and making farming more predictable and less dangerous.

Gianessi (2002) reported significant estimates of production cost savings by transitioning to GM crops: a projected net savings of \$30 million/year saved in California by switching to Liberty Link processing tomatoes (\$30 million/year saved in hand weeding costs, \$9 million saved in cultivation costs, and an increase of \$9 million in pesticide costs); and a production increase of \$125/acre by switching to *Bt* sweet corn in Florida.⁹³ Klee (2002) specifically referred to farmers' enthusiasm for growing GM sweet corn to reduce pesticide use. Pesticide sprayings not only cost money, but also increase on-farm exposure to toxic substances. Redenbaugh (2002) provided evidence in the form of a slide titled, "Farmer Activism - Biotech Crop Production without Approvals" mentioning plantings of *Bt* cotton in India, Roundup Ready™ soybeans in Brazil, and *Bt* maize in Mexico. While he did not condone behavior contrary to national or international law, he interpreted such action as strong evidence for the enthusiasm of farmers to access biotech's benefits.

By assuming these collective benefits of agbiotech for growers, promotional science constructs a political landscape on which opposition to GM crops signifies opposition to farmers and their interests. Klee, for example, presented the failure of the

⁹³ Gianessi referred to forty case studies of economic projections for switching to GM crops, conducted by the National Center for Food & Agricultural Policy and available at <http://www.ncfap.org/whatwedo/40casestudies.php> (accessed 18 March 2005).

agricultural market to embrace *Bt* sweet corn, out of fear of consumer rejection, as a tragedy for growers who were eager to reduce pesticide use.

Consumer-oriented benefits (increased nutrition, new flavors, delayed ripening) will tip the risk-benefit calculus, making GM crops attractive to consumers.

Promotional science assumes that technological progress (discussed as an assumption below) will overcome consumer resistance. At the Hort-Biotech Workshop, participants generally showed great optimism for the combination of output-oriented agricultural biotechnologies and increased communication of safety by science (see above) to encourage consumers to embrace GM food. The hard question was not whether consumers will accept GM food, but when and at what cost.

5.4 Progress

The first generation of GM crops will give way to future technologies that offer innovative and more complex benefits to producers and consumers.

At the Gene Flow Conference, Goodman (2003) described current transgenic technologies as “low hanging fruit” and “proof of principle.” Such references implied a future of great promise. In the language and graphics of economics, Nicholas Kalaitzandonakes⁹⁴ (2002) made a similar claim at the Hort-Biotech Workshop. He instructed the audience to think of current agricultural biotechnology as a “platform, like cars or computers.” His slide showed a well-known graph of innovation vs. time, with an S curve representing the slow rate of innovation during early periods of technological

⁹⁴ Nicholas Kalaitzandonakes is Associate Professor, Department of Agricultural Economics, University of Missouri, Columbia, MO.

development, followed by a period of rapid innovation, and ending with a leveling off as the technology matured. His slide included an arrow (“you are here”) that pointed to a position very low on the curve (early time period, slow innovation), graphically suggesting that major breakthroughs in agbiotech were just around the corner. The PIFB website provides the full narrative:

Proponents assert that agricultural biotechnology has the potential to improve the nutritional value of foods, reduce crop losses to pests and drought, slow down soil erosion, reduce the use of chemical pesticides, and increase food security in the developing world. They believe biotechnology could enable animals and fish to grow faster and be more disease-resistant; produce trees that grow quickly or with improved pulp and paper characteristics; or alter ornamental trees and grasses to require less care and be more stress and disease-resistant. While many of these potential applications have not yet been tested for their technical or economic feasibility, *it is clear* that the current generation of genetically modified crops is but the first of many possible applications of biotechnology to agriculture. Today’s *relatively simple* gene manipulations are *likely* to yield to more complex applications as scientific knowledge grows [emphasis mine] (PIFB 2005a).

Locating current technologies as proofs of principle, low hanging fruit, and relatively simple situates controversy over agbiotech as a minor bump in the road toward fulfillment of the technology’s ultimate promise. Within this framing, opposition is misplaced as it attacks early versions of GM crops, and it threatens to delay or halt the beneficial progress toward more advanced technologies. More broadly, anything we can do to speed innovation (e.g., increase public support of research, streamline government regulation, deter or undermine activist opposition) will bring society significant returns in the form of safer and more powerful biotechnologies.

The progress and success of the agbiotech industry are public goods.

Promotional science strongly connects public and private benefits of advancing agbiotech. In the *California Agriculture* issue, Alston (2004) rhetorically moved from

assuming technological progress (see previous) to a rationale for public support of development:

The technological potential for GM horticultural crops appears great, particularly when we look beyond the “input” traits that have dominated commercial applications to date, to opportunities in “output” traits, such as pharmaceuticals and shelf-life enhancements. Because delays in socially beneficial technologies mean forgone benefits, there may be a legitimate role for the government in facilitating a faster rate of development and adoption of horticultural biotechnology products (p. 88).

It is critical to note that Alston did not advocate for the government to establish a parallel R&D track that would focus on developing and distributing socially beneficial technologies; instead the government had a “legitimate role in facilitating” an implied, but unspoken, private sector. Neal Gutterson⁹⁵ (2002) included a slide in his presentation that stated, “When a societal need is not met by companies due to lack of economic incentive, governmental intervention in the form of support for needed development is warranted.” Again, the vision was not one of government labs (e.g. nuclear weapons research), but of “support” for research that would eventually become commercialized products. The annual report from the Seed Biotechnology Center (a primary sponsor of the Hort-Biotech Workshop) was sent to all workshop participants. It included a message from Bill Van Skike, President of the California Seed Association and an advisor to the Seed Biotechnology Center (SBC):

The Annual Report documents the continuing activities of the SBC that provide tangible research, educational, and *public service benefits to the seed industry*...The political challenges facing the industry, including the movement to ban genetic engineering approaches to crop improvement, make the need for scientific knowledge crucial. The SBC’s role as an educational and scientific conduit for such an issue is extremely *beneficial to the industry, as well as to the general public* [emphasis mine] (Seed Biotechnology Center 2004, 3).

⁹⁵ Neal Gutterson works for DNA Plant Technology.

The phrase, “public service benefits to the seed industry,” was an odd textual construction, but revealed the deep assumption linking private progress to social benefit. One speaker at the Gene Flow Conference took particular aim at this conflation of public and private good:

[Light] emphasized that, in general, where deep disagreements exist, a mere market advantage does not constitute a public good. In such a case, if no overriding public good can be identified by a particular GM application or where the environmental or health risks are too high, then the application should not be permitted or at least should not be supported with public funds (PIFB 2003, 28).

Light’s argument *against* conflating market advantage with public good provided evidence for the dominance of this assumption at the Gene Flow Conference. As discussed above, Light’s presentation challenged a number of the assumptions that controlled the discourse at the Gene Flow Conference, but the timing of his panel prevented his perspective from having an impact beyond the short question and answer session following his panel.

5.5 Control

A strong intellectual property rights (IPR) regime is necessary to support innovation in agbiotech, although the system requires nominal reform to reduce the ‘patent thicket.’

As discussed earlier within an analysis of character development at the Hort-Biotech Workshop (Section 3.3), workshop participants made a variety of suggestions for reducing the tendency of IPR to hinder innovation. The favored solution was to create a quasi-public organization either to serve as a clearinghouse for information about patent holdings or to develop and hold several key technologies in order to license them more broadly for research and commercialization. As Jefferson (2002) stated, “Imagine a

toolkit that cures constipation.” This focus on institutional innovation to speed technical innovation stays faithful to the broad paradigm of IPR as a key component of technology policy:

[Hort-Biotech Workshop participants] also concurred that a policy shift in how intellectual property rights are assigned is necessary. Researchers, public and private, increasingly obtain intellectual property rights to their inventions and either license or transfer ownership of these rights to commercial interests. Once this is done, the inventions, or more importantly, the tools to further inventions, are no longer available to the public unless future researchers can negotiate time consuming and often expensive agreements with the intellectual property owners. Workshop participants suggested establishment of an intellectual property clearinghouse and/or a technology pool to give researchers greater access to collections of agricultural biotechnology tools and materials (“Horticultural biotechnology issues aired at Monterey workshop” 2002, 5).

The discussion centered on *redistributing* IPR rather than *re-defining* it. No ethical critique of IPR enters the discourse of promotional science – the relevant context is one of utility/function rather than of metaphysical questions about ownership (e.g., should life-forms be patentable?).

Governmental regulation of GM crops is adequate to protect human and environmental health, and should be streamlined and relaxed in some places.

Redenbaugh and McHughen (2004) described the agbiotech regulatory regime in the U.S. as thorough, listing an impressive array of “key data requirements” including: “product description...molecular characterization...toxicity studies...nutritional data...substantial equivalency...allergenicity...natural toxicants...[and] environmental impact” (p. 108). They went on to argue that “As further experience is gained with biotech methods, regulatory requirements should be relaxed for categories of products posing little health or environmental risk” (p. 114). Clark et al. (2004) communicated a similar confidence and expressed concern that regulatory costs had gotten out of control:

Extensive safety testing is required for regulatory approval (deregulation) of biotech crops beyond what is required for varieties bred using traditional methods (see page 106). If the trait has already been approved in other crops, the costs are lower as prior data can be used to support an application. However, for novel traits likely to be of interest for horticultural crops, the costs could be millions of dollars (p. 97).

At the Hort-Biotech Workshop, Klee (2002) took a more critical stance: “We have to break this log jam [referring to regulatory barriers to horticultural biotechnology crops].” Likewise, in discussing the contentious concept of substantial equivalence,⁹⁶ Means (2002) argued that “policies have to catch up with reality,” implying that the regulatory stance had not relaxed according to the growing evidence of safety and benefits of GM crops. At the Gene Flow Conference,

Villalobos outlined the steps that Mexico has taken to prepare for the use of GMOs, such as developing physical infrastructures and human resources. This includes research institutions and highly trained scientists; the development of requisite regulatory and risk assessment expertise; the formation of a regulatory framework; and the signing of the Cartagena Protocol on Biosafety (PIFB 2003, 27).

Although several speakers expressed concern about the lack of a strong regulatory regime surrounding transgenic pharmaceutical crops (e.g., Gálvez 2003), the only presentation to suggest that U.S. regulations might be inadequate occurred at the very end of the final panel. Bill Lambrecht⁹⁷ (2003) suggested that the U.S. needed regulation that “inspires trust”; interestingly, however, the PIFB booklet described his argument as less obviously critical: “He urged the U.S. to reconsider its resistance to regulatory reform, suggesting that a clear regulatory structure would inspire significant confidence among people” (PIFB 2003, 29). Regardless of whether PIFB underestimated⁹⁸ Lambrecht’s critique, the call for reform stemmed primarily from a perception problem (trust, confidence)

⁹⁶ Substantial equivalence is a regulatory term used to describe a condition in which a novel product is ‘substantially equivalent’ to an older, accepted product. The FDA, for example, assumes that transgenic foods are substantially equivalent to conventional foods except for the expression of the targeted protein(s).

⁹⁷ Bill Lambrecht is a journalist for the St. Louis Post-Dispatch and author of *Dinner at the new Gene Café*.

⁹⁸ Thank you, George W. Bush.

rather than a true safety concern (inadequate testing of technologies that would enter the environment and the human food supply). In sum, while promotional science does not hold up current regulatory policy over GM crops as ideal, the points of criticism address issues of regulatory efficiency, relaxation due to increasing confidence in safety, and public comfort.

Agbiotech opponents are irrational and often disguise their motivations. The public is vulnerable to their campaigns, but education of the public by scientists promises to improve the public acceptance of agbiotech.

Many speakers at the two events characterized opposition by consumers and other actors as irrational, irresponsible, interest-based (e.g. to raise money for NGOs), and marginal to scientific discourse. At the Hort-Biotech Workshop, Klee (2002) included a slide that stated, “While cost is a factor, the more important barrier to GE crops is FEAR” [emphasis original]. Zischke (2002) also commented that for consumers, the “fear factor still exists,” but that consumer-oriented products would overcome this barrier. Batkin (2002) hoped that “some realism gets inserted back into the debate.” These examples demonstrated strong boundary-work to distinguish agbiotech opponents as disconnected from data and logic. At the Gene Flow Conference, Raven (2003b) described in disbelief the Zambian government’s rejection of GM maize as food aid and “letting one-hundred thousand people starve.” He also ridiculed the “environmental literature” as attributing the “collapse of Oaxaca to transgenes.” Ortega (2003) relied less on hyperbole but dismissed the allocation of resources to publicizing environmental concerns about GM crops as wasteful compared to resources allocated to research.

Other speakers were more diplomatic in their evaluation of the causes of opposition. At the Gene Flow Conference, Fernández stated: “Any event that the public could perceive as harmful, regardless of whether or not negative consequences actually exist, will impact and change producers’ decisions and consumers’ behaviors. In this sense, he said, it was important to pay attention to the public demand for minimum thresholds [of contamination] before introducing new transgenic varieties” (PIFB 2003, 22). In other words, the public was vulnerable to misperceptions, but the proper response was not to dismiss the public’s fears but to show a degree of strategic sympathy. Ezcurra (2003) characterized opposition to agbiotech as a combination of “concerns and myths” and expressed confidence in the ability of “scientific knowledge” to “overcome the unknown.” He thus not only marginalized opposition (myths vs. scientific knowledge) but implied that concerns emanate from the “unknown,” as if ignorance were the cause of opposition.

In *California Agriculture*, James (2004) defended promotional science against one of the oft-repeated statistics that challenged agbiotech: polling data that reported the percentages of consumers or citizens who rejected GM food or wanted mandatory labeling. First she exposed the ignorance of consumers with regard to agricultural production:

[In the 2001 CSPI survey] only 40% of consumers said that they would purchase processed foods that were labeled as having been made from crossbred corn. Since nearly all corn varieties currently being used are crossbred, stated resistance to consuming this type of corn reveals a lack of basic knowledge about agriculture and how food is produced (p. 101).

Rhetorically this undermined opposition to agbiotech by characterizing consumers as ill-prepared to judge agbiotech fairly. Next, she criticized the survey methodology:

One notable example described by Shanahan et al. (2001) is a survey conducted by the Harris Poll in 1993. In a question designed to measure attitudes about the relative risks and benefits of genetic engineering, the dinosaurs in the movie *Jurassic Park* were given as an example of genetic engineering. The reference to *Jurassic Park* evokes a very negative image, so it is not surprising that 57% of respondents said they thought the risks of genetic engineering outweighed the benefits (the most negative response to this type of question in the surveys reviewed by Shanahan et al. 2001) (p. 102).

James' intended impact was to call into question the validity of such surveys, which often served to bolster opposition to agbiotech as a 'popular' position. Instead, James implicated unsavory survey techniques as falsely influencing the results – effectively derationalizing the apparent opposition. Lastly, James cited a study that showed consumers' willingness to purchase GM food in a supermarket study. Aside from the reduction of the public to consumers (discussed previously), James demonstrated that the rational public actually accepted agbiotech quite readily. In sum, she discredited the opposition, explained the apparent public rejection of agbiotech, and reassured promotional science of the actual (market) openness of consumers to GM food.

Apart from the activist representatives at the Gene Flow Conference, two presenters spoke against the assumption of opposition as irrational. Light (2003) did not argue for or against the scientific validity of arguments against agbiotech, but he situated that very debate as outside the path to policy resolution. According to him, consensus would fail to emerge “no matter how much evidence is presented because the underlying disagreement represents a fundamental metaphysical divide rather than a disagreement about the facts of the matter.” Much like preferences for kosher food, society should allow individuals to opt out of GM food in order to “preserve the coherence of their worldview” (PIFB 2003, 28). Most directly, he argued against the “divisive strategy” of characterizing one view as “non-scientific.” In a similar vein, and immediately following Light in the program, Lambrecht (2003) emphasized the role of culture in understanding

opposition to agbiotech. Thus, neither speaker allied himself with agbiotech opponents, nor defended the rationality or scientific validity of arguments against GM crops, but they challenged the *dismissal* of the opposition on logical or empirical grounds, which had become the dominant pattern at the Gene Flow Conference.

5.6 Summary

This chapter has brought together empirical data from Chapters 3 and 4 in order to understand the practices and assumptions that create discursive power to promote agbiotech. Viewing the events as ‘scientific performances’ has revealed how narrative, character development, stage management, and audience construction combined to produce a set of claims about biotechnology, global food systems, regulatory policy, democratic participation, and technology governance. These claims connect with one another to form a coherent world-view that encourages the research, development, and commercialization of GM crops. Detailing the assumptions that support this world-view serves to map the terrain of promotional science, upon which the first sparks of dissent (contrarian science) ignite controversy.

Chapter 6 Case Studies of Contrarian Science

This chapter introduces three cases of contrarian science: Ignacio Chapela's discovery of transgenic DNA in Mexican maize (Chapela Maize); John Losey's study of *Bt* corn pollen's lethal effects on monarch butterfly larvae (Losey Monarch); and Arpad Pusztai's experiments comparing the nutritional and developmental effects of GM potatoes vs. conventional potatoes fed to laboratory rats (Pusztai Potato). I present these three case studies of contrarian science within a dramaturgical framework, slightly modified from the typology engaged in Chapters 3 and 4. In order to introduce each case, I address four questions:

1. What characterized the scientific approach to the research question? *How did the question emerge? What hypotheses were tested? What methodology was chosen?*
2. What claims were made? *How have the scientists interpreted their data? What arguments did the data support, both in the published scientific paper and within media immediately following the research publication?*
3. What were the narrative implications and significance? *What did these claims mean in the context of debates around GM crops? What stories did they tell about how to situate this technology in society?* I consider both scientific and media discourse.
4. What audiences were constructed as the research and corresponding narratives were communicated? *Who had access to the results? How was access controlled? What mediums enabled scientific research to reach wider public audiences?*

Wherever possible, I focus on very limited time-frames to answer these questions. This strategy serves two purposes: first, to avoid repeating data that is more critical to

Parts Two (resistance) and Three (responses), and second, to emphasize the fleeting moment of contrarian science. Most narratives of these controversial cases delve straight into the controversy without bothering to examine the early moments of unfolding research, claims, and narrative. These early moments clearly anticipate controversy – scientists involved in such research do not remain completely ignorant of the political context of their work – but it is worth investigating the character of contrarian science *whose specific claims have not yet faced resistance*.

6.1 Chapela Maize

The first case study of contrarian science centers on David Quist and Ignacio H. Chapela's (2001) publication in *Nature* that announced the presence of transgenic DNA fragments in the genomes of landraces of Mexican maize. Following the style of scientific prose in a premier journal, the peer-reviewed research article presented the research process and results as linear, clear, and confident (Latour 1987), but my interviews with the authors revealed important aspects of science-making that contribute to the understanding and analysis of contrarian science.⁹⁹ In addition, media accounts of the research before and immediately after the *Nature* release clarified the significance of the research in advance of, and perhaps anticipating, the resistance that would soon follow.

⁹⁹ Unless otherwise noted or cited, my presentation of this case depends on a series of formal interviews conducted with David Quist (Quist 2003, 2004) and Ignacio Chapela (Chapela 2003a, 2004a) and their published article in *Nature* (Quist and Chapela 2001). Direct quotations include precise references.

Research question and methodology

As is often the case in the practice of science, the experiment that defined the eventual research publication emerged from laboratory work aimed in quite a different direction. David Quist, a graduate student at UC Berkeley in Ignacio Chapela's laboratory, traveled to Mexico in October 2000 to run an educational workshop on transgenic detection as part of an ongoing research and capacity-building relationship between the Chapela lab and UZACHI (Union de Comunidades Zapoteco Chinanteca, an indigenous scientific initiative in Oaxaca, Mexico). After consulting the scientific literature and collaborating with several colleagues, Quist developed a protocol to test for the presence of transgenic DNA in maize samples. Quist and Chapela assumed that the highlands of Oaxaca would be free of transgenic maize, especially given the Mexican moratorium that had prohibited the planting of transgenic maize since 1998. Nevertheless, they saw the value in teaching the technique as an educational tool that might have practical utility in Oaxaca sometime during the next decade or sooner if the moratorium were lifted. Guided by this assumption, Quist brought positive controls (transgenic maize from the U.S.) with him for demonstration purposes. After helping the workshop participants obtain samples from local maize fields and extract DNA from ground-up samples, Quist ran the PCR¹⁰⁰ protocol on his own, the night before demonstrating the technique at the workshop. His positive controls and water negative

¹⁰⁰ PCR (polymerase chain reaction) is a laboratory technique that allows an investigator to make many copies of DNA fragments. Typically, investigators place the post-PCR samples on a gel medium infused with an electric charge. The fragments move at different rates, providing a measurement of the relative lengths of various fragments (more precisely, populations of fragments). Depending on how the original DNA was fragmented (specific enzymes cut DNA strands within a target sequence), the pattern of bands on the gel indicate properties of the original intact DNA.

controls were “behaving,” but the local samples showed a “faint signal,” suggesting the presence of transgenic DNA. Although he had followed the protocol precisely, Quist “hoped that cross-contamination was to blame.” Using the only phone available in the locale, he contacted Chapela. Although they had initiated the workshop, they had never previously discussed the implications of finding transgenic DNA in landraces of Mexican maize. Quist recalled:

For me, holy shit if this holds out to be true! My God! And I ran the tests the next day, and I didn't run the samples that came up to be transgenic positive because I couldn't confirm it...Ignacio and I discussed this, [and we wanted] to be sensitive to not say “Oh yeah, we found transgenic DNA, but maybe it's not true. Okay, bye, I'll see you later.” And then go back to the U.S. and then do the tests there. I didn't feel like we could make a claim based on that very preliminary result. So I didn't use those samples because then if rumors get started, or what have you, it would be a really dangerous thing. So that's when we collected more samples and brought them back to Berkeley to be able to run them under more stringent, controlled conditions (Quist 10/10/2003).

What had begun as a demonstration project had suddenly become an experimental question: *had transgenic DNA entered remote populations of maize landraces in Oaxaca?*

Quist returned to the U.S., aware of the significance of potentially proving the presence of transgene flow in Mexico, and eager to repeat the analysis in a more familiar and complete laboratory setting. Quist repeated the PCR-based transgenic detection protocol under the supervision of Chapela. They compared samples from the following sources:

Table 2: Quist and Chapela's Maize Samples

Sample	Purpose
Whole cobs of native landraces of maize sampled from four standing fields in Oaxaca	Experimental target
Bulk grain from the local stores of Diconsa, the Mexican agency that distributes subsidized food (supplied largely from U.S. sources)	Potential source of transgenic DNA (The U.S. supply includes transgenic maize. While designated for consumption, Mexican farmers may have planted the seeds)
Cob samples of Peruvian blue maize	Negative control (minimum chance of transgene flow to this variety in this location)
20-seed sample from a 1971 collection of maize in Oaxaca	Negative control (seeds collected before the advent of transgenic maize)
Bulk-grain seeds of Yieldgard Bacillus thuringiensis (<i>Bt</i>)-maize (<i>Bt1</i> ; Monsanto Corporation)	Positive control
Bulk-grain seeds of Roundup-Ready maize (RR1; Monsanto Corporation)	Positive control

Their method of detection relied upon the widespread use of the Cauliflower Mosaic Virus (CaMV) as a standard promoter inserted during the process of plant transformation.¹⁰¹ Virtually all commercialized transgenic crops include the insertion of the CaMV promoter. In addition, Quist and Chapela tested for the presence of the nopaline synthase terminator sequence from *Agrobacterium tumefaciens* (T-NOS), a second common element necessary to control expression of inserted genetic sequences and the *Bt* gene. Collectively, these methods led Quist and Chapela to conclude that transgenic DNA had “introgressed” into landraces of Mexican maize.¹⁰²

¹⁰¹ A promoter encourages the expression of a gene. In the case of transformation (inserting foreign DNA with recombinant DNA techniques), the promoter is linked to the foreign genetic sequence to enable the host organism to produce the intended protein. In other words, promoters are genetic sequences that ‘turn on’ gene expression of a nearby sequence.

¹⁰² ‘Introgression’ is one of many terms to describe the presence of DNA foreign to a particular population, and one that was critiqued by population geneticists who invoke the term in a much narrower realm related to the stability of a gene in a controlled breeding population during successive generations. Activist groups

The second part of the investigation asked quite a different question: namely, in what context or contexts was CaMV inserted? Two scientific thrusts drove this secondary inquiry. First, Quist and Chapela judged the introgression finding as politically significant, but scientifically limited. “So what?” they asked themselves. In other words, they felt compelled to push their research to engage questions about the implications of transgenic DNA in Mexican landraces of maize. As Quist recalled, other scientists might have approached the ‘so what’ question at the trait level – what would the *Bt* gene do to landraces of Mexican maize in terms of fitness and ecological effects – but Quist and Chapela were “thinking on the DNA level.” They were familiar with research that demonstrated DNA fragmentation and unintended changes in expression, which led them to wonder about the genetic context for insertion. Second, Quist and Chapela brought an ecological lens to their inquiry. Rather than taking an agronomic perspective (how will gene flow affect the farming of maize landraces?), Quist and Chapela viewed transgenic introgression as occurring in an ecological context – an environment continuous with but larger than agricultural spaces and timeframes. Quist located their approach as part of the emerging field of ‘gene ecology’.

To answer this second question, Quist and Chapela employed the method of inverse PCR (iPCR), which sequenced the DNA regions immediately before and immediately after the CaMV promoters. While the method could not produce a lengthy sequence with a certain genetic identity, comparing relatively short sequences of the

have preferred “contamination” (e.g., Food First 2002), which clearly carries strong negative connotations of pollution and loss of purity. Industry groups have used the term, “adventitious presence” (Biotechnology Industry Organization 2005b), which communicates a much more benign phenomenon of something ‘out of place’ but perhaps having no impact or significance. While no term is thus completely objective, I will use the words ‘introgression’ or ‘presence’, unless intentionally communicating a value judgment (e.g., an emerging narrative of ‘contamination’).

regions flanking the CaMV promoter to known sequences available at GenBank¹⁰³ would contribute to the task of exploring the context of transgene insertion in the environment, ‘downstream’ from laboratory conditions and after wild events of pollination and replication.

Factual claims

The first part of the *Nature* publication addressed the claim of introgression. The introduction to the article stated, “Here we report the presence of introgressed transgenic DNA constructs in native maize landraces grown in remote mountains in Oaxaca, Mexico” (Quist and Chapela 2001, 541). Quist and Chapela’s Figure 1 (p. 541) displayed pictures of gels for each of the relevant samples. Bands on the gels indicated the presence of CaMV in four samples of criollo varieties of maize, in the Diconsa sample, in the Roundup Ready maize, and in the *Bt* maize. No such bands were present for the negative control from Peru or the internal negative control for the PCR reaction. More specifically, the text indicated that four of five criollo samples included CaMV, that two of six criollo samples and the Diconsa sample included T-NOS, and that one criollo sample included a *Bt* gene. Quist and Chapela also cited an investigation by the Mexican government that affirmed their findings:

During the review period of this manuscript, the Mexican Government (National Institute of Ecology, INE, and National Commission of Biodiversity, Conabio) established an independent research effort. Their results, published through official government press releases, confirm the presence of transgenic DNA in landrace genomes in two Mexican states, including Oaxaca. Samples obtained by the Mexican research initiative from sites located near our collection areas in the Sierra Norte de Oaxaca also confirm the relatively low abundance of transgenic DNA in these remote areas. The governmental research

¹⁰³ GenBank is a publicly available online database of sequenced DNA. Scientist post their results on this database and later researchers can look for similarity to experimental sequences.

effort analysed individual kernels, making it possible for them to quantify abundances in the range of 3-10% (Quist and Chapela 2001, 541).

In their article, Quist and Chapela suggested that a *potential* source for that contamination was U.S. maize distributed through Diconsa. The *San Francisco Chronicle* offered a different hypothesis and used the less benign, but more accessible, term for introgression: “Genes from bio-engineered crops can jump fields and contaminate native crops” (Kay 2001).

The secondary claims¹⁰⁴ were less definitive than the first, mostly because the underlying method (iPCR) did not support the kind of ‘yes’ or ‘no’ answer that PCR had provided in offering proof of the presence of transgenes in DNA samples. Quist and Chapela reported two significant secondary claims:

1. Flanking regions of CaMV were diverse, suggesting multiple loci of insertion and/or multiple introgression events. Some of the flanking sequences matched known transgenic maize sequences, others matched known criollo maize sequences, and still others failed to match any GenBank sequences.
2. In some of the introgressed DNA, the “transgenic DNA construct seemed to have become re-assorted and introduced into different genomic backgrounds, possibly during transformation or recombination” (Quist and Chapela 2001, 542).

In a UC Berkeley (2001) press release, Quist explained, “If this contamination was the result of a single gene transfer event, we would expect to find the transgenic DNA in a consistent location on the criollo genome. Instead, we’re finding it at different points

¹⁰⁴ I refer to “secondary” claims for three reasons: 1) they do not fall under the topic of the title of the published paper, 2) media reports announcing the research mention the introgression finding first, and 3) Chapela referred to them as “secondary” in official correspondence about his tenure case (e.g., Chapela 2003d).

along the genome.”¹⁰⁵ Thus, Quist and Chapela presented their data as basis for questioning broad issues of the stability of transgenes and their flow through agricultural/ecological contexts. Other actors quickly took up these claims and collectively formed narratives that explained the significance of these contrarian results. These narratives were more significant as scientific performances that reached much broader publics than the regular audience of *Nature*.

Narrative significance – implications

Together, the *Nature* article and the media surrounding it constructed a narrative about the significance and implications of Quist and Chapela’s findings. Recognizing the plethora of narratives produced and advocated by diverse interests, I draw two boundaries in this section to limit my discussion. First, I focus on media leading up to and including the announcement of the research article. This boundary eliminates much discussion about significance that evolved during the active period of controversy (resistance and responses). Second, I restrict this analysis to the original research article (Quist and Chapela 2001); news articles in *Nature* (Dalton 2001), the *New York Times* (Yoon 2001), and the *San Francisco Chronicle* (Kay 2001); and the UC Berkeley press release (University of California 2001) announcing the *Nature* article. While all of these media sources had their own biases and institutional interests, they captured a moderate view in

¹⁰⁵ When a professor publishes in a premier scientific journal, it is customary for the university to issue a press release announcing the results. In the case of Quist and Chapela’s article, the university obtained copies of the manuscript from *Nature* before publication, sought opinions of its own scientists (some of which were not favorable), and issued the press release. A deeper analysis of the institutional forces, characteristics, and consequences of UC Berkeley’s commitment to Quist and Chapela’s research goes beyond the scope of this dissertation, but raises important questions about the powerful and complex context of public universities.

relation to narratives constructed by overtly partisan organizations such as Greenpeace or AgBioWorld.

Broadly, the narrative that emerged included four dimensions. First, the discovery of transgenic DNA in Mexican maize signaled *contamination*, at an ecological, cultural, and agricultural level. Second, the contamination story combined with secondary claims to question the *technical reliability and safety* of GM crops. Third, the apparent surprise of the results raised issues of *institutional trust* with regard to governmental regulation and industrial oversight. Fourth, the findings pointed to major opportunities for further research in the fledgling field of *gene ecology*.

Contamination

The language and imagery of contamination and pollution performed significant rhetorical work in forging a narrative of concern. Interestingly, the research publication did not use either term – favoring instead words such as “introgression,” “introduction,” “presence,” and “flow,” but a related metaphor appeared nonetheless: “Our results demonstrate that there is a high level of gene flow from industrially produced maize towards populations of progenitor landraces. As our samples originated from remote areas, it is to be expected that more accessible regions will be *exposed* [emphasis mine] to higher rates of introgression” (Quist and Chapela 2001, 542). ‘Exposure’ carried a slightly less negative connotation than ‘contamination,’ but still suggested cause for concern. On the other hand, the UC press release and the *SF Chronicle* and *NY Times* articles used forms of the word ‘contamination’ in their lead sentences. This rhetoric

implicated transgenic DNA as dangerously infiltrating the relatively pure and desirable genomes of Mexican maize landraces.

One aspect of contamination as concern stemmed from the genetic diversity of maize landraces as a global resource for future developments in maize seed. Quist and Chapela (2001) referred to the “genetic diversity of crop landraces and wild relatives in areas of crop origin” as “essential for global food security” (p. 541). The UC press release cited Chapela to explain:

Genes from genetically modified crops that spread unintentionally can threaten the diversity of natural crops by crowding out native plants, said Chapela. A wealth of maize varieties has been cultivated over thousands of years in the Sierra Norte de Oaxaca region, providing an invaluable “bank account” of genetic diversity, he said. Chapela added that genetically diverse crops are less vulnerable to disease, pest outbreaks and climatic changes. “We can't afford to lose that resource,” said Chapela (University of California 2001).

This utilitarian view of genetic diversity as a ‘natural resource’ to be protected from contamination sounded an alarm. The implication was that transgenic DNA would crowd-out, replace, or destroy the genetic diversity that future seed breeders would need to respond to agricultural challenges or crises.

A second aspect of the contamination narrative focused on maize not at the genetic level but at an ecological scale. Quist and Chapela (2001) mentioned “direct effects on non-target species” and the transfer of ecologically-relevant traits to landraces or wild relatives (p. 541). The *NY Times* cited Dr. Norman Ellstrand, evolutionary biologist at UC Riverside: “He said the real worry was that other foreign genes – like pharmaceutical-producing genes being developed in crops – could also find their way quickly and unnoticed into distant food sources” (Yoon 2001). Although Ellstrand’s comment focused on foreign genes, the concern manifested at an ecological scale – the ecology of agricultural production that included humans as ultimate consumers.

The third aspect of the contamination narrative broadened the concern beyond both the genetic and ecological scales to the cultural impacts. *Nature* described Oaxaca as “a rural southern state where maize is revered by indigenous people” (Dalton 2001).

Likewise, the *NY Times* stated:

In addition to being one of the world’s most important crops, corn is viewed with a *near religious reverence* in Mexico, with seeds of native varieties passed down from generation to generation. Until now, scientists said researchers had assumed that these varieties, some of which are grown only by subsistence farmers in remote areas, were *pristine*...Scientists may eventually be able to quantify the biological effects of the contamination, but some say the *cultural cost* in a country where corn is a symbol of the Mexican people may be harder to measure. “*The people are corn*,” said Dr. Chapela, who is Mexican, “*and the corn is the people*” [emphasis mine] (Yoon 2001).

This framing launched the contamination narrative out of biology and into the realms of national politics, ethnic identities, and spirituality. If the people were the corn, and the corn was contaminated, the people were contaminated. The *NY Times* thus situated maize in a cultural context and implied that the outsider/insider relationship between transgenic DNA and genomes of maize landraces mapped onto an even more significant outsider/insider relationship between biotechnology and local Mexican culture. Chapela’s quote cemented this analogy and even indirectly engaged discourses of the genetic manipulation of humans by merging the identities of plant and person.

Technical reliability and safety

A second narrative theme addressed the technical reliability and safety of GM crops. The *NY Times* reported, “Scientists said the results also indicated that crop genes might be able to spread across geographic areas and varieties more quickly than researchers had guessed,” and quoted Norman Ellstrand as saying “It shows in today’s modern world how rapidly genetic material can move from one place to another” (Yoon

2001). The article implied that scientists had underestimated the technical process of gene flow. Likewise, the UC Berkeley press release (2001) stated, “Agricultural experts and proponents of biotech crops maintain that corn pollen is characteristically heavy, so it doesn’t blow far from corn fields by the wind. Chapela said this assumption may need to be reevaluated in light of the recent findings in Mexico.” Here, a simple technical assumption – the distance corn pollen travels – became uncertain, undermining the promotional narrative of transgenic containment.

At a deeper technical level, Quist and Chapela’s (2001) secondary finding (of multiple points of insertion and potential instability of transgenes) spoke directly to the issues of reliability and safety. They stated, “Long-term studies should establish whether, or for how long, the integrity of the transgenic construct is retained” (p. 542). While the authors did not specify the consequences of transgene instability, the obvious possibilities would include decreased technical performance over successive generations (with saved seed) and unintentional effects due to portions of the transgene interacting with other functional units in the genome.

Institutional trust

A related narrative theme exposed institutional failures that eroded public trust in the regulation and development of GM crops. The SF Chronicle quoted Chapela: “For a long time, we’ve been assured that the transgenic crops wouldn’t move out from where they were planted so there was no risk of cross-contamination...Finding it in such a sensitive, delicate place makes it evident that the assurances weren’t true” (Kay 2001).

Each of the above sources also mentioned the de facto moratorium in Mexico banning the planting of GM maize. For example:

Mexico imposed a moratorium in 1998 on new plantings of transgenic maize. The closest region where bioengineered corn was ever known to have been planted is 60 miles away from the Sierra Norte de Oaxaca fields, said Chapela. "It's not clear if the moratorium was poorly enforced, or if the contamination occurred before the moratorium was enacted," said Chapela. While new plantings are banned in Mexico, it is still legal to import biotech corn into the country. "Whatever the source, it's clear that genes are somehow moving from bioengineered corn to native corn," he said (University of California 2001).

This passage demonstrated a series of related institutional critiques. First, Chapela raised the possibility that the moratorium was ill-conceived, given that GM maize had been planted in Mexico before 1998 – political institutions ignoring biological reality. Second, Chapela suggested that the moratorium may have been poorly enforced – political and social institutions shirking their duties. Third, the reminder of the legality of importing GM maize as food/feed called into question the coordination among different institutions, which either failed to cooperate or neglected to think broadly about policies and practices that could result in GM maize pollinating Mexican varieties of maize.

Lastly, the narrative subtly transformed the 'surprise aspect' of the research into a critique of scientific and regulatory institutions. The *NY Times* quoted Chapela commenting on studies by the Mexican government that confirmed his then unpublished findings: "These are the extremes, the places where you would really not expect to find contamination... The only reason they found it there is because that's the only place they've looked" (Yoon 2001). Chapela, using the word 'contamination' which did not appear in the *Nature* article, implied that transgenic DNA had likely introgressed into maize throughout Mexico, but that no one had yet bothered to conduct the research. In other words, the scientific and regulatory communities were caught by surprise because

they had ignored a straightforward question – in hindsight, a rather obvious question with a predictable answer. Thus, the reality that Chapela and Quist’s findings attracted the attention of both the scientific community and the popular media tarnished the credibility of institutions that *should have previously considered and measured transgenic ‘contamination.’*

Gene ecology

The fourth narrative dimension advocated for the expansion, if not the creation, of the scientific field of gene ecology. In their introduction, Quist and Chapela (2001) wrote:

The degree of genetic connectivity between industrial crops and their progenitors in landraces and wild relatives is a principal determinant of the evolutionary history of crops and agroecosystems throughout the world. Recent introductions of transgenic DNA constructs into agricultural fields provide unique markers to measure such connectivity (p. 541).

Thus, transgene flow – even if undesired – had utility as a research tool because it presented the opportunity to measure the ecology of gene flow in agricultural ecosystems. More specific to the behavior of transgenes, Quist and Chapela advocated for long-term studies that could “establish whether, or for how long, the integrity of the transgenic construct is retained, and whether the relatively low abundance of transgene introgression detected in the 2000 harvest cycle in Oaxaca will increase, decrease, or remain stable over time” (p. 542). These ideas emerged from the iPCR findings of multiple contexts of insertion of CaMV, and also from the reality that understanding evolutionary and ecological process would require measurement over time. While one study certainly could not establish or define the boundaries of an emerging scientific field, the narrative surrounding the Quist and Chapela study presented an argument for integrating molecular

biology and ecology (in the choice of both questions and methods) to understand the significance of transgenes in non-laboratory environments.

Audience construction

Despite the dissimilarities with the promotional events described in the previous chapter, the Chapela Maize case embodied aspects of science as performance. The narrative themes discussed above demonstrate how both scientific and popular media can combine to create an extensive discourse of the significance and implications of technical research that goes well-beyond the relatively careful language of reporting research results. This section explores the construction of audiences for that narrative, organized historically to emphasize the unfolding aspect of audience construction in this case.

An early attempt at audience construction around the issue of transgenic contamination of Mexican maize predated Quist and Chapela's research. In 1999, Greenpeace launched a campaign to convince the Mexican government to stop importing maize from the U.S. to avoid "polluting Mexican corn varieties." The Mexican Minister for Agriculture responded by explaining that the imported maize would not be planted and that even if it were, it would fail to grow because the maize was treated with a fungicide that made the seed sterile. In March 1999 Greenpeace took U.S. maize samples from boats docked at Veracruz, showed that they included *Bt* maize made by Novartis, and even grew some of the seeds. Although they took the harvested transgenic maize to the Mexican Ministry of Agriculture, the government took no action (Rowell 2003, 150). While Greenpeace failed to *activate* their audience, at least in any official capacity, their

actions may have prepared the Mexican government for the later performance by Quist and Chapela.

The very first move to construct an audience for the research involved the ‘deconstruction’ of an audience. After Quist saw the surprising results of running the PCR gels the night before his workshop demonstration, he and Chapela decided to *exclude* the intended audience (UZACHI workshop participants). Quist justified this decision in terms of scientific uncertainty (running the experiment a few times in an unfamiliar lab was not scientifically convincing); in the language of performance, more rehearsals were needed before the show could go ‘on’. Quist and Chapela thought it irresponsible to construct an audience for a high-impact performance that might simply be in error.

After verifying the results in their university lab, Quist and Chapela made two major audience decisions that reflected both strategic and ethical intent. First, they approached *Nature*, a premier scientific journal, to consider publishing their results. This satisfied the multiple strategies of professional advancement (Chapela was coming up for tenure review at UC Berkeley), securing expert review through peer-review, and communicating their results to a broad, public audience (scientists all over the world read *Nature*, and it is a frequent source for science news in the popular media). Second, Chapela felt their results were so significant that he approached a number of contacts within the Mexican government to share preliminary results. Quist and Chapela recognized that releasing their findings directly to the *media* would doom their chances for a scientific publication and also discredit their study in the eyes of the scientific community, but they felt an ethical obligation to give Mexican agencies the opportunity to take scientific and political action as quickly as possible (Quist, 10/10/03).

Constructing this limited audience thus contributed to the power of their science to affect policy and practice, but also endangered the legitimacy of their intended publication.

In fact, the Mexican Biosafety Commissioner, despite being informed by Chapela of *Nature*'s strict policy prohibiting the announcement of scientific results in advance of journal publication, participated in a press conference in early September 2001, at which he revealed Quist and Chapela's findings. At the press conference, Greenpeace learned of the study and was unwilling to wait until after the *Nature* publication to take action. Having learned of Greenpeace's intention to make the 'transgenic contamination of Mexican maize' a matter of public record, *Nature*'s news division published an article on 27 September 2001 entitled, "Transgenic corn found growing in Mexico" (Dalton 2001), coinciding with a Greenpeace press release the same day entitled, "Serious genetic contamination revealed in Mexican maize" (Greenpeace 2001b). The *Nature* article explained:

The disclosure of scattered plots of transgenic corn in the states of Oaxaca and Puebla was made by a government official earlier this month. A research team at the University of California at Berkeley, which is preparing work on the topic for publication, has subsequently accused the official of breaching confidentiality by his disclosure...A native of Mexico, Chapela confidentially shared preliminary results of his research earlier this year with Mexican government officials. The officials then set up a research team to conduct similar studies. On 4 September, at a subcommittee meeting of an international food-safety organization, the Codex Alimentarius Commission, Chapela's discovery was revealed publicly by Fernando Ortiz Monasterio, director of Mexico's Biosafety commission. Within days, the information had reached the Mexican Congress and the press. Chapela says he had told Ortiz [Monasterio] and other Mexican officials that he was planning to publish his research, and that public disclosure would undermine this. He adds that Ortiz [Monasterio]'s "breach of confidentiality" will "degrade the quality of information" his team was compiling. Ortiz [Monasterio] denies breaching confidentiality, but acknowledges that he did reveal Chapela's research results in a public forum (Dalton 2001).

The *New York Times* followed with its own extensive story on 2 October 2001, nearly two months in advance of Quist and Chapela's official research publication:

Mexico's Ministry of the Environment and Natural Resources made the announcement on Sept. 18 that contaminated corn had been found in 15 different localities. The announcement credited Dr. Chapela with the initial discovery but described only the results from government-led research. Neither Dr. Chapela's team nor the Mexican teams' work has yet been published (Yoon 2001).

This odd series of media stories, quite beyond the control of Quist and Chapela, served to create a broad audience primed for the peer-reviewed *Nature* paper. Far from torpedoing the research out of the scientific literature, Ortiz Monasterio's actions increased the value of Quist and Chapela's research to *Nature* as a scientific finding with global scientific and political appeal. On the other hand, Chapela's decision to sidestep the conventional scientific practice of seeking peer-review before announcing results to anyone outside the relevant scientific community certainly raised the ire of some of his colleagues. Thus, by the time Quist and Chapela's publication appeared in *Nature* on 29 November 2001, a significant audience had already been assembled: agencies and personnel in the Mexican government, activist NGOs, readers from the *New York Times*, and readers of *Nature* who had already been exposed to Dalton's news article. The combination of the strong narrative themes outlined in the previous section, with this assemblage of political and scientific actors as audience, created a contrarian thrust powerful enough to attract significant attention from promotional science: sparks of scientific dissent made visible.

6.2 Losey Monarch

Research question and methodology

Much as Quist and Chapela's controversial research question about transgenic contamination of Mexican maize arose through material and experiential circumstances, John Losey's question about the potential for *Bt* corn to harm monarch butterflies occurred during fieldwork for a different study. Losey, a newly appointed assistant

professor of entomology at Cornell University, had received a grant from the U.S. Department of Agriculture's Agricultural Research Service to study the emergence of pest resistance to *Bt* corn. Losey hypothesized that weeds in and around cornfields could host the European corn borer – the target pest of the *Bt* toxin – serving as a kind of ecological refuge to delay the evolution of resistance. This was a critical question as concerns had been raised about the probability of *Bt* resistance developing rapidly in *Bt* crop fields that expressed the *Bt* toxin throughout all plant parts and continually throughout the growing season (as opposed to the spatially and temporally limited application of conventional pesticides).

In the process of conducting that research, Losey noticed the large amount of milkweed in and around the cornfields. "You really can't miss it, it's a large abundant plant," Losey says. Milkweed is a wholly unsatisfactory food for the European corn borer. It is, however, the only source of nutrition for the monarch butterfly larvae. Because of the milkweed's close proximity to *Bt* corn, and hence its pollen, monarch larvae could eat pollen that had fallen on milkweed. "I wanted to know if the butterfly larvae would eat the pollen, and if they did, would *Bt* pollen harm them," Losey says. "It was a first step in the research. Because the *Bt* toxin affects lepidopterans [butterflies and moths] to widely varying degrees, it wasn't altogether clear whether the monarch larvae would suffer any ill effects at all" (PIFB 2002, 7).

Losey focused on pollen because of its visibility on milkweed leaves, and its property of being the only mobile part of the *Bt* corn plant. Losey's research project thus transformed into one investigating non-target effects of *Bt* corn. The eventual publication in *Nature* began:

Although plants transformed with genetic material from the bacterium *Bacillus thuringiensis* (*Bt*) are generally thought to have negligible impact on non-target organisms, *Bt* corn plants might represent a risk because most hybrids express the *Bt* toxin in pollen, and corn pollen is dispersed over at least 60 metres by wind. Corn pollen is deposited on other plants near corn fields and can be ingested by the non-target organisms that consume these plants (Losey, Rayor et al. 1999).

While Losey and his co-authors framed their concern about non-target effects broadly, their methods focused on the narrow question of the toxicity of *Bt* corn pollen to monarch butterfly larvae.

Losey's team chose the laboratory experiment as their method of investigation, a decision for which they would later receive harsh criticism. They collected corn pollen from a *Bt* corn developed by Novartis (N4640) and from a non-GM corn hybrid. In the laboratory, they placed five three-day-old monarch larvae onto each of three treatments of milkweed leaves. The study included an experimental treatment and two controls: sprinkling the milkweed leaves with *Bt* corn pollen; sprinkling the milkweed leaves with pollen from non-GM corn; and plain milkweed leaves. "Pollen density was set to visually match densities on milkweed leaves collected from corn fields...Milkweed leaf consumption, monarch larval survival and final larval weight were recorded over four days" (Losey, Rayor et al. 1999). This methodological approach permitted Losey's team to separate out the effects of pollen dusting from the effects of *Bt*, an important strategy given the artificial circumstances of feedings conducted in the laboratory.

Factual claims

For reasons that will become clearer in Part Two, it is critical to separate out the factual claims made by Losey et al. and the hypotheses generated by the study.

The claims present in the article specifically referred to the laboratory experiment.

Losey et al. (1999) wrote:

In a laboratory assay we found that larvae of the monarch butterfly, *Danaus plexippus*, reared on milkweed leaves dusted with pollen from *Bt* corn, ate less, grew more slowly and suffered higher mortality than larvae reared on leaves dusted with untransformed corn pollen or on leaves without pollen.

Regarding feeding behavior, the article presented a graph (“Figure 1b”) that showed cumulative leaf consumption per larvae during the four-day study. Monarch larvae ate 30% less leaf area when non-GM pollen was present, compared to the bare control, which the authors hypothesize as “a gustatory response of this highly specific herbivore to the presence of a ‘non-host’ stimulus.” However, this hypothesized response did not explain the tremendous difference between the *Bt* and non-GM pollen treatments – monarch larvae ate nearly 50% less leaf area when sprinkled with *Bt* pollen compared to non-GM pollen. Growth rates of the larvae (measured by weight) roughly correlated with feeding behavior. While Losey et al. did not claim to understand the biological mechanism (a question outside the domain of their experiment), they clearly claimed that *some property* of *Bt* corn pollen *could* interfere with the normal feeding behavior and growth of monarch larvae.

More poignantly, Losey et al. (1999) claimed that *Bt* corn pollen *could* kill monarch butterfly larvae. Only 56% of larvae feeding on leaves sprinkled with *Bt* corn pollen survived the four day study. “Because there was no mortality on leaves dusted with untransformed pollen, all of the mortality on leaves dusted with *Bt* pollen seems to be due to the effects of the *Bt* toxin.” Figure 1a made the point clearly in a bar graph with gradually diminishing numbers of live monarch larvae in the experimental treatments, while both controls lacked any mortality. Not only did the mortality claim take on greater significance as a ‘life or death’ question, but also it distinguished the measurement of mortality from the measurement of feeding behavior; although monarch larvae fed less on non-GM pollen-treated leaves, the difference was not enough to *kill* them – while something about the *Bt* pollen made the experimental treatment lethal.

Narrative significance – implications

I draw upon a small collection of public sources to describe the narrative significance and implications of Losey et al.'s (1999) publication: the *Nature* article itself, published in “Scientific Correspondence,” a forum for short research articles peer-reviewed by two outside reviewers; a *Washington Post* article (Weiss 1999a) and a front-page *New York Times* article (Yoon 1999a), both published the same day as the *Nature* publication; and a Cornell press release (Cornell News Service 1999a).

Threatened monarch survival

While Losey et al. (1999) made claims about the impact of *Bt* corn pollen on monarchs in the laboratory conditions they created for the experiment, they made no hard claims about the effects of *Bt* corn on monarch survival in agroecosystems. All media sources listed above made this distinction adequately,¹⁰⁶ but the emerging narrative certainly pointed to a strong likelihood of *Bt* corn threatening monarch survival. As pointed out frequently, this increased the salience of this research dramatically, given the monarch's cultural status in the U.S. “‘It's sort of the Bambi of the insect world,’ said Marlin Rice, a professor of entomology at Iowa State University in Ames. ‘It's big and gawdy and gets a lot of good press. And you've got school kids all across the country raising them in jars’” (Weiss 1999a). On a more scientific level, Rayor, one of Losey's co-authors, described monarchs as “a flagship species for conservation” (Cornell News Service 1999a). The status of monarch butterflies, as charismatic mini-fauna, raised the

¹⁰⁶ For example the *Washington Post*'s headline was “‘Biotech vs. ‘Bambi’ Of Insects? Gene-Altered Corn May [emphasis mine] Kill Monarchs” (Weiss 1999a).

stakes of the question of whether Losey et al.'s findings would transfer from the laboratory to the field.

The sources presented both evidence and expert-derived opinion to suggest that the observed experimental mortality and morbidity signaled actual environmental conditions. Losey et al. (1999) cited multiple scientific publications to argue that a) monarch larvae feed exclusively on milkweed leaves, b) milkweed occurs frequently around the edges of cornfields, c) corn pollen is released for eight to ten days during the time when monarch larvae feed, and d) half of the summer monarch population emerges from the Midwestern 'corn belt,' suggesting "that a substantial portion of available milkweeds may be within range of corn pollen deposition." The *NY Times* reported: "How much milkweed is close enough to corn fields to be at risk of receiving a dusting of pollen is unknown. But as Dr. Marlin Rice, entomologist at Iowa State University put it, in many heavily farmed states, 'if you're a monarch, odds are you're going to be close to a cornfield'" (Yoon 1999a). Finally, anticipating the objection to extrapolating results from a laboratory-based study, the *Washington Post* reported: "although the work was confined to a laboratory, Rayor said, ongoing field experiments by scientists in Iowa are generating similar results" (Weiss 1999a). Thus, although the laboratory study only proved the *potential* for harm to monarchs, the scientific and media discourse immediately began to paint a picture of a strong likelihood of *Bt* corn having a significant impact on North American monarch populations.

Technical safety

The narrative theme of technical safety expanded beyond the specific question of the potential for *Bt* corn to harm monarch butterflies. The *NY Times* reported:

The *Bt* toxin itself is already known to be lethal to many butterflies and moths. Researchers said this suggests that butterfly or moth species other than the monarch could be affected by the transgenic plant, particularly those that live on plants like milkweeds that are often found in and around corn fields and could be dusted by *Bt* corn pollen. But researchers note that the effect of *Bt* corn pollen on populations of wild insects is unknown (Yoon 1999a).

In other words, Losey's monarch study was not so much new information as a reminder that the large-scale deployment of *Bt* toxin in GM crops *could* harm populations of wild insects. The *Washington Post* quoted David Andow, a University of Minnesota insect ecologist, who broadened the significance of Losey's research to the international level: "In Europe...where the landscape is even more finely divided and where endangered species are closely associated with agricultural landscapes, it will be even more important to look at the relationship between native butterfly species and *Bt* corn" (Weiss 1999a). Although framed with uncertainty, the narrative challenged the technical safety of *Bt* crops by pointing to pathways of unintended environmental harm with serious consequences. The *Washington Post* also contextualized the research by reminding readers of the controversy over insects developing resistance to *Bt* crops – an agricultural issue with environmental links, and also the motive for Losey's original USDA research: "*Bt* corn already is under fire because of concerns that widespread planting may speed the development of pests resistant to *Bt* sprays, which are harmless to people and cherished by organic farmers." Connecting environmental concerns (undermining organic farming), agricultural concerns (fostering resistance so as to make *Bt* sprays and crops useless), and

ecological concerns (harming non-target pests) challenged the view of GM crops as technically safe.

Increasing the salience of this questioning of technical safety, the narrative explicitly framed both *Bt* crops as a technology with a broad and growing reach and the Losey study as a ‘first’. The Cornell press release (1999a) reported that U.S. farmers planted more than 7 million acres of *Bt* corn in 1998 and that “[a]t least 18 different *Bt*-engineered crops have been approved for field testing in the United States.” The *NY Times* mentioned that the EPA had approved *Bt* potatoes and *Bt* cotton in addition to *Bt* corn (Yoon 1999a). Losey et al. (1999) wrote:

With the amount of *Bt* corn planted in the United States projected to increase markedly over the next few years, it is imperative that we gather the data necessary to evaluate the risks associated with this new agrotechnology and to compare these risks with those posed by pesticides and other pest-control tactics.

Thus, the stakes of technical safety were high, requiring further research and an implied caution about the expanding acres planted with *Bt* crops. Secondly, the *NY Times* emphasized the novelty of Losey’s findings, writing that the research “provides the first evidence that pollen from a transgenic plant can be harmful to nonpest species. As such, the study is likely to become part of the growing debate about whether genetically engineered crops may have unforeseen effects on the environment.” Quist and Chapela (2001) also cited the Losey study as a reference to “direct effects on non-target species” in their introduction to their *Nature* publication.

Despite the predominance of a narrative that questioned the technical safety of *Bt* crops, a counter-narrative also occurred. Losey qualified his concern quite clearly: “we can’t forget that *Bt*-corn and other transgenic crops have a huge potential for reducing pesticide use and increasing yields. This study is just the first step, we need to do more

research and then objectively weigh the risks versus the benefits of this new technology” (Cornell News Service 1999a). Reminding readers of the potential benefits of GM crops and placing the decision as properly belonging in a risk-benefit framework, rhetorically complicated the narrative questioning the safety of *Bt* crops. In a similar move that contextualized the technology in a decision framework beyond questions of ecological safety, the *NY Times* quoted a farmer who favored the technology for what were most likely purely agronomic reasons:

“It’s an amazing technology,” said David Linn, a corn and soybean farmer in Correctionville, Iowa, who plants *Bt* and regular corn. “Does it kill more monarchs or not? That’s so far down on the list of things we have to decide about” (Yoon 1999a).

In other words, while scientists and butterfly aficionados might fret about whether transgenic corn harms monarch butterflies, other insects, and even organic farmers, conventional farmers and perhaps even environmentalists who care about reducing pesticides might strongly favor *Bt* technology. The *NY Times* continued, “Whatever level of threat *Bt* corn pollen turns out to pose, it is almost certainly less damaging to monarchs and insect diversity in general than the spraying of insecticides. But Obrycki [another scientist] said that in many areas of the country, farmers do not typically spray for corn borer.” The narrative thus remained complex – even in a moment of minimizing the relative harm of *Bt* corn, the promotional narrative of ‘reduced pesticide use’ confronted a technical observation that undermined the promotional argument.

Institutional trust

The narrative surrounding the Losey Monarch case paralleled the Chapela Maize narrative with regard to raising questions about the trustworthiness of institutions generally relied upon by the public to protect against technological harms. The theme

emerged in two flavors – a general concern about the shortcomings of scientific/regulatory institutions, evidenced by their lack of previous knowledge of the phenomenon raised by Losey’s team, and a specific concern about the EPA’s regulatory oversight.

At the general level, the *NY Times* raised the specter of Losey’s research as an unwelcome surprise that revealed inadequate scientific attention to potential downsides of GM crops: “Academic researchers praised the study as a first step toward understanding a previously unsuspected risk. ‘Nobody had considered this before,’ said Dr. Fred Gould, insect ecologist at North Carolina State University. ‘Should we be concerned? Yes’” (Yoon 1999a). Gould’s concern communicated a dual meaning – the risk itself (harm to monarchs) was worthy of further consideration, and the reality that no scientist, regulatory agency, or biotech company had thought to test for this ecological consequence ahead of time reflected an institutional gap in the risk assessment of GM crops. The article went on to quote Dr. Margaret Mellon, director of the agriculture and biotechnology program at the Union of Concerned Scientists: “Why is it that this study was not done before the approval of *Bt* corn? This is 20 million acres of *Bt* corn too late. This should serve as a warning that there are more unpleasant surprises ahead.” This quote framed the Losey Monarch study as an indicator of regulatory failure with potentially dire consequences, given the extensive acreage of *Bt* corn. Here, the ecological significance of the study paled in comparison to its institutional significance and predictive power of ‘unpleasant surprises’ that would emerge because of a lack of regulatory foresight.

The *Washington Post* contributed to the narrative about institutional trust by focusing criticism more precisely at the specific level of regulation of GM crops by the EPA: “Whatever the actual ecological impact, the monarch’s popularity is likely to put pressure on the already embattled agricultural biotechnology industry and on the Environmental Protection Agency (EPA), which approved the crops” (Weiss 1999a). The EPA had granted limited approval to commercialize *Bt* corn in 1995, with the unusual step of requiring the re-registration of *Bt* corn in 2001 – allowing an opportunity to review adverse experiences during the five-year approval. The EPA did not require testing on monarch butterflies/larvae, assuming that *Bt* corn could *theoretically* be toxic to them but would never pose a true threat because monarchs would not frequent corn fields (PIFB 2002, 7). The *Washington Post* continued:

Some experts criticized the EPA, which is already being sued by environmentalists for alleged shortcomings in its ecological risk assessment for *Bt* corn. The agency demands evidence from companies that new varieties will not directly harm beneficial species, such as honeybees. But it does not require tests for “second tier” organisms that might eat those species, or for species such as monarchs that simply live nearby. “All of this is adding up to show that EPA does not have a program to protect against these risks and is not in a position to detect these kinds of problems,” said Jane Rissler, a senior staff scientist at the Washington-based Union of Concerned Scientists.

Rissler’s critique mirrored her colleague (Mellon) quoted by the *NY Times* (above) – framing the true ecological significance of the Losey Monarch case as the background to a more critical public issue: the failure of regulatory institutions, specifically the EPA, to anticipate and test for adverse consequences of GM crops.¹⁰⁷

¹⁰⁷ “‘Nobody ever claimed [the *Nature* paper] was more than it was. It simply raised the possibility of risk,’ Mellon says. ‘This would not have been a front-page story in the *New York Times* if EPA had examined this carefully at any level’” (PIFB 2002, 9).

Audience construction

The construction of audience around the Losey Monarch case lacked the convoluted character of the Chapela Maize case, but similarly reflected an assemblage of diverse actors who quickly engaged the narrative themes outlined above.

Whether for strategic or ethical reasons, Losey began to create an audience in advance of the appearance of his *Nature* publication.

[O]nce his paper had been accepted for publication by *Nature*, Losey informed both Monsanto and Novartis (now Syngenta), makers of the *Bt* corn varieties, about its pending publication and sought their advice. “Our goal wasn’t to blindside anyone,” Losey says. “And, in our conversations with industry, we got some helpful suggestions about wording parts of the paper and the direction of future studies” (PIFB 2002, 9).

Pew suggested that this action to construct an industry audience in advance resulted in constructive collaboration, but Losey’s comments at the “Pulse of Scientific Freedom in the Age of the Biotech Industry” (University of California, Berkeley, 10 December 2003) communicated a different flavor:

They [Novartis and Monsanto] requested a meeting here at Cornell. They came and suggested some different wording in the paper. In fact, people from Novartis came out and said we’d really rather you not publish this. You should go back and get more data. My thought at the time was, This corn was already out on 20 million acres; the time for getting more data was 5 years ago before it was released.... If we sat on it another year longer, it would be another year it would be out there on 20 million acres with unknown effects.

This quote demonstrated the potential for one dimension of audience construction (collaboration with or notification to industry) to threaten the broader dimension of constructing a public audience. As Losey made clear, however, he went forward with the *Nature* publication.

The simultaneous publications in *Nature*, the *NY Times* (a front page article), and the *Washington Post* made a significant media splash that constructed a broad and engaged public audience. The *Washington Post* ran the headline, “Biotech vs. ‘Bambi’ Of

Insects? Gene-Altered Corn May Kill Monarchs” and led with the sentence, “A popular new variety of corn plant that has been genetically modified to resist insect pests may also be taking a toll on the monarch butterfly, one of the most beloved insects in the United States, new research suggests” (Weiss 1999a). This textual combination struck emotional chords to construct audiences well beyond *Nature*’s typical audience of scientists within educational, industrial, and regulatory organizations.

“I knew there would be a lot of interest in the results of this paper because it involved *Bt* corn and monarch butterflies, two things of interest to the general population,” Losey says. What followed, however, was a media firestorm that lasted through the summer. “I don’t think I’d ever seen that level of interest in any paper, let alone one I had published,” he says. Public advocacy and environmental groups leapt on Losey’s results as confirmation that EPA’s registration system for bioengineered crops was not stringent enough to adequately protect people and the environment. Greenpeace demonstrated in front of the U.S. Capitol dressed as monarch butterflies that collapsed as they were felled by “killer GM corn.” “Once we heard about the *Nature* paper, we called reporters and sent out press releases for days,” says Margaret Mellon of the Union of Concerned Scientists in Washington, D.C. “We worked very hard to make this a high-profile issue because without media attention we knew nothing would be done,” she says (PIFB 2002, 8).

This passage suggested that the process of audience construction had quickly escaped the control of Losey and his colleagues, similar to the fallout from Ortiz Monasterio’s press conference in the Chapela Maize case. In both cases, media institutions (including NGOs that deliberately fostered media coverage of a story) exponentially increased the reach of a scientific performance designed for a technical audience (readers of scientific journals). This phenomenon of audience expansion, combined with the narrative themes outlined above, created a context that anticipated the quantity and quality of resistance.

6.3 Puzstai Potato

Research question and methodology

The genesis of Puzstai's research question on health risks of GM crops reflected a complex institutional environment. As a senior scientist at the Rowett Research Institute, a well-respected scientific institution specializing in nutrition, Puzstai responded to a call for proposals in 1995 from the Scottish Office Agriculture, Environment and Fisheries Department for research investigating the safety of GM food. His team won the £1.6 million contract over 28 other competing research organizations (Rowell 2003, 80-1).¹⁰⁸ He credited his success to the well-respected and precise animal models that his team had developed, their collective expertise, and their extensive animal facilities (Puzstai 2003).

The scientists' primary task was to establish credible methods for the identification of possible human/animal health and environmental hazards of GM. The idea was that the methodologies that they tested would be used by the regulatory authorities in later risk assessments of GM crops. For the first time, independent studies would be undertaken to examine whether feeding GM potatoes to rats caused any harmful effects on their health, bodies or metabolism (Rowell 2003, 80).

Apart from the Rowett, two other institutions participated in the wider investigation.

Complementing Puzstai's rat feeding studies, the Scottish Crop Research Institute (SCRI) and the Durham University Biology Department focused on the effects of GM crops on target pests and beneficial insects (Rowell 2003, 81).

¹⁰⁸ This section draws heavily on the work of Andrew Rowell, an investigative journalist who compiled his work on the Puzstai affair into his book, *Don't Worry It's Safe to Eat: The True Story of GM Food, BSE and Foot and Mouth* (2003). Chapter 5 ("Hot Potato") and Chapter 6 ("The 'Star Chamber'") offer the most comprehensive narrative of the Puzstai controversy that has been published to date. Although I have examined a number of primary sources and conducted an extensive interview with Arpad Puzstai myself (Puzstai 2003), I remain deeply indebted to Rowell's careful presentation of a very thorough piece of journalism.

Much as Quist went to Mexico intending to focus on a *methodology* by designing an experiment that he and Chapela *expected* to show negative results, so also did Pusztai design his study to maximize the likelihood of demonstrating ‘substantial equivalence’ between GM and conventional potatoes. Pusztai described himself at the start of the study as a “believer in this technology” and viewed his position as a Rowett scientist to offer a “credible” and “independent” endorsement of the safety of GM food (Pusztai 2003). Stanley Ewen, one of his colleagues and the eventual co-author of the published study, commented that they “didn’t expect any differences, this was only a test to develop a test to confirm the safety of these things” (Rowell 2003, 89). With that aim, Pusztai genetically engineered a potato¹⁰⁹ to express a lectin called GNA (*Galanthus nivalis*), a protein isolated from the snowdrop plant that he had worked with since the late 1980s. “The GNA gene was selected because our studies had shown that GNA caused no harmful effects on the mammalian gastrointestinal tract’, says Pusztai” (Rowell 2003, 81). By choosing a trait with no *expected* negative health impacts, Pusztai’s research posed the question of whether the *process of genetic modification* could introduce unexpected health risks.

Much of the controversy surrounding the Pusztai study addressed the methods employed, but my interview with Pusztai (Pusztai 2003), an independent audit report conducted within the Rowett (Rowell 2003, 87-8, 97-100), and the eventually published peer-reviewed paper in the *Lancet* (Ewen and Pusztai 1999a) support the following broad

¹⁰⁹ Pusztai attempted to obtain a “bona fide” GM potato from Monsanto for the study, but was unsuccessful. He commented that agbiotech companies were frequently uncooperative in providing materials or sequences for studies unless they reserved the option to control the dissemination of results in some way. Consequently, Pusztai contracted for a small biotech firm in Cambridge, Axis Genetics, to modify the potato according to his specifications (Pusztai 2003).

description. In a logic similar to Losey's dual control groups, Pusztai's team compared groups of young rats, fed on one of three potato preparations (either raw or cooked): 1) conventional potato, 2) conventional potato with GNA added as a supplement, and 3) transgenic potato expressing GNA. Such an approach allowed the research team to control for effects of the GNA lectin and focus on the effects of genetic transformation. They conducted both short-term (10 day) and long-term (110 day) trials. They measured the rats' growth, organ development, and immune responsiveness (using Con A as one of the reagents¹¹⁰), and took histological samples of various regions of the gastrointestinal tract. According to Pusztai, the Biotechnology and Biological Sciences Research Council, the leading funding agency in the United Kingdom for bioscience research, reviewed and passed the methodology as proposed in his grant application.

Audience construction

Unlike the previous two case studies, Chapela Maize and Losey Monarch, for which I presented the construction of audience as the final piece of analysis, the Pusztai Potato case demands an earlier treatment. Like the Chapela Maize case, media treatment began *before* scientific publication of results, although it was Dr. Pusztai himself who provided the media early access to his findings (rather than indirectly, through a leak to the press). Considering the construction of audience at three points during the controversy sheds light upon subsequent analyses of the factual claims, the narrative significance, and the implications of the research.

¹¹⁰ As will be discussed later, a great deal of confusion emerged when the Rowett's press release indicated that the GM potatoes had been transformed with Con A, a known mammalian toxin, rather than the GNA lectin.

Phase one: Pusztai on television

In late 1997, Pusztai's team became concerned about preliminary results from their GM potato study. There were "unexpected and worrying changes in the size and weight of the rat's [sic] body organs. Liver and heart sizes were getting smaller, and so was the brain. There were also indications that the rats' immune systems were weakening" (Rowell 2003, 82). With approval from his supervisor, Professor James, Pusztai appeared on *Newsnight*:

"We are putting new things into food which have not been eaten before," Pusztai said on the programme. "The effects on the immune system are not easily predictable and I challenge anyone who will say the effects are predictable." "Nobody phoned me afterwards," recalls the lectin specialist. "I was happy with what he said on *Newsnight*," says James [the Director of the Rowett] (Rowell 2003, 83).

As a result of this episode, Granada Television's *World in Action* became interested in Pusztai's work, contacted the researcher, and filmed him in late June 1998. According to Pusztai, a Rowett press officer was present during most of the interview (Pusztai 2003; Rowell 2003, 84). Subsequently, *World in Action* produced a short piece (under 3 minutes) that aired on 10 August 1998 entitled, "How Safe is Genetically Modified Food?" Prior to the show, *World in Action* distributed a press release to a limited audience, embargoed until 10 August, entitled, "New Health Fears Over 'Frankenstein' Food." The press release combined with the television broadcast created a flurry of attention in the popular media (articles in the *Guardian*, *The Independent*, the *Telegraph*, *The Times* and coverage by the BBC) – effectively creating a national, and presumably international audience for the Pusztai Potato study.

As discussed in the Chapela Maize case, preempting scientific publication with popular media attention put the perceived scientific credibility and the publication

potential of the Pusztai Potato research at risk, but two motivations enabled the interviews to occur. First, Pusztai described believing that the public had a right to know of his preliminary results promptly, given their significance with regard to health concerns. At “The Pulse of Scientific Freedom in the Age of the Biotech Industry” (University of California, Berkeley, 10 December 2003), Pusztai commented that the money for his GM potato studies came from public tax dollars, and that the public therefore had a right to expect him to report back promptly any potential problems. Second, institutional motivations on behalf of the Rowett played a role. According to Pusztai, the director encouraged media attention as a lever to attract additional funding (Pusztai 2003). James, the Director of the Rowett, testified in a Parliamentary hearing in October 1998:

We therefore had an agreement that he could take part [in the *World in Action* interview] (because this is a publicly funded research programme done collaboratively). I also felt it was important to show that we were operating in the public as well as industrial and other interests, and that we were doing work on this topic. We agreed, however, that we should not release any unpublished data (1998).

James not only acknowledged the public nature of the research, but saw media exposure as a chance to solidify the position of the Rowett as an institute serving the public interest. Procedurally, James and Pusztai agreed that data would not be discussed – an arrangement to protect the assumed future scientific audience who would only endorse research that had not been published previously.

The House of Commons Science and Technology Select Committee directly challenged Pusztai on his choice of going to the media in advance of scientific publication.

[Chairperson] did you think it was appropriate to discuss on television the results that you had, before those results had been subject to peer review?

(/Dr Pusztai/) This is a debatable point, that what I disclosed were results; they were certainly not data. It was a long-standing policy of the Institute to have a sort of cautious approach to GM-related matters, and we all felt, including Professor James, that the route we had to take should be a very, very gradual and well-researched route. So, in a sense, what I expressed there was nothing surprising, considering the background of the Institute's work in that direction....

[Chair, Question] 129. If some experiments had not been done, do you think it is appropriate that an eminent scientist such as yourself should go on television and discuss sophisticated experiments when the work was not complete, knowing perhaps, as you did, the programme was going to be a controversial programme and was going to take a hard stance against genetically modified foods?

(/Dr Pusztai/) I was not sure at all in my own mind, when I did do it; I did not know what would be the final outcome of the programme; the programme had changed quite a bit, and I had exactly 150 seconds in it.

[Chair, Question] 130. And you did not know, at the time you were asked to go on the programme, it was likely to be a hostile programme?

(/Dr Pusztai/) No; no, not at all (1999).

As this testimony demonstrated, while Pusztai and James had control over *whether* to create a television audience for the Pusztai Potato study, the construction of that audience remained partially out of their control. As suggested by the question of whether the show was a “hostile programme,” the editing of the program along with other production decisions to frame the piece had significant effects on the construction of an audience.

As a final note, in contrast with the Losey Monarch case, Pusztai did not seek a direct audience with industry when he began to suspect the potential for negative social impacts from GM crops. According to Pusztai, Monsanto attempted to reach him immediately after the televised interview, but his director blocked any such conversation from taking place. This was especially significant given that Monsanto had a number of contracts with the Rowett and its consulting arm, Rowett Research Services (Pusztai 2003).

Phase two: Select and limited audiences

Soon after James suspended Pusztai from his position at the Rowett and forbade him to speak publicly about his case, James set up an audit committee, ostensibly to

‘clear’ Pusztai’s name (Rowell 2003, 87). According to Pusztai, however, the outcome of the audit would not have affected James’ decision not to renew his contract, making the audit rather insignificant in terms of its potential to reestablish Pusztai’s professional reputation (Pusztai 2003). Nevertheless, the audit committee, composed of Pusztai’s former supervisor at the Rowett (Andy Chesson), another Rowett scientist (Harry Flint, Head of Gut Microbiology and Immunology), a scientist from SCRI (Professor Davies) and the retired head of the Institute of Animal Health (Professor Bourne) (Rowell 2003, 97), represented an official audience who would then perform for larger political and scientific audiences.

Although prevented from speaking in public about his experiments or interactions with the Rowett, Pusztai was allowed to communicate privately with scientific colleagues. He shared his views, the audit report, and his written response to the report with a number of colleagues. This small, but scientifically potent, audience responded by issuing a memorandum in February 1999, signed by 30 scientists from 13 countries, that argued for the exoneration of Pusztai and the need for further work to explore the importance of his preliminary findings. The publication of this memorandum led to another flurry in the popular media (e.g. “Scientist in Frankenstein Food Alert is Proved Right,” *The Mail on Sunday* [London], 31 January 1999) (Rowell 2003, 100).

Finally, The Royal Society entered the fray by establishing itself as a proper audience for judging Pusztai’s work. On 19 February 1999, The Royal Society announced its intention to establish “an independent expert group to examine the issues related to possible toxicity and allergenicity in genetically modified plants for food use” (quoted in Rowell 2003, 103). Their main, if not single focus was to review Pusztai’s

research. They gathered what data they could (a source of additional controversy, given that no study had been published), recruited a number of external reviews, convened an expert committee, and issued a “damning verdict against Pusztai” on 18 May 1999 (Rowell 2003 103-8).

Phase three: The Lancet publication

After Pusztai’s television appearance in August 1998, Stanley Ewen began to conduct a more detailed and careful histology of the rat gut samples. This work served as the basis for a submission to *The Lancet*, a premier medical journal. Contrary to media reports which blasted the journal for its intention to publish an “unworthy” article, four of six scientific reviewers supported publishing the article on the basis of scientific merit. Despite such maligned press attention and documented pressure on Richard Horton, the editor of *The Lancet*, not to publish the research, Ewen and Pusztai’s research letter, “Effect of diets containing genetically modified potatoes expressing *Galanthus nivalis* lectin on rat small intestine,” appeared on 16 October 1999 (Rowell 2003, 114-9). The issue also included a commentary by the editor who clarified the review process of the Ewen and Pusztai submission and also criticized high-ranking scientists for improperly downplaying concerns about GM food safety (Horton 1999); a second commentary that criticized Ewen and Pusztai for being “incomplete” and methodologically weak (Kuiper, Noteborn and Peijnenburg 1999); and a research article on the binding of snowdrop lectin to human white blood cells which “supports the need for greater understanding of the possible health consequences of incorporating plant lectins into the food chain” (Fenton, Stanley et al. 1999). The significant editorial and scientific attention in this issue of *The*

Lancet, as well as a multitude of correspondence pieces published in the following issue (13 November), combined to engage a wide audience for the Pusztai Potato study.

Factual claims

According to the logic of the previous section, I present the factual claims in two installments: claims made around the *World in Action* television program, and those made as part of the published paper in *The Lancet*.¹¹¹

Pusztai on television

Prior to the *World in Action* broadcast, Granada Television's press release announced two scientific claims about the Pusztai Potato study: "that rats fed on genetically modified potatoes suffered [1] stunted growth and [2] damage to their immune systems after 100 days" (quoted by Rowell 2003, 84). These claims were widely repeated by other media sources that covered the story (e.g., Highfield 1998b).

During the television show itself, Pusztai's desire for clarity and credibility operated in tension with his agreement with James not to discuss details of his research, including not mentioning the name of the transgene inserted into the potato (GNA). Thus, while much of the 150 second interview dealt with broader issues of significance and opinion, some of the conversation put forth specific scientific claims:

Andrew Brittain [presenter for *World in Action*]: "Rats have [been] fed two different kinds of genetically modified potato, which are not on sale and have never been eaten by humans. The rats ate them for more than 100 days, the human equivalent of 10 years."

Professor Arpad Pusztai: "The immune system takes about 10 days to get in top gear. So, if we do a short-term trial, we wouldn't have seen the end result."

¹¹¹ During the second phase, limited and select audiences, no distinctive scientific claims were made by Pusztai or his colleagues.

Andrew Brittain: “Animals fed on one kind of research potato remained perfectly health[y]. But rats given the other set did show ill-effects”...
Professor Arpad Pusztai: “The effect was slight growth retardation and an effect on the immune system. One of the genetically modified potatoes, after 110 days, made the rats less responsive to immune effects” (transcript provided in Bourne, Chesson et al. 1999).

Thus, the first significant scientific claim addressed the utility of animal models for human health research. Although not stated with absolute clarity, Brittain’s explanation of a 100-day rat-feeding trial being equivalent to 10 human years implied that one could extrapolate from rats to humans in a direct fashion. Second, Pusztai echoed the ‘stunted growth’ claim of the press release by reporting “slight growth retardation.” Third, Pusztai described the immune effect in terms of a reduction in response.

The Lancet publication

Although Pusztai intended the research for publication since before his television interviews, and Ewen and Pusztai submitted an article for peer-review in December 1998, a number of factors (many related to the resistance to the Pusztai Potato study, described in Part Two) delayed formal publication until October 1999. Because this scientific publication was directed to a technical audience and came after further analysis that had not been done prior to the *World in Action* taping, the claims were much more precise than those listed above. At the same time, the article addressed only a subset of the findings emerging from Pusztai’s research – differences in organ growth/development and immune system effects were not addressed. The following claims, all stemming from the histological work on rat gut samples, were made in *The Lancet* publication entitled, “Effects of diets containing genetically modified potatoes expressing *galanthus nivalis* lectin on rat small intestine”(Ewen and Pusztai 1999a):

1. The GNA lectin had a measurable effect on some parts of the gastrointestinal (GI) tract, regardless of whether expressed from the GM potato or added as a supplement (e.g., “The presence of GNA in the diets, irrespective of whether originating from GNA-GM potatoes or from parent-potato diets supplemented with GNA, was associated with significantly greater mucosal thickness of the stomach when compared with parent-potato diets”).
2. The GNA-GM potatoes caused some measurable and statistically significant differences in the GI tract *not found* with conventional potatoes spiked with GNA or plain conventional potatoes.
 - a. “Crypt length in the jejunum of rats fed on raw GNA-GM potato diets was significantly greater than in those given parent-line or parent-line plus GNA potato diets. However, the increase in jejunal crypt length was not seen in rats fed boiled GNA-GM potatoes.”
 - b. “Rats fed boiled GNA-GM potatoes had significantly thinner caecal mucosae than rats given boiled parent potatoes, with or without GNA supplementation.”
 - c. “Intraepithelial lymphocyte counts per 48 villi were 7·6 (SD 2·7) in rats fed on boiled parent potatoes, compared with 10·3 (3·3) in rats fed boiled transgenic potatoes ($p < 0\cdot01$). With raw potato diets, the intraepithelial lymphocyte counts were again significantly different: 5·3 (2·0) and 9·3 (2·6) in parent and GM potatoes, respectively ($p < 0\cdot01$).”

- d. “Rats fed on GNA-supplemented parent potatoes had significantly shorter colonic crypt lengths than those fed on parent potatoes of GNA-GM potatoes; the reason for this finding is not clear.”
3. The transformation *process* (as a result of properties of the transgenic construct or positioning effects of the inserted construct) caused the differences claimed in #2. “We suggest that the promotion of jejunal growth was the result of the transformation of the potato [emphasis mine] with the GNA gene, since the jejunum of rats was shown to be stimulated only by GM potatoes but not by dietary GNA (table 1), in agreement with a previous study in which the dietary GNA concentration was 1000-fold higher than the one used in this study.”
 - a. “Thus, we propose that the unexpected proliferative effect was caused by either *the expression of other genes of the construct, or by some form of positioning effect in the potato genome caused by GNA gene insertion*. Because caecal thickness was similar in rats given boiled parent potatoes in the presence or absence of spiked GNA, we suggest that the decrease in caecal mucosal thickness seen in rats fed boiled GM-potato diets was the *consequence of the transfer of the GNA gene into the potato*” (emphasis mine).
 - b. “In conclusion, the stimulatory effect of GNA-GM potatoes on the stomach was mainly due to the expression of the GNA transgene in the potato. By contrast, the potent proliferative effect of raw GNA-GM potatoes on the jejunum, and the antiproliferative effect of boiled transgenic potatoes on the caecum can be attributed only partly to GNA

gene expression. Other parts of the GM construct, or the transformation, could have contributed to the overall effects.”

In summary, the research demonstrated statistically significant negative health effects of feeding rats GM potatoes, with evidence for the process of genetic modification bearing responsibility for these unintended effects.

Narrative significance – implications

Much as the Chapela Maize and Losey Monarch cases drew significant media attention, so also did the Pusztai Potato research. These media complemented the more primary sources of narrative data: the *World in Action* program in August 1998 and *The Lancet* publication in October 1999. I draw upon a limited number of sources to describe the narrative significance of the Pusztai Potato study at these two crucial moments – points at which contrarian science became available for public consumption and scrutiny.¹¹² Apart from the television program and the scientific publication, the following analysis stems from a 12 August 1998 press release from the Rowett Research Institute and news reports from the BBC News Online Network, *The Independent* and *The Guardian* about the Pusztai Potato research in August 1998 and October 1999. Three narrative themes emerged: the safety of GM food, the technical safety of the process of transformation, and institutional trust, including regulatory policy.

¹¹² Between these two events the media covered the controversy quite thoroughly. I will cover such developments in Parts Three and Four as they have more to do with resistance and response than the appearance of contrarian science. One might argue that *The Lancet* publication itself was a form of response, but I treat it as contrarian science because the intention to publish preceded the controversy over the unpublished findings announced on television.

Safety of GM food

The strongest narrative theme situated the Pusztai Potato study as a challenge to the safety of GM food in terms of its effects on human health. The presenter on *World in Action* framed the study as “trying to find out whether long-term consumption of GM foods may affect health” and emphasized its significance by referring to it as “believed to be the only one of its kind.” During the interview, after referring to the findings of stunted growth and immune suppression in rats fed the GM potatoes, Pusztai commented, “If I had the choice, I would certainly not eat it till I see at least comparable experimental evidence which we are producing for our genetically modified potatoes” (quoted in Bourne, Chesson et al. 1999). This expert opinion, offered by Pusztai, represented a powerful statement to the public about the wisdom of eating GM food. The BBC made the link between rat feeding studies and human health clear in their lead sentence: “New research shows that genetically modified (GM) food can stunt the growth of rats and damage their immune system, prompting more concerns about the effect on humans” (“Experiment fuels modified food concern” 1998).

The narrative questioning the safety of GM food was not presented uniformly, however. The Rowett’s press release declared it “premature to conclude whether or not there are data of concern to those assessing the safety of foods with transgenic lectins” (Rowett Research Institute 1998b). James, director of the Rowett, downplayed the significance of the study, saying that “the experiment was only one of many specifically concerned with the safety of potential new foods, none of which was available commercially” (Radford 1998a). If the press had publicized (or known of) Pusztai’s inability to secure a commercially available product for testing because of the lack of

industry cooperation (see footnote 109), this detail (distancing the Pusztai Potato study from relevance to current and actual food supplies) might have carried less weight.

The media surrounding *The Lancet* publication continued the food safety theme. Several weeks before the appearance of the scientific article, *The Independent* announced that the study would “reignite fears that GM foods may endanger human health” (Lean 1999). *The Guardian* quoted a Greenpeace spokesperson who emphasized how the credibility of publication in a premier medical journal made the Pusztai Potato study a powerful challenge to the safety of GM food: “Scientific concerns about the safety of GM foods are clearly real...The fact the work is of sufficient quality to be published raises questions about the safety of GM food” (Meikle 1999). The BBC framed the Pusztai Potato study as a landmark event in the consideration of GM food safety: “The scientific research that was largely responsible for sparking the intense debate in the UK over the safety of genetically-modified (GM) foods has finally been published” (“GM controversy intensifies” 1999). *The Lancet* publication itself, while certainly reaching a more narrow audience, echoed the message:

The possibility that a plant vector in common use in some GM plants can affect the mucosa of the gastrointestinal tract and exert powerful biological effects *may also apply to GM plants containing similar constructs* [emphasis mine], particularly those containing lectins, such as soya beans or any plants expressing lectin genes or transgenes (Ewen and Pusztai 1999a).

Ewen and Pusztai made it clear in their article that the significance of their research was in no way limited to GM potatoes, commercialized or experimental; their data challenged the safety of all GM food due to the widespread use of standard transformation techniques and promoters.

Technical safety of genetic modification

The previous quote from Ewen and Pusztai (1999a) pointed beyond the issue of food safety to the technical safety of the process of genetic modification. *The Lancet* publication (especially point #3, above) made it clear that the authors saw their research as challenging the safety of GM foods at a very fundamental level – a criticism enabled by the comparison between GM potatoes expressing GNA and conventional potatoes spiked with GNA. Because of the misinformation about ConA and whether Pusztai had even used GM potatoes in the first round of publicity (August 1998), the media did not produce the specific narrative about technical safety until the second round (October 1999). *The Guardian* reported that Pusztai’s “work had sought to establish whether the effects of GM materials and non-GM materials were ‘substantially equivalent’ in every respect. ‘Up to now, people have said they are the same. That is not true with GM potatoes. They are compositionally different’” (Meikle 1999). This issue of substantial equivalence went beyond the narrative of food safety to a challenge of the technology itself. *The Independent* stated: “*The Lancet* has allowed Professor Ewen and Dr. Pusztai to conclude that the damage they witnessed to the intestinal linings of rats fed GM potatoes might be due to the process of genetic modification” (Connor 1999a). The BBC quoted Dr. Vyvyan Howard, a toxico-pathologist at Liverpool University, as saying that the Pusztai Potato study “highlights the fact that this technology is unpredictable” (“GM safety research stokes new row” 1999) and later reported that “the two Aberdeen researchers believe the GM device used to carry the new gene into the potatoes may be the source of the problem” (“GM controversy intensifies” 1999). Thus, at least by the

time Pusztai and Ewen published their research, the narrative challenging the safety of basic elements of the transformation process had emerged.

Institutional trust

The Pusztai Potato research entered public discourse in the shadow of several other major European food scares (e.g. BSE, hoof and mouth). The editor of *The Lancet* wrote an extensive commentary in the same issue as the Ewen and Pusztai (1999a) publication:

[Liam] Donaldson [the Chief Medical Officer of England] and [Robert] May [the UK's chief scientific advisor] urged the UK government to develop a comprehensive research strategy into GM food technology, including study of its potential effects on health. These responses reflect an appropriately cautious approach towards the science of genetic modification. They reflect the real concern expressed by both "single-interest groups" and a wider public. These anxieties may seem odd,- even irrational, given that GM foods were introduced in the USA without any sign of consumer anxiety. Why? Because Europe now lives in a post-BSE (bovine spongiform encephalopathy) age, one in which society has learned that the epidemic of BSE was brought on by *unchecked industry-driven changes in farming practices and that the denials of risk by government and scientific authorities were worthless* [emphasis mine]. That concern is now spreading beyond the UK (Horton 1999).

The BBC reported Horton's reference to BSE as a causal agent in the public's demand for a "more open debate on GM technology" ("GM controversy intensifies" 1999). Thus, the challenges to the safety of GM food and to the technology itself (outlined above) added yet another layer of concern around the public's trust of regulatory and scientific institutions.

One aspect of this narrative suggested that scientific institutions had been asleep at the wheel – failing to properly conduct responsible safety assessments. On the *World in Action* program, Pusztai said:

We're assured that this is absolutely safe. We can eat it all the time. We must eat it all the time. There is no conceivable harm, which can come to us. But as a scientist looking at it, actively working in the field, I find that it's very, very unfair to use our fellow citizens as

guinea pigs. We have to find guinea pigs in the laboratory (quoted in Rowell 2003, 86).¹¹³

Equating the distribution of GM food to humans with a scientific feeding experiment rhetorically undermined the public's trust in science to test products before their release into the marketplace. This theme was taken up in media reports as well: "The Liberal Democrat environment spokesman, Norman Baker, said the results showed that 'we have become the guinea pigs in a gigantic experiment'" (Radford 1998a). Striking a similar chord and tone, Patrick Holden, Director of the Soil Association, wrote in a letter published in *The Independent*:

[W]e are all very much aware of the links between nutrition and our health. When the scientists who claimed to know everything about the building blocks of life, and exercised that knowledge with a total disregard for those of us who objected, are proved wrong, it is time to put up our hands and say enough is enough (Holden 1999).

Less alarmist versions of this narrative theme emerged as well. Ewen and Pusztai (1999a) introduced their article by stating:

Genetically modified (GM) plant products are becoming increasingly common in the human food-chain, yet in contrast to the general acceptance of the need for the biological testing of novel foods and feedstuffs, few studies have been carried out on the possible effects of GM products on the mammalian gut mucosa.

The Independent highlighted both the paucity of other relevant research and the importance of identifying potential conflicts of interest around such investigations:

The research is important because few papers have so far been published on the health effects of GM foods, despite the rapidity with which they have spread on to supermarket shelves. Indeed Dr Pusztai, who was not available for comment on the news, began his experiments because he could find only one previous peer-reviewed study, led by a scientist from Monsanto, the GM food giant, which had found no ill-effects (Lean 1999).

¹¹³ In hearings sponsored by a Select Committee of the House of Commons in March 1999, Pusztai was asked, "Can you just confirm that your comments about the possibility of the general public being used as guinea-pigs was a general expression and was a general concern, and was not necessarily relating to your experiments or the potatoes you used in your experiments?" He responded, "It was a general comment. Having come across what is submitted to the various Novel Foods and other committees, knowing exactly the extent of what is required, and compared it with our own experience, I thought that it was perhaps a fair comment; maybe it was not a very wise comment but it was a very fair comment, at the time" (1999).

The narrative implication of this passage suggested to readers that what little science had been done on the safety of GM food might have been inadequate, perhaps partly because of the presumed bias introduced by an interested corporate sponsor.

While a great deal of discourse challenged the public's trust in science, a counter-narrative emerged to reassure the public that 'science' had eventually responded appropriately. *The Lancet* editor wrote:

So why publish the paper? The answer lies partly in a February, 1999, statement from the UK's chief scientific adviser, Robert May. While criticising the researchers' "sweeping conclusions about the unpredictability and safety of GM foods", he pointed to the frustration that had dogged this entire debate: "Pusztai's work has never been submitted for peer review, much less published, and so the usual evaluation of confusing claim and counter-claim effectively cannot be made". This problem was underlined by our reviewers, one of whom, while arguing that the data were "flawed", also noted that, "I would like to see [this work] published in the public domain so that fellow scientists can judge for themselves... if the paper is not published, it will be claimed there is a conspiracy to suppress information"...Ewen and Pusztai's data are preliminary and non-generalisable, but at least they are now out in the open for debate, as are the results, also published in today's *Lancet*, of Brian Fenton and colleagues. Only by welcoming that debate will the standard of public conversation about science be raised. Berating critics rather than engaging them-and criticising reports of research, as the Royal Society did with the Pusztai data, before those data were reviewed and published in the proper way-will only intensify public scepticism about science and scientists (Horton 1999).

Horton thus claimed that the publication of the Pusztai Potato study should reassure the scientific community and the public that 'science was alive and well'; that *The Lancet*, at least, understood its proper role as an institution to provide space for rational presentation of results and debate.

More so than the Chapela Maize or Losey Monarch studies, the Pusztai Potato study immediately produced a narrative about policy implications, perhaps because of its more direct implications for safeguarding human health. On one extreme, the research was framed as a reminder of how the regulatory system would/could/should work:

Foods Minister Jeff Rooker said the government had no immediate plans to call a moratorium on genetically modified foods, but agreed that repeated testing of them would be desirable... Mr. Rooker said the potatoes used in the experiment were not on

sale and would not be approved for sale. “The potatoes have gone wrong because this particular potato damaged the immune system of the animals it was tested on. The fact is, that product wouldn’t have got through the regulatory process to be allowed to be marketed as a product” (“Experiment fuels modified food concern” 1998).

Rooker thus reassured the public that regulatory institutions had the capability to screen unsafe GM food before commercial release. A majority of voices, however, disputed this claim and used the Pusztai Potato study as evidence. For example:

Ian Gibson, Labour MP for Norwich North and a member of the Commons science and technology committee, said he was worried by the Rowett institute’s findings and called on the Government to act. He said ministers should consider calling a moratorium on the sale of genetically modified (GM) products while more tests were carried out (Radford 1998a)

and

A growing proportion of processed food available in Britain contains GM ingredients but there are no legal requirements for warning labels on packaging. / MP wants ban / Liberal Democrat environment spokesman Norman Baker, who disclosed last week that GM food had been removed from the menu at the House of Commons, called for a ban. “The government has been irresponsible and spineless in allowing GM foods into our diet without demanding to see definitive proof that it is safe. The only proper thing to do now is to ban GM ingredients from all foodstuffs (“Experiment fuels modified food concern” 1998).

Thus, according to some, politicians, regulators, and scientists had so badly abused public trust that a moratorium on GM crops was the only appropriate response. The Pusztai Potato study served as a beacon for the inadequacy of oversight and as scientific rationale for significant policy intervention. As Radford (1998b) wrote in *The Guardian*, “The *World In Action* broadcast was a gift for those who want a moratorium or ban on the research.”

6.4 Discussion

The metaphor of contrarian science as the first spark of scientific dissent is quite fitting. Each of the three case studies represented a fairly discreet experiment, but shone

brightly enough to attract the attention of a diversity of social actors. The research approaches differed enough from mainstream scientific inquiry and the claims were ‘hot’ enough to draw focus within scientific and political communities. These moments of contrarian science quickly became implicated in narratives of social and technological governance. The flames of controversy began to flicker even before the appearance of overt resistance.

While going against the grain of promotional science, contrarian science should not be construed as the ‘opposite’ or ‘flipside’ of promotional science. Promotional science marks a complex category in agbiotech. It is more than a Kuhnian paradigm or a Fleckian thought collective because it engages institutions and power structures that permeate the social world. A set of interconnected assumptions give it life and give it the power to organize technoscientific discourse and practice. Promotional science thus weaves together a world-view, a collective of practitioners and spokespersons, and an institutional pattern that reaches into the realms of research, governance, and commerce. I view contrarian science as equally complex, but understandable in relation to promotional science rather than as its mirror image.¹¹⁴ As an early step down the path of scientific dissent, contrarian science is less-organized ideologically, institutionally, and socially than promotional science. The spark does not attack the campfire, but carries the potential to start a new fire on different terrain.

¹¹⁴ An analogy may help to clarify this perspective. The notion of the ‘contrarian investor’ does not indicate an individual who opposes the institutional legitimacy of the market, nor who does the exact opposite of mainstream investors at all times. Instead, the contrarian investor approaches the market with a different set of assumptions about behavior, trends, value, and potential. Similarly, the contrarian scientists in Chapela Maize, Losey Monarch, and Pusztai Potato do not oppose ‘science’ or seek to counter every proposition of promotional science, but their work reflects different behaviors, trends, values, and potential.

As with all sparks, however, the question of ultimate impact depends upon how long they can retain their heat in flight, when and where they land, and their ability to transfer their potential for fire. Most of these answers must wait for Parts Two and Three, but this chapter has offered a few insights.

First, these sparks emerged from other fires. Chapela, Losey, and Pusztai had scientific credentials, extensive expert knowledge, and powerful institutional affiliations and resources. Their research may have been too marginal in numerous respects to stay within the protective fires of mainstream science, but they will always carry the signature of that origin. While each of them reached out (to varying degrees) to audiences beyond the boundaries of legitimate science, they published in major scientific journals and invoked the highly technical communication style of science.

Second, these sparks rose into atmospheres filled with powerful air currents. Media professionals, science advocates, NGOs, government officials, and corporate representatives took up these sparks and began to blow them around immediately. At times these air flows made the sparks brighter, at other times nearly snuffing them out.

Third, these sparks in flight immediately began a self-consumptive process. Launching and maintaining a spark takes tremendous energy, and these scientists have mostly left the reassuring heat of their fires of origin. Even before the advent of resistance, we can sense the wear and tear on contrarian scientists to explain their findings, control their dissemination, and insulate their credibility.

Fourth, contrarian scientists recognize at some level that their research will be controversial, but they have not yet faced the *particular forms* of resistance that challenge the credibility of their research. At some level, and for some period of time, these

scientists hope that their contrarian findings will be accepted as legitimate and alter mainstream scientific discourse accordingly. My case studies do not demonstrate this phenomenon, but maintaining awareness of the possibility of this alternate pathway (away from dissent) strengthens the significance of analyzing contrarian science as a necessary, but not sufficient, precursor of scientific dissent.

At stake are not just the scientific claims of three scientists bucking disciplinary trends and challenging institutional hierarchies, but implicit world-views that organize the social projects of agriculture, knowledge production, and technological governance. The coherencies within the world-views are as striking as the sharp conflicts between them. Concerns about gene flow into criollo varieties of maize, non-target effects of *Bt* corn, and unexpected health consequences of eating GM potatoes weave together diverse stakeholders into a complex critique of the agbiotech project. Underlying messages about the dangers of technology, the disasters of industrial agriculture, and the distortions created by corporate power lie just beneath the surface of the narratives that surround the three case studies.

Importantly, however, contrarian science emerges within the protective institutional and symbolic confines of 'science'. This contrasts with social movements or legislative actions that seek to regulate agbiotech from an explicitly political and external standpoint. As such, performances of contrarian science represent a unique challenge to the hegemony of promotional science because both contrarian and promotional science defend their legitimacy on similar grounds – rational inquiry, goals defined by public interest, claims based on data, etc. In other words, contrarian science embodies precisely

the kind of expertise that can most effectively challenge the narrative claims of promotional science.

PART TWO

RESISTANCE

Introduction

The sparks of contrarian science are hot with the potential to ignite scientific controversy. Some sparks may quickly burn out for rather benign reasons – the nugget of fuel was too small to sustain the glow (inadequate scientific significance to draw ongoing attention) or the spark landed on bare rock (inability to connect with networks of concern in the socio-political realm). The case studies I explore, however, glow brightly enough to attract substantial attention and possess trajectories that aim for the dry kindling of social debates over agbiotech. The flames of controversy are imminent.

Engaging this ‘campfire’ metaphor, the traditional (and normative) view of science envisions a protective and stable wire screen that separates the fire of science from the campers of society. The mesh catches most of the sparks (contrarian science), protecting the credibility of science as an institution that warms and heats predictably rather than burning indiscriminate holes in the social fabric. Only the occasional spark passes through or over the screen – a deserving spark, perhaps, which campers will tolerate as a small price to pay for the benefits of the fire. The wire screen is entrusted to the scientists, who repair it, modify it, and occasionally move it to a new, more relevant fire, and society wisely keeps its hands out of the fire as much as possible (Polanyi 2000 [1962]). The action behind the screen is the domain of experts, and the public’s main responsibility is to keep the fire well-fueled (with people and money). The infrequent

escapes of sparks signal potential paradigm shifts (Kuhn 1970 [1962]) and provide some excitement for the campers.

Over the last thirty years, STS has shifted our gaze into the dynamics of the burning logs (Latour 1987); the complexity of the construction and positioning of the screen-boundary (Gieryn 1999); and the frequency and significance of transactions around, across, and within the campfire (Krimsky 2003). More radical scholars have attended to how the fire was built (Noble 1977); the practice of selective fire suppression (Martin 1981; Proctor 1995); and the hot and smoky quality of everything at the campsite (Haraway 1997). Emerging from this mix are questions about the myriad ‘appropriate’ and ‘inappropriate’ ways to manage sparks of scientific dissent. Does a lack of sparks signal a well-built fire or an overly protective screen? Should we be concerned about fellow campers who stand at the ready with buckets of water to squelch hot sparks that escape? Should we, the public, take down the screen and learn more about tending the fire in order to create the heat and light that we want? In sum, what mix of expertise and democratic control should manage the sparks of scientific dissent?

These questions arise within the discourses of scientific controversies. Their answers are no less or more controversial than the technical claims championed by scientific opponents. In other words, *the struggles over how to manage scientific dissent are at least as messy as struggles over the ‘facts’ in question*. It is dark at the campfire, smoke blows in our faces, and very few of us are sitting still and singing ‘Kumbaya.’ This chapter thus has very little hope of answering the question of whether Chapela Maize, Losey Monarch, and Pusztai Potato, as cases of contrarian science, were ‘appropriately’ or ‘fairly’ treated. As an analyst with a great deal of data from supporters

and opponents, I might be able to locate some behaviors as well-within trusted scientific norms and others as reprehensible, but it is the gray area in between that shows how even these categories are constructed and contested. I therefore embark on a slightly different journey: focusing on the experience and performance of resistance to contrarian science.

Resistance¹¹⁵ is the spectrum of behaviors that work to reduce the credibility of a scientific claim and/or the person who advances that claim. This definition intentionally encompasses a great deal of action – everything from the rejection of an article through peer-review, to a denial of academic promotion, to a published critique in mainstream media, to personal intimidation around a particular line of research. As an analytical strategy, this allows me to avoid the ‘trap of judgment’ (see Chapter 2). Depending on one’s position in a scientific controversy, for example, the denial of tenure can appear either outrageous and indicative of institutional corruption, or justified according to accepted professional norms. Instead, I focus on contrarian scientists’ experiences of resistance as the subjective experience that helps to shape diverse performances of scientific dissent (Part Three).

Thus, I aim to explore the heterogeneity of performances of resistance to contrarian science. While these performances target scientific claims and individual scientists, they also play to larger audiences of experts and laypersons with two significant ramifications. First, performances of resistance communicate to others (including would-be contrarians) the ‘costs’ of conducting contrarian science in a given arena. Harsh resistance, whether justified or not, warns others to avoid provoking similar

¹¹⁵ In other contexts, ‘resistance’ connotes an underdog status, as in guerillas being the ‘resistance’ in a political conflict; this dissertation, however, engages the term to access the metaphor of physical resistance in terms of opposing a force or flow.

treatment and becoming the next target. At a more subtle level, a journal with a reputation for rejecting manuscripts based on a novel methodology discourages scholars from submitting such manuscripts, simply to avoid frustration and wasted effort. These forms of control resonate with Laura Nader's (1997) 'controlling processes' and Michel Foucault's (1995 [1977]) power and 'discipline'.

Second, the construction of audiences for these performances has major implications for how controversy plays out in public fora. Actors reference resistance during scientific controversy – either as evidence of a lack of credibility, or as evidence of a kind of subversive credibility. For example, framed one way, the denial of tenure undermines a professor's legitimacy – institutional processes have carefully considered a bid for tenure and found the credentials lacking. Other actors might frame the resistance within a narrative of corruption and intellectual suppression – *increasing the credibility of the scholar who was 'good enough' to raise the ire of powerful interests*: "If the 'evil' forces bothered to resist you, you must really have something important (threatening) to say." This theme emerges as an important strategy for contrarian scientists to respond to resistance in Part Three.

Chapters 7-9 focus on resistance in each of the three case studies of contrarian science: Chapela Maize, Losey Monarch, and Pusztai Potato. The approach respects that resistance unfolded in a particular sequence. Early modes of resistance set the stage for later interventions, and later examples of resistance take for granted a familiarity with the history of the case. This is not to suggest a neat or strict temporal relationship among contrarian science, resistance, and response – these phenomenon overlap and swirl as highlighted in the first section on the Chapela Maize case. But history does matter, and

with some exceptions, the performances of resistance take for granted a certain knowledge of the controversies by their audiences.

Chapter 7 Resisting Chapela Maize

7.1 Preface to Resistance: Transgenes and Mexican Maize in Science

In order to underscore the conceptual and temporal complexity of pathways of scientific dissent in the Chapela Maize case, I begin with a brief discussion of an exchange in the journal *Science* that occurred nearly two years *before* Quist and Chapela's (2001) publication. In February 2000, *Science* published a letter from two Mexican scientists, "Transgenic Maize in Mexico: No Need for Concern" (Martínez-Soriano and Leal-Klevezas 2000). The authors stated:

Recently, some biotechnology companies have requested authorization to plant and market transgenic maize in Mexico. Several ecological groups have raised concerns about the potential risks of introducing such plants to Mexico, where maize originated. The main concern regarding the possible effects on the native maizes and relatives has little if any scientific basis; it is more related to cultural factors rather than biological ones.

By framing the issue as a cultural concern rather than a biological concern, the authors discouraged contemporary *and* future scientific interventions into gene flow from transgenic maize to Mexican maizes and teosintes.¹¹⁶ They made three technical claims to support this view: 1) "Any transgene transferred inadvertently to native maizes can be removed from the progeny by selecting against the incorporated trait," 2) "transgenes cannot be established in a natural population of teosintes" because cultivated maizes include the trait of a stable cob, which prevents natural seed dispersion, 3) "fixation of a (trans)gene or allele in a teosinte population would be impossible if it did not confer an evolutionary advantage to the species" and thus a hypothetically transferred transgene

¹¹⁶ Teosintes are understood to be the wild ancestors of varieties of cultivated maize. Teosintes still grow wild in Mexico and occasionally interbreed with maize.

would simply die out in a teosinte population because transgenic traits would not effectively increase fitness in wild populations. Together, these claims suggested that gene flow from transgenic maize to Mexican maizes and teosintes was unlikely, manageable agronomically, and of no scientific concern. It is interesting to note that while the authors acknowledged cultural factors that *could* validate concerns about introducing transgenic maize in Mexico, the conclusion provided in the title, “no need for concern,” effectively subordinated – if not obliterated – cultural preferences with respect to scientific basis.

The following month, *Science* published two letters under the heading, “Transgenic Crops: A Cautionary Tale,” which responded to Martínez-Soriano and Leal-Klevezas (2000). One letter, submitted by two Israeli scientists (Abbo and Rubin 2000), pointed out that (trans)genes unrelated to cob stability could flow from maize to teosintes without reducing ecological fitness. The authors went on to describe gene flow between cultivated crops and wild varieties in other parts of the world, and argued that “it is imperative to ensure that such [transgenic] crops are grown only outside the range of their wild progenitors. Otherwise, the most valuable gene pools for future food supplies will be at risk.” A second letter, submitted by a collection of Mexican and American scientists (Nigh, Benbrook et al. 2000), countered a number of the other claims made by Martínez-Soriano and Leal-Klevezas (2000). The authors emphasized the “limited state of our current knowledge” and suggested that “what little evidence is available seems worrisome.” First, the authors pointed out the diversity within the teosinte species and cited published work that demonstrated gene flow from cultivated maize to teosintes varieties. Second, the authors challenged the notion that transgenes could be selected out

of a teosinte population after introgression: “Although perhaps technologically feasible, there is no practical way for farmers or breeders to select out genes for *Bt* or glyphosate resistance, for example, given the scale at which landraces are grown in Mexico.” Third, the authors noted that maize farmers actively promote diversity among their crops which would tend to spread transgenes into multiple varieties of teosintes and maizes. The letter ended with a commentary that explicitly connected scientific debate and research with politics and regulation:

We believe that the genetic and ecological risks of introducing transgenic crops into the centers of origin of agronomic crops are largely unknown. We must not get beyond the science. The effects may prove, in most cases, of little consequence, but we should not find out by default or accident. Regulatory decisions involving the introduction of transgenic plants should be based on thorough scientific research, which in the case of maize, at least, has not yet been conducted.

Presuming that these two groups of authors had no knowledge of Quist’s preliminary findings in Oaxaca, their letters held open a space for the eventual Quist and Chapela (2001) publication. If left unchallenged, the Martínez-Soriano and Leal-Klevezas (2000) letter might have predisposed *Nature*’s editors, an obvious audience to this exchange, to judge Quist and Chapela’s finding of transgenic contamination as insignificant and perhaps too unbelievable to publish. This example demonstrates how contrarian science and resistance need not appear ‘in order,’ but rather denote conceptual categories that refer to the complex landscape of technoscientific claims and counterclaims that occur in a particular context of knowledge and politics.

7.2 Negotiations with Nature about publishing

From the time that Quist and Chapela first submitted their manuscript to *Nature* in March 2001 until the publication of their “Letter to Nature” in November 2001, the peer

review and editorial process provided numerous challenges to their claims. This process, almost always hidden from public scrutiny, revealed a number of themes that would emerge again.¹¹⁷

After submitting a “Letter to Nature” of three figures and approximately 1800 words on 20 March 2001, *Nature* responded with a request that Quist and Chapela reformulate their article in the format of a “Brief Communication” (approximately 600 words), because of “enormous space constraints” (email from Rosalind Cotter, Editor of Brief Communications, to Quist, 30 March 2001). Quist and Chapela resubmitted a “reformatted and condensed version” (Quist Chronology) on 2 April. This editorial decision constrained their ability to explain and defend their claims, but also encouraged the authors because someone within *Nature* saw their work as worthy enough for review.

Round one of peer review involved two referees. The first (R1) judged the paper important and worthy of publication, offering three fairly minor suggestions for revision. The second (R2) responded with a substantive scientific criticism and a recommendation against publication:

This is an interesting study, but to me it says more about failures of internal governmental policies (e.g. containment of GM crops) than it does about scientific issues to do with the risks of GM cultivation...I am not convinced that this work is strong enough for Nature. It is an interesting journalistic piece, but I think it is premature (email from Cotter to Quist, 10 May 2001).

R2’s scientific challenge addressed the choice of controls: “Surely the appropriate control would be samples of seed from the native plant that were stored before GM maize was

¹¹⁷ The following analysis makes use of a combination of data sources: 1) a personal chronology written by David Quist, entitled, “Background information on the technical, political and academic scope of the entire response process to our initial Nature publication v414:541-543” (Quist Chronology); 2) a substantial collection of saved emails between Quist/Chapela and *Nature* from March 2001-May 2002 and associated documents, provided to me by David Quist; and 3) personal interviews with Quist and Chapela.

grown in Mexico.” Together, R1’s exuberance over the significance of the study and R2’s high degree of reluctance led *Nature* to request that Quist and Chapela revise their manuscript with R2’s criticism in mind. Subsequently *Nature* would reconsider the manuscript for publication. This segment of peer review, although delaying rapid publication, appeared constructive in creating the potential for a stronger scientific publication as an end result. In retrospect, Chapela judged this first round of peer review as “honest”, “reasonable”, and “thoughtful”, although he saw the request for a ‘proper’ historical control as a “trick question.” Because of the incredible genetic diversity of maize landraces over space and time, one might never really satisfy the challenge of obtaining an appropriate historical control (Chapela 6/24/04). Along with submitting a revised manuscript on 14 May, incorporating R1’s suggested improvements, Chapela and Quist wrote to *Nature*:

The referee makes an interesting theoretical suggestion for a useful control. It is difficult, however, to know what (s)he suggests by “native plant”. If one could be assured that the pre-GM maize was of the same landrace as used in this research, then clearly pre-GM maize samples from the same population would have been most appropriate controls for this study. However, the level of genetic heterogeneity in maize landraces, along with variable farmer seed trading practices of the region make such resolve equivocal at best, and usually impossible. It might be relevant to note that not even the most controlled breeding programs can maintain fully accountable lines over a few generations, and this would be far more difficult in the case of open-pollinated landraces (Quist Chronology).

Thus, they disputed the methodological challenge by R2 as impractical in the context of their study.

Nature sent the manuscript out for round two of peer review on 22 May. On 14 June, *Nature* informed Quist and Chapela that their revision had not successfully changed the opinion of R2 and rejected their article without opportunity for appeal. R2 stated:

The authors have made no attempt to address my fundamental criticism that the appropriate control was Mexican seed collected before GM crops had been introduced. I

just don't think the paper is anywhere near interesting enough or careful enough to merit publication in *Nature* (email from Cotter to Quist, 14 June 2001).

The institutional decision not to involve any other referees, as indicated by a prior portion of Cotter's email, placed great power (and confidence) in the judgment of a single scientist, one who had already demonstrated severe lack of enthusiasm for the work (in contrast to R1). R2 either did not read Quist and Chapela's rebuttal to the suggestion of a historical control, or found it spurious and chose not to acknowledge or respond to it. R2's lack of specific criticism beyond the issue of historical control, using adjectives such as "interesting" and "careful", suggested an unwillingness to engage with the science as presented. It is significant that in this publication decision, *Nature's* editors found R2's evaluation to be credible and thorough enough to justify the rejection of the manuscript. Although this may be common in the scientific publication process, this case illuminates the grey area between personal and professional judgment within the peer review process. Manuscript rejection comes from a place of power and credibility (a journal) with some degree of transparency, but a lack of accountability and only moderate discursive engagement.

Although Quist and Chapela disagreed with R2's demand for a historical negative control, they deemed this additional work as worthwhile in the quest to publish their paper in *Nature*. Chapela obtained historical samples from CIMMYT (see discussion below on the details of this procurement), they ran their PCR experiments on these samples, and resubmitted a revised manuscript to *Nature* on 19 July (Quist Chronology). Several weeks later, *Nature* informed them that their appeal was successful, and that their manuscript would be sent out for another round of peer review (email from Cotter to Quist, 9 August 2001).

On 9 October, two months after the decision was made for a third round of review and two weeks after *Nature* had announced the transgenic contamination of Mexican maize (Dalton 2001), *Nature*'s Deputy Biological Sciences Editor, Rory Howlett emailed Quist and Chapela:

Your manuscript has now been seen by two new, independent referees whose reports are attached. While both find your work of interest, it is clear, particularly from the comments of reviewer 4, that further information is required before the strength of the conclusions can be fully assessed. As emphasized by this reviewer, and as already evidenced by intense media interest, the work is likely to come under close scrutiny, and it is important that the paper itself should contain all of the data and methodological details needed to satisfy knowledgeable and potentially sceptical readers. For these reasons, and after consultation with the Editor, Philip Campbell, we have decided that it would be best to reformat the paper as a Letter to *Nature*, allowing you the space fully to address the points raised in the new reports.

The reviewer's criticisms were constructive and detailed, and Quist and Chapela revised their manuscript accordingly and submitted a 1967-word, two figure manuscript on 19 October, along with a detailed response to the comments by R3 and R4 (Quist Chronology). Howlett sent an email to Quist on 26 October, informing them that R4 had been "entirely satisfied with the revisions" and offering "in principle" to publish the paper.

This series of negotiations in advance of publication demonstrate the potential for resistance to be constructive as well as destructive to a piece of research. It also showcases how resistance can occur out of public view, with only the contrarian scientist(s) as audience. Finally, as scientific institutions, journals such as *Nature* bring together a potent mixture of credibility (peer reviewers, reputation) and power (publication decisions) that determine what knowledge (published manuscripts) is ultimately produced.

7.3 Intimidation by Mexican officials

Chapela shared his findings with the Mexican government in advance of the peer-reviewed publication. According to Chapela, scientists had discussed the notion of transgenic contamination in the early 1990s, but the potential problem had drifted off the radar of Mexican agencies. Chapela anticipated significant media attention to his results, and did not want Mexican officials to be blindsided. He surmised that they would be so desperate for information that they would “run scared” into the willing arms of the US Embassy and industry sources. Instead, he wished to give high-ranking scientists an opportunity to think about transgenic contamination without the pressure of media on their doorstep. He contacted Mexican scientists (e.g., Exequiel Ezcurra, Victor Villalobos, Jorge Soberón), CIMMYT, CGIAR, and even the Rockefeller Foundation.¹¹⁸ The Mexican Biosafety Committee responded by organizing an off-the-record, nearly secret meeting. Various scientists and specialists in corn genetics were invited to this informal symposium held in Cuernavaca in August 2001. Chapela’s findings were the impetus and the focus, but unspoken in the official organization of the meeting. Chapela recalled: “Eventually the moment came when they said, ‘Now we want to hear what this guy has to say.’ All I had was an overhead projector with a gel and the bands, and this is what we’re finding. And the whole thing just blew up. People were shocked” (Chapela 11/5/2004).

¹¹⁸ Chapela reported that Rockefeller never responded to his overtures. They knew in advance of his findings, but did not engage in any conversation. This silence was, itself, a form of resistance by communicating a lack of interest in the significance of the findings and/or a lack of trust in Chapela as a scientist worthy of engagement.

The dominant response at the meeting was a flurry of ideas of how to capitalize on Chapela's findings in terms of attracting new research funding. Several scientists discussed a potential project to introduce a new transgene into Mexican maize as a marker to quantify transgene flow (Chapela 11/5/2004). One scientist directly challenged Chapela's publication plans at the meeting.

Dr. Suketoshi Taba, Head of maize germplasm bank, CIMMYT

The day before attending the meeting in Cuernavaca, Chapela went to CIMMYT to secure a maize sample that could serve as a historical control (requested by *Nature's* reviewers). Without divulging the details of his findings, Chapela approached Dr. Suketoshi Taba, the head of CIMMYT's maize germplasm collection, to ask for historical seed samples that could be used to compare with transgenic seeds. Dr. Taba readily agreed and arranged for Chapela to obtain the seeds from a technician that very day. After several hours of waiting and searching for a sufficiently old sample, Chapela secured 50 seeds from the 1970s – no longer viable but appropriate for DNA extraction. He thanked Taba on his way out, and they acknowledged cordially that they would see one another in Cuernavaca the next day (Chapela 11/5/2004).

At the meeting, Chapela noted Taba's "curious and shocking" response to the presentation of his findings. Chapela recalled:

I could see that it was dawning on him what this was about. And then he said, "You know, this is not good. I don't think this should be published. We get the message. We get the message. I think there is a problem here, but we're going to address it. But I don't think it should be published. Our jobs are at stake. It is our jobs, your job, my job, it is dangerous" (Chapela 11/5/2004).

Other participants were not particularly sympathetic to Taba's position, but Chapela believed he understood the source of Taba's anxiety. In one of their PCR tests, Chapela

and Quist found unexpected contamination in the CIMMYT collection (Hodgson 2002), a finding that, if proven,¹¹⁹ could bring scandal upon CIMMYT (Chapela 11/5/2004).

Chapela and Taba shared a friendly breakfast on Thursday, talking about research around transgenics, and Chapela flew home with the seeds on Saturday.

When Chapela arrived in his UC Berkeley office on Monday morning, he was shocked to find that Taba had emailed him with a furious request for Chapela to return the seeds. Chapela recalled that the email was

really angry and threatening with lawsuits and all kinds of stuff, that I had stolen seeds from the collection, that I must return these seeds, or else I was going to be [*doesn't finish sentence*]. Incredible. He went all the way to the director of CIMMYT, to the board of directors [of CIMMYT], to the CGIAR people. It became an international thing that I was stealing seeds from CIMMYT...[It was a] huge scandal that carried on for months and months, where I am a bio-pirate for them. [When we were eating breakfast in Cuernavaca,] he didn't mention anything about the possibility that I could be stealing seeds...By Monday I had just broken every possible rule in the world (Chapela 11/5/2004).

Chapela confirmed with Quist that they had already ground the seeds, then replied to Taba by email, apologizing for being unable to return the whole seeds, but offering to send back ground seeds or extracted DNA (Chapela 11/5/2004).

Control over the raw materials of investigation (seeds) created a significant power relationship. Taba's attempt to recover the sample could have undermined that portion of Chapela's research, possibly even discouraging *Nature* from publishing the study at all. Taba's drastic change of heart – from cooperating and conversing pleasantly to accusing Chapela of stealing seeds – appeared to Chapela as a strategy to maintain ignorance in the face of potential knowledge. Taba made no overtures, for example, of wanting to conduct the research himself.

¹¹⁹ According to Chapela, CIMMYT would not give them further access to their collection (Chapela 11/5/2004).

Ortiz Monasterio meeting with Chapela

The Mexican Biosafety Commissioner, Fernando Ortiz Monasterio, publicized Chapela's results to the media (jeopardizing *Nature's* willingness to publish data by putting it into the public domain). Chapela also accused Ortiz of direct intimidation. According to Chapela, in September 2001, an aide to Ortiz Monasterio waited all day for Chapela to finish other meetings and then escorted him to a government office building in a rough part of Mexico City. Chapela recalled the scene as it unfolded on the twelfth floor:

The office space was absolutely empty...There were no computers, no phones, the door was off its hinges, there were cardboard boxes as a table. The official [Ortiz Monasterio] is there with his cell-phone beside him. We are alone in the building. His aide was sitting next to me, blocking the door...He spent an hour railing against me and saying that I was creating a really serious problem, that I was going to pay for. The development of transgenic crops was something that was going to happen in Mexico and elsewhere. He said something like I'm very happy it's going to happen, and there is only one hurdle and that hurdle is you...[I replied,] So you are going to take a revolver out now and kill me or something, what is going on? (quoted in Rowell 2003, 152).

Chapela remembered being shown around the empty offices and out the windows that looked down on dump sites, all of which increased his apprehension. Ortiz Monasterio made it known that he wished Chapela and Quist would withdraw their submitted *Nature* publication (Smith 2003, 222).

When Chapela made it clear that he had no intention of withdrawing the manuscript, Ortiz Monasterio changed tactics. According to Chapela:

After he told me how I had created the problem, he said I could be part of the solution, just like in a typical gangster movie. He proceeded to invite me to be part of a secret scientific team that was going to show the world what the reality of GM was all about. He said it was going to be made up of the best scientists in the world and you are going to be one of them, and we are going to meet in a secret place in Baja, California. And I said, "who are the other scientists?" [And he said], "Oh I have them already lined up, there are two from Monsanto and two from DuPont." And I kept saying "Well that is not the way I work, and I wasn't the problem, and the problem is out there" (quoted in Rowell 2003, 152).

Ortiz Monasterio wanted the five scientists to meet at a private resort and complete their research in just six weeks. He reassured Chapela that the work would be submitted to *Nature* – so he would still have a prestigious publication to bolster his academic career (Smith 2003, 224). Chapela remembers Ortiz Monasterio’s twisted explanation of his responsibility for biosecurity in Mexico: “I’ll tell you what biosecurity is really about, it is about securing the investment of people who have put their precious dollars into securing this [sic] technologies, so my job is to secure their investment” (quoted in Rowell 2003, 153).

The end of the meeting was most disturbing as the threats became very personal. Chapela recalled: “He brings up my family...He makes reference to him knowing my family and in ways in which he can access my family. It was very cheap. I was scared. I felt intimidated and I felt threatened for sure. Whether he meant it I don’t know, but it was very nasty” (quoted in Rowell 2003, 152). Several months later, Ortiz Monasterio acknowledged meeting Chapela, but denied threatening him in any way. He located the meeting in an office of the Ministry of Health on the fifth floor, and recalled that they had discussed “the issues of the presence of maize, the importance of publishing, that what we were doing is research, and that when we have the results from our own researchers, we will share with him” (BBC Radio 4, ‘Seeds of Trouble’, 7 January 2002, cited in Rowell 2003, p. 153).

Chapela’s experience of this interaction reveals a number of powerful modes of resistance. First, Ortiz Monasterio sat in a position of power, not as a supervisor of Chapela, but as someone who commanded political influence, controlled financial resources that could come to bear on Chapela’s science, and could access webs of state

power that might include security and intelligence operations. Second, Chapela experienced severe intimidation that connected his behavior as a scientist with his personal safety and family's well being. Whether ludicrous or not, Chapela actually wondered whether he was to be killed, or his family harmed, for his scientific claims. Third, Chapela interpreted Ortiz Monasterio's offer to work on an elite scientific team as attempted manipulation – an effort to suppress the data (and create alternate knowledge), while giving Chapela currency (both financial and professional). Fourth, Chapela experienced Ortiz Monasterio's words as undisguised in their motives and rationale. Ortiz Monasterio did not question the validity of Chapela's methods, or pretend to be on Chapela's side in his concern for transgenic contamination or independent research; instead he laid bare the economic connections between his vision for 'biosecurity', his political stance on the acceptability of GM crops, and his demand that Chapela withdraw his *Nature* manuscript. It is worthwhile to consider how effective this resistance *could have* been. Chapela could have withdrawn his *Nature* manuscript with an explanation that he was on the verge of collecting new and more significant data, joined Ortiz Monasterio's team of corporate scientists, cooperated in publishing a study downplaying the significance or existence of transgenic DNA in Mexican maize, and completely avoided the controversy over his premier publication.

Ortiz Monasterio's next tactic involved divulging Chapela's findings to groups that would publicize the contamination event. Chapela recalled:

'I had said to him', says Chapela, 'that if the information was released before it was published in *Nature* then *Nature* would think twice about publishing it'. 'He fed it directly to Greenpeace, which is a lot easier to discredit than *Nature*,' says Chapela, adding that [Ortiz] Monasterio knew that 'the media coverage would seriously threaten publication in *Nature*' (Rowell 2003, 153).

Ortiz Monasterio acknowledged sharing the results, but denied breaking confidentiality (Dalton 2001). However, by *publicizing* scientific results in a particular way and in a particular medium (stage management), Ortiz Monasterio nearly undermined the scientific credibility and power of Quist and Chapela's data. What this suggests is the distinction between public knowledge about science (via a Greenpeace press release and subsequent media attention) and institutionalized scientific knowledge. Even if knowledge (scientific findings) cannot be prevented from reaching the public, opponents can reduce the status of that knowledge. In the context of ongoing resistance (coordinated or not), weakening the institutionalized legitimacy of scientific knowledge sets up that knowledge for easier dismantling in the future. Notable as well is that Greenpeace, an organization sympathetic to Chapela's research stance, participated in this action, nearly undermining a premier publication that they would come to cite as a key reference in establishing 'genetic contamination.' (e.g., Greenpeace 2001a). Thus, resistance can look like promotion (publicity).

Fax from Villalobos

While the meeting with Ortiz cannot be documented beyond the reports of the participants, Chapela did receive a fax from another government official with a similar tone of intimidation. Victor Villalobos, an Underminister for agriculture and a colleague of Ortiz Monasterio, faxed Chapela a letter intimating that he would be held responsible for negative financial consequences stemming from his publication. Chapela was not surprised by the approach as he considered the Ministry of Agriculture to be "riddled

with conflicts of interest. There [sic] are just working as spokespeople for DuPont, Syngenta and Monsanto” (quoted in Rowell 2003, p. 153).

The threats involved a mixture of potential economic and political consequences that would affect Chapela personally and professionally. The appearance of the faxed letter in advance of publication seemed to have served no other purpose than to encourage Chapela to withdraw or denounce the manuscript. It is notable that Chapela’s Mexican origin made him more vulnerable to intimidation from Mexican officials, despite living and working at an American university. As a Mexican citizen and with a network of professional contacts and family members in Mexico, Chapela’s life could have been severely disrupted if he were to feel unwelcome or unsafe in his native country.

7.4 Challenges to Credibility

After Quist and Chapela’s (2001) *Nature* publication appeared on 29 November, attention quickly shifted from the manuscript to the credibility of the authors.

Virtual attacks by manufactured identities

On the day of Quist and Chapela’s *Nature* publication, a number of disparaging emails appeared on pro-agbiotech listserves. While these were decried as a “smear campaign” by Chapela’s allies over the next several months, the character and source of this immediate and virtual resistance was not discovered until May 2002. Jonathan Matthews, co-founder of the Norfolk Genetic Information Network, published an article in *The Ecologist* that detailed his investigation with Andy Rowell, a freelance journalist:

Chapela and Quist came under immediate attack in a furious volley of e-mails published on the AgBioView listserv. AgBioView correspondents calling themselves 'Mary Murphy' and 'Andura Smetacek' claimed Chapela and Quist's research was a product of a conspiracy with "fear-mongering activists". The conspirators' aim, apparently, was to attack "biotechnology, free-trade, intellectual property rights and other politically motivated agenda items." These claims prompted a series of further attacks from others. Prof Anthony Trewavas [a known scientist from the University of Edinburgh], for example, denounced scientists like Chapela who had "political axes to grind". Trewavas demanded Chapela be fired unless he handed over his maize samples for checking (Matthews 2002).

Rowell (2003) quoted Murphy's email, the first attack on AgBioView:

The activists will certainly run wild with the news that Mexican corn has been "contaminated" by genes from GM corn not currently available in Mexico...It should also be noted that the author of the *Nature* article, Ignacio H Chapela, is on the Board of Directors of the Pesticide Action Network North America (PANNA), an activist group...[He is] not exactly what you'd call an unbiased writer" (p. 155).

Rowell then described Smetacek's posting, which led the AgBioView bulletin following Murphy's email, appearing under the headline "Ignatio [sic] Chapela – activist FIRST, scientist second":

It read: "Chapela, while a scientist of one sort, is clearly first and foremost an activist". "Searching among the discussion groups of the hard-core anti-globalization and anti-technology activists Chapela's references and missives are but a mouse click away." Smetacek argued that the article was "not a peer-reviewed research article subject to independent analysis". Her email included detailed information on the author and tried to undermine his credibility. "A good question to ask of Chapela would be how many weeks or months in advance did he begin to coordinate the release of his 'report' with these fear-mongering activists [Greenpeace, Friends of the Earth]? Or more likely, did he start earlier and work with them to design his research for this effect?" (p. 155).¹²⁰

In his article, Matthews first traced Smetacek¹²¹ (through earlier posts) to the Center for Food and Agricultural Research (CFFAR) which was ostensibly a "public policy and research coalition," but appeared not to exist beyond its website.¹²² According to

¹²⁰ *The Guardian* reported: "The next day, another email from 'Smetacek' asked 'how much money does Chapela take in speaking fees, travel reimbursements and other donations... for his help in misleading fear-based marketing campaigns?'" (Monbiot 2002).

¹²¹ "Smetacek has, on different occasions, given her address as 'London' and 'New York'. But the electoral rolls, telephone directories and credit card records in both London and the entire US reveal no 'Andura Smetacek'" (Monbiot 2002).

¹²² Rowell (2003) explained that the CFFAR website was suspended soon after the scandalous story broke, but that the original page remained available on archive sites (p. 158-9).

Matthews, the purpose of the CFFAR.org website was “to associate biotech industry opponents with terrorism. This mission is facilitated by fabricated claims.” Matthews then connected CFFAR.org with the Bivings Group,¹²³ which had developed “Internet advocacy” campaigns since 1996 for corporate clients, including Monsanto. An article in *The Guardian* added:

The Bivings Group specialises in internet lobbying. An article on its website, entitled *Viral Marketing: How to Infect the World*, warns that “*there are some campaigns where it would be undesirable or even disastrous to let the audience know that your organisation is directly involved...* it simply is not an intelligent PR move. In cases such as this, it is important to first ‘listen’ to what is being said online... Once you are plugged into this world, it is possible to make postings to these outlets that present your position as an uninvolved third party... Perhaps the greatest advantage of viral marketing is that your message is placed into a context where it is more likely to be considered seriously.” [italicized section later eliminated by Bivings¹²⁴] (Monbiot 2002).

Matthews went on to write:

Bivings designs and runs Monsanto’s websites and Theodorou [see footnote 123] is believed to have been part of Bivings’ Monsanto team. Mary Murphy would also seem to connect to Bivings. Or so it would seem from the evidence of a fake Associated Press article on the bulletin board of the foxbghsuit.com website. It was posted by “Mary Murphy (bw6.bivwood.com)”.¹²⁵ Between them Smetacek and Murphy have had 60 or more attacks published, often very prominently, by Prakash on the AgBioWorld listserv (Matthews 2002).

The Guardian contacted The Bivings Group about the two identities; Bivings said it had “no knowledge of them” (Monbiot 2002). Monbiot also wrote to Mary Murphy to ask whether Mary Murphy was her real name:

¹²³ Matthews (2002) reported that “THEODOROV, MANUEL” registered the CFFAR.org domain, and that “emmanuel theodorou” signed a pro-agbiotech petition launched by AgBioView’s list editor, C.S. Prakash. On the petition, Theodorou listed, “POSITION: director of associations. ORGANIZATION: bivings woodell, Inc. DEPARTMENT: advocacy and outreach.”

¹²⁴ Rowell (2003) reported that Bivings changed their on-line version of this passage, eliminating the italicized part, after the story broke in the UK Press. Bivings also reduced their 15 client references to Monsanto websites down to one (Monsanto.com) (p. 158).

¹²⁵ “‘Mary Murphy’ uses a hotmail account for posting messages to AgBioWorld. But a message satirising the opponents of biotech, sent by ‘Mary Murphy’ from the same hotmail account to another server two years ago, contains the identification bw6.bivwood.com. Bivwood.com is the property of Bivings Woodell, which is part of the Bivings Group” (Monbiot 2002).

[S]he replied that she had “no ties to industry”. But she refused to answer my questions on the grounds that “I can see by your articles that you made your mind up long ago about biotech”. The interesting thing about this response is that my message to her did not mention biotechnology. I told her only that I was researching an article about internet lobbying (Monbiot 2002).

Rowell (2003) reported:

BBC *Newsnight* then took up the story. A spokesperson for Bivings admitted to a researcher from *Newsnight* that “one email did come from someone ‘working for Bivings’ or ‘clients using our services’”. But once again they denied an orchestrated covert campaign. Bivings later argued that they had “never made any statements to this effect”, saying that BBC *Newsnight* had been “wrong” (p. 157).

Regardless of the degree of coordination of the campaign, the discourse emerging online to discredit Chapela and the discovery of the questionable identities of two of the key contributors to that conversation suggested an important pattern of resistance with central themes of credibility and power. Importantly, this performance remained accessible to scientist and laypersons alike, with significant impacts on how the technical controversy was understood.

First, the initial move of discreditation relied upon an attack on the *character* of the scientists rather than the scientific research (despite AgBioWorld serving as a forum for scientists who would presumably understand a higher degree of technical analysis than a more public forum). The accusations of being an “activist” and associating closely with activist groups implied that such character qualities and affiliations polluted their scientific claims.

Second, the forum for accusations (the audience) allowed rumor (e.g. that the Quist and Chapela article was not peer reviewed) and speculation (e.g. that Chapela had coordinated his research with activist NGOs). AgBioWorld served as an incubator for the development of an intense campaign, with enough momentum to eventually transform

into a stronger and more technical critique that could reach and influence a wider audience.

Third, the disputed links between industrial interests and these ‘fake persuaders’ at least suggested the potential for enormous disconnect between the face of resistance (personal emails) and the sources of power that support those faces (PR and biotech firms). In performance terms, the actors on stage may enjoy hidden (and potentially coordinated) support that makes them harder to oppose. Simultaneously, powerful interests that might be excluded from a public stage can participate through covert representation in controversy.¹²⁶ Such covert campaigns (or lack of transparency) render the resistance extremely slippery to oppose – unless they are uncovered, in which case the scandalous discovery operates to discredit the resisters and make even broader claims about the landscape of resistance.

Agbioview sign-on letter

The virtual attacks described above fed into more formalized and public performances. AgBioView issued a press release on 19 December 2001, began circulating a petition for scientists to sign in February 2002, and issued a second press release on 1 March publicizing the results of that petition effort.

¹²⁶ An exchange over letters to the editor in the *San Francisco Chronicle* regarding the Chapela Maize controversy suggested such a situation. The first letter, criticizing an article that showed some sympathy to Chapela, was signed by “Mike Lassner, Foster City” (“Reader Finds Column Misleading,” 19 May 2002). A follow-up letter (“Writer’s Motive Questioned,” 26 May 2002) uncovered a potential corporate connection with the first letter: “On a whim, I decided to use an Internet search engine to see what affiliations (if any) the letter author had, and it quickly indicated that he has a Monsanto e-mail address. Immediately I began to wonder: Did Lassner have honest concerns that motivated his letter, or is his response merely another example of Monsanto’s shady public relations practices? The letter is written as if it were from an individual, yet the affiliation suggests that it is more likely a piece of corporate propaganda.”

The first press release, “Scientists Say Mexican Biodiversity is Safe: Concerns About Cross-Pollination Unfounded,” declared that “scientists around the world are reaffirming that Mexico’s biological heritage is safe and that biotechnology will actually protect biodiversity, not harm it” (AgBioWorld Foundation 2001). The text made the following arguments:

1. “The claims of cross-pollination have not been verified and have been called into question by scientific experts.” This claim engaged classic rhetoric that might apply to any science (verification and doubt by experts). Here it was somewhat disingenuous given that the Mexican government had also previously announced separate data that did confirm the presence of transgenes in maize landraces (e.g., Dalton 2001).
2. If transgenic DNA has crossed into Mexican maize, it would be short-lived if it were undesirable: “if an undesirable gene is transferred into certain plants, seed from those plants will not be planted the following year and will be eliminated from the gene pool... unless farmers select them for their increased productivity...[which] would result in improving the native varieties”. This claim echoed the resistance that appeared years before (discussed in section 7.1), although because it appeared after the Quist and Chapela paper, it implied that transgene fragments (and promoters and markers) would, by themselves, be subject to farmer selection (or that only fully intact genes, which were not the subject of Quist and Chapela’s inquiry, were worthy of attention).
3. The worry over a pristine status for Mexican maize is misplaced: “corn itself is a wholly un-natural plant created by thousands of years of selective breeding by

farmers...Gene flow between commercial and native varieties is a natural process that has been occurring for many decades”. This claim engaged the rhetoric of science and the trope of the ‘natural’. The audience must assume that a) transgenes will behave no differently than conventionally introduced genes and b) the “many decades” of gene flow had only positive or neutral effects.

4. The actions of activist organizations have overblown the significance of Quist and Chapela’s article: “Organizations with vested interests and hidden agendas have used these tenuous claims and a campaign of hysteria to discredit modern biotechnology”. This claim resisted the *performance* of Chapela Maize, as extended by other actors, rather than the scientific results or even the scientific credibility of the authors.
5. The theoretical threat to biodiversity pales in comparison to the predictable threat of restrictive regulation: “‘The biggest threat to Mexico is not out-crossing from biotech crops, but activism that prevents farmers from adopting more productive and environmentally beneficial agronomic practices,’ said Dr. Prakash [head of Agbioview]. ‘That’s the only real damage this whole scenario will cause’”. This contextualized the research question in assumptions of promotional science. By shifting attention from biodiversity to agronomy, this claim explicitly argued for a focus away from ‘scientific issues’ and toward political and economic issues. This would have seemed ironic for a forum primarily used by scientists, but strategically challenged not just Chapela Maize, but any such investigations with the potential to increase the regulatory and political burden of extending biotechnology to farmers.

Within two months, AgBioView began circulating an online sign-on letter entitled, “Joint Statement in Support of Scientific Discourse in Mexican GM Maize Scandal” (AgBioWorld Foundation 2002a). In his online introduction (dated 24 February 2002), Prakash referred to this letter as a “counter-statement” in response to a prior sign-on letter organized by NGOs to support Quist and Chapela (Food First 2002). The AgBioView “Joint Statement” made the following claims:

1. Transgene flow to Mexican maize is not a concern: “the kind of gene flow alleged in the *Nature* paper is both inevitable and welcome. It is inevitable because of the nature of maize, and it is welcome as demonstrated by the standard practices landrace custodians have used to improve their varieties for thousands of years.” More strongly worded than the prior press release, this claim framed Chapela Maize as a piece of irrelevant science and echoed the promotional assumption of continuity between traditional breeding and biotechnology.
2. Quist and Chapela’s results are doubtful: phrases such as “The research supposedly demonstrated...presumably through cross pollination...[T]he key research method employed is highly prone to false positives, and the *Nature* paper failed to use standard techniques to ensure accuracy and confirm results,” and references to critiques of Quist and Chapela’s methodology that were submitted to *Nature* and published in *Transgenic Research*.
3. The activist discourse purporting to defend Quist and Chapela from unjust attack is actually a hindrance to the proper progress of science:

It must be stated clearly and unequivocally: scientists have a fundamental ethical obligation to rigorously examine the results and methodology of reported research. This

is in fact how science corrects mistakes and ever more closely approximates truth and understanding. Far from being “mudslinging” or “intimidation,” all scientists worthy of the name understand that relentless double-checking and independent third party evaluations are the cornerstones of the scientific process. Such relentless criticism and re-examination is perhaps most important when it leads in directions that may conflict with a point of view driven by politics or activism, rather than science. We the undersigned scientists declare our support for appropriate and necessary scientific discourse and debate, especially in areas marked by widespread misunderstanding and misrepresentation, such as agricultural biotechnology.

Countering the NGO “Joint Statement,” AgBioView’s letter explained the intense pattern of scrutiny of Chapela Maize not as a politicized campaign masking as science, but as science trying to break through a politicized campaign. The letter countered forces that sought to insulate Quist and Chapela from attack and to de-legitimize the scientific challenges. What was noteworthy, perhaps, was that the activists would have undoubtedly agreed with the last sentence, which also informed the title. AgBioView thus framed its resistance in language that appealed broadly. What the activists would dispute, however, was what signified “appropriate and necessary scientific discourse and debate” and how agbiotech was misunderstood and misrepresented.

On 1 March, the AgBioWorld Foundation announced “that nearly one hundred prominent scientists have signed a petition calling for greater scrutiny of [Quist and Chapela’s article]” (AgBioWorld Foundation 2002b). This description of the sign-on letter represented a stretch from the original text: the petition defended the critiques that had already been produced, but it did not call for additional scrutiny. The remainder of the text mostly echoed the previous press release and sign-on letter, with one exception: “Despite the inadequacies and misrepresentations of this particular study, gene flow from biotechnology-improved corn, as with all corn, is most likely occurring at some frequency and will certainly be demonstrated and accurately characterized through further studies” (AgBioWorld Foundation 2002b). While the earlier texts downplayed the

significance of Quist and Chapela's "contamination" claim, they did not go so far as to support the primary claim. How does one explain a discreditation campaign that culminated in the acceptance of the veracity of the targeted science? From a narrowly scientific viewpoint, such a rhetorical strategy appeared to undermine the entire project! Two other perspectives, however, offer some insight. First, from a strategic point of view, Prakash and his colleagues may have truly believed that the discovery of transgenes in Mexican maize was inevitable – there was no point in countering what would surely become a 'fact'. But the process of broadly de-legitimizing Quist and Chapela as scientists allowed the dismissal of the second claim without much public fanfare. Second, viewing the Chapela Maize case as a performance shows that the interests of promotional science need not resist particular facts, but protect the broader political and scientific landscape for the continued research and development of biotechnology. Quist and Chapela's results can be 'true' as long as they do not really matter. What matter are the ability of Quist and Chapela to act as credible spokespersons (interpreting their claims, calling for additional research), the impact of Chapela Maize on regulatory policy, and the synergy between science and activism that Chapela Maize galvanized.

7.5 Formal Scientific Critiques

Christou editorial

In February 2002, the journal *Transgenic Research* published a three-page editorial on Quist and Chapela's article entitled, "No Credible Scientific Evidence is Presented to Support Claims that Transgenic DNA was Introgressed into Traditional Maize Landraces in Oaxaca, Mexico" (Christou 2002). Christou made it clear that this

was not his own private analysis, boosting his own scientific legitimacy to stand in judgment:

Members of the Editorial Board of Transgenic Research, and a number of other scientists with many decades of experience in the area of transgenics, have provided comments that indeed demonstrate that the data presented in the published article are mere artifacts resulting from poor experimental design and practices.

This charge, that the Chapela Maize data were “mere artifacts” echoed the AgBioView critique in questioning the scientific credibility of Quist and Chapela and their *Nature* publication, but did not make counter-claims to their published conclusions by providing additional data or predicting that a ‘proper’ investigation would yield no introgression. In fact, Christou stated that “the *eventual* introgression of transgenes from commercial hybrids into landraces and wild relatives is likely *should they be grown in close proximity*” (emphasis mine). Thus with some qualifications, Christou supported the notion that transgenic DNA would introgress into maize landraces.

The editorial resisted Quist/Chapela in four ways: by castigating *Nature* for publishing the manuscript, by criticizing the methods used as flawed and/or inappropriate, by suggesting that the results were too unlikely or bizarre to believe, and by claiming that the results (even if true) were insignificant.

The editorial implied that *Nature* had committed a serious error in its peer review process. Christou stated that it was “very surprising...that a manuscript with so many fundamental flaws was published in a scientific journal that normally has very stringent criteria for accepting manuscripts for publication.” The final paragraph stated, “It is disappointing that the editors of *Nature* did not insist on a level of scientific evidence that should have been easily accessible if the interpretations were true.” By directly criticizing *Nature*’s peer review process, Christou challenged the credibility transferred to Quist and

Chapela's article through the prestigious reputation of a premier journal. Assuming that the status of *Nature* as an elite scientific publication depends generally on its perceived high standards of peer review, impugning a momentary lapse of selection stripped the targeted manuscript of a great deal of status – reducing its power to establish credible knowledge.

Much of the editorial detailed the shortcomings of Quist and Chapela's methods and interpretations. Christou judged their method for detection as flawed: "The authors do not employ measures to eliminate *any* [emphasis mine] source of contamination, and therefore do not rule out the most likely explanation for the results they observed." He critiqued the use of PCR as prone to contamination (especially nested PCR) and argued for the superiority of "[a]n old-fashioned, but more reliable Southern blot" or growing out the plants to test for antibiotic resistance or transgene expression. He judged the inverse PCR results as "technically flawed" and reported likely errors in matching sequences to known regions in the maize genome in the second part of the paper. At the level of presentation, Christou called attention to a missing track in Quist and Chapela's "Figure 1" (the Diconsa sample) and questioned the interpretation of that figure. Lastly, he noted the failure to present data on the historical negative control. Because the editorial claimed expert status – above Quist, Chapela, and *Nature*'s editors and reviewers – these technical criticisms challenged each of the conclusions in Quist and Chapela's article and indirectly attacked the professional character of Quist and Chapela through implications of incompetence that bordered on fraud.

Also relying on the expert status assumed by Christou, the editorial presented the results as too unlikely to be legitimate. Regarding the claim of introgression, he argued

that Quist and Chapela's failure to find "intact inserts, which are more likely to be present than fragments of unknown origin, casts further doubt that the results observed come from a transgenic plant source." Regarding the finding of multiple insertion events, he commented, "There are few transgenic events in commercial maize and all the inverse PCR results would be expected to conform to one or other of these." With respect to transformation explaining the multiple loci of CaMV, Christou explained, "Transformation is not a likely explanation, since the sequences in these transformation events are well accounted for, and stray CaMV promoters just are not present in these events." In essence, these criticisms placed Quist and Chapela's results so far outside the boundary of expected knowledge that their conclusions deserved no attention. The implied message was that Chapela and Quist were conducting research well beyond their area of expertise, and *Nature* was fool enough to fail to recognize their naiveté.

Lastly, in line with the AgBioWorld rhetoric described above, the editorial framed Quist and Chapela's results of introgression as insignificant *even if they were true*. Christou explained that the lack of evidence of fully-functional genes made concerns about effects "scientifically unwarranted." This perspective assumed either that only the transfer of intact transgenes mattered (ecologically, agronomically, evolutionarily) or that the transfer of transgenic fragments could not occur. This critique challenged both the credibility of Quist and Chapela (why did anyone bother to conduct such an insignificant experiment?) and those actors that translated Chapela Maize into a call for political action against GM crops (assuming a 'science-based' policy framework, Quist and Chapela's conclusions did not warrant a political response whatsoever).

Nature, Act II, Scene 1 – The critics submit letters to Nature

In December 2001 and January 2002, *Nature* received five letters challenging the validity of Quist and Chapela's article. Following *Nature*'s protocol, Quist and Chapela were asked to respond to four of them (Kaplinsky et. al, Metz and Fütterer, Parrot, and Ranger), each in a 500 word response, later extended to 700 words for two of the critiques. Quist and Chapela submitted those responses on 28 January and those responses, along with the letters of critique, were sent out for peer review (Quist Chronology). These letters paralleled some of the public denunciations described above, but with great potential to challenge Quist and Chapela on more equal footing – on the pages of *Nature* itself.

After peer-review, *Nature*'s editors found the critiques quite convincing, but offered Quist and Chapela the opportunity to produce additional data to substantiate their claims:

From these reports, the technical problems associated with your PCR results would appear seriously to undermine the main conclusions presented in your paper. Although some of the referees acknowledge that your conclusions may still stand, they are agreed that definitive evidence is still urgently needed to substantiate them. We therefore ask you to supply this evidence in the form of a DNA blot demonstrating transgene integration, with the appropriate controls, as requested by our first referee. Should you be in a position to provide these data, we would be prepared to include the blot (after this has been seen by the reviewers) in a reply from you to the brief communications comments. However, in the event that you are unable to supply a definitive Southern blot, we are likely to ask you to retract your paper...Because of the publicity surrounding this paper and the urgent need to alert the community to this uncertainty, we need to put a time limit of 4 weeks from the date of this message on the return of your supporting experimental evidence (Email from Rosalind Cotter [Brief Communications Editor] and Ritu Dhand [Chief Biology Editor] to Quist, 14 February 2002).

Because *Nature*'s editors controlled the influential space of the pages of their journal, they had the power to induce Quist and Chapela to take on the stressful task of reinforcing their main claim of introgression via other means in only four weeks.

Nature, Act II, Scene 2 – Quist and Chapela attempt to produce more data

After four weeks, Quist and Chapela found themselves unable to optimize their experiments to demonstrate transgene introgression with a DNA blot. During this time, one of the critiques was published on a public website, breaking *Nature*'s embargo, and making it ineligible for publication in the journal. *Nature* also had informed Quist and Chapela that they should write a short piece responding to their critics, but present their non-PCR data as proof of introgression to avoid the need to rebut every criticism surrounding the PCR approach (Email from Cotter to Quist, 7 March 2002). On 13 March, Quist and Chapela submitted a report on their progress to *Nature*, with an email stating:

Unfortunately, we cannot make any definitive statement based on these results. It is paradoxical to note that the reason for our reluctance to conclude anything definitive from our results is that we have obtained false positives in some of our negative controls, the very reason that has been quoted as invalidating our PCR approach, with which, it must be said, we did not have this problem. As discussed in our brief report, the specifics of our samples represented a serious challenge for these hybridization experiments, and we were glad to see that in principle we should be able to resolve the question using this dot-blot approach once we optimized the method for our specific conditions. Hard as we tried, the time within the deadline was not enough for this purpose, but we will continue with this process (Quist Chronology).

According to Quist and Chapela, *Nature* edited their report (not written with publication in mind) into a “reply” that would have been published along with the letters of critique. The edited report explained the rationale for seeking non-PCR evidence, justified the use of a dot blot technique rather than a Southern blot, and admitted that experiments thus far had yielded false positives from controls. The “reply” concluded: “In view of the time constraints imposed by the public debate surrounding our paper, we are still seeking verification from DNA-hybridization evidence for the presence of CaMV DNA in our

criollo maize samples” (galley proof of “Brief Communications, Quist and Chapela reply,” pdf file labeled “1395c Quist,” provided to author by Matthew Metz).

In a rapid turn of events, Quist successfully optimized the dot blot experiment and he and Chapela wrote to *Nature* on 18 March to inform them of their success. They also explained their opposition to the galley proof that *Nature* had asked them to sign for publication:

We are extremely concerned about the wording and content of the galley proofs sent to us on Friday. First of all, we do not agree that this column can be portrayed as a reply to either the Kaplinsky et al. or the Metz & Fütterer letters. Kaplinsky et al. explicitly state that they do not challenge our main statement of transgenic introgression into landraces, and Metz & Fütterer question this statement only weakly and indirectly. By forcing us to focus on this question, we do not have an opportunity to address their challenge to the more complex results obtained in our iPCR method. Second, presenting weak hybridization results as our only response to the wide campaign of attacks, mostly in the internet but also in other publications such as the editorial in *Transgenic Research*, gives the wrong impression that we do not have any other responses. At the very least, we feel that it should be made clear that both the suggestion that we should necessarily perform the hybridization experiments, and then the pressure to produce results from these experiments within an extremely short time period came from *Nature* (Quist Chronology).

Nature sought the opinion of a referee, who judged the dot blot inadequate. Dhand explained:

Your new data have now been assessed by an independent reviewer¹²⁷ whose comments are attached. You will see that the reviewer - an authority in the field of plant molecular genetics - does not find the new evidence compelling, and believes that the salient claims in the original paper remain unproven. Under the circumstances and after discussion with Philip Campbell [Editor in Chief], we do not believe that it would be appropriate to publish the latest version of your response to the criticisms raised against your paper, or to refer to the new dot-Blot data as supporting evidence given that these new data have now been subject to peer review and found to be unsatisfactory. We enclose a further edited version of your response that we trust you will now find acceptable (Email from Dhand to Chapela and Quist, 21 March 2002).

¹²⁷ In a reply to *Nature*, Quist and Chapela disputed the ‘independence’ of this reviewer by pointing out that the text of the reviewer contains verbatim language from a hostile reviewer from a previous round of peer review. They requested that *Nature* seek a “truly independent reviewer” (Email from Quist and Chapela to Dhand, 21 March 2002).

Chapela and Quist judged the new text as essentially a retraction of their original paper (Chapela 6/24/04), and rejected the new response (a marked-up version of the previous galley proof with all mention of DNA hybridization experiments removed [Quist Chronology]). Quist and Chapela wrote: “[W]e respectfully but vehemently disapprove of the text sent to us as representing a legitimate response to our critics” (Email from Chapela and Quist to Dhand, 21 March 2002). At this stage, the argument remained out of public view, but with enormous potential political consequences. The refusal to accept Quist and Chapela’s additional data and the censoring of all of their responses to letters of critique would have meant the publication of disparaging letters accompanied by an anemic reply – suggesting that the original authors had no ability to refute the claims of the critics. Seemingly procedural decisions (i.e. stage management) have enormous impact on the character and power of resistance.

Nature, Act II, Scene 3 – Final negotiations for publication

The following day, 22 March, *Nature* sent Quist and Chapela a new version of their ‘reply’ that included their additional dot-blot data but nothing in response to the two critical letters to be published (Kaplinsky 2002; Metz and Fütterer 2002). Quist noted: “Understanding that their patience was probably getting thin to our consistent objections of their handling of our response, we capitulated to further protest by instead making a few requests in the final proofs” (Quist Chronology). In their email to *Nature*, Quist and Chapela argued:

1. Please do NOT use the word “reply” to characterize our piece. What is available in the galley proofs we received this morning does not address the questions raised by the two letters included. At the very least, the reader will be confused, but most probably he/she will simply conclude that we do not even understand the point being raised. We suggest the use of “Quist & Chapela provide further data”. The editorial

comment should be changed accordingly: what is included in the proofs does not comprise in any way our “response”, but simply additional data.

2. Request a statement of conflict of interest from all authors (see justification below).
3. In the editorial comment, make a balanced statement about the reality of the review process, namely that two of three reviewers found that our main conclusion is not challenged. By focusing on the one reviewer who categorically opposed our paper, *Nature* gives the wrong impression that this is the general opinion among the peers (Quist Chronology).

Nature's Editor in Chief responded to Quist and Chapela directly:

We have considered your latest e-mail. Despite previous agreement to the contrary, we are willing to include a response from you to the two contributions that I sent you, provided it is received tomorrow morning our time...We must ask you to condense both of your previous responses (attached) into one opening section of the revised single document, the second portion of which would be the existing contribution reporting the new data, essentially unchanged. This allows you a total of 250 additional words of text (Email from Campbell to Quist and Chapela, 26 March 2002).

Quist and Chapela followed these instructions and prepared a 726 word version of their response, which they submitted to *Nature* the next day (Email from Quist to Campbell, 27 March 2002). The final version, published on *Nature*'s website “Advanced Online Publications” on 4 April, appeared in print on 10 April 2002.

Although experienced within the relative privacy of *Nature*'s editorial process, the negotiations over the format and framing of a reply created enormous pressure on Quist and Chapela to defend their claims, while operating within an institutional framework in which their only source of power was to withhold their signatures from text they found objectionable. It was clear that *Nature* was going to publish critiques of Quist and Chapela's article; at stake was the inclusion and presentation of their corroborating data and defense against technical critiques.

Nature, Act II, Scene 4 – For the world to read...and to judge?

With the complex editorial and publication negotiations completely out of public view (as they always are), *Nature* published the two critiques, “Suspect Evidence of

Transgenic Contamination” (Metz and Fütterer 2002) and “Maize transgene results in Mexico are artefacts” (Kaplinsky, Braun et al. 2002); the reply, including new data (Quist and Chapela 2002); and an “Editorial note” (Nature Editor 2002a).

The two critiques challenged Quist and Chapela’s article along similar themes as those found in the *Transgenic Research* editorial published two months earlier (Christou 2002), although neither directly chastised *Nature* or its peer review process for publishing the research in the first place. Regarding Quist and Chapela’s claim of introgression, Kaplinsky et al. suggested that “Southern blots of individual kernels could provide much more reliable information” but focused most of their attention on the iPCR-derived claims. Metz and Fütterer, however, challenged Quist and Chapela’s use of the word introgression; objected that “results from the historical negative control sample are omitted as data not shown, with two lanes of data being excised from the gel in the authors’ Fig. 1”; and accused Quist and Chapela of careless methodology that could have resulted in contamination of their samples. While neither critique explicitly doubted that transgenic corn could have been growing in Mexico, the wide-ranging and detailed criticisms undermined the credibility of Quist and Chapela’s article in front of *Nature*’s readers and audiences of other media that reported the story (e.g., Abate 2002; Mann 2002).

The second claim, of multiple sites of introgression and transgene instability, received much harsher critique. Metz and Fütterer judged the iPCR methodology as flawed, bemoaned the lack of negative controls, and corrected what they interpreted as an unfaithful citation to the work of another scientist (Pawlowski). Echoing Christou’s assertions of results being too far outside the boundary of expectations, they wrote: “We

examined the sequences of the reported i-PCR products (Fig. 1) and found that none contains a *reasonable* number of the features that *would be expected* in a *legitimate* product of amplified genomic DNA flanking the anchor sequence” (emphasis mine). Kaplinsky et al. accused Quist and Chapela of using poorly designed primers and misidentifying sequences to the extent that the reported results had no merit. Together, the two critiques provided a highly technical and unforgiving challenge to the iPCR data and interpretation, although neither provided data or specific citations to refute the possibility of multiple transgenic insertion sites or instability.

Less directly than Christou, the two letters challenged the significance of Quist and Chapela’s claim of introgression. Metz and Fütterer reminded readers that transgenic crops had already been grown illegally elsewhere “with soybean in Brazil and cotton in India.” Kaplinsky et al. stated that “[t]ransgenic corn may be being grown illegally in Mexico,” but immediately emphasized the political consequences of poor science, backgrounding any concerns (scientific, cultural, ecological, economic) that might arise from transgenic introgression: “It is important for information about genetically modified organisms to be reliable and accurate, as important policy decisions are at stake.”

From one perspective, the published letters honored the scientific tradition of critique, in the spirit of “organized skepticism” (Merton 1973 [1942]). The authors provided technical analysis of a published paper they judged to be flawed and important enough to challenge in a formal arena. From another perspective, however, they functioned as a blanket of resistance to a set of questions represented by Chapela Maize. Neither letter called for improved work to demonstrate the stability and predictability of transgenes in ecological contexts; neither affirmed the importance of measuring and

evaluating transgene flow into landraces or wild relatives; and neither praised Quist and Chapela for asking questions that had thus far gone unasked within elite scientific discourse. Without ascribing motives to any of the authors or *Nature*'s editors, the publication of these critiques as written missed the opportunity to call for better science in response to supposedly inadequate science.

Along with the critiques, *Nature* published an "Editorial Note" that perhaps did more to discredit Quist and Chapela's findings than any other form of resistance discussed thus far. The full text read:

In our 29 November issue, we published the paper "Transgenic DNA introgressed into traditional maize landraces in Oaxaca, Mexico" by David Quist and Ignacio Chapela. Subsequently, we received several criticisms of the paper, to which we obtained responses from the authors and consulted referees over the exchanges. In the meantime, the authors agreed to obtain further data, on a timetable agreed with us, that might prove beyond reasonable doubt that transgenes have indeed become integrated into the maize genome. The authors have now obtained some additional data, but there is disagreement between them and a referee as to whether these results significantly bolster their argument. In light of these discussions and the diverse advice received, *Nature* has concluded that the evidence available is not sufficient to justify the publication of the original paper. As the authors nevertheless wish to stand by the available evidence for their conclusions, we feel it best simply to make these circumstances clear, to publish the criticisms, the authors' response and new data, and to allow our readers to judge the science for themselves (Nature Editor 2002a).

This passage was not a retraction,¹²⁸ although it has often been mischaracterized as such. At the U.N. Convention on Biological Diversity in The Hague in the spring of 2002, Dr. Tewolde Igziabher, Head of the African Region, and delegate from Ethiopia, advocated for public access to 'event-specific' molecular sequences in order to monitor gene flow, referring to the serious contamination of Mexican maize. "The delegate from Australia rebutted Tewolde's statement by telling the conference that *Nature* had retracted the

¹²⁸ According to Chapela, *Nature* formally asked them to retract the paper, but he and Quist refused. Later, after Quist and Chapela submitted a report on their lack of sufficient progress to achieve confirmation of their initial results, *Nature* created a letter (much of it cut and pasted from the submitted report) and asked for Quist and Chapela's signatures for publication. The two refused after recognizing that the letter would have essentially retracted their original paper (Chapela, 6/24/04).

paper, giving the impression that the science was suspect and the problem does not exist” (Ho 2002). Likewise, During Chapela’s tenure review, an external referee and his own department chair referred to the paper as “retracted” (Chapela 2003d). The misrepresentation even extended to a scientific paper published within the Nature Publishing Group (Stewart, Halfhill and Warwick 2003).¹²⁹

As evidence of the uniqueness of *Nature*’s behavior, the editors went to the trouble of attempting to clarify the situation in June 2002, when two *Correspondence* pieces also appeared criticizing *Nature*’s editorial treatment of the controversy (Suarez, Benard et al. 2002; Worthy, Strohman et al. 2002). The editors commented:

It is highly unusual for *Nature* to publish a paper whose principal conclusion is shown to be not necessarily false but unsustainable on the basis of the reported evidence. The paper was not formally retracted by its authors or by *Nature*. In the circumstances, *Nature* considered it appropriate for the record to make clear to readers its revised view of its original decision to publish (Nature Editor 2002b).

While these editorial passages functioned on one level to increase the transparency of a controversial process by commenting on editorial decisions and practices usually invisible to readers, a more critical analysis exposes contradictions, silences, and subtleties that performed significant boundary work on Quist and Chapela’s data.

First, a published withdrawal of editorial support was unprecedented in *Nature*’s history.¹³⁰ Scientific research constantly revises and reverses earlier findings and interpretations. Retraction does not apply in such cases – the historical record of

¹²⁹ At the request of Quist, *Nature Reviews Genetics* published a correction to the Stewart et. al paper in April 2004, Volume 5, page 310, reaffirming that *Nature* did not retract the Quist and Chapela (2001) paper.

¹³⁰ During an interview, Chapela called my attention to the ‘Benveniste affair’, a controversy that raged in *Nature* during the 1980s over an experiment that purported to demonstrate scientific credibility for homeopathy, specifically water’s molecular memory after extreme dilution (for an overview, see Maddox 1988). Although Benveniste was highly discredited by critics and *Nature*’s editor in unusual ways, important differences preserve the uniqueness of *Nature*’s disavowal of Quist and Chapela (2001).

publication suffices to demonstrate the falsification or revision of earlier claims.

Retraction is reserved for cases of gross negligence or fraud (e.g., Schön, Meng and Bao 2003). By providing an editorial note to accompany a pair of criticisms to the Quist and Chapela (2001) paper, *Nature* inserted a unique voice in the performance of this controversy. Allan McHughen, a researcher at the Crop Development Center at the University of Saskatchewan in Canada, commented:

I guess if I have an issue with anybody, it is with *Nature* and the way they handled the whole thing. Even if it had been done poorly [Quist and Chapela's research], the fact is that they [*Nature*] accepted it, they reviewed it, and they published it using presumably fair, reasonably consistent criteria. Once it's done and there's data that alter those findings, they publish those data and let the authors of new papers present their data in contrast (Lepkowski 2002).

The two published critiques provided no new data, as discussed above, only criticisms of method and revised interpretation of Quist and Chapela's data. *Nature*'s editorial action thus displaced Quist and Chapela's claims with a shadow – the claims lost their scientific credibility but no new claims took their place. Michael Pollan, former *New York Times* journalist and professor in UC Berkeley's journalism school remarked:

That's where the withdrawal of the paper is so corrosive. As long as the paper was out there and *Nature* stood behind it, the industry then has to do additional science to dispute it, and that would have been to the benefit of everybody. But the fact that the paper is gone obviously has lifted the burden to defend...The answer to flawed science is more science, and in this case we didn't get more science (interview by author, 9 December 2003).

In other words, *Nature*'s withdrawal drastically reduced the ability of the Quist and Chapela piece to stand as a piece of contrarian science *worthy of attention or refute*.

Second, the editorial note fell silent on three key issues. A) It failed to distinguish between the two findings of the original paper. Although somewhat linked in their dependence on similar primers and identical samples, the claim of introgression depended upon PCR and the claim of multiple insertion sites depended upon iPCR. The published

critics focused attention on the latter – and Quist and Chapela admitted some mistakes in their original interpretation of the iPCR data – but their dismissal of the introgression claims depended mostly on accusations of sloppy science (see above). B) The editors characterized the dispute over the validity/relevance of the additional data as between a single referee and the two authors (“The authors have now obtained some additional data, but there is disagreement between them and a referee as to whether these results significantly bolster their argument”). The following sentence, referring to “the diverse advice received,” backgrounded the reality that only one reviewer was officially consulted. C) The editors failed to discuss the significance of allowing their “readers to judge the science for themselves.” From one perspective, this strategy was generous to Quist and Chapela – instead of privately condemning their science they allowed the critiques and response to appear on *Nature*’s stage, with the possibility that the scientific (and lay) audience would be convinced of the truth of their claims. From another perspective, the strategy was unjust to the two authors – without hard evidence to overturn their original results, *Nature* retroactively revoked the support of its peer reviewers who had negotiated an acceptable article with Quist and Chapela in 2001. Either way, this strategy undermined the institution of peer review, and tarnished the reputation of *Nature* as a proper arbiter of valid and reliable scientific claims. Together, these silences denigrated the specific claims of Quist and Chapela’s article as well as the contrarian approach inherent in their research.

Third, the editorial note improperly characterized Quist and Chapela’s publication of additional data as a “response” to criticism, when in fact they were not given opportunity to address the published critiques adequately in that forum – *Nature*’s

editorial decisions prompted the emphasis on new data over rebuttal of critiques. The heading, “Quist and Chapela reply,” further misled readers, who might have expected answers to more of the specific accusations. The note thus staged the Quist and Chapela “reply” in a way that made it appear inadequate, at best, and intentionally avoidant or dumbstruck, at worst.

Other media took up this performance on *Nature*’s pages as worthy of significant attention. *Science* published a news article explaining the developments of the Chapela Maize controversy, entitled, “Mexican Maize: Transgene Data Deemed Unconvincing” (Mann 2002). The article explained the controversy quite thoroughly, and quoted two interest groups to frame the range of reactions:

The Competitive Enterprise Institute, a pro-market advocacy group in Washington, D.C., hailed the reversal as proving that “antibiotechnology activists often rely on faulty data.” Meanwhile, the antibiotech ETC Group charged that *Nature*’s “flip-flop” is “just an obfuscation of the real issue ... that a Centre of Crop Genetic Diversity has been contaminated, and no one is doing anything about it.”

Andrew Rowell (2002) wrote an article much more sympathetic to Chapela and Quist, calling attention to the timing and potential impact of *Nature*’s actions:

This week sees crucial negotiations at the UN Convention on Biological Diversity in The Hague. The *Nature* statement could not have come at a better time and the biotech industry is naturally gleeful. “Many people are going to need that (*Nature*’s editorial) reference”, says Willy De Greef from Syngenta, the world’s leading agribusiness company, “not least those who, like me, will be in the frontline fights for biotech during the Hague negotiations”.

The *San Francisco Chronicle* ran an article that more precisely distinguished the scientific challenges to the two claims by Quist and Chapela (Abate 2002). The article affirmed the challenges to their second claim, but took seriously the question of transgene stability, exploring the opinions of a number of experts:

Hugo Dooner, a transgene expert at Rutgers University in New Jersey, said the stability of transgenes seems so obvious based on the findings of other experiments that no one has yet conducted a single study to prove it formally.

George Bruening, a plant pathologist at the University of California at Davis, said the commercial success of genetically engineered crops offers convincing proof that transgenes behave like normal genes...If transgenes broke apart or jumped around the genome, biotech firms would not be able to supply GE seeds of consistent quality and farmers would not be planting millions of acres of such crops each year, he said...

But Paul Gepts, an evolutionary geneticist at UC Davis, said notwithstanding any technical shortcoming in Chapela's paper, the Berkeley professor was correct to question the prevailing assumption. "People claim that they (transgenes) are stable, but I am not sure they have looked at this in a systematic way," Gepts said, adding, "We ought to know whether these transgenes remain in place."

Rhetorically, this treatment raised the interesting question of whether Quist and Chapela's line of inquiry was valid, quite apart from their methods and conclusions. The *Chronicle's* presentation left some room for the question as scientifically legitimate, given that the first two scientists appeared to *assume* the stability of transgenes – a practice often derided in science.

7.6 Replication as Resistance, and Resistance to Replication

In early February 2002, CIMMYT announced having tested its own seed banks of maize for the presence of transgenic DNA. They reported that none of their experiments revealed the presence of the CaMV 35S sequence. In addition, they noted that tests conducted in October and November 2001 on 42 Oaxacan maize landraces from the 2000 growing season showed no transgenic contamination (CIMMYT 2002). This directly countered Quist and Chapela's findings, as the seed samples analyzed by CIMMYT matched the year of samples collected for the *Nature* article. While it was possible that CIMMYT's 42 samples did not include transgenic contamination, and Quist and Chapela's did, the failure to find any contamination does undermine the credibility of Quist and Chapela's claims. In other words, while CIMMYT's work did not represent a

true replication of Quist and Chapela's study (identical samples or locations of fields), their report spoke to the question of whether transgenes had introgressed into landraces of Mexican maize. The CIMMYT press release made no direct comment on the credibility of Quist and Chapela's results, but their announcement presented their finding as careful scientific work on an impressive number of samples.

The Mexican government conducted a variety of tests to confirm the introgression of transgenes into maize landraces. The first, completed and announced before Quist and Chapela's *Nature* publication, corroborated their findings and added a quantitative estimate of contamination at 3-10% (Quist and Chapela 2001). More studies followed, and in August 2002, Mexico's National Institute of Ecology (INE) officially announced finding transgenic DNA in samples of maize plants (Enciso L. 2002b). Studies from El Centro de Ecología de la UNAM and El Centro de Investigaciones Avanzadas del IPN (Cinvestav) de Irapuato were submitted to *Nature* for publication, but rejected for technical reasons and "contradictory explanations." "One [reviewer] said that it was a discovery so 'obvious' that it didn't deserve to be published" (translation mine) (Enciso L. 2002a),¹³¹ "whereas the other said the results were 'so unexpected as to not be believable'. The *Nature* editor said the papers had been rejected on 'technical grounds'" (Rowell 2003, 167). *Nature*'s rejection of these manuscripts disallowed the appearance of peer-reviewed proof of Quist and Chapela's first claim of transgenic contamination – by denying the science both the legitimacy of appearing within the pages of *Nature*, and the

¹³¹ Original Spanish text: "Ahora que el gobierno mexicano confirmó los resultados con estudios del Centro de Ecología de la UNAM y del Centro de Investigaciones Avanzadas del IPN (Cinvestav) de Irapuato, rechazó la publicación con argumentaciones técnicas y explicaciones contradictorias de los especialistas que los revisaron. Uno decía que era un descubrimiento tan "obvio" que no merecía ser publicado, y el otro, que se tenía que investigar más, según explicó hace unos días el presidente del Instituto Nacional de Ecología, Exequiel Ezcurra" (Enciso L. 2002a).

opportunity to inform a wider international audience that remained less accessible to Mexican government reports. Furthermore, the news of how the papers were rejected communicated a schizophrenic posture about a realm of contrarian science – what hope would other scientists have in repeating similar experiments and getting them published in *Nature* or elsewhere?¹³²

7.7 Conditional invitation to GeneFlow conference

Although much of the rancor of the Chapela Maize controversy had settled down by 2003, Chapela still faced challenges to his reputation and ability to speak about his research. During the summer of 2003, a friend and colleague of Chapela from Mexico contacted him to invite him to participate in Pew’s Gene Flow Conference in Mexico City. According to Chapela (interview, 9/11/2003), the invitation came late, after the list of speakers had already been determined, indicating that the organizers in charge had not wished to include Chapela, despite the fact that his research with Quist had inspired Pew to organize the conference:

The way they did it was contacting me from a low, mid-managerial level, by someone saying, Well you know I’ve been pushing for you really hard and the bosses finally gave their authorization for me to call you. And everybody is interested in giving you an opportunity to show the science behind this. Will you be able to just come and in ten minutes say what the science says about the case and try to stay away from anything else?

Thus Chapela could participate as a speaker, as long as he avoided discussing the political context of his experience or his opinion of the controversy. Chapela

¹³² The first empirical research about transgene flow in Mexican maize after the Chapela Maize controversy was published in the Proceedings of the National Academy of Sciences (PNAS) in 2005. It reported an “absence of detectable transgenes in local landraces of maize in Oaxaca, Mexico” using seed samples from the 2003 and 2004 growing seasons (Ortiz-Garcia, Ezcurra et al. 2005). Quist and Chapela posted a preliminary critique of the article on the “Pulse of Science” website the same day that the PNAS article appeared online (Chapela and Quist 2005). As of 2 November 2005, a more formal commentary had not yet been released by Chapela.

begrudgingly accepted the invitation as a favor to his friend, but then rescinded after learning more about the program:

But then I saw what the line up was, forget about it. The whole thing is just so loaded...I see the line up and it just totally fits with the Pew profile – which is the appearance of dialogue, the appearance of debate, the appearance of, you know, insightful and careful discussion, without allowing the possibility of crossing a very, very shallow bottom. To be more explicit about it, for example,...transgenesis, per se should not be allowed to be questioned because then we'd really be in trouble. So industry calls that product not process principle. It's an absolute propaganda dictum.

On one level, Chapela excluded himself – he declined an invitation to speak. From Chapela's perspective, however, the circumstances of his invitation clearly demonstrated strong rejection of his scientific personae. Pew's belated and narrow invitation drew a boundary around the small part of Chapela's expertise that would have been welcome at the conference. Much of Chapela's decision not to attend involved his own analysis of how little he could say and explain under the circumstances vs. his appearance creating a veil of legitimacy and balance to an event he judged controlled and framed by promotional science.

7.8 Denial of tenure

Unfortunately for Chapela, his tenure review at UC Berkeley began in September 2001 and continued in the wake of the controversial *Nature* publication. The process was anything but smooth.

According to Chapela, between October 2001 and February 2002, twelve scholars responded to requests and wrote letters in support of his tenure. On 29 March 2002, an ad-hoc committee composed of faculty from his department (Environmental Science, Policy and Management – ESPM) unanimously recommended tenure. In response, his department voted in favor of awarding him tenure (32 for, 1 against, 1 abstention). His

department chair and the dean of his college (College of Natural Resources – CNR) both recommended tenure. Lastly, a campus-wide ad-hoc committee of five faculty unanimously recommended tenure on 3 October 2002 (Chapela n.d.). Because of the perceived controversy surrounding Chapela’s case, the campus administration had enlarged this committee from the customary three members to five members (interview by author of Wayne Getz [member of ad-hoc committee], 13 February 2004).

The first negative signal came with a request by the Vice Provost of the university requesting the ad-hoc committee chair to “re-evaluate the case with new external letters” that he himself had requested. Subsequently the chair resigned, without informing the other committee members, and denounced the existing report of his committee without explanation (Chapela n.d.). The motive for seeking additional letters was not specified publicly, and this created tension between the campus administration and the faculty involved in the review represented.

The first documented challenge to Chapela’s tenure arrived in the form of an external letter, dated 23 February 2003. This contrasted with two other external letters received in this round, which did recommend tenure (Chapela n.d.).

In a somewhat unusual procedural step, the Budget Committee (campus tenure review committee made up of faculty from various departments) requested two additional external letters in May 2003; one reviewer declined and one submitted a letter supporting tenure. Soon after, the Budget Committee issued its preliminary decision to deny tenure on 5 June 2003 (Chapela n.d.).

With a negative tenure decision likely, but not finalized in an announcement from the Chancellor’s Office, Chapela experienced a severe lack of communication between

him and the campus administration about the status of his employment. Although Chapela knew it was customary for professors denied tenure to have their contracts extended for one year out of professional courtesy, the administration was silent on his status as his two-year terminal contract approached expiration on 30 June 2003 (despite numerous inquiries from Chapela to officials involved in the review of his case). This uncertainty led Chapela to stage “open office hours” outside of California Hall, UC Berkeley’s main administration building, from 26-30 June as a form of protest (discussed in Part Three). Aside from the symbolic value of keeping his academic position, Chapela worried about his ability to pay his bills should his UC Berkeley contract simply run out. He received notice of a one-year extension of his contract on the first morning of this event (hand delivered at 7 A.M. on 26 June, dated 19 June) (Chapela n.d.).

In the fall of 2003, two additional external letters arrived, both recommending tenure; the Dean of CNR again recommended tenure (for the fifth time); and the chair of ESPM reiterated his support for tenure (for the third time). Nevertheless, on 20 November, the Budget Committee issued its final decision – to deny Chapela tenure, and the Chancellor endorsed this decision on the same day (Chapela n.d.). For Chapela, the rejection signified UC Berkeley’s lack of confidence in his science, inability to distinguish between controversial research and a discreditation campaign, and failure to protect space for contrarian science. Chapela faced an uncertain contract extension and a professional reputation doubly damaged by the *Nature* controversy and the tenure rejection. In sum, Chapela lost his most important symbol of professional credibility, the confidence of an elite research institution. Although the tenure rejection was eventually overturned in May 2005, giving Chapela tenure retroactively to June 2003, Chapela

suffered through an extraordinarily difficult professional and personal journey to gain official sanction from UC Berkeley. His current work, on the science and policy of biotechnology, unfolds in the shadow of this experience.

7.9 Summary

In the wake of his *Nature* publication, Chapela faced challenges to the credibility of his work from a multitude of actors: journal reviewers, journal editors, professional colleagues, campus administrators, advocacy organizations, manufactured internet identities (linked to corporate interests), government officials, and a group of scientists represented in a public letter. From one perspective, many of these challenges had the potential to improve Chapela's science. We can only imagine, for example, what improvements Chapela and Quist might have made to their original submission if they had consulted with scientists such as Kaplinsky and Metz (both known by Quist and Chapela, given their training within CNR's Department of Plant and Microbial Biology); or how Chapela's tenure case might have played out differently had he attempted to undertake a more thorough investigation of patterns of transgenic introgression in Mexican Maize. While these criticisms have some merit, they fail to respect the drama of the controversy. In regard to the former, The UC Berkeley-Novartis controversy, which preceded Quist and Chapela's publication, had already created an enormous rift between faculty in PMB and faculty in ESPM – especially Quist and Chapela, who were outspoken critics. Regarding the latter, Chapela discussed his difficulty in attracting research support from major scientific institutions after the controversy around his article – even hearing from one program officer that it was not worth his time to apply.

The totality of the experience was resistance on multiple fronts. Attacks focused on the relevance of the research question, the type of methodology employed, the care with which the methodology was carried out, the presentation of the data, the interpretation of the data, the communication of significance, the failure to submit more 'definitive' proof, Chapela's political affiliations, his publication record, and more generally, his failure to adhere to the 'rules of the game'. In dramaturgical terms, his character was attacked, stages were managed to expose his supposed incompetence, and narratives were created to deflate the import of his work. At times, Chapela was the only audience to these performances of resistance, but most became available to wider publics within and outside of the scientific community.

Considering resistance as performance thus informs an understanding of scientific dissent as a pathway. First, forms of resistance can create opportunities to strengthen contrarian science, either in anticipation of more forceful dissent or integration with mainstream science. Second, resistance can provide the motive and incentive for an active response of dissent. Third, resistance plays on the larger stage of scientific controversy, becoming a part of the narrative that supports and challenges scientific credibility.

Chapter 8 Resisting Losey Monarch

Unlike Chapela, Losey did receive tenure in the wake of his controversial research on the lethal effects of *Bt* corn on monarch butterflies. Losey explained at the “Pulse of Scientific Freedom in the Age of the Biotech Industry” (University of California, 10 December 2003) that while a few notable scientists at Cornell had challenged his research, those faculty were not “well liked” in his home department and he was given tenure in the fall of 2003. Prior to this vote of confidence, however, Losey and his colleagues faced many challenges to the legitimacy of their work.

8.1 Resistance to Publication

University scientists, industry representatives, and editors from *Nature* and *Science* discouraged Losey from publishing his research. According to Anthony Shelton and Mark Sears who published an overview of the controversy in *The Plant Journal* in 2001:

Prior to submission, Losey asked several people to review the article; the senior author of this paper [Shelton] was one of the reviewers who recommended against publication because of methodological problems, lack of field data and potential for misrepresentation of the study, but urged that a more careful study, including field aspects, be conducted to address the questions the authors were asking. The authors decided to publish their findings and, according to Knight (2000) [an article published in AgBioView on the AgBioWorld website] in an interview with Losey, the article was submitted to and rejected by *Science* then resubmitted to *Nature*, which accepted it as a ‘Scientific Correspondence’.

Shelton viewed the study as too preliminary and methodologically weak to be published, and too dangerous because of its potential to be misrepresented, presumably by anti-biotech activists.

Losey also shared his results with industry representatives from Novartis and Monsanto prior to publication. According to Shelton and Sears, “Industry representatives learned of this potential paper, were concerned about its publication, and requested a meeting with Losey and his co-authors.” Again, the concern reflected the fear that the research could be “misinterpreted to the media and the public” (Shelton and Sears 2001) and the view that additional data was needed:

Monsanto’s Vice President of Scientific Affairs, Eric Sachs, sent a Monsanto entomologist, Steven Spangler, to discuss the data with Losey. Sachs says the concern at the time was that the data weren’t robust enough and the study wasn’t thorough enough for publication. “Even if we didn’t think the results were robust, we did recognize that [Losey] was raising important questions,” Sachs says. “We felt that we could reasonably predict that the harm to monarchs would be quite limited, but we knew we could only get a complete answer through a risk analysis. And it was obvious we needed much more research.” (PIFB 2002, 9-10)

This theme of discouraging publication because of incomplete data and a potential for public misinterpretation raises key questions about the role of science and scientific dissent in particular. What standard is appropriate in this case? In the context of Shelton and Sears and Spangler’s judgment, Losey’s decision to move forward appeared foolhardy and even arrogant. Why shouldn’t incomplete and possibly misleading data be kept quiet so as not to frighten the public or interrupt technological ‘progress’? Losey’s memory of the meeting, however, and his historical framing of the need for more data, placed the action in a different light:

They [Novartis and Monsanto] requested a meeting here at Cornell. They came and suggested some different wording in the paper. In fact, people from Novartis came out and said we’d really rather you not publish this. You should go back and get more data. My thought at the time was, This corn was already out on 20 million acres; the time for getting more data was 5 years ago before it was released. We didn’t have a definitive answer, and I don’t think we still [do]... If we sat on it another year longer, it would be another year it would be out there on 20 million acres with unknown effects (College of Natural Resources 2003).

From Losey’s point of view, the delay of publication was irresponsible ecologically –

more time that potentially harmful technologies would spread on more acres of farmland. As will be shown below, the publication of Losey's study did have a major effect by spawning extensive research into the potential harm of *Bt* corn to monarch butterflies and other non-target pests. One can imagine that if Losey had followed the advice of Shelton or Novartis, such research would have been undertaken much later or on a much smaller scale. Evaluating the 'social benefit' of this resistance thus becomes a question of competing values for research: the value of alerting the public and scientific community of a potential problem worthy of additional research and possible political response (policy), and the value of research as a more definitive representation of reality (theory – in this case, the ecological effects of GM technology).

8.2 Disparaging Remarks

Almost immediately after publication, the Losey Monarch paper received disparaging comments. On the same day as the print version of *Nature* was released, John Beringer, the Chairperson of the UK Advisory Committee on Releases to the Environment stated on the BBC Radio *Today* program that the Losey Monarch manuscript had not been peer reviewed and that the work might have been "flawed." In a statement published the following month in *Nature*, Beringer (1999) referred to and apologized for this error (*Nature* conducts peer review for all Scientific Correspondence), but defended his disparaging characterization of the study as a needed, but unheeded warning: "My suggestion that the work might be flawed *was not intended as a slight* but was a reminder to the press that preliminary observations should not be overinterpreted. Regrettably, most reporting of the communication has almost entirely ignored the need

for such caution.” While Beringer may have been targeting the press with his characterization of the Losey Monarch paper as “flawed,” within scientific discourse such description performed strong boundary work to exclude the research from scientific credibility. Adjectives such as “incomplete”, “partial”, “preliminary”, or “only suggestive” might have communicated a more nuanced critique of the Losey Monarch results without undermining the scientific value of the study itself.

Several months later, Cornell produced a press release that quoted Shelton ridiculing the Losey Monarch study:

“If I went to a movie and bought a hundred pounds of salted popcorn, because I like salted popcorn, and then I ate those the salted popcorn all at once, I’d probably die. Eating that much salted popcorn simply is not a real-world situation, but if I died it may be reported that salted popcorn was lethal,” Shelton said in an interview. “The same thing holds true for monarch butterflies and pollen. Scientists have a duty to be incredibly responsible for developing realistic studies” (Cornell News Service 1999b).

By rhetorically linking the absurdity of humans eating ridiculous amounts of popcorn with Losey’s study of monarch larvae fed on milkweed dusted with *Bt* corn pollen, Shelton implicitly argued that the Losey Monarch experiments were unrealistic and therefore that Losey had been an irresponsible scientist. These claims hearkened back to the different value one could place upon scientific research – as truth vs. initiator of a path of inquiry. How do we distinguish responsible science? Answering “realistic studies” simply begs the question.

Anthony Shelton and Richard Roush’s (1999) commentary appearing in *Nature Biotechnology*, “False Reports and the Ears of Men,” inspired the Cornell press release. Their critique drew upon *Henry IV: Part Two*, in which “Shakespeare describes a world in which Rumor [a character who stuffs ‘the ears of men with false reports’] creates false

appearances that set in motion a chaotic series of events.” Shelton and Roush rhetorically equated the communication of Losey’s results with rumor:

In many ways, Rumor seems to be playing a similar role in the current debate over the inherent risks of the use of genetically modified plants. And this distortion, however entertaining, is having profound consequences in the real world of science and public policy... We believe that few entomologists or weed scientists familiar with butterflies or corn production (and the control of milkweed) give credence to the *Nature* article, but the public and its policy makers have reacted in a knee-jerk fashion: immediately after publication of the *Nature* correspondence, there was a nearly 10% drop in the value of Monsanto stock, possible trade restrictions by Japan, freezes on the approval process for *Bt*-transgenic corn by the European Commission (Brussels), and calls for a moratorium on further planting of *Bt*-corn in the United States. Was this reaction justified based on what can only be considered a preliminary laboratory study or could Rumor still be more entertaining than fact? (Shelton and Roush 1999)

While Shelton and Roush decried the disproportional public response to the Losey Monarch paper, their assignment of agency remained less clear. Without explicitly absolving Losey (or *Nature*) of responsibility for the spread of ‘rumor’, Shelton and Roush implicated the scientist and the journal as having played into an entertaining drama rather than staying safely within the scientific discourse of ‘fact’. They dismissed the results of the study rather cleanly (with reference to the paucity of qualified scientists who gave “credence” to the article), but the challenge struck much deeper. If the public was bound to ‘overreact’ to contrarian science, those communicating contrarian science (researchers, journals, other media) had an extra burden to somehow limit the significance and impact of their work. This critique emerged despite clear language by Losey in the Cornell press release that minimized the study’s power to measure risk without more research:

We need to look at the big picture here. Pollen from *Bt*-corn could represent a serious risk to populations of monarchs and other butterflies, but we can’t predict how serious the risk is until we have a lot more data. And we can’t forget that *Bt*-corn and other transgenic crops have a huge potential for reducing pesticide use and increasing yields. This study is just the first step, we need to do more research and then objectively weigh the risks versus the benefits of this new technology (Cornell News Service 1999a).

Losey thus faced conflicting responsibilities – first, his internal sense of duty to alert the public to potential ecological harm from *Bt* crops, and second, the duty implied by calls for him to withhold his data until a higher degree of certainty and precision could be obtained.

8.3 Attacks on Methodology

Much of the resistance challenged the methodology of the Losey Monarch study. Critiques centered around four themes: lack of quantification and precision, poor replication of ‘field’ conditions within the laboratory, inappropriate controls, and poor interpretation of data.

Lack of quantification and precision

While many aspects of Losey et al.’s methodology were highly quantitative, their measurement of pollen sprinkled on leaves was qualitative. *Nature Biotechnology* reported:

Willy De Greef [worldwide head of regulatory and government affairs] of Novartis also considered that the data presented could not be used to come to any conclusion about the real effect of *Bt* toxin-containing pollen even on monarch larvae. “The impact would depend how much of the milkweed available to the monarch larvae was affected,” he said. “If it was 75%, then this might be important: if it was 10%, then it would probably not be important” (Hodgson 1999).

De Greef implied that the lack of quantification undermined the study because it might have demonstrated an effect when there was none. The *Washington Post* also noted that scientists had “criticized the Cornell researchers for not actually measuring *Bt* doses in the study” (Weiss 1999a). Other media revealed a more passionate resistance to the lack of precision. At the European Plant Biotechnology Network’s Phytosfere meeting held in

Rome in June 1999, a delegate from the Netherlands commented, “If I had measured out pollen by dropping it onto leaves with a spatula [the method the Cornell researchers used]...I would expect to be chopped into little pieces during peer review” (Hodgson 1999). The graphic quality of this comment revealed true disdain, again suggesting that whatever level of social significance implied by the data, science should not have emerged in partial or preliminary form. Losey admitted this shortcoming, but argued that there would have been no “systematic error in one direction or another,” and even reported that his next phase of research would include quantifying “the concentration of pollen at various distances from the field” and doing “a dose-response for each hybrid [corn]” (Hodgson 1999).

Similar to the charge of inadequate quantification, the presumed lack of appropriate controls represented another shortcoming of the Losey Monarch paper.

Beringer (1999) wrote:

It is clearly stated in the text, for example, that pollen for the non-*Bt* (*Bacillus thuringiensis*) maize control was from an unrelated, untransformed hybrid. However, there is no reported control to demonstrate that pollen from the transformed variety was not toxic in the absence of the functional *Bt* gene.

Nature reported that Losey et al. used an unrelated corn hybrid for their “pollen-plus control” for reasons of availability, but that they planned to repeat experiments using an isogenic line. Losey doubted that it would affect the experimental outcome (Hodgson 1999), but critics who called attention to this detail raised the possibility that the observed monarch mortality had been just an artifact of poor experimental design. This both undermined the results and also weakened Losey’s credibility as a competent and careful scientist.

Poor laboratory design

Critics also derided Losey et al. for conducting an unrealistic laboratory study when field data would have shown more reliable results. The *New York Times* reported bluntly: “Representatives from Novartis Agribusiness Biotechnology, Monsanto and Pioneer Hi-Bred International Inc., the top sellers of *Bt* corn, challenged the significance of the findings for monarch caterpillars, also known as larvae, outside the laboratory” (Yoon 1999a). *The Washington Times* published a letter from a University of Nebraska professor entitled “Butterflies Bearing Grenades” that was much more condescending: “The emerging trend toward publicizing little laboratory studies is going to cause big problems for scientific credibility if it is not reined in quickly” (Foster 1999). As Chris Henke noted in “Dreaming the Butterfly,” a paper delivered at the 2004 American Sociological Association meeting, framing the Losey Monarch study as an example of “force feeding” undermined the agency of the monarchs and argued for the importance of testing for ecological effects in the ‘natural’ conditions of the field. The *Wall Street Journal* ran an oft-cited opinion piece that reflected this rhetoric:

Numerous entomologists and botanists I’ve interviewed say that Mr. Losey’s lab conditions were so artificial as to have little in common with those in the great outdoors. The researchers coated the leaves of milkweed plants -- believed to be the only food monarch larvae eat -- with *Bt* corn pollen. Mr. Losey’s larvae had no clean leaves to choose; it was eat pollen or starve. In contrast, in the wild, if larvae come across a leaf with *Bt* or any other pollen on it, they leave for another leaf. “They’re not interested in eating anything but a nice clean leaf,” says Warren Stevens, senior curator of the Missouri Botanical Gardens in St. Louis (Fumento 1999).¹³³

As Henke noted, this privileging of field over laboratory went against powerful explanatory threads in STS, which locate part of science’s tremendous authority in its

¹³³ It is interesting to note that Fumento uses the title “Mr.” rather than “Dr.” or “Professor” to refer to Losey.

practice of bringing elements of ‘nature’ into the laboratory in order to control them and subject them to new levels of scrutiny ((Latour 1999 [1983]; Latour and Woolgar 1986 [1979]). At one level the critique was a reasonable request for laboratory conditions to more closely match ‘natural’ conditions; at another level, the critique re-framed the study from a placeholder of potential phenomena that demanded concern and further action (research) to a study that prescribed a particular political, commercial, or social response (policy). Ironically, by making more of the study than the authors intended, critics created a more vulnerable piece of science as their target.¹³⁴ Lambasting this re-framed target not only portrayed the data as unrealistic, but chipped away at the question posed by the study.¹³⁵ Above, for example, Stevens made the claim that monarch larvae would only eat “a nice clean leaf,” which, if true, would make Losey et al.’s investigation pointless in the first place. Val Giddings, BIO’s Vice President of food and agriculture announced:

Even if the reported results are validated, there are strong reasons to believe they are not relevant to Monarch caterpillars in the wild. Monarch migration and egg laying patterns ensure that the primary period of larval feeding and growth throughout nearly all the Monarch range takes place well before any nearby corn produces pollen. Ongoing monitoring of *Bt* corn fields by companies since their introduction further shows that very little pollen lands on adjacent milkweed leaves. It is thus highly likely that in the natural setting, outside the laboratory, most Monarch larvae would never encounter any significant amounts of corn pollen. This means the real potential for any negative impact is negligible (Biotechnology Industry Organization 1999).

If the potential for any negative impact were negligible, the motivation to repeat or refine the study would evaporate quickly. The target thus drifted from results to purpose.

¹³⁴ Losey himself referred to this phenomenon as “shooting at scientists for saying things they didn’t say” and “building a straw man and cutting that down” (College of Natural Resources 2003).

¹³⁵ By contrast, Beringer wrote in *Nature*: “Of course it is desirable to point out the potential harm that may arise from pollen dispersal, which in this case could be very important, but the data reported by Losey *et al.* do not directly pertain to this issue” (Beringer 1999).

8.4 Attacks on Significance

Another theme of critique focused not on the research methods but on the significance of the results. As *The New York Times* reported on the same day the Losey Monarch study was published, “Rich Lotstein, vice president of public affairs for Novartis Agribusiness Biotechnology, said, ‘Even if Dr. Losey’s results are real, which they could be, the exposure is still minimal, and the impact is extremely small, if any’” (Yoon 1999a). Critics engaged three rhetorical strategies: arguing that the results were not surprising, backgrounding the study in comparison to other threats to monarch survival, and framing the proper comparison as between *Bt* crops and conventional agriculture with pesticides

Bt harming monarchs is no surprise

In a news article in July, *Nature* reported:

Critics of the work have particularly highlighted a number of weaknesses of the paper. The first is that it is unsurprising that a lepidopteran species such as the monarch should be affected when fed plant material that contains a protein used precisely because of its lepidopteran-specific killing properties (Hodgson 1999).

Likewise, Professor John Foster wrote, “there probably was not an entomologist in the world who was not aware that corn pollen containing the *Bt* gene could harm butterflies—if butterflies ate corn pollen, which they don’t”¹³⁶ (Foster 1999). This criticism not only reduced the significance of the data, but also undermined the credibility

¹³⁶ Losey et al.’s data contradicted this viewpoint. As *Nature* explained in a news article in July 1999, “Some of the Cornell group’s unpublished data throw up another interesting thread that needs to be untangled in subsequent work. Apparently, caterpillars feeding on leaves dusted with nontransgenic pollen grew significantly larger than those on the leaves with no pollen. ‘It’s as if the pollen provides a little extra package of nutrients,’ explained Losey. In fact, it seems likely that that this ‘extra package’ formed a substantial component of the larval diet. The pollen-plus control larvae got fatter even though they consumed only 70% as much leaf area as the pollen-minus control larvae” (Hodgson 1999).

of scientists who would undertake such ‘pointless’ research and bother to publish it. Val Giddings, spokesperson for BIO, made a similar point:

With this letter by John Losey to *Nature*, old issues have been resurrected to raise questions about the potential impact on Monarch butterflies...Reports of the potential for effects from these *Bt* corn hybrids on Monarch butterflies or other lepidoptera are not new. They have been reported in the scientific literature and regulatory review documents since at least 1986. Both the Environmental Protection Agency and the U.S. Department of Agriculture have been provided data on the potential for impacts on non target species from *Bt* pollen for years (Biotechnology Industry Organization 1999).

Giddings thus argued from a historical and institutional perspective that the Losey Monarch study lacked significance because the questions had already been addressed by scientists and regulatory organizations.

Keep threats to monarchs in perspective

Discourse around the Losey Monarch study often made reference to the existence of multiple threats to monarch butterflies. In some cases, the reminders of other threats took on an educational air. Val Giddings commented, “As conservation groups have noted, the primary threat to the monarch butterfly is the loss of crucial winter habitat in southern California and central Mexico... Other threats come from habitat degradation along butterfly migratory routes, pesticides, and other human activities” (PR Newswire 1999). Likewise, *The Washington Post* reported:

Other experts...ventured that *Bt* probably ranks low compared with other monarch threats. “I don't think it's a very big issue,” said Rice of Iowa State. Much of the monarch's habitat was lost when tall-grass prairies were converted to farmland, he said, and deforestation in Mexico clearly is a big problem. Moreover, he said, “milkweed is considered a noxious weed, and farmers do their utmost to get rid of it” (Weiss 1999a).

On one level, these statements contextualized the potential threat of *Bt* corn to monarchs, sending a message to their audience that anyone who cared about monarch butterflies should invest their time and effort elsewhere.

Other examples undermining the significance of Losey Monarch took on a sarcastic tone. Giddings commented, “It’s not an exaggeration to say more monarchs succumb to high-velocity collisions with car windshields than ever encounter corn pollen” (PR Newswire 1999). *The Washington Post* also quoted Rich Lotstein, a spokesperson for Novartis: “It all has to be put in the proper perspective...Please keep in mind that monarchs are probably impacted by cars in the Midwest, too” (Weiss 1999a). In both cases, the speakers referenced a graphic and violent death scenario for butterflies, and one that was likely familiar to most readers. Rhetorically, this placed their audience in the position of weighing their personal driving habits with monarch butterfly survival, as if to communicate that a certain level of monarch mortality was unavoidable without major changes in modern lifestyle and agricultural practice. In addition, by foregrounding other threats in a comparative context, this discourse backgrounded the question of *additional but preventable threats*. While habitat destruction in Mexico may have had the largest impact on monarch butterflies, and a certain number of butterflies were killed on roadways, those that escaped those threats would face the additional threat of *Bt* corn pollen, if the Losey Monarch study were instructive. A strategy to protect the global monarch population would not simply focus on the relative percentage impact of different threats, but also take into account the degree and potential for eliminating particular threats to reduce the overall impact. This would likely be true even within a fairly narrow risk analysis and management paradigm.

Compare Bt crops to conventional agriculture?

The most frequent argument to undermine the significance of the Losey Monarch paper involved calling attention to the appropriate comparative context for measuring the potential harm of *Bt* corn on monarchs. Critics argued both that monarchs specifically endured greater threats from conventional pesticide applications to corn, and more generally that *Bt* corn had desirable ecological consequences.

Pimentel and Raven (2000) argued that “considering the gains obviously achieved in the level of survival of populations of Monarch butterflies and other insects by eliminating a large proportion of the pesticides applied to the same crops, the widespread cultivation of *Bt* corn may have huge benefits for Monarch butterfly survival.” Likewise, the PR Newswire (1999) quoted Dr. Chris DiFonzo, Michigan State University Field Crops Entomologist & Pesticide Education Coordinator: “*Bt* corn is a much safer method of pest management, and has less detrimental impact on all aspects of the environment -- monarchs included -- than the use of broad-spectrum insecticides.”

At other moments, critics condensed concerns about monarch survival within larger ecological and health questions about agricultural impacts. Pimentel and Raven (2000) commented, “In evaluating the use of *Bt* corn and its possible environmental damage, it is important to take into account the serious public health and environmental damage caused by the use of pesticides in U.S. agriculture generally.” By extending the concerns to issues of public health, they effectively trumped the Losey Monarch study as investigating a narrow, and less important (from an anthropocentric viewpoint) aspect of unintended agricultural impacts. Giddings elaborated this widened view of *Bt* corn’s significance:

Ongoing monitoring by companies of *Bt* corn fields since their introduction also shows that insect biodiversity and population densities in *Bt* corn fields is significantly higher than in fields treated with chemical pesticide sprays. *Bt* corn thus helps enhance beneficial insect populations that would otherwise be threatened by the use of pesticidal sprays. This further leads to significant improvements to water quality and environmental conservation for insect eating birds, small mammals and other life (Biotechnology Industry Organization 1999).

The argument was simple and straightforward – public good would be served best by focusing on the broad benefits of *Bt* technology rather than the marginal harm done to one species of butterfly. Even Losey himself echoed this perspective: “I still think the proven benefits of *Bt* corn outweigh the potential risks... We can’t forget that *Bt* corn and other transgenic crops have a huge potential for reducing pesticide use and increasing yields” (PR Newswire 1999).¹³⁷

8.5 Connecting with the ‘Technology as Progress’ Narrative

On 25 June 1999, just over one month after the Losey Monarch publication, the *Wall Street Journal* published a letter from Michael Fumento, a senior fellow at the Hudson Institute and a frequent contributor to the *WSJ*. The letter was entitled “The World Is Still Safe for Butterflies” and attacked both the methods and significance of the Losey Monarch paper according to most of the themes outlined above. It differed, however, in the way that the condemnation of the study was used as a platform to make a

¹³⁷ Losey’s perspective, however, remained nuanced: “According to Losey, in those fields that are going to be treated against the European corn borer, *Bt*-corn clearly is the most environmentally sound option. However, that still may not make it the preferred option. The European corn borer is very hard to treat conventionally with insecticides. It has to be treated either early—before its second instar when it bores into the corn stalk—or late in the corn-growing season when second generation insects emerge. Late spraying requires specialized equipment or airplanes, and although late spraying is effective, it can only realistically be performed on large fields away from human habitations or roads. The net result is that less than 20% of the corn crop is actually treated. Thus, adverse environmental effects from conventional treatments only accrue to 20% of corn plantings. In contrast, *Bt* corn already represents 30% of the area, and this proportion is increasing” (Hodgson 1999).

much broader point about contrarian views of agbiotech. Fumento concluded his letter with the following four paragraphs, each analyzed separately.

The issue here is not just corn but biotech in general. The British Medical Association has demanded a moratorium on planting all biotech crops, and even Prince Charles -- whose occupation perhaps gives him a special affinity for the monarch butterfly -- asks: "If [biotech] plants can do this to butterflies, what damage might they cause to other species?"

Fumento broadened the concern and shifted the frame to consider how political forces might (over)react to the Losey Monarch study. Prince Charles' question appeared misguided in the light of Fumento's previous condemnation of the study, but forthright to an audience who could forgive the Monarch's lack of expert knowledge on his namesake's butterfly research.

Such actions and words appeal to people who don't realize that virtually everything we eat is already the result of man's [sic] handiwork. The difference is that formerly two animals or plants were crudely crossbred in hopes the right genes would come out of the mix. Biotechnology allows a single gene to be isolated from an organism and inserted into another, with much faster and more predictable results. It also allows crossing species that formerly could not have been combined.

Fumento drew upon essential claims of promotional science (precision, continuity of biotechnology with plant breeding, predictability) to disrupt and invalidate Prince Charles' question.

Biotechnology can enable plants to protect themselves not just from insects but from weeds and fungi and to rely less on fertilizer. Bioengineered crops will provide much higher yields on the same amount of land and grow in less hospitable soils and climates. They will contain far more vitamins, protein and other nutrients. Some, when eaten, will vaccinate people against scourges like cholera and malaria.

Fumento continued with narratives of promotional science and laid bare the promotional assumptions about the future of biotechnology.

We ought not allow such progress to be brought to a screeching halt by a beautiful little insect and a letter to a science journal.

The conclusion named the advance of biotechnology as progress – a frame supported by the assumptions of promotional science – and implicated not just the Losey Monarch publication, *but those who would use it as justification for political action*, in the prevention of progress. Interpreted broadly, Fumento’s letter marginalized dissenting science as a force that, if taken seriously, would threaten ‘progress.’ In this performance, caution and careful questioning had no role in the development of agbiotech – the choice offered to the audience was ‘progress’ or a ‘screeching halt.’

8.6 Monarch Research Symposium

Soon after the publication of Losey Monarch, industry leaders formed a consortium with the USDA and university scientists to respond to the growing public anxiety about *Bt* corn’s threat to monarch butterflies. The consortium, the Agricultural Biotechnology Stewardship Working Group (ABSWG), was charged with a short-term research task:

to assess the potential risk to monarch populations from *Bt* corn pollen under natural environmental conditions. These studies focused on determining the potential effects of *Bt* pollen on monarch larval growth and survival (hazard), and evaluating the probability that larvae would consume *Bt* pollen (exposure). The panel urged the public not to over-react to the initial study reports, but to allow the scientific community to complete a thorough, science-based risk assessment (Shelton and Sears 2001).

Conducting further research in itself would not undermine the credibility of the Losey Monarch study, but framing of the study as a risk assessment and urging the public not to “over-react” hinted at an expectation that the further research would result in reassurances to the public that the fears generated by the Losey Monarch study were unwarranted.

The research program, costing more than \$100,000,¹³⁸ culminated in the *Monarch Research Symposium* (MRS), held in Rosemont, Illinois (near Chicago) on 2 November 1999. The following analysis draws from four media performances of this event: *The Washington Post* article entitled, “Gene-Altered Corn’s Impact Reassessed; Studies Funded by Biotech Consortium Find Little Risk to Monarch Butterflies” (Weiss 1999b); *The New York Times* article entitled, “No Consensus on Effect of Genetically Altered Corn on Butterflies” (Yoon 1999a); Pew’s report on the MRS within their larger publication entitled, “Three Years Later: Genetically Engineered Corn and the Monarch Butterfly Controversy” (PIFB 2002); and a “Trip Report” (first-person account of the event) written by Becky Goldberg and distributed by the Environmental Defense Fund (Goldberg 1999). The differences among these performances provide insight into how the MRS did function to challenge the credibility of the Losey Monarch study, and also how the interpretation of such action can vary greatly according to institutional perspective.

The four articles judged the conclusions of the MRS quite differently, both in terms of the probable threat of *Bt* corn to monarchs and the level of uncertainty still present after the studies. The *Post*’s sub-heading declared that “Studies Funded by Biotech Consortium Find Little Risk to Monarch Butterflies” and led with the sentence, “Genetically engineered corn plants appear to pose only a modest and perhaps insignificant threat to monarch butterflies” (Weiss 1999b). The article acknowledged some degree of uncertainty, describing results as “mixed” and stating that there was “still a lot to learn about the complex ecological interactions in question,” but of the five

¹³⁸ According to Eric Sachs of Monsanto, industry sources had provided 60% of this amount, with 40% coming from government and other sources (PIFB 2002, 10),

studies mentioned, four reduced or eliminated the potential threat to monarchs and one had mixed results. In contrast, the *NYT* headline proclaimed a lack of consensus at the conference and led with a sentence that reported “conflicting assertions about the risks that genetically engineered corn might pose to the monarch butterfly” (Yoon 1999b). The article quoted a research entomologist as saying that “there was a lot of information presented that was positive about *Bt* corn” but emphasized the uncertainties and unknowns (e.g., “where monarchs were really coming from and what proportion were likely to be growing up on plants with harmful amounts of pollen”) and described the studies as predominantly “still far from completion and none peer-reviewed or published.” The Pew report focused on the politics of the conference (see below) but included a substantive comment that “several field studies suggested that monarchs were unlikely to be exposed to *Bt* corn pollen because the caterpillars were not present during pollen shed” (PIFB 2002). Echoing the *NYT* article, EDF announced that “Most of the reports were preliminary: many of the scientists had not yet finished analyzing their data, sample sizes were sometimes small, and some scientists’ research methodology was questionable” (Goldburg 1999). The report noted that “Researchers did not address potential fitness effects on monarchs of ingestion of *Bt* corn pollen,” and described a question and answer session in which conference participants revealed that a study with reassuring conclusions may have been invalid because the investigator did not test the potency of the pollen before the experiment, when “at room temperature *Bt* toxins in corn pollen lose their potency after the pollen is stored for a week or so.” Of the seven studies discussed, four implicated *Bt* corn as a probably cause of harm to monarchs, and three

had mixed results. EDF clearly communicated that there was no consensus among meeting participants.

One explanation for these differences among the articles could have been source differences. Of the four quotations in the *NYT* article, one expressed concern about the threat of *Bt* corn to monarchs, two minimized concern, and one was fairly neutral. Of the five relevant quotations in the *Post* article, four minimized concern, one was neutral (Losey), and none emphasized concern. But why would two reporters from elite media, who had both written stories previously to announce the Losey Monarch study, have exposure to such different sources or choose to rely upon different sets of sources?

Viewing the MRS as a performance in itself, rather than strictly a scientific meeting, suggests a powerful explanation. Both the Pew and EDF reports described a press conference that had taken place before the meeting. As the *NYT* reported, “The Agricultural Biotechnology Stewardship Working Group, the industry group that organized the symposium, issued a statement this morning saying scientists *were expected to conclude* [emphasis mine] that the altered corn pollen did not harm the monarch” (Yoon 1999b). According to Pew:

The Biotechnology Industry Organization (BIO) played a role, however, by helping with the media coordination of the event. “The ABSTC had enormous scientific talent so there wasn’t much BIO could help with on that front,” says Val Giddings, vice president for food & agriculture at BIO. “We figured there would be a feeding frenzy at the meeting so we decided to help with the media management.” The first thing that BIO did was have a pre-meeting teleconference with some of the scientists who were presenting data from their studies the following day. Then, Giddings and his staff wrote a press release based on the conference call stating the scientific participants were likely to conclude *Bt* corn wasn’t a hazard to the monarch butterfly. “That meeting was a travesty,” says Lincoln Brower, a monarch biologist at Sweet Briar College in Sweet Briar, Va. “While the seminar was still in progress, Carol Yoon [of the *New York Times*] got a press release saying scientists agreed that *Bt* corn had a minimal impact on monarchs, which not all researchers agreed with. The following press conference was highly orchestrated by the companies and the TV cameras ignored those scientists expressing concerns.” (PIFB 2002).

Pew then quoted a university researcher and a Monsanto VP as disagreeing with Brower's assessment – that the results had not vindicated the Losey Monarch paper's original conclusions and that media work should be expected around science with political consequences. EDF was less generous:

Unfortunately, the scientific nature of the symposium was obscured during the afternoon when it became clear that industry was using the symposium to deliver a message to the media that pollen from *Bt* corn posed no threat to monarchs...Subsequently, a packet of articles from the November 2 editions of several newspapers, including the Los Angeles Times, the Chicago Tribune, and the St. Louis Post-Dispatch appeared in a press room, and copies began to circulate among participants. The articles all reported that the symposium would conclude that *Bt* corn pollen posed little risk to monarchs - even though the articles had been written before the meeting had taken place!!...In short, by the end of the day it became abundantly clear that the major purpose of the symposium, from the perspective of its sponsors, was not careful and deliberate evaluation of just completed, and in some cases, still incomplete scientific research. Instead, the meeting was designed and press interactions were orchestrated to provide the impression of scientific consensus when, in fact, no such consensus existed among meeting participants. There was apparently a consensus opinion among a segment of the scientists attending that *Bt* corn poses no significant risk to monarchs, an opinion that they formed prior to the meeting (Goldburg 1999).

Yoon apparently disrupted the intended MRS performance, an element clearly missed by Weiss from *The Washington Post*. His lack of mention of Yoon's interjection (a newsworthy moment of the conference) suggested strongly that he relied upon the orchestrated performance rather than the live performance of debates and conversations among participating scientists.

Yoon's disruption became an opportunity to reinforce the narrative (supporting contrarian science) that industry and regulatory agencies¹³⁹ were conspiring to minimize perceptions of ecological effects of GM crops. However, the planned choreography (while poorly executed) by BIO and the ABSTG consortium still resulted in significant

¹³⁹ EDF commented, "It is understandable that an industry organization such as BIO would manage the meeting to assure its 'spin' was conveyed in media stories. The USDA's role and participation is harder to explain and much more disappointing, given USDA Secretary Glickman's ostensible commitment to sound science and an 'arms length' regulatory process to both characterize and manage the risks posed by genetically engineered organisms" (Goldburg 1999).

media coverage that reduced the credibility of the Losey Monarch findings and of actors pushing for regulatory change on the basis of the original study. It also remained clear that institutional differences (EDF as an environmental NGO with suspicion of biotech, Pew as an initiative portraying balance but often leaning toward biotech as a useful tool with great potential [see Chapter 4]) affected the representation of resistance and even counter-resistance.

8.7 Summary

As with the Chapela Maize case, the significance of resistance to the Losey Monarch study emerges in its pattern. Attacks on methods, calls for additional research before publication, arguments for the relative insignificance of the questions posed by Losey et al., and the coordination of the MRS (a mixture of research funding, institutional collaboration, and media work) work together both to inform Losey of the hostile context in which his research appeared and also to tell a story to broader audiences about that context. Resistance thus carries the dual potential of intellectual suppression and public education of the politics of science.

Losey Monarch also lays bare the interactions of knowledge, power, and credibility in resisting scientific credibility. The MRS, for example, created knowledge (additional research) to complicate (and perhaps overturn) the narrative that Losey et al.'s study initiated. The power that made this possible stemmed largely from industry's financial and institutional resources, which both funded the research and also coordinated the media. Lastly, the participation of regulatory agencies and scientists was key to maintaining the credibility of the MRS as a collaborative, rather than self-interested,

event. One could attribute the lack of success of this performance to an overuse of power (orchestrating the press conference) that diminished the importance of knowledge (the actual discussion at the conference), which then undermined the credibility (for some audiences) of the symposium.

Chapter 9 Resisting Pusztai Potato

As in the first two case studies, challenges to the Pusztai Potato study began well before the publication of results in a scientific journal. Pusztai's experience shared many similarities with Losey and Chapela in terms of theme, but unfolded in a more severe manner. While this dissertation does not attempt to correlate factors of a controversy with the severity of resistance, one cannot ignore a prime difference that may have had some impact on the enthusiasm behind challenges to the Pusztai Potato study: Pusztai's claims directly implicated GM food as a health concern rather than raising the ecological questions of transgene flow and non-target effects.¹⁴⁰

9.1 Pusztai Removed from the Stage, Misinformation Takes the Stage

Pusztai was not permitted to participate in the variety of performances of media coverage and critique following the *World in Action* program on 10 August 1998. Pusztai's silence began without apparent malicious intent. He recalled: "For 2 days, it [the television appearance] was regarded as the greatest thing since sliced bread. And everybody was heaping praises on the Rowett Research Institute. Meanwhile I more or less faded out of the picture because the director was running the show. He was getting all the credits for it" (Pusztai 2003). *The Guardian* reported on 11 August that Pusztai was "unavailable for comment, but Prof James [the director] went to bat for the laboratory" (Radford 1998a). According to Pusztai, the director even blocked Monsanto

¹⁴⁰ An expert might infer that Chapela's claims around transgene instability did speak to health concerns, but Quist and Chapela's framing of the results and the publication venue (*Nature*) emphasized environmental concerns.

from contacting him directly to speak with him about his research and associated claims (Pusztai 2003).

The consequence of Pusztai's silence became significant as the controversy unfolded and false reports began to emerge. Most significantly, an error appeared in the Rowett's second press release of Monday, 10 August – that Pusztai had fed rats potatoes spiked with a lectin from the South American Jack bean, Concanavalin A (Con A), rather than GM potatoes modified to express the snowdrop lectin (GNA). Con A was known to be toxic to mammals, while Pusztai had chosen GNA precisely because of its benign effects (Rowell 2003, 84). Pusztai did not have access to review this press release until Friday, 14 August, and was confused when interviewed early in the week about experiments with Con A:

I could not understand, when people had originally phoned me on the 11th, asking: “Why is it that they are talking about Con A, we have not done Con A genetically modified potato experiments?” So if you ask the question and the question is the wrong question, it is very difficult to give a coherent answer, because I wanted to talk about GNA-GM potatoes and people were starting to ask me: “Have you done any Con A-GM experiments?” and I had to say: “No” (1999).

In those early conversations before James prohibited Pusztai from discussing the issue at all, Pusztai spoke from a place of ignorance because he had been mostly excluded from the official communication between the Rowett and the media about his research and television appearance.

The error quickly snowballed into a powerful negation of all that Pusztai had said.

On Wednesday, 12 August, the BBC reported:

Professor Phillip James, Director of the Aberdeen-based Rowett Research Institute, said Dr Arpad Pusztai had been interpreting the wrong data. “Dr Arpad Pusztai had got himself, under the intense pressure of media interest and huge complex experiments, into a state where he actually thought he was looking at the transgenic study when he was not.” Professor James described the mistake as tragic. “He went too fast, too early” (“Health Genetics Scientist Suspended” 1998)

James' statement made Pusztai appear rushed and so incompetent as to confuse his own studies. On Thursday, 13 August, *The Independent* reported:

Biotechnology companies were crowing last night after the scientist who claimed to have evidence that genetically modified foods could harm health was suspended from his job when his results were shown to be false... The effects are not comparable and the entire experiment must be redone, said Andrew Chesson, the institute's head of research. He said Professor Pusztai admitted that his claimed results were misleading. "It is extremely embarrassing . . . He was a very senior member of staff. We assumed his work was beyond reproach"....Colin Merritt, technical director of the biotech giant Monsanto, said: "It is an awful mistake, and these revelations are absolute dynamite" (Arthur 1998).

With Pusztai 'off stage' and unavailable for comment, the misrepresentation of his experiments both undermined the credibility of the contrarian results he had mentioned on television and also did severe damage to his reputation as a careful and thoughtful scientist. This not only invalidated what a contrarian scientist such as Pusztai *had said*, but also what he *might say* to defend himself later. On the same day, *The Guardian* reported:

Prof James said he assumed there had been a muddle in the results. He instituted an investigation which removed Dr Pusztai from his own research project. "It's tragic really, because this guy is a distinguished biologist with a world record and has had international prizes, he is a fellow of the Royal Society of Edinburgh," he said. "I had little option. He totally agreed, apologised, knew he had presented us with enormous embarrassment and would do everything in his power to co-operate in any way he could" (Radford 1998b).

James thus created a narrative of a tragic fall from credible scientist to an apologetic man backpedaling a muddled experiment. James later regretted the "confusion" after learning that Pusztai had not confused Con A experiments with GNA experiments (1998), but the error continued to be repeated well into 1999 (Rowell 2003, 85).

On one level, the origin of the Con A misunderstanding was irrelevant to the effects described above. Pusztai's enforced silence removed him from a position to correct such factual errors, which made the misinformation contagious enough to strongly

undermine the research and the researcher. On another level, Rowell's (2003) mention of Monsanto's possible role deserves mention:

'I think that is the most likely source of the misinformation, and that the whole Con A potato story originates with Monsanto', speculates Pusztai. Indeed on *GMTV*, earlier that morning [Monday, 10 August] Dan Verakis from Monsanto, without mentioning the gene's name, implied that Pusztai's results were 'no great surprise', 'It is well known that that particular gene that was tested creates a protein that could have some issues with the immune system,' said Verakis, talking about Con A, not the snowdrop lectin (p. 85).

Monsanto's own statement suggested that they did not learn this information until Wednesday, 12 August 1998 (Monsanto Company 1998), but Verakis' strange statement made on 10 August did appear to be the first confused reference attributing Pusztai's results to the use of a protein known to be toxic. Even if Monsanto did fabricate the Con A controversy as a strategy to create confusion around a potentially explosive study, they could not have predicted how well the actions at the Rowett would prevent Pusztai from making a corrective response.

9.2 Enforced Silence and Suspension

According to Rowell (2003), the misunderstanding might have been cleared up on Tuesday afternoon, in advance of many of the media reports that communicated the misinformation about Con A. On Tuesday, 11 August at 3:00 PM, Pusztai, Susan Bardocz (his wife and colleague), Philip James (Rowett Director), Ian Bremner (Rowett Deputy Director), Andrew Chesson (Rowett Head of Research), and the team's immunologist met. At the meeting, Bardocz provided a five-page document that summarized all of the work that their team had undertaken in the previous three years, and which clearly showed that "long-term GNA feeding studies had been undertaken, including checking for any kind of immune response" (p. 87). Pusztai recalled that

Bremner agreed to “use Susan’s report to write an authoritative press release to be released the next morning, clearing up the controversy over Con A and GNA lectin and backing up Pusztai” (p. 87).

Instead, James silenced and suspended Pusztai on the morning of Wednesday, 12 August, and no such press release went out. According to Pusztai:

James started to use the official restrictive rules under which all scientists have to work in our academic system [in Britain]. First he suspended me, then, by instituting an audit, he gagged me on 12 August. All my data was confiscated. My phone was redirected to his office and my emails were intercepted. The director then wrote me a series of letters in which he explicitly threatened me with legal action if I spoke to anyone in or outside the Rowett about our work. He also warned Rowett staff of the dire consequences if anyone spoke to me (Pusztai 2000).

Not only was Pusztai unable to speak about his research to clarify errors or defend his methods, but he lacked access to his data and his lab team in order to conduct further studies that might have boosted the credibility of his claims. Pusztai recalled:

Then I had been gagged for seven months, I was threatened by legal action if I say anything without the director’s prior written permission. So I kept my silence. So all the information which most people knew about had emanated, from this seven months, in which I could not correct anything, I couldn’t say anything. So all the information, the good, bad, and the indifferent, all came from the institute, all from the scientific establishment...I was rather busy, but we were quite literally in a vacuum. Because as we understood later, as it transpired, the Director sent out an email to all staff members to stop talking to us. We were sent to Coventry (English saying), an expression when you are separated from any member of the institution or the workplace. And indeed this is what happened. People didn’t talk to us. We were almost feeling like lepers. And that was socially, in this institute where you knew everyone and they avoided you, there were no conversations (Pusztai 2003).

Pusztai made it clear that his Rowett colleagues had not begun avoiding him because of a sudden change in their evaluation of his professional or personal competence. He explained that all of the retired members of the institute “rallied around” him, and several of them later signed the scientific memorandum supporting him (Pusztai 2003). James thus had invoked his power as director to isolate and silence Pusztai. Aside from

signaling institutionally-sponsored resistance (against a scientist and the credibility of his work), this action created significant openings for further action by other actors.

As with the Con A controversy, *the cause* of James' drastic switch from supporting Pusztai immediately after the television appearance and agreeing to release a clarification to the press that would have vindicated Pusztai, to 'sacking' him the day after, did not alter Pusztai's experience of being suspended and gagged. Indeed, there is no clear documentation of James' change of heart, and his public statements were somewhat elusive and contradicted other documented evidence (Rowell 2003, p. 87-99; 1998). A mixture of evidence, however, points to the influence over James by British Prime Minister Tony Blair's office. Pusztai recalled:

[I]t is still debated by many people what it was that happened. Most people think there was some political pressure on the director by the biotech industry via the political establishment, most particularly by Tony Blair himself or one of his top aides. That is reasonably certain even though we may not be able to document it. There had been several, at least two telephone calls to the director on that fatal Tuesday afternoon (Pusztai 2003).

At least two media articles appeared in February 1999 that obliquely supported this assertion. *The Guardian* reported, "Some of the scientists who have viewed the evidence believe that the circumstances surrounding Dr Pusztai's removal and the closing down of his research team cannot be understood outside of political and commercial [sic] parameters" (Gillard, Flynn and Rowell 1999). *Science* quoted Alan Simpson, Labor MP (Member of Parliament) as commenting, "This raises questions about the extent to which the biotech industry seeks to permeate every level of government" (Enserink 1999).

Rowell (2003) examined the question in great depth, presenting several distinct lines of evidence pointing to telephone calls made by Blair's office to James on Tuesday, 11 August 1998. Two Rowett employees informed Pusztai that two phone calls were put

through from the Prime Minister's office to James on that afternoon (p. 89). Later a person in senior management at the Rowett informed Pusztai and Bardocz that "Bill Clinton had phoned Blair and told him to sort out the problem" (p. 89). Stanley Ewen (the scientist who later published with Pusztai in *The Lancet*) recalled learning the same information through a fourth source, a senior manager within the Rowett. Ewen sat next to Professor Asim Dutta-Roy at an orthopedic fundraiser in Aberdeen on 24 September 1999. They engaged in informal conversation, and Ewen brought up James' about-face in supporting Pusztai. Dutta-Roy explained that "there wasn't one, but two phone calls from Tony Blair on Tuesday," and Ewen understood the explanation as phone calls from Monsanto to President Bill Clinton, to Blair, to the Rowett (p. 89).

Ewen says, when he heard this, his 'jaw dropped to the floor. 'The conversation is sealed in my memory', he recalls. 'He [Dutta-Roy] very quickly cleared off, very sharpish. I think he realized that he had let the cat out of the bag. That was the feeling I got. Immediately great remorse overcame him and he went off very quickly" (p. 90).

Professor Robert Ørskov, who worked at the Rowett for 33 years, directed the Rowett's International Feed Resource Unit, consulted for the UN FAO, and was a fellow of the Royal Society of Edinburgh, was also told about the phone calls (p. 90). He commented:

Clinton rang Blair and Blair rang James – you better keep that man [Pusztai] shut up. James didn't know what to do. Instead of telling him to keep his mouth shut, they should have told him to say it needs more work. But there is no doubt that he [James] was pushed by Blair to do something. It was damaging the relationship between the USA and UK, because it was going to be a huge blow for Monsanto, if it was the cauliflower mosaic virus [CaMV] promoter which was the method they used for genetic modification (p. 90-1).

Rowell also wrote that James was "adamant the phone call never happened." He quoted James as saying, "There is no way I talked to anybody in any circumstances...It's a complete pack of lies. I have never talked to Blair since the day of opening of Parliament in 1997" (p. 91). Thus, without documentation, the phone calls remained unproven, but

they remain an important part of the stories (performances) surrounding the Pusztai Potato controversy.

9.3 *Resistance in Audit's Clothing*

As mentioned above, James initiated an audit of Pusztai's work, which corresponded with his suspension and 'gagging'. James testified:

I immediately invoked a system which I was familiar with in the Medical Research Council. In other words I suddenly had to say, "Hang on, there is confusion here. We must not allow confusion to occur in something of such enormous public interest. Therefore, please, Dr Pusztai, stand aside. I will appoint people with great authority and independent of me or the Scottish Office or anybody else to assess all the data that seemed to have been talked about" (1998).

Rowell (2003) argued, however, that only one of the four members on the audit committee was "external," the group lacked a nutritionist despite reviewing nutritional studies, and the audit team took only ten hours to review three years work (p. 97-8). From this perspective, the audit – a performance of independent and thorough review – lacked the legitimate authority to pass judgment upon Pusztai and his work, creating the potential for creating negative judgment disproportionate to actual expertise and credibility.

The Audit Committee produced a report mixed with critique and support of Pusztai's research, but the summary comments condemned Pusztai rather soundly.

"The Audit Committee is of the opinion that the existing data do not support any suggestion that the consumption by rats of transgenic potatoes expressing GNA has an effect on growth, organ development or the immune function." Thus the previous suggestion that the research results demonstrated adverse effects from feeding genetically modified potatoes to rats was unfounded (Rowett Research Institute 1998a).

This summary sentence, referred to by many media, completely undermined the credibility of the Pusztai Potato study. Because audiences had no access to the

controversies around the composition of the audit committee, its rushed timeline, or its failure to consider Pusztai's perspective on his own data, the "Audit Committee" carried significant weight as a credible and powerful jury of scientists.

Several aspects of the report did provide some vindication of Pusztai, but they rarely appeared in media coverage. As *Science* reported, the audit acknowledged that the transgenic GNA potato studies had been carried out (Enserink 1999), which countered some of the discourse that painted Pusztai as a confused and sloppy scientist. Rowell (2003) noted this as well, explaining that the audit included dates of completed experiments, showing that the feeding experiment had finished well before the filming of the *World in Action* program. In addition, the committee declared that the immune response results were "in most cases, far too variable to reach statistical significance," which actually bolstered the argument that in *some cases* the data *were statistically significant*. If some data were statistically significant, Pusztai had the right to at least suggest the claims he made on *World in Action* and call for more research. For Professor Rhodes, who later signed a memorandum backing Pusztai, the audit report did not match the conclusions. He suggested that "a 'fairer' conclusion would be that the experiments 'have shown statistically significant alterations in lymphocyte function', which deserve further study'" (Rowell 2003, p. 98-9). Unfortunately for Pusztai, the performance of the audit report as the unraveling of Pusztai's conclusions had much greater momentum than alternate interpretations such as Rhodes'. Thus, the audit functioned to denigrate Pusztai's contrarian science.

Three actions surrounding the audit gave greater insight to the action as part of a larger strategy to challenge Pusztai's science. Pusztai explained:

In the press release by the institute, the institute director said that after the audit I *will*, Dr. Pusztai *will* retire. So you notice that it was not dependent on the outcome of the audit. It was to retire. The British are hypocrites. They did go through the business of the audit, but the result had been predetermined and arranged and it was actually announced” [emphasis reflects speaker’s intonation] (Pusztai 2003).

Indeed, the Rowett’s press release of 12 August stated: “This morning the director suspended Dr. Pusztai from all responsibility...and Dr. Pusztai will now retire from the Institute” (Rowett Research Institute 1998b). Media picked up on this message and reports indicated that Pusztai’s career had essentially ended (“Health Genetics Scientist Suspended” 1998; Highfield 1998a). If James had no intention of adjusting his treatment of Pusztai according to the audit, the audit and the simultaneous forced retirement resemble a strategy of suppression rather than inquiry and discipline. Oddly, *The Telegraph* reported on Thursday, 13 August: “Prof James stressed that it remains possible that Dr Pusztai will be proved right. He said: ‘But he was wrong about the details of his experiment, as portrayed to us, and it is because of that second point that I have decided that he ought to retire because he did mislead us and has admitted to that’” (Highfield 1998a). The preponderance of evidence – other than James’ statement – suggested that Pusztai never admitted to misleading anyone about his experiments. Thus, James appeared to have judged Pusztai’s incompetence sufficiently enough to force retirement in advance of the audit.¹⁴¹

Second, Pusztai’s wife, Susan Bardocz, was also suspended by James in relation to the audit report. She had requested to be relieved of her duty during the audit, but upon its completion was informed that she, too, would be suspended. Asking for a reason, she

¹⁴¹ Pusztai provided further evidence of the audit’s lack of legitimacy: “I discovered later that the director had no right to set up the audit because I was not accused of scientific fraud by the Rowett – the only legitimate scientific reason for an audit. Drawing erroneous conclusions from our GM-potato work – the offence of which I was wrongly accused – was not a serious enough offence to warrant an audit. To my mind, the entire point of the audit was to create a ‘show trial’” (Pusztai 2000).

was told, “because you are Arpad’s wife” (Pusztai 2003; Rowell 2003). Bardocz stood up for herself, intimating with irony that such rationale would “stand up beautifully in court.” A “harsh silence” followed, and she was then advised to keep a “low profile” until she reached the age of early retirement – the deal she struck with the Rowett included her promise not to speak of the controversy to anyone (Pusztai 2003).

Third, when Pusztai’s suspension did end after the audit, he was effectively prevented from continuing his work as a scientist. Pusztai recalled:

Even with me, after the audit, theoretically my suspension ended. So I could have just gone back to the work, in a sense. But that was a theoretical business, in practice what happened was, I had four graduate students, PhD students at the time. They were straightaway detached from me. They were attached to other people. All the group members including the foreign EU postgraduates and postdoctoral, were personally my postdoctorals, were detached and attached to other groups. So I was left without a lab, without any people. So theoretically I was no longer suspended, but I was more or less confined to my office. Because I had no lab. Susan [his wife] was told that she would be allowed to continue with work which was originated by her [nothing to do with GM], but of course she didn’t have any staff either. So it is very nice on paper and it has been said time and time again that we were quite free to do more work. Our suspension was not there. But we were not given any opportunities to do it (Pusztai 2003).

The audit thus occurred in a context in which Pusztai and his wife were both silenced and isolated professionally. Their experience was not of singular actions that challenged their research or their professional identities, but of a collection of action. That collection – whether coordinated by higher political influence or not – constricted the possible responses by Pusztai and Bardocz, as detailed in Part Three.

9.4 *Lancet Publication*

As discussed previously, Pusztai always intended to publish the Pusztai Potato study in a peer-reviewed scientific journal. Some of the criticism he endured revolved around his choice to speak about his research to the public media before scientific

publication – and much discourse referred to the benefit of Pusztai publishing his data officially. In a press release in February 1999, the Rowett Institute said:

[T]he Institute has always encouraged him to submit his findings for publication in a reputable, peer reviewed scientific journal and continues to do so. Dr Pusztai's experiments and conclusions could then be scrutinised by all scientists working in the field and made available to the public and policy makers (Rowett Research Institute 1999).

Consistent, however, with the Chapela Maize and Losey Monarch cases, the publication of the research in a prestigious journal failed to quell controversy. Instead, discourses emerged that criticized the methodology of the study, the process of peer review, and the editorial decisions by *The Lancet*. The following analysis treats the challenges to Ewen and Pusztai's (1999a) *Lancet* publication as a performance.

The prelude: backstage revolts and threats

On 11 October 1999, several days before the appearance of the Ewen and Pusztai publication, *The Independent* reported that peer reviewers had “found the study to be defective in design, execution and interpretation,” but that *The Lancet* had chosen to publish the study “on the grounds that publication of even flawed research could be in the public interest” (Connor 1999b). This general statement undermined the credibility presumed by any published peer reviewed article, especially those appearing in prestigious journals such as *The Lancet*. The article quoted one peer reviewer who broke the code of anonymity:

One referee, Professor John Pickett, an authority on plant chemistry, is so outraged by the journal that he has decided to voice his concerns in public. “It is a very sad day when a very distinguished journal of this kind sees fit to go against senior reviewers,” said Professor Pickett, the head of biological and ecological chemistry at the government's Institute of Arable Crops Research at Rothamsted near Harpenden in Hertfordshire.

Breaking the tradition of anonymity and public silence communicated a strong message that *The Lancet's* editorial process had gone awry – Picket essentially claimed higher status than the editorial board on the basis of his ‘seniority’ and perhaps the obviousness (to him) of the shortcomings of the Ewen and Pusztai manuscript. *The Independent* quoted other referees who disparaged the publication: calling Ewen and Pusztai’s conclusions “wild speculation”; re-interpreting the data as only supporting the obvious conclusion that rats fed raw potatoes suffer nutritional deficiencies; and criticizing the statistics, methodology and analysis as “flawed” to the point that if the manuscript had been a doctoral thesis, it would have been rejected. In addition, *The Independent* cited three other sources with high scientific credibility who lambasted the publication: Steven Cox, the Royal Society’s executive secretary, who demanded that *The Lancet* mention the reviewers’ “reservations”; John Gatehouse, a scientist at Durham University and former colleague of Pusztai, who referred to “unsupported assertions” and “anecdotal” aspects of the study and commented that the claim that GM plants caused undefined health problems was “simply unscientific; it is the attitude of medieval witchcraft trials”; and Professor Martin Chrispeels of UC San Diego who judged simply, “This isn’t science. It wouldn’t be published in a serious plant biology journal. Their conclusion is not correct” (Connor 1999b). The level of animosity, especially the ‘witchcraft trials’ reference, echoed the assumption of promotional science that contrarian science is irrational and a-scientific.

Although not announced until 1 November, *The Guardian* reported that a leading member of the Royal Society had threatened *The Lancet's* editor, Richard Horton, just two days after *The Independent* published Picket’s announcement:

Dr Horton said he was called at his office in central London on the morning of Wednesday October 13, two days before the *Lancet* published a research paper by Arpad Pusztai, the scientist at the centre of the GM controversy. Dr Horton, editor of the *Lancet* since 1995, said the phone call began in a 'very aggressive manner'. He said he was called 'immoral' and accused of publishing Dr Pusztai's paper which he 'knew to be untrue'. Towards the end of the call Dr Horton said the caller told him that if he published the Pusztai paper it would 'have implications for his personal position' as editor (Flynn and Gillard 1999).

Horton refused to name the caller, but *The Guardian* identified him as Peter Lachmann, the former vice president and biological secretary of the Royal Society and president of the Academy of Medical Sciences. Lachmann acknowledged phoning Horton with criticism, but denied making threats or calling Horton "immoral" (Flynn and Gillard 1999). Even assuming that neither man intentionally misled the reporter, Horton clearly experienced the interaction as a harsh attack on the credibility of the Ewen and Pusztai manuscript. More significantly, the public and scientific audience that learned of this phone call through the popular media 'witnessed' an example of politically-charged intimidation against a scientist of extremely high status – the editor of *The Lancet*. As part of the unfolding performance, this communicated that the forces resisting the Pusztai Potato research viewed themselves as above the lines of traditional authority in the scientific community.

Act 1: Presenting "Pusztai Potato," with reservations

The Ewen and Pusztai article appeared in the 16 October 1999 issue of *The Lancet*, along with a lead editorial by Richard Horton, a scientific critique by Harry Kuiper, a letter by the Royal Society's Vice President Patrick Bateson responding to an earlier letter published in *The Lancet* by Ewen and Pusztai, and a related research article by Brian Fenton, Kiri Stanley, Steven Fenton, and Caroline Bolton-Smith.

Horton's (1999) editorial, entitled "Genetically modified foods: 'absurd' concern or welcome dialogue?" led with a critique of discourse by prestigious scientists (including Lachmann) who lamented the public's concern and mistrust of GM technology. He announced that six reviewers had peer reviewed the Ewen and Pusztai manuscript, and that even after three rounds of revision, the committee of six failed to unanimously recommend publication. Horton clarified that part of the motivation for publishing Ewen and Pusztai was to put their research in the stream of official scientific discourse – published in a journal of record wherein critique and reply might occur. Horton stated clearly that publication did not represent a "vindication" of Pusztai's earlier work. Thus, the editorial offered a mixed endorsement of Ewen and Pusztai's research. On one hand, Horton's explanation detracted from the automatic credibility bestowed upon peer reviewed articles in *The Lancet* that would be assumed for Ewen and Pusztai's published manuscript. On the other hand, Horton framed the potential bias of the scientific community against work like Pusztai's as a real problem in need of response. In the language of this dissertation, Horton proclaimed the unfortunate domination of promotional science in squeezing out opportunities for contrarian science. His final paragraph was illustrative:

The comments by Lachmann, Sykes, and Gosden are therefore disappointing because they reflect a failure to understand the new, and apparently unwelcome, dialogue of accountability that needs to be forged between scientists and the public. Risks are not simply questions of abstract probabilities or theoretical reassurances. What matters is what people believe about these risks and why they hold those beliefs. Ewen and Pusztai's data are preliminary and non-generalisable, but at least they are now out in the open for debate, as are the results, also published in today's *Lancet*, of Brian Fenton and colleagues. Only by welcoming that debate will the standard of public conversation about science be raised. Berating critics rather than engaging them-and criticising reports of research, as the Royal Society did with the Pusztai data, before those data were reviewed and published in the proper way-will only intensify public scepticism about science and scientists.

The final sentence, a clear jab at the Royal Society, reflected an ongoing conflict between Horton and leaders of the Royal Society with regard to their involvement in the Pusztai Potato case (see Rowell 2003, Chapter 6).

Fenton et. al (1999) published a research article entitled, “Differential binding of the insecticidal lectin GNA to human blood cells.” Although the authors made no specific claims about the safety of genetic modification as a process, their study questioned the possible health consequences of plant lectins in the human diet. Specifically, Fenton et al. tested the ability of snowdrop lectin (GNA) to bind to human white blood cells. While not a significant contribution to the Pusztai Potato controversy, the appearance of this article reaffirmed *The Lancet*’s commitment to providing a forum for discussing possible negative effects of GM food crops. In opposition to the promotional narrative that GM critics are anti-scientific and irrational, this research article marked the territory of unexplored consequences of GM as scientifically relevant and important.

Kuiper’s (1999) commentary, “Adequacy of methods for testing the safety of genetically modified foods,” criticized the Ewen and Pusztai manuscript on technical grounds. Kuiper’s points included: 1) lack of compositional consistency between GM potatoes and parental lines; 2) study diets were protein deficient; 3) inconsistent biological changes in rats’ digestive systems, potentially indicating nothing more than “adaptive” changes to diets of raw potato starch; and 4) too few animals per group. Kuiper concluded, “Therefore the results are difficult to interpret and do not allow the conclusion that the genetic modification of potatoes accounts for adverse effects in animals.” The commentary went on to criticize the significance of the Fenton et al. publication and generally defended current safety testing of GM food technologies as

adequate. In sum, Kuiper challenged the validity and significance of Ewen and Pusztai's article and echoed the promotional narrative that regulatory science had thus far kept up with developments in agricultural biotechnology.

Bateson's (1999) correspondence, "Genetically modified potatoes," addressed a letter published in the 21 August 1999 issue of *The Lancet* by Ewen and Pusztai. He emphasized the rigor of the Royal Society's review of the Pusztai Potato study earlier that year, and noted Pusztai's apparent failure to cooperate in the process. An important component reassured the audience of the independence of the review process:

The working group was careful to choose referees with relevant expertise and with no vested interest. The referees had no previous involvement that might be regarded as potentially distorting their judgment, and had not commented publicly on the research. The anonymity of such referees is preserved so that they may remain free to make a frank and honest judgment based solely on the quality of the research.

By reminding readers of the previous judgment of the Royal Society of Pusztai's data as flawed and inadequate, Bateson denigrated the Ewen and Pusztai publication – without mentioning the possibility that new or alternate data might have contributed to *The Lancet* publication (see Rowell 2003, p. 115). His emphasis on the independence of reviewers countered the narrative that linked critics of Pusztai to vested interests in biotechnology. In addition, Bateson's description of Pusztai's lack of cooperation strongly undermined Pusztai's professional credibility, creating a narrative of a mischievous and marginal scientist afraid to face the challenge of 'true' scientific experts. Lastly, the correspondence challenged the professional reputation of *The Lancet* by showcasing the Royal Society's confident and expert review of Pusztai's work with *The Lancet*'s apparently schizophrenic support of its own publication. In a sense, if critics could convince their audiences that the Ewen and Pusztai publication was an aberration

rather than normal peer-reviewed science, they could effectively strip the credibility normally bestowed by peer review and publication.

Act II: Lancet resistance continues

The 13 November issue of *The Lancet* included a number of pieces of correspondence about the Ewen and Pusztai publication.

Allan Mowat, from the Department of Immunology and Bacteriology, University of Glasgow, published a commentary on Ewen and Pusztai's article that offered extremely technical criticisms of the methodology. While Mowat acknowledged that Ewen and Pusztai raised "intriguing questions," the extensive challenges to their methods undermined the major claims of the original article. He presented his perspective as rather obvious, suggesting that Ewen and Pusztai had made unnecessary errors; he concluded, "Appropriate methods for studying the enteropathic effects of lectins are available and are comparatively simple and inexpensive" (Mowat 1999).

Carl Feldbaum, of the Biotechnology Industry Organization (BIO), wrote a scathing critique of *The Lancet's* decision to publish Ewen and Pusztai's article. He wrote that "*The Lancet* has placed politics and tabloid sensationalism above its responsibility to report and assess new science...[and] has jeopardised the journal's credibility, especially among readers and contributors in the scientific community" (Feldbaum 1999). He jabbed at *The Lancet* for a biased political orientation and simultaneously characterized promotional science as the noble endeavor: "I doubt if *The Lancet* would have published Ewen and Pusztai's research if it had implied the safety of biotech foods. But that is fine. Those who work hard to apply biotechnology to agriculture have no interest in flawed

data.” This quote simultaneously challenged the scientific credibility of the journal and the quality and significance of Ewen and Pusztai’s data. Lastly, Feldbaum presented a standard portion of the promotional narrative:

In the USA, biotech crops and foods have been tested more than any other agricultural products in history. We have a regulatory system that applies science-based policies to guard the health of consumers and the environment. That system would have trashed Pusztai’s potatoes if they had been submitted for approval. Too bad *The Lancet* failed to exercise such oversight on research submissions.

Ironically, in reassuring his audience of the quality of the U.S. regulatory system, Feldbaum claimed that Pusztai’s potatoes would have failed to win approval, thus supporting Ewen and Pusztai’s conclusions that exposed health concerns of the GM potatoes they studied. This contradiction was similar to critics of Quist and Chapela who derided their methods but accepted their conclusions as obvious. In this subtle rhetorical move, Feldbaum managed to promote the science of agbiotech as safe for consumers while dismantling the credibility of a prominent scientific foray into questioning the safety of GM crops.

A team from the Histopathology Unit, ICRF (London) published a short and limited critique of Ewen and Pusztai’s methods, but with a tone of collegiality. The commentary ended with a call for further research: “We hope these comments will help to ensure that if these studies are repeated (as they should be), robust, rapid, and reliable methods for assessment of cell proliferation are used” (FitzGerald, Goodlad and Wright 1999). Thus, while calling into question the quality of the published article, the authors supported the endeavor and made suggestions to advance the progress of contrarian science.

Peter Lachmann wrote a very measured critique of Ewen and Pusztai's article, listing his criticisms numerically and avoiding any emotional language. This presentation contrasted strongly with the publicized story of him being accused of threatening Horton in advance of the Ewen and Pusztai publication). Lachmann's five points were: 1) a group of rats fed potatoes spiked with GNA should have been included, 2) control data for rats fed a normal laboratory diet should have been included for comparison, 3) the article failed to mention whether the assays were conducted "blind" in a coded methodology to prevent experimenter bias, 4) Ewen and Pusztai's demonstration of changes in the rat gut did not automatically translate into known or referenced pathologies, 5) the lack of a pre-existing hypothesis probably undermined the statistical significance of the reported comparisons (accusation of "data dredging") (Lachmann 1999). Collectively, these five points challenged the conclusions of Ewen and Pusztai and suggested a certain professional carelessness of the authors that further lessened their credibility as individual professionals.

Sir Aaron Klug, President of the Royal Society, published a letter defending the organization's decision to review Pusztai's unpublished data in May 1999 and criticizing *The Lancet* for publishing the Ewen and Pusztai manuscript. Klug justified the Royal Society's involvement in judging Pusztai's science:

The Lancet criticised the Royal Society last May for its "breathtaking impertinence" in reviewing Arpad Pusztai's work before it had been published. Richard Horton now repeats that criticism [16 October issue (Horton 1999)]. We commented on Pusztai's unpublished work because he himself had commented on it, so extensively that it had become a matter of public interest. Since a one-sided debate was raging on the back of unvalidated experimental data, the Royal Society had a duty to examine such evidence as it could secure from all sources, including Pusztai himself. That is impertinence only if you endorse scientists flouting normal practice and rushing to the press with unvalidated data and invalid conclusions (Klug 1999).

In this first paragraph, Klug focused on Pusztai's unscientific behavior – “flouting normal practice” by extensively commenting on unpublished data (a curious accusation given Pusztai's long period of silence enforced by the Rowett) and “rushing to the press.” Klug's presentation of the Royal Society's behavior, by contrast, exuded professionalism, responsibility, and even generosity (examining evidence provided by “Pusztai himself.”). His second paragraph shifted the critical eye to *The Lancet*:

In introducing the Ewen and Pusztai research letter Horton helpfully describes the ambivalence of the referees and emphasises the value of having the data out in the open. He also states that the data are nongeneralisable. It is therefore surprising that the journal allowed the paper to appear with two general conclusions in the final paragraph. In the circumstances, Horton's comments on the “failure to understand the... dialogue of accountability” are somewhat ironic (Klug 1999).

While Klug's comments laid bare the ongoing feud between the Royal Society and *The Lancet*, the letter called attention to the unusual presentation of Ewen and Pusztai's article in the first place, and argued that *The Lancet*'s own introduction should have negated Ewen and Pusztai's ability to make general claims.

9.5 Summary

Pusztai Potato offers several insights into resistance as performance. First, struggles over the credibility of contrarian science (or scientists) implicate struggles over credibility between other actors and institutions (e.g., between *The Lancet* and The Royal Society). From the perspective of a public audience, these webs of arguments make the controversy that much more complicated to understand. Second, the narrative context of critique has enormous implications for how resistance is experienced. The team from the Histopathology Unit that criticized Ewen and Pusztai's methods did so within a narrative that supported their goals. This contrasted sharply with similarly technically-based

challenges embedded in narratives of promotional science, or those that fell silent on the larger context of such research. Third, what might appear as stable scientific institutions in general, become contested and malleable within the context of scientific controversy. The audit initiated by James fell short of the idealized practice of an independent, external review – according to Puzstai the audit was unwarranted, lacked sufficient independence, and had no relevancy to the decision to force his retirement. In a similar manner, the institution of peer review was torn apart in the context of Ewen and Puzstai’s publication. Despite significant discourse within the journal and mainstream media, confusion remained over what the process of peer review had actually accomplished in terms of measuring the credibility of a piece of science. In this sense, resistance can work to dismantle not just a piece of research, or a single scientific career, but the legitimacy of established scientific practices.

9.6 Conceptualizing and Mapping Resistance

Part Two has provided an empirical basis for looking at the challenges that three contrarian scientists and their research faced and experienced. With respect to the metaphor of contrarian science as the first spark of dissent, these challenges have appeared variously as fire-screens that knocked sparks back into the fire to heat them up before eventual escape (journal reviews), winds that blew the sparks toward a less flammable target (re-framing the significance of results or calling for additional research), stomping feet that smothered the spark before it had a chance to burn (professional suspension), or buckets of water that soaked nearly everything (denial of

tenure). These actions, which I have collected into the heuristic category of resistance, have impacts upon contrarian scientists as well as broader audiences of the controversies.

Although many authors have provided tremendous insight into the philosophical, sociological, and political aspects of 'resistance' in scientific controversy, most fail to capture the broad landscape and heterogeneity described in this dissertation. Merton's (1973 [1942]) notion of "organized skepticism" highlights the contribution of ritualized critique in the practice of science, but ignores the political context of resistance. Fleck's (1979 [1935]) theory of the development of scientific facts acknowledges the role of thought collectives (a nod to different perspectives and assumptions held in different disciplines), but falls short of exposing the significance of links between certain communities of thought and other centers of social power. Latour's (1987) "trials of strength" expose the practice of accumulating allies as a political and intellectual process of making 'facts' that are too fortified to challenge, but the approach falters when extended to cases of suppression or intimidation (corrupted or 'fixed' trials of strength with participants bringing very different reservoirs of 'strength' to the contest). Gieryn's (1999) analysis of controversies through the lens of boundary-work calls attention to the multiple strategies to attack the credibility of a scientist or scientific claim, but the model too readily assumes that the response of a dissenter should be to play the legitimacy game and rhetorically expand the boundary to broader inclusion. Martin's (1999) focus on intellectual suppression and censorship foregrounds the role of social power in controlling scientific speech and action, but falters a bit when attempts at suppression

increase the visibility (and even credibility) of contrarian science within a controversy.¹⁴² Likewise, Proctor (1995) demonstrates the role of powerful interests to control the paths of knowledge and ignorance around scientific issues with high political and economic stakes, but the grey area between suppression and legitimate scientific challenge remains relatively murky.

This dissertation does not neatly respond to each one of these theoretical challenges, but begins to approach the heterogeneity of resistance. As the cases demonstrate, resistance takes many forms, and that heterogeneity can present contrarian science with complementary challenges.

¹⁴² In other words, suppression may describe the motive, but not the outcome. The act of resisting can increase the stage presence of contrarian science, by drawing attention to issues of larger significance, by expanding the audience, or by unintentionally connecting with tropes of corruption, suppression, or greed.

Table 3 summarizes the diversity of resistance in terms of the targets of the resistance, the messengers, the apparent intentions or goals, and the audiences exposed to the resistance.

Table 3: Heterogeneity of Resistance

Target	Messenger	Intention	Audience
<ul style="list-style-type: none"> • Research question • Methodology • Data • Results or interpretation of data • Significance • Manuscript • Discipline • Publication • Person • Professional credibility • Institutional credibility • Political and financial credibility 	<ul style="list-style-type: none"> • University scientist • Government scientist • Industry scientist • Journal referee • Political official • Editorial board • University administration • University press office • Professional advocate (NGO, trade association, etc.) • Organization 	<ul style="list-style-type: none"> • Silence • Suppress • Intimidate • Influence • Discourage further research • Improve further research • Improve quality before publication • Confuse • Shift attention from one scientific question or controversy to another • Prioritize ‘scientific’ concerns over cultural, social, or ethical issues • Reduce credibility 	<ul style="list-style-type: none"> • Contrarian scientist • Scientific community (narrow field or broad) • Regulatory agencies • Political constituencies • Public media • Scientific media • Would-be contrarians • Organization

This characterization has several implications. First, the wide diversity of *targets* suggests that defending against resistance becomes increasingly complex as multiple sites of vulnerability are challenged. Responses thus may be limited both by choice and capacity. Second, the mix of *messengers* with high credibility among diverse social sectors can complement one another without necessarily cooperating or coordinating strategies or tactics. University scientists, for example, may challenge contrarian science in a way that vastly increases the reach and power of a corporate campaign to discredit a

piece of research because of its policy implications. Third, while motivation is difficult to assess empirically, the *intentions* of resistance appear to vary widely and have diverse effects. Actors with different intentions risk being judged as working on the ‘same team’ even if motivation and values differ enormously (e.g., university scientists ‘in bed’ with industry because they challenge contrarian research). Fourth, different *audiences* have different quantities and qualities of power to enact social or intellectual change. No single actor (supporter or challenger) can control the totality of audiences in a given controversy as it unfolds. Fifth, this mode of analysis encourages the consideration of resistance as it is experienced and witnessed rather than simply as disaggregated actions of criticism, reprimand, censure, or pressure. I do not propose a grand theory of resistance to contrarian science. Instead, the cases and associated analyses point to the need to continue asking difficult questions about the practice of resistance, in all its diversity, and the potential to develop new conceptual tools that comprehend the complexity of experiences and performances of resistance.

PART THREE

RESPONSES OF DISSENT

Introduction

The pathway to, and character of, scientific dissent is not entirely explained by understanding the scientific context of the controversy, the performances of contrarian science, and the patterns of resistance to contrarian scientists and their claims. In researching the three case studies, I witnessed a third phase of the emergence of scientific dissent: *how contrarian scientists respond to the resistance they face – how they, in fact, become scientific dissenters*. While these responses depend upon the stories presented in the dissertation thus far, they are not determined by them. Elements of personal choice, institutional involvement, and political opportunity each play a role. More generally, pathways to scientific dissent are iterative,¹⁴³ but it is useful to consider a set of responses as an end-point in this analysis – pausing to reflect upon the significance of heterogeneous forms of scientific dissent.

I view scientific dissent as a distinctive performance of science. The potential for dissent emerges when momentum builds behind a particular formation of science – in the case of agbiotech, promotional science. The first sparks of dissent appear with contrarian science, but at this stage, before resistance has occurred, dissent remains a latent phenomenon. In other words, dissent becomes manifest only after promotional and

¹⁴³ By iterative I mean to suggest that experiences of resistance and dissent inform later actions and choices by scientists, their allies, and their opponents.

contrarian forces have engaged one another in struggle. While Losey, Pusztai, and Chapela knew of the contrarian nature of their claims, they submitted their work to the scientific community with at least some hope of having their research folded into the ‘normal’ path of knowledge production. What they experienced, instead, were patterns of resistance that then provoked a diversity of responses from themselves and their allies. The value of waiting to label or categorize scientists as ‘dissenters’ until they have faced resistance becomes clear in the intricacies of response outlined in this chapter. Here, dissent becomes a performance – in the face of academic, disciplinary, commercial, and political pressures for the science of agricultural biotechnology to evolve in certain ways.

The following analysis of the diversity of scientific dissent acknowledges a spectrum of strategies, defined by the poles of *agonistic dissent* and *dissident science*. The spectrum describes the degree to which contrarian scientists respond to resistance within conventional norms of scientific discourse. The political theorist, Chantal Mouffe, in *The Return of the Political* (1993), envisions a new democratic order using the term “agonistic pluralism.”¹⁴⁴ This model of engagement respects the need for disagreement and controversy as a path to negotiated governance, but redefines the approach to conflict by shifting how opponents perceive one another:

[W]ithin the context of the political community, the opponent should be considered not as an enemy to be destroyed, but as an adversary whose existence is legitimate and must be tolerated. We will fight against his ideas but we will not question his right to defend them. The category of the ‘enemy’ does not disappear but is displaced; it remains pertinent with respect to those who do not accept the democratic ‘rules of the game’ and who thereby exclude themselves from the political community (p. 4).

For Mouffe, agonism represents an attractive alternative to ‘antagonism’, which disrupts the ability of a community to use conflict constructively by violating the norms of

¹⁴⁴ Earlier literature in political theory has explored similar notions (e.g., Berlin 1969).

engagement and creating enemies rather than opponents. As described below, antagonism does not map neatly onto dissident science, but importing the idea of agonistic dissent into controversy studies creates a way to reference responses that adhere to the ‘rules of the game’ of scientific debate. In other words, *agonistic engagement follows the norms of scientific discourse when facts are contested*. While I view these norms as constructed, and therefore contestable and malleable, at any given moment these norms present themselves as structures commanding a degree of respect.¹⁴⁵ Importantly, the performance of agonistic dissent reifies these structures (rules) by reinforcing assumed boundaries between politics and science (keeping science objective and apolitical) and between scientists and publics (e.g., Gieryn’s [1999] boundary of autonomy). Practices of agonistic dissent as responses to resistance encompass a set of behaviors and rhetorical strategies well-predicted by several pillars of STS theory (Kuhn 1970 [1962]; Latour 1987; Merton 1973 [1942]), but not well elaborated in the larger context of understanding pathways of dissent.

In contrast to agonistic engagement, other responses to resistance violate the norms of scientific communication in controversy. These phenomena, mostly overlooked by theorists of scientific controversy, locate the practice of *dissident science as a form of dissent that challenges both dominant forms of knowledge and the conventional norms of scientific discourse*. Thus, dissident scientists combine intellectual struggle with social action. Unlike Mouffe’s ‘antagonism’, dissident action focuses less on the creation of enemies than on reforming relationships that control knowledge production. Such

¹⁴⁵ Daniel Kleinman makes a similar argument to gain analytical traction in the agency vs. structure debate with regard to the formation and consequences of networks of scientific practice (Kleinman 2003).

dissident strategies emphasize the political nature of scientific controversy, call attention to the institutional contexts that produce (or inhibit) research, and shift the terrain of debate away from fact-making to envisioning alternative social orders for conducting science.

The agonistic-dissident spectrum organizes my analysis of the diversity of scientific dissent in the three case studies. Chapter 10 begins with a discussion of agonistic responses – those that reside well within the norms of scientific discourse. Specifically, I describe examples of contrarian scientists responding to resistance with factual arguments and additional evidence. Second, I present a set of responses that involves the recruitment of additional scientific allies as a source of support (and power) for contrarian views. These remain mostly agonistic – in the sense that Latour (1987; 1986 [1979]) theorizes the creation of scientific legitimacy as the assemblage of allies (through the use of instruments, representations, and citations) and an enrollment of actors in a network (Latour 1999 [1983]) – but can also veer toward the dissident by revealing the political (popular) force of gathering supporters. Third, I discuss several examples of contrarians responding with claims about disciplinary territory and its relation to expertise within controversy. An agonistic move in the sense of supporting the structural separations between disciplines in science, such arguments also include a dissident character by undermining the authority of established thought collectives. Because other work in STS has characterized these strategies reasonably well, my analysis aims only to echo other theories and provide a slightly newer frame of understanding.

Chapter 11 focuses on responses that tend toward dissident science. I take a richly empirical approach because locating dissidence represents a more significant leap from existing efforts to understand the landscape of dissent. I discuss three scientific performances: 1) Black Canvas - an event at the Berkeley Art Museum to which Chapela (who boycotted the organized panel) sent a black canvas to ‘appear’ on his behalf; 2) Open Office Hours – office hours held by Chapela in front of the university administration building for five days and nights, in protest of the secrecy surrounding his then protracted tenure case; and 3) “The Pulse of Scientific Freedom in the Age of the Biotech Industry” – a panel discussion on the UC Berkeley campus organized by Chapela and including Losey, Pusztai, and Tyrone Hayes (a Berkeley professor whose research on the ecological effects of a major herbicide had generated significant resistance of the type elaborated in Part Two).

Chapter 12 extracts a set of dissident strategies from a range of performances of scientific dissent. The strategies not only clarify the underlying approaches for a variety of the tactics, but also expose the risks of moving away from agonistic engagement. In the chapter’s discussion, I review the major differences between agonistic dissent and dissident science (Table 4, p. 392) and discuss their implications for understanding boundary work that separates science from politics and scientists from publics.

Chapter 10 Expected Responses

This chapter begins to chart the territory of dissent with three strategies that broadly move across the agonistic-dissident spectrum. As explained in the introduction, STS has recognized these behaviors as important aspects of the practice of science, and scientific controversy in particular. The chapter aims to show, however, that seemingly conventional responses can begin to take on more dissident qualities.

10.1 Dispute Facts and Provide Additional Evidence

In all three case studies, the contrarian scientists responded to much of the resistance by disputing facts, defending methods, and strengthening evidence that supported their initial claims. These practices echo behavior described within traditional controversy studies (e.g. Collins and Pinch 1982) and generally satisfy expectations held by scientific audiences, evidenced by a lack of criticism of this strategy even by opponents who disagreed with the content of the claims.

Chapela Maize

Quist and Chapela reluctantly accepted *Nature's* demand of producing additional data to support their original claim and spent significant time crafting responses to letters of critique (Quist and Chapela 2002). Although they lacked control over the publication of additional evidence and technical arguments, they participated in good faith with the clear goal of defending their claims in a key forum for natural scientists. Likewise, in official letters composed during his tenure case, Chapela argued against claims that his

work had always provoked controversy with detailed explanations of how his previous research had become accepted over time:

My first contribution to scientific advance, my PhD work on endophytic fungi, was published at a time when the established view was that such a functional group did not exist -or was represented only in very limited cases. It took a decade for the work in my dissertation to be fully accepted, but now scholarly meetings are convened and books written on the topic. I believe that a similar path awaits my more recent discoveries, and I am eager to hear from confirmatory, rectifying or disproving work following on the original research from my laboratory (Chapela 2003d).

Chapela thus positioned himself as a scientist eager and willing to accept the ongoing knowledge created by the ‘march’ of science toward truth. In this way, he dissented from accepted beliefs but embraced the normative process within science of allowing evidence to define what is considered truth.¹⁴⁶

Losey Monarch

A news article that appeared in *Nature Biotechnology* in July 1999 detailed some of the resistance to the original article and described Losey’s response (Hodgson 1999). First, Losey addressed the technical issue of the density of pollen dusting the milkweed leaves (a major theme of criticism). He defended his original results as “not stringently quantitative” but unlikely to produce “systematic error in one direction or the other.” This technical claim about the reliability of qualitative data kept the controversy within traditional scientific discourse about method and interpretation. Second, Losey reported that he planned to quantify pollen concentrations to create a dose-response curve and improve controls in the next phase of research. In proposing new and better experiments, Losey acknowledged criticism but rose above it as a scientist willing to exert additional

¹⁴⁶ This insight echoes Gieryn’s (1999) observation that boundary-work reifies the significance and strength of the boundary even when it attempts to shift that boundary’s position.

effort for closer approximations of truth. Third, Losey provided strong data to undermine the criticism that the proper comparison for evaluating *Bt* corn should be fields treated with insecticides. Specifically, Losey reported that because of difficulties in spraying for the European corn borer, only 20% of corn fields are sprayed for this pest in a given year. *Nature Biotechnology* concluded, “Thus, adverse environmental effects from conventional treatments only accrue to 20% of corn plantings. In contrast, *Bt* corn already represents 30% of the area, and this proportion is increasing.” This final strategic response defended against the claim that the original study had no social value (assuming *Bt* corn eliminated vast amounts of pesticide) with hard data – albeit data about farmer behavior rather than biological interactions.

Although a variety of actors resisted the Losey Monarch study in multiple ways, significant institutional forces within and around the scientific community pushed for additional studies to clarify the questions raised by the original study. This paralleled the Chapela Maize case with respect to the follow-up studies conducted to confirm the contamination of maize landraces by transgenic DNA (conducted by various Mexican agencies), but differed from the complete lack of follow-up research on Quist and Chapela’s second finding, that of potential transgenic instability in ecological contexts. Moves to create additional evidence fall within the realm of agonistic dissent for two reasons: first, continuing the line of questioning is itself a challenge to the narratives of promotional science; and second, the reliance on, and patience for, data to emerge to increase the power of contrarian science to influence policy and public opinion maintain traditional views of science as an input to policy and of scientists as objective creators of knowledge. An article in the *Cornell Chronicle* in October 2000, quoted Losey:

I haven't changed my position [on GMOs]. We need to determine the extent of those risks as we do for any other pest management tactic. Then we can determine which tactic is the most environmentally sound and the most economically viable. I'm neither a detractor nor a proponent. I am completely open to being convinced by the data (Friedlander Jr. 2000).

This short statement reveals a great deal about the framing that surrounds dissent of an agonistic character. First, Losey referenced a risk-benefit analysis (although not by name) as the appropriate decision tool – a technical approach foregrounding science as an input and backgrounding the value judgments that inform and define a risk-benefit strategy. Second, Losey emphasized his neutral status as “neither a detractor nor a proponent,” which protected his credibility as a scientist with no political motivations. Third, he reaffirmed his commitment to the ultimate power of data – in language that de-contextualized data from any institutional, professional, or personal agency.

Pusztai Potato

Unlike Losey and Chapela, Pusztai was prevented from conducting additional research under the suspension and forced retirement by the Rowett Institute. Nevertheless, Pusztai created channels to defend the technical quality of his work and eventually did produce additional evidence to support his preliminary statements made on television – cooperating with Stanley Ewen to publish a paper in *The Lancet* (Ewen and Pusztai 1999a). Perhaps because of his suspension and forced retirement, which made the carrying on of his normal duties impossible, Pusztai produced a series of incredibly detailed responses to technical critiques – disputing interpretations, defending his methodologies, and correcting misunderstandings about his research. Examples include:

1. A complete report on all research conducted with GNA-GM potatoes, written by Pusztai in October 1998, to supplant the Audit Committee's report, which Pusztai found incomplete and misleading (Pusztai 1998).
2. An email written by Pusztai to a producer of the Equinox television program, which planned to run a segment on the Pusztai Potato controversy (Email from Arpad Pusztai to Katherine Ainger, 18 March 2000). Pusztai published this exchange on his website,¹⁴⁷ bemoaning the show's unfair treatment and failure to include his perspective. The text of the email included technical clarifications of the details of his research.
3. An extensive reply by Ewen and Pusztai, published in *The Lancet* in November 1999, directly responding to three critiques published in *The Lancet* the previous month (Kuiper, Noteborn et al. 1999; Lachmann 1999; Mowat 1999). In this reply, Ewen and Pusztai defended their experimental design (especially the issue of proper diets and controls); argued that a lack of consistency of changes did not imply an absence of concern given that the statistically significant differences that had been found undermined assumptions of substantial equivalence; differentiated their own experiments from typical industrial safety tests that measured effects of recombinant forms of *Bt* toxin (produced with *E. coli* rather than the transformed plant); defended their method of tissue fixation as standard in human histopathology; assured readers that experiments were conducted double-blind; specified their organizing hypothesis; and defended their statistical methods (Ewen and Pusztai 1999b).

¹⁴⁷ <http://www.freenetpages.co.uk/hp/a.pusztai/neenan.htm>. Accessed 4 November 2005.

These actions emulate strategies of agonistic dissent by focusing on the details of the research methods with respect to standard scientific practice.

Like Losey, Pusztai framed his position as a scientist without significant political affiliation. In an article in *The Ecologist*, Pusztai wrote:

It is worth remembering at this point that I have never claimed that all GM foods are unsafe, or that biotechnology per se is dangerous. All I have said is that my work suggested GM foods may pose dangers to human health, and that more work needs to be done on this subject, particularly as GM foodstuffs already accepted have never been tested by methods similar to those used in our GM potato studies (Pusztai 2000).

Rhetorically, Pusztai distanced himself from the ‘anti-biotech’ community but maintained his contrarian stance by calling for additional research, justified by the tentative results of his studies.

10.2 Recruit Additional Allies

A second strategy visible in two of the case studies involved the recruitment of additional allies within the scientific community. This response is well-predicted by Latour’s model of science as a quest to enroll enough allies to make challenging a particular fact unfeasible (Latour 1987; Latour and Woolgar 1986 [1979]). Although each of the three cases included traditional methods such as the use of specialized equipment and citations of articles (actions clearly toward the agonistic end of the spectrum), I briefly describe more personal examples of contrarian scientists reaching out to other scientists for support. These responses thus operate in a more dissident way because they expose the political nature of scientific support in a way that the use of citations and scientific equipment makes invisible. From one perspective, all such responses involve the recruitment of allies, but only those that make such behavior explicit work to undermine the veneer of scientific objectivity. Science derives great authority from

distancing itself from ‘popularity contests’ – breaking sharply with notions of democratic order – and when such political recruitment becomes visible it challenges conventional assumption that the scientific method protects against the tyranny of the majority.

Chapela Maize

The allies recruited by Quist and Chapela as a response to resistance did not resemble the type of actors predicted by Latour’s model. Neither scientist made moves to assemble specialists in corn genetics, gene ecology, molecular biology, or transgenic testing to defend their claims or produce additional research. While agencies within the Mexican government conducted a number of tests to measure contamination levels of Mexican maize, these findings did not function as gathering expert support for Quist and Chapela’s claims. In fact, although the studies confirmed the contamination (Quist and Chapela’s first claim), they did not use PCR as a method and therefore offered no technical support to the credibility of Quist and Chapela as competent scientists. None of the work conducted under the auspices of the Mexican government touched the questions raised by the second finding of transgenic instability. As discussed later in Chapter 11 and Chapter 12, the expanding network of actors around Quist and Chapela took on a different character.

Losey Monarch

As discussed above, Quist and Chapela conducted further research (the dot blot test) to confirm their finding of contamination, but they worked in relative isolation. Losey, on the other hand, became part of a community of scientists who organized and published further research on the ecological effects of *Bt* corn.

As discussed in Part Two, the first conference organized to review new data in November 1999, the Monarch Research Symposium (MRS), became mired in controversy because of the dominance of industry in funding the research and framing the event for the media. One month after the MRS, however, the EPA issued a data call-in for questions around *Bt* corn and monarch butterflies. In light of the recent failure of the MRS to create credible scientific consensus, the USDA spearheaded the creation of a consortium of public agencies, industry groups, university scientists, and environmental NGOs to set research priorities and distribute funding that came half from the USDA and half from industry. As a result, forty scientists participated in a second *Bt*-monarch conference in March 2000 to discuss the most pressing scientific questions that should inform the EPA's policy to re-register *Bt* corn.¹⁴⁸ Although Losey had not accepted funds from industry to present at the MRS, he did conduct research under a grant provided by the consortium (PIFB 2002). After some controversy involving questions of publication timing, confidential business information, and public access to data before the re-registration of *Bt* corn,¹⁴⁹ Losey and the other scientists eventually published their

¹⁴⁸ “At that meeting, researchers identified five short-term research objectives: 1) determine the importance of cornfields for sustaining the monarch population; 2) continue laboratory studies to determine how monarch caterpillars are affected by different amounts of *Bt*; 3) determine the abundance and location of milkweed; 4) determine monarch distribution, abundance and survival in *Bt* and non-*Bt* corn fields; and 5) collect data to see if what happens in the laboratory actually happens in the field. [The steering committee] adopted these priorities and put out a request for proposals for scientists to design studies that addressed the questions and the grants were made in April 2000,” USDA’s Hellmich says. “The plan was to develop the data and present them in a peer-reviewed scientific journal so the data and conclusions would be above reproach” (PIFB 2002, 13).

¹⁴⁹ “With the research completed in the fall of 2000, the consortium met again to analyze and interpret the data. They also set about writing the six papers that would be submitted to *The Proceedings of the National Academy of Sciences (PNAS)*, which requires that two outside reviewers examine the papers. The industry wanted the data sent to EPA in order to meet its data call-in deadline of March 2001. The scientific team, however, was concerned that the public release of the data to EPA would jeopardize their ability to publish in a respected peer-reviewed scientific journal. For that reason, the scientific team requested that industry submit the information as confidential business information (CBI) until the papers had entered publication. As CBI, the information is protected by statute and EPA can’t disclose it to the public record. As it turns

research in the *Proceedings of the National Academy of Science (PNAS)* in October 2001 (Hellmich, Siegfried et al. 2001; Oberhauser, Prysby et al. 2001; Pleasants, Hellmich et al. 2001; Sears, Hellmich et al. 2001; Stanley-Horn, Dively et al. 2001; Zangerl, McKenna et al. 2001).

While Losey clearly did not take a leadership role in forming this community of scientists, he participated willingly and recalled the process as “a brief, shining, Camelot-like period” at the “Pulse of Scientific Freedom in the Age of the Biotech Industry” (UC Berkeley, 10 December 2003). This dissertation does not attempt to explain why the Losey Monarch case unfolded so differently than the Chapela Maize case in this regard, but several hypotheses informed by the data emerge as possible contributing factors: 1) research about non-target effects of *Bt* was less threatening to vested interests than research on transgenic instability, making Losey’s study a less intimidating starting point for additional research; 2) Losey operated within an established field of entomologists (an existing community of support) while Quist and Chapela sought to stake out a new area of inquiry, gene ecology; 3) tensions created by U.S.-Mexico political and scientific relationships inhibited a more cooperative response to Quist and Chapela’s article; 4) the political context in terms of major policy decisions around agbiotech at the time of the Losey Monarch study was less intense, allowing research to proceed without threatening policy already ‘on the table’; 5) Chapela’s more dissident approach created a hostile

out, that move created a serious conflict. EPA needed to make a decision on *Bt* corn by September 30, 2001, in order to allow the corn to be bought in time for planting for the 2002 growing season. But given the long process involved in readying studies for publication and peer review, the studies were unlikely to be published in *PNAS* until after September 30th. Public interest advocates were upset, arguing that the data should be made public so that the public would have an opportunity to review and comment on the data in advance of EPA’s decision to re-register *Bt* corn. EPA responded by working out a deal with the companies allowing public access to the original data in ten reading rooms across the country starting on August 24, 2001” (PIFB 2002, 13).

environment for scientists or other actors to attempt to parse out scientific questions that could be answered by further research; 6) the resistance to Quist and Chapela was so effective that it removed the perceived need to do additional research, whereas the resistance to Losey et al. tended to open up particular lines of inquiry that seemed feasible and relevant for investigation; 7) cultural differences between Cornell and UC Berkeley created distinct environments of support, challenge, and ‘political’ opportunity.

Pusztai Potato

Unlike Losey, Pusztai took an active role in creating a network of expert support, despite prohibitions on his ability to speak publicly about his research. This response not only brought him personal comfort during a challenging time, but resulted in a public show of support by experts. A portion of an interview with Pusztai illustrates how his actions made this possible:

AP: Our scientific colleagues who had known us for a long time personally, not just as a sort of drinking mate, but also having done experiments together. I mean, no matter what Professor James or anyone else did say, they had personal experience and they knew that that was untrue [the allegations against Pusztai]. It cannot be true. It is just as simple as that.

JD: You had a community of people who were confident in your work...

AP: Yes, yes.

JD: And those folks didn't desert you during this controversy.

AP: No. Without them it would have been very difficult to survive. Now because it was important that it's not as character witnesses they would support me, but they would support what we stood for. Looking at the contract, it was obvious that in confidence, if it is not published, I can talk about science to scientific colleagues. Will let them see the things. Can ask them that it will not just be my views but their views, as well. Are these experiments and results valid? Are they good, bad? Can they serve as a way to do further research? Will they be a foundation for further research? And I gave them the results in confidence, the only thing I wanted of them is a peer review, just like when you submit a paper to a journal, you get the peer reviewer, first part to say whether it is acceptable, whether it is publishable. So that's what they gave. And then eventually, two senior people who could fuse this all together, and transform it into a publishable memorandum, and they sign it, the twenty-four of them actually signed it with their affiliation, so that it would be on record. It's not just one side of story, the Rowett and the scientific establishment; this is the other side of story, and these were all active, working scientists (Pusztai 2003).

The Guardian subsequently ran the headline, “Dr. Pusztai Vindicated! International Scientists Back Shock Findings of Suppressed Research into Modified Food.” The article called the memorandum “unprecedented,” and described how twenty scientists had demanded the “professional rehabilitation” of Pusztai (Gillard, Flynn et al. 1999).

Science reported later the same week that Pusztai had shared copies of the audit report, his own rebuttal to it, and a transcript from the *World in Action* show with numerous scientific colleagues. A protein chemist, Edilbert Van Driessche of the Vrije Universiteit in Brussels, collected the responses and spoke at a press conference in the House of Commons, stating that Pusztai’s scientific claims about immune, organ, and growth effects were valid, and that the data in the audit report “appeared to be arbitrarily selected and biased towards brushing aside the conclusions of the experimental findings.” *Science* also reported that Pusztai’s supporters had pointed to the follow-up study with Stanley Ewen as further credible evidence that transgenic potatoes had unanticipated biological effects (Enserink 1999). Thus, Pusztai’s engagement of a network of scientific allies proved to be a strong response in terms of creating rebuttals to resistance that were credible enough to catch the attention of major media.

Recruiting allies can function primarily as an agonistic strategy of dissent, but it carries a hint of dissidence, especially as shown later when the assemblage of support transcends the boundary of the legitimate scientific community. One perspective on the differences among the three cases in this regard is the different community resources available to the contrarian scientists in the face of resistance. In addition to being well on his way to achieving tenure at Cornell, Losey quickly found himself in a community of entomologists actively seeking to refine his experiments and address related questions.

Pusztai, although researching questions much more at the margin of his discipline, had a strong network of established scientists who trusted his scientific character because of a career of credible interactions. Chapela enjoyed support from his department, but his department's interdisciplinary status and recent establishment most likely provided him little protection and support among molecular biologists and agricultural scientists.

10.3 Claims of Disciplinary Territory

Fleck's (1979 [1935]) concept of thought styles and the difficulty for distinct thought collectives to understand one another predicts an element of controversy traceable to disciplinary standpoints. Extending Gieryn's (1995) notion of boundary-work into this realm further anticipates that contrarians might invoke disciplinary boundaries to shield themselves from resistance. In all three case studies, contrarians invoked this strategy to protect the credibility of their own research from 'outside attack.' In the same way that the recruitment of allies operates on the spectrum of dissent, these disciplinary claims exhibit characteristics of both agonism and dissidence. Respecting domains of expertise carries an agonistic tone, but arguing for the establishment or primacy of fledgling disciplines that carry political overtones represents dissident tendencies.

Chapela Maize

In Quist and Chapela's published "reply" in *Nature*, they responded to some technical criticism by staking out new disciplinary territory, or at least differentiating their approach from that of their critics. They wrote: "As altered DNA species should also be an important focus of ecological research, we disagree with our critics who assume that only intact transgenes are worthy of attention in our study" (Quist and

Chapela 2002). Although presented in a context that referred specifically to the criticism of Quist and Chapela's failure to find whole transgenic constructs to prove introgression, the sentence made the understated assertion that an entire new focus of ecological research was at hand. Quist and Chapela thus responded to resistance by exposing its limited perspective, indicated by a scientific assumption that they categorically rejected.

In the same letter, Quist and Chapela further defined their approach as outside of conventional microbiology or genetics:

Our analysis of Oaxacan maize is unique for several reasons. First, we wished to document changes that occur within diverse populations of landraces (rather than single varieties or lines), for which no markers, restriction-enzyme digestion maps or linkage analyses have been developed. Second, we could not have predicted which (or how many) specific transgenic constructs (or derivatives) were present in the samples that we analysed. Third, our samples of ground, pooled kernels from individual maize cobs do not represent individual genomes. All of these factors render the application of DNA-hybridization methods difficult. To minimize confusion in interpreting the multiplicity of bands that would have been created by Southern hybridization with our samples, we chose to use dot blotting for our experiments (Quist and Chapela 2002).

The authors made three related rhetorical moves: 1) conventional tools of microbiology and genetics were unavailable given the scale of their inquiry, which emerged at an ecological scale far beyond the controlled environment of a laboratory; 2) the complexity of their research environment eliminated the possibility of using more obvious and traditional methods to show the presence of transgenes – a claim that the nature of their question, rather than the inadequacy of their methodological approach, drove their unconventional technical choices; 3) critics' calls for a Southern hybridization revealed their inexperience in even considering the type of question Quist and Chapela had constructed. Together, these claims attempted to negate some of the resistance to Quist and Chapela's research by drawing a new boundary around a field of 'transgenic ecology'.

In his tenure appeal, Chapela also invoked the strategy of staking out new disciplinary territory. In a letter to the dean of the College of Natural Resources, written in response to a set of external reviews and a letter from his chair that pre-dated the denial of tenure, Chapela wrote:

The field of mycology does include organisms (fungi) that are central to my research interests -hence my consistent involvement in this field- but falls short of providing a framework for the future development of microbial ecology. Efforts to box my research as exclusively mycological are therefore misguided...Given that this research opened a new direction in the conceptual understanding of transgenic DNA, it should not be surprising that the outside observer should find it difficult to place this work in any specific field.

Such difficulty is evident in the request from Vice Provost deVries as quoted in Chair Beissinger's letter, as well as in the letter from Referee R, where s/he suggests that I have strayed from my "other major expertise: mycology." Although biology in the middle-third of the 20th Century was cast as a mosaic of expertise divided along the boundaries of taxonomic groups, recent advances have allowed our discipline to return to its roots, where Darwin could legitimately use earthworms, humans or finches in his pursuit of biological ideas. Far from being bounded by a specific systematic group of organisms, my research has rather focused on a set of questions pertaining to the abundance and distribution of microbes, and their roles as symbionts of other organisms. It is in this conceptual field where my work with transgenic DNA as a symbiont of maize plants finds equal footing as other research in my laboratory...Thus I consider [it] retrograde to criticize my work on the basis of the diversity of systems with which I have worked (Chapela 2003d).

In this passage, Chapela made the interesting move of staking out intellectual territory in opposition to the disciplinary segregation within biology. Despite this apparent anti-disciplinary posture, his strategy remained similar in that he defended his work as being bound by different standards and expectations than the boundaries assumed by his critics.

Losey Monarch

In comparison to Chapela, Losey engaged the strategy of staking out alternative disciplinary territory in a much more subtle manner. Together with a group of colleagues, Losey published an article in *BioScience* entitled, "Transgenic Insecticidal Corn: Beyond Insecticidal Toxicity to Ecological Complexity" (Obrycki, Losey et al. 2001). The thrust

of this article argued that the analysis of the potential negative consequences of *Bt* corn had remained too narrow and failed to consider more broadly ecological dynamics. Obrycki et al. summarized this theme visually by comparing two approaches to assessing the risks from *Bt* corn:

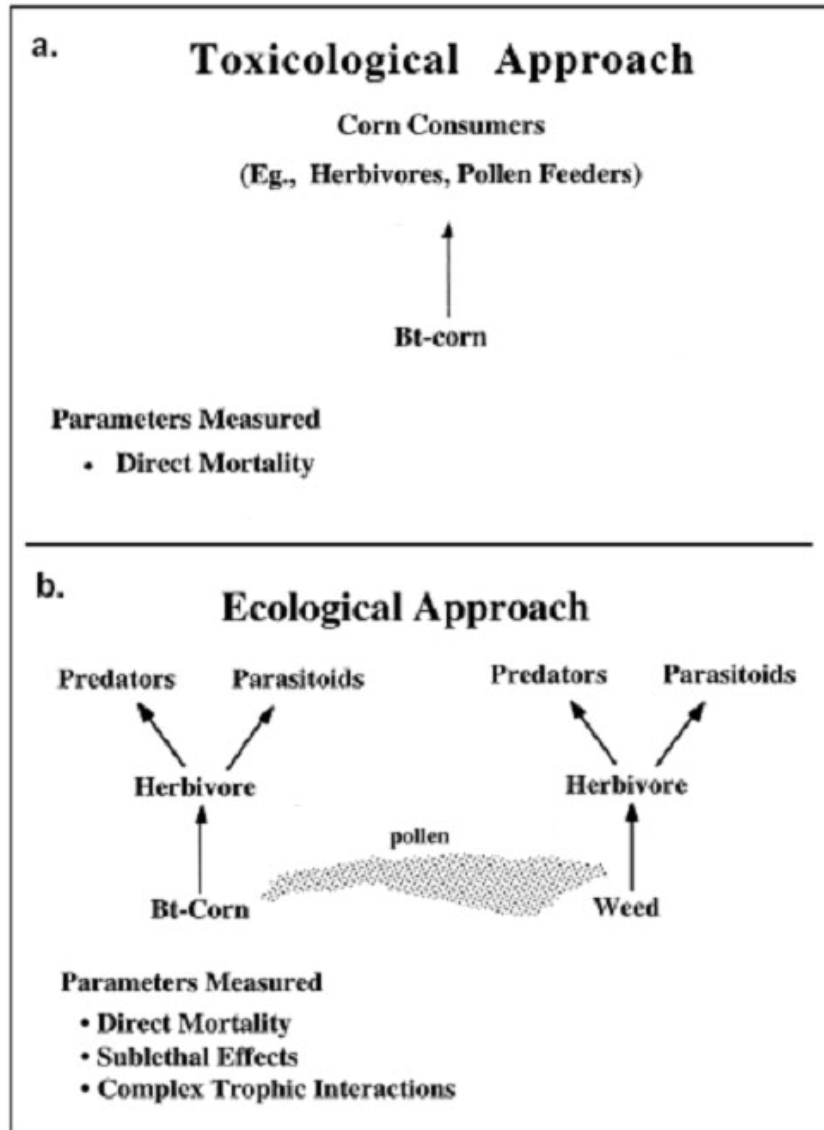


Figure 3: Distinguishing Approaches to Risk Assessment

(Obrycki, Losey et al. 2001, 354)

This figure distinguished Obrycki et. al.'s approach as respecting the complexity of the ecological environment, as opposed to the single-parameter approach of measuring direct toxicity only. Ironically, the original study by Losey et al. (1999) fit the more narrow approach, but Obrycki et al. provided a kind of cover from the field of criticisms. By staking out new territory for the scientific analysis of the safety of *Bt* crops, Obrycki et al. subsumed the Losey Monarch study as one part of a larger project – immune from some of the criticism levied against it because it was only a partial effort. Furthermore, the resistance to Losey Monarch became less potent in the shadow of this article because the message of caution became fortified by a framework of inquiry rather than a particular laboratory experiment. In this sense, Losey and his colleagues engaged quasi-disciplinary boundaries to further the project of contrarian science.

Pusztai Potato

Pusztai's responses invoking disciplinary protection were even less ambitious than Losey's. One example involved a rejection of criticism based on the professional expertise of the critic:

You bet that I did not accept their ill-judged and biased criticisms which have come from mainly their incompetence in nutritional studies. This incidentally also apply [sic] to most of the "experts" such as Dr. Gatehouse in your programme. Dr Gatehouse is, I am sure, a reasonable molecular biologist but as he has never done a single nutritional study in his life or even less published one, I think I am justified to reserve my judgment on his competence on our nutritional studies. To show how highly I regarded his expertise in molecular biology I only have to tell you that I brought him into our programme despite opposition from other partners (Email from Arpad Pusztai to Katherine Ainger, 18 March 2000).

Pusztai's parsing of Gatehouse's expertise (high in molecular biology, low in nutritional studies) highlighted the importance of disciplinary boundaries in signifying credibility.

Unlike Chapela and Losey, Pusztai was not attempting to stake out new disciplinary terrain, but only protect an existing discipline from encroachment by non-experts.

In a move that resonated with Losey's publication with Obrycki and colleagues about an ecological approach to safety testing, Pusztai defended the need to conduct complex and interdisciplinary research in light of the failure of 'substantial equivalence' to secure the safety of GM crop technology:

Although it is argued by some that small differences between GM and non-GM crops have little biological meaning, it is clear that most GM and parental line crops fall short of the definition of "substantial equivalence." In any case, this crude, poorly defined and unscientific concept outlived its possible previous usefulness and we need novel methods and concepts to probe into the compositional, nutritional/toxicological and metabolic differences between GM and conventional crops and into the safety of the genetic techniques used in developing GM crops if we want to put this technology on a proper scientific foundation and allay the fears of the general public. We need more science, not less (Pusztai 2001).

Critically, the "more science" that Pusztai advocated was within a particular approach – one that engaged "novel methods and concepts" rather than an approach based within the framework of substantial equivalence.

10.4 Discussion

As suggested by the evidence above, dissent that exhibits primarily agonistic tendencies does not quell controversy, just as Mouffe's agonistic pluralism does not envision democracy without conflict. The strategies discussed above invite further cycles of resistance as technical claims, credibility of allies, and disciplinary boundaries become the terrain of struggle over what knowledge becomes accepted as scientific 'truth'. Nor does agonistic engagement imply that dissenters automatically maintain cordial and cooperative professional relationships with their opponents. 'Trials of strength' (Latour 1987) can be cruel and at least discursively violent. But regardless of how disturbing the

controversy becomes to scientific or lay audiences, when parties respect ‘the rules’ (agonistic engagement) they maintain and strengthen the broad outlines of the boundaries of acceptable scientific conduct. Indirectly, these strategies further the performance of science as an objective, apolitical, and expert (non-public) method. Agonistic strategies thus support a narrow focus on controversial knowledge production that backgrounds the political and institutional context of science-making.

Chapter 11 Performances of Dissident Science

The discussion in the previous chapter of selected responses that fall toward the heuristic category of agonistic dissent does not carry much meaning on its own. As described, scientists can and do respond to resistance with techniques familiar within the STS controversy literature – disputing facts and evidence, recruiting additional scientific allies, and drawing new disciplinary boundaries. These behaviors, or similar ones, may be sufficient to narrate the unfolding of many scientific controversies, but they only describe a portion of the responses undertaken by the scientists within the three case studies of this dissertation. I suggest that the following discussion and exploration of responses challenge the limits of agonistic dissent in multiple ways. While I do not mean to imply a harsh and clear line between agonism and dissidence, the descriptions below stretch our understanding of responses to include strategies that appear to violate the norms of scientific discourse. The question that emerges is how do such violations both serve as functional responses to resistance and also challenge conventional perspectives on the practice and politics of science.

This chapter presents three performances of dissent that include many features of dissident science. The first, “Black Canvas,” centered on an art exhibition that explored the arena of biotechnology. Chapela was invited to appear as a panelist in a public event associated with the exhibition. While this context was not precisely scientific, Chapela’s choices of participation revealed a strategy of engagement that violated conventional expectations of a scientist. In the second event, “Open Office Hours,” Chapela staged a quasi-protest outside of UC Berkeley’s main administration building during his initial

quest for tenure. Exploring this response demonstrates how dissident science challenges not only mainstream knowledge, but assumptions about credible knowledge production. The third event, also organized by Chapela, brought Losey and Pusztai to UC Berkeley for an event entitled “The Pulse of Scientific Freedom in the Age of the Biotech Industry.” This explicit performance included a diversity of agonistic and dissident responses by the three scientists, but as a whole represented an intentional challenge to the political, scientific, and academic context of agbiotech.

11.1 Black Canvas¹⁵⁰

Description of the event

In the fall of 2003, the Berkeley Art Museum (BAM) and the Pacific Film Archive organized an exhibit entitled *Gene(sis): Contemporary Art Explores Human Genomics*. The exhibit included a number of visual and interactive installations. Pictures of the infamous transgenic bioluminescent bunny, Alba (a project of Eduardo Kacs) adorned the entrance ramp to the exhibits, the exterior of the museum, and the web page.¹⁵¹ In addition, the Director of Education and Academic Relations of BAM organized a series of lectures entitled, “Thinking Through Genomics” to occur within the auditorium space. The first scheduled conversation on 14 September 2003, “Making Worlds: Artists, Scientists, and Genomics,” included Ignacio Chapela, Roger Brent (Professor of Biopharmaceutical Sciences, UCSF, and President and Director, The Molecular Sciences Institute, Berkeley), Catherine Wagner (Artist), Gail Wright (Artist),

¹⁵⁰ Portions of this section were originally presented by the author at the 2005 annual meeting of the Society for Social Studies of Science (4S) in Pasadena, California (Delborne 2005).

¹⁵¹ <http://www.bampfa.berkeley.edu/exhibits/genesis/>. Accessed 5 August 2005.

Meredith Tromble (Artist and Writer), and Iain Boal as moderator (Director, Environmental Politics Colloquium, International and Area Studies, UC Berkeley, and History of Consciousness Program, UC Santa Cruz). While not a debate, per se, it was clear that Brent and Chapela were invited so as to include promotional and contrarian scientists.

As the panel began, Chapela's chair remained empty. Unbeknownst to the organizers of the event, the other participants, and the audience, Chapela had decided not to appear in person, but instead to have a statement read in his absence. An African-American student, Halima O'Neil, from Chapela's department carried in a large black canvas to rest in Chapela's chair, the manuscript to be read, and twenty copies of visuals to accompany the reading (which she was to collect before leaving). Chapela had been intentional about working with a student of dark skin color, and the manuscript included her as a co-author. In an interview afterward, he explained that they had envisioned O'Neil's role as an actress, with the responsibility to make clear that the black canvas was to hold Chapela's place. Chapela reported that she had been too afraid to make much of her role and "tried not to interfere," which worked against the intent of their performance (Chapela 10/23/2003).

When it was Chapela's turn to speak, Boal (as moderator) declared, "The mystery of Ignacio Chapela's absence is now solved," and proceeded to read aloud Chapela's manuscript, entitled "Black Canvas" (Chapela 2003c). The reading included six parts:

1. A polite, yet sarcastic introduction including the statement:

I am thankful for the efforts to get us to talk, thirty years into the age of the transgenized biosphere, about what it is that we have been doing to ourselves, to our medicalized, medicated ecology, in this last quarter-century. As if we had any idea about it. I am thankful for your unjustifiable trust in my knowledge of microbial ecology, as if that

knowledge really gave me anything to say about the cultural consequences of the transgenization of our internal and exterior environment.

2. An accusation that Alba (the transgenic bunny) was a hoax – an image manufactured with photoshop evidenced by the fluorescent green of its pupil (which should have reflected red from the retina) and the singular pose of every available image. Assuming it was a hoax, Chapela pondered, “The question then remains; why should anyone be so mesmerized by its faked luminescence?”
3. An accusation that another installation was a hoax, evidenced by “a dye craftlessly added onto the medium” to create the appearance of green fluorescence. He echoed his earlier question: “So I ask again, why our dazzled immobility under the faked power of this light?”
4. A harsh critique of James Watson, a prominent figure in biotechnology viewed as a founder of modern genetics and featured in the exhibit with multiple quotes and references.
5. An interweaving of famous historical works of art (which he reproduced in packets passed through the audience) and three Spanish words to make claims about the social project of biotechnology. Chapela presented the Spanish terms “because I believe that what we are witnessing here must have been re-lived in the past in that language. Let me try at least three words: Engaño, Desengaño, Sueño.”¹⁵²
6. A closing to justify his absence:

¹⁵² Chapela defined each of the terms for the primarily English-speaking audience: “Engaño -deceit, deception, but also treason, betrayal, breach of faith”; “Desengaño is disillusionment, disenchantment, although often also sobering-up and realization”; Sueño as dream, but with more complex possibilities: “And in closing our eyes, which sueño will we go into: Dream? Tiredness? Inattention? Boredom? Or imagination and desire?”

Much as I would have liked to paint with you, by framing us between watson [sic] and rabbit I was handed a black canvas upon which my colours cannot play. Too much centrally dogmatic, fluorescent knowledge for our eye to be able to think. So Halima O'Neil and I bring it back to you today with these thoughts. We send it also as a space-holder for all those people, those dreams which did not yet happen, all those who are not in our cool loop of fluorescent illumination. Today, in the California of Ward Connerly, Peter Atkinson and Warren Hellman. The suicide in CanCun, the wafts of transgenic pollen over the Sahara. In this sense, I believe my absence is useful, and perhaps forgivable in its impolite inability to dialogue with you today.

Following this explanation, Chapela connected his own absence to his recent experience at the Biodevastation conference in Missouri, during which harsh security measures against conference participants led to one of his co-panelists missing her panel and leaving an empty chair. He ended by noting a number of contrarian scientists whose work and identities received no mention in the exhibit.

The question and answer session after the remaining panelists had spoken included some discussion of Chapela's choice of performance. Some offered harsh criticism that he chose not to engage personally – taking offense and questioning his commitment to the issues at hand. Others offered some defense, vouching for Chapela's character. But according to Chapela, only later did he receive high praise for his performance, which came mostly from within the art community and from organizers and participants in the *Gene(sis)* exhibit (Chapela 10/23/2003).

Defying expectations, eschewing rational discourse

By not showing up, refusing to engage in active discussion, Chapela defied the expectation of scientists to participate in a debate of evidence. Although he offered his own argument in words and images (in absentia), his inability to listen to other panelists and participate in discussion with the audience effectively removed him from the type of

rational discourse that was expected by the BAM audience. The venue and theme of the conversation did not emphasize an opportunity for factual debate about Chapela's specific contrarian claims, but the program construction made it quite clear that Chapela was expected to play the role of scientist 'concerned about biotechnology' in opposition to Brent, the 'biotechnology enthusiast'. Brent made some overtures to speaking about the relation of art and science, but he faithfully brought his perspective and knowledge gained through the practice of his science. In one sense, Chapela brought his perspective and knowledge to the performance as well, but in a manner that eschewed rational discursive engagement – part of a scientist's expected role in the public sphere.

Second, Chapela's contribution emphasized the wisdom of 'art' over 'science', rather than maintaining the status of science as the prominent path to truth. As mentioned above, he downplayed the importance of his scientific background in giving him standing to speak at all. More significantly, his highly sophisticated discussion of historical works of art completely overshadowed any explicit reference to his scientific work. For example:

Look again at the famous mirror and the six pairs of eye-reflections staring at you while you are invited to participate in Velázquez' light experiment in *Las Meninas*. What do you see? Yourself as Fernando Rey y Soberano in the supreme luxury of familial peace and splendor. *Las Meninas* as exquisite mirror to the icons and aspirations of today's suburban life. Connecting our fluorescent hues to Velázquez' spirals of gold and silver light is the promise that, this time, it could be you who could retire at 31. If only your Novartis stock held for a few months (Chapela 2003c).

The content of the presentation, the verbal style and tone, and the incorporation of text and images into a premeditated and explicit *performance* together privileged artistic over scientific inquiry. On one hand, this approach seemed entirely appropriate for an event in an art museum, but the audience's predominantly negative reaction (and surprise) at

Black Canvas suggested that the expectations of Chapela to engage as an agonistic dissenter were more significant than the desire for artistic ingenuity.

Third, despite being a scientist whose work had been strongly resisted, Chapela passed on the opportunity to defend his own research or his professional status (both under attack in the fall of 2003). Although the audience of Black Canvas did not necessarily represent scientific peers with the power to restore scientific credibility to Chapela, the complete lack of engagement around his own controversies in a public and intellectual setting nearly suggested a fear or unwillingness to address the resistance to his work.

Fourth, instead of aligning himself with centers of power within science to boost his own credibility, Chapela acted to distance himself even further from ‘legitimate’ actors and institutions. He disparaged James Watson, mocked the technical claims around the transgenic bunny, and unapologetically denounced the project of biotechnology:

...**engaño** is that to which we have been subjected with the illusion that we had in “Biotechnology” anything resembling what we have learned to recognize as a **technology** at all --with design, control, and predictability as key characteristics. Let alone anything of value to society (emphasis original) (Chapela 2003c).

Chapela thus challenged some of the key assumptions of promotional science head-on, but without providing any ‘scientific’ evidence to bolster his claims. Furthermore, he aligned himself with other scientists who had been marginalized from the mainstream scientific community, asking “Where is Rosalind?¹⁵³ Where is Edwin Chargaff? Where Susan Oyama? Lynn Margulis? Terje Traavik?”

¹⁵³ Presumably Rosalind Franklin, a scientist who had a significant role in the discovery of the double helical structure of DNA, but who has received scant attention or credit in comparison to Watson and Crick.

The dramaturgical use of the black canvas and the empty chair had greater resonance with protest and traditions of political solidarity than fulfilling the role of a scientist invited to participate on the panel. Although the art community appreciated this move in retrospect as a “great performance piece” (Chapela 10/23/2003), the real-time audience displayed no consensus and perhaps leaned toward condemnation. Chapela took this gamble as preferable to simply not showing up at all out of disgust with what he saw as the dominating context of promotional science at the exhibit and the lack of any truly critical art installations. Following the presentations by the panelists, one audience member thanked the panelists “who did come,” and labeled the “no-show” as “anti-dialogue” and “staring at himself in the mirror.” More than a few audience members applauded after this comment. This exchange revealed that the audience, a mix of the art community, the scientific community, and laypersons, sensed the degree to which Chapela had broken expectations of engagement. The only spoken defense came from a panelist who had interviewed him for her exhibit piece, knew of his commitment to the issues, and declared that she would give him the “benefit of the doubt.”

Given the controversy over Chapela’s chosen approach in *Black Canvas*, it is tempting as an analyst to judge the failures and successes of the strategy. To do so, however, would make the same error as wanting to distill the arguments about Quist and Chapela’s research or Chapela’s tenure case down to their essences in order to judge them, up or down. These judgments do happen, of course, and are perhaps informed by analyses such as mine, but my purpose comes at a different angle to the event. Instead of asking about outcome, I direct the inquiry toward understanding: what do Chapela’s choices teach us about the palette of responses available to dissenting scientists? *Black*

Canvas reveals the beginning of a pattern that is no less engaging or serious than agonistic response, but draws from different sources of credibility and sketches alternative relations among the public and institutions of science. In this sense, it begins to chart the territory of dissident science.

11.2 Open Office Hours¹⁵⁴

At the end of June 2003, Ignacio Chapela's two-year terminal contract was due to expire, and he had received no official word from administrators on the status of his tenure application or the likelihood of a contract extension. Chapela saw the need for a public action, but dismissed conventional protest strategies such as a sit-in or hunger strike. Instead, on Thursday, 26 June at 6:00 A.M., Chapela set up "Open Office Hours" outside of California Hall, the main administration building of UC Berkeley. Except for short bathroom breaks, he remained in his open office for five days straight, twenty-four hours per day.¹⁵⁵ The Open Office Hours functioned as an explicit response to the resistance Chapela had endured regarding his application for tenure, but its dramaturgical qualities revealed a posture of dissent much more complex than conventional strategies of fighting over facts and challenging boundaries of credibility.

Characters

Without question, Ignacio Chapela played the starring role in this performance.

His management of his character, from his email announcement sent around the world to

¹⁵⁴ Portions of this section were originally presented by the author at the 2003 annual meeting of the Society for Social Studies of Science (4S) in Atlanta, GA (Delborne 2003).

¹⁵⁵ I was present at the Open Office Hours for approximately 15 hours, spread over four separate visits. I engaged in conversations with Chapela and his visitors, recorded several interactions, and took notes about what I observed and experienced.

his interactions with various visitors over the five days, revealed much about the strategy of his response.

One component of Chapela's character involved his self-presentation as the dutiful, dedicated professor. Chapela's announcement declared:

Beginning at 6 o'clock this morning, as I enter the final days of my contract as a faculty member at the University of California at Berkeley, I intend to mark and celebrate them, by doing what I believe a professor in a public university must do: to further reason and understanding...I believe that I have contributed to the mission of the university and my heart and intellect are also vested in its health and growth...In the face of such lack of transparency and accountability [surrounding my tenure case], I choose to hold office hours in public, in the open, and in the midst of our beautiful campus. I do so in celebration of my vocation and my time at Berkeley, and not in the expectation that such an action will change the course of the decision process, whatever that might be (Chapela 2003b).

To some degree, Chapela carried this agonistic posture throughout the event. He encouraged visitors to browse his library of books; he gave impromptu lectures to groups of undergraduate and high school students; he sponsored an outdoor slide-show (projected onto the side of California Hall) and lecture by Grey Brechin, author of *Imperial San Francisco* (1999), entitled "The University of California: Courses to and from an Organic Vision"; and he participated in countless intellectual discussions about topics ranging from the biology of genetic engineering, to the economics of global agricultural trade, to corporate-university relations, and to the peculiarities of the tenure process in a public university. Chapela closed his email announcement as follows:

At a time of rampant obscurantism and irrationality, I am proud of the privilege vested in me by the public as a professor at Berkeley. In fulfillment of the duty attached to that privilege, I intend to share the light of rationality during office hours over the next five days, together with those who might wish to join me.
Fiat lux.

By emphasizing his status as a professor and projecting this role upon a character that might otherwise be mistaken for a protester, Chapela strived to present himself as a dedicated intellectual fulfilling his duties in the final days of his academic appointment.

This tension between dutiful scientist and brazen activist spanned the spectrum between agonistic dissent and dissident science.

A second critical component of Chapela's character during the office hours involved his status as a victim of a corrupt academic process. Chapela's announcement explained:

It has been suggested that the extraordinary delay in reaching a decision on my tenure case without ostensible reason may be the result of, even retribution for, my advising our campus, academe, the government and the public against dangerous liaisons with the biotechnology industry, as well as my concerns regarding the problems with biotechnology itself. Without doubt, the uncertainty and reproach implicit in the silence on campus surrounding my case has had grave consequences for my professional, public and personal life. But such are the wages of doing work that has significance for the world, and it will be up to those sifting through the files of this case to discern the twists and turns that brought us to this moment, and to pass the judgment of history on the motives and actions of those involved, within and beyond our community (Chapela 2003b).

Many discussions involved the particulars of Chapela's tenure case, and several participants even began an attempt to collect data from the administration about the historical median and range of days required to complete a tenure review at UC Berkeley. One participant used Chapela's cell phone to call a campus administrator (inside California Hall) to request this data. Yet, while openly discussing his perspective on the irregularities of his tenure case, Chapela did not perform the role of angry protestor willing to fight for his tenure by any means necessary. Instead, he maintained an outward appearance nearly of disinterest in how his case would be resolved – an attitude that matched norms of conduct for an academic scientist who privileged inquiry over results.

The boundary between 'characters' and 'audience' blurred significantly during the Open Office Hours. One might even argue that my role of participant-observer and recorder played a part in the performance – one openly acknowledged publicly by Chapela on several occasions as introductions were a constant practice during the five

days. For the purpose of analysis, however, I will discuss several key participants whose characters contributed to the performance in its entirety.

Iain Boal, a long-time friend of Chapela and a historian of science (and the moderator at the Black Canvas event), spent many hours supporting the performance, both in-person to provide company to Chapela and participate in discussions with visitors, and behind the scenes organizing logistics and assisting with writing projects. In describing the way the event came together in his mind during the planning stages, Chapela recalled that after making a “personal and emotional” commitment, he made a “social” commitment by socializing the writing of the announcement (Chapela 6/30/2003). Boal contributed to this effort, and also distributed an announcement to invite the public to attend Grey Brechin’s slideshow. He began:

To: commoners everywhere
Up against the Wall,
Come one, Come all.

At dusk this evening, Sunday June 29th, on necessarily short notice, and as a gesture of collective solidarity with Ignacio Chapela, you are invited to an open-air slideshow, to be projected onto California Hall, which by day houses the Chancellor and the committee which, to its shame and with grave consequences for his professional and personal life, has balked at granting Ignacio Chapela security of tenure. David Quist and Ignacio Chapela were the scientist-messengers who carried the news back north that transgenic (GM) corn had contaminated the ancient reservoir of maize landraces in Oaxaca, a result considered at the time implausible, even fraudulent, by the gatekeepers of biotechnology (Boal 2003).

Boal thus acted in the role of supporting actor, but his intellectual background and commitment to radical politics influenced strongly the performance experienced by audience members near and far.

Unlike Boal, who had historical connections to Chapela, another participant, “Bradley,” showed up in response to the email announcement and played a pivotal role in the performance. After learning of the Open Office Hours from the “Organic Consumers”

listserv (which he signed up for at a Greenpeace anti-GMO event), Bradley, a UC Berkeley graduate, came to the event and quickly decided to offer his support even though biotechnology had not been a primary concern for him. He spent two full nights with Chapela, meditating in a yoga position and keeping watch over the site. He explained that he participated “on principle” and felt no need to learn all of the technical arguments or details of Chapela’s tenure case. Over the two nights, he interacted with several homeless persons, two pairs of belligerent drunks who wanted to engage about the politics of GM research, and the campus police who explained that while Chapela and Bradley were free to be on campus throughout the night, they were not permitted to sleep. Chapela felt great appreciation for Bradley who brought spiritual strength and physical protection during the most chaotic and stressful moments of the Open Office Hours (Chapela 6/30/03).

Stage management

Chapela thought carefully about the symbolism of staging the Open Office Hours. In an interview the day before he began, Chapela explained that he had contacted some reporters with whom he had strong relationships and also announced his intentions at a dinner hosted by the International Forum on Globalization in Sacramento the night before.¹⁵⁶ He described a conversation with David Noble, a colleague and radical critic of the corporate influence on science and technology, in which they drew parallels between

¹⁵⁶ The IFG dinner was part of the NGO-led response to the USDA Ministerial Meeting on Agricultural Technology in Sacramento, CA. A coalition of NGOs had organized a series of events including a teach-in (at which Chapela spoke), a rally and march through downtown Sacramento, and direct action to attempt to shut down the ministerial meeting. IFG organized a dinner for the invited ministers from around the world to hear alternative views on the wisdom of promoting agbiotech in developing countries.

Julia Butterfly Hill sitting in a redwood tree to protect the forest from the lumber industry and Chapela sitting on campus to protect the public university from the biotech industry (Chapela 6/25/2003).

Symbolically, the spectacle of a professor sitting outside, holding office hours in a transparent office, contrasted with the lack of transparency in Chapela's tenure review.

As the *Daily Californian* reported:

California Hall was chosen as the campsite because it is the "black box" where many academic decisions are made. Chapela criticized the administration for obscuring the decision-making process. "Academic decisions are complex, and keeping them private is OK, to a certain extent," he said. "But when that privacy is abused it becomes secrecy, and that is not OK." Chapela's new "office" has no walls. It is transparent, to symbolize the need for transparency higher up in the university, he said (Wittmeyer 2003).

Chapela thus communicated his primary message through the physical staging of the performance.

Chapela taped a sign on the concrete wall with his name, professorial title, and department affiliation. The sign indicated "Office Hours 00:01 – 24:00," and concluded with the following quote:

"I shall collect plants and fossils, I will be able to make observations in astronomy with excellent instruments - I shall chemically dissect the elements of the air. But that is not the main goal of my travels. My eyes shall be aimed at the harmony of cooperating forces, the influence of the inanimate on the animate creation of animals and plants" - Alexander von Humboldt, 1799.

Chapela chose this quote as a motto for practicing science in a particular way. According to Chapela, von Humboldt was a naturalist "all but banned in the English-speaking world," and who differed from conventional colonial scientists by remaining open to explanations coming from the subjects of study. In other words, von Humboldt emphasized "relationships" over "facts in isolation," which positioned the scientist in a cooperative rather than extractive relationship with wildlife and indigenous people

(Chapela 10/21/2004). This aspect of staging connected Chapela's performance with not just an unusual scientific approach, but with one that had been actively excluded and had deep implications for relationships among humans, science, and environments.

Chapela located himself against a low concrete wall, in the shade of a large tree, and brought with him numerous props that set the scene for his performance:

1. A metal rolling cart with books to serve as his library. Chapela invited colleagues to contribute books to his collection, in order both to have the "presence" of respected friends and to offer other visitors something to read when they had time.¹⁵⁷
2. A sign-in book, in which Chapela encouraged all visitors to write their thoughts and reflections.
3. A Mexican blanket, the first object Chapela ever bought.¹⁵⁸
4. A sombrero purchased the day before, supplemented by a second straw hat brought by an ESPM faculty member as a donation.
5. An oil lamp from Switzerland given to Chapela by a Brazilian friend.
6. Three chairs to offer some seating, although many visitors stood, sat on the wall, or sat on a blanket on the ground.

Chapela also sought to exclude some objects from the performance. He intentionally avoided corn imagery, not wanting "to just make it about corn," although a friend brought a candle in the shape of a cob of corn, which he allowed. Second, he recalled being overwhelmed with efforts by activists who had come to support him:

¹⁵⁷ The collection included: a book by an artist who drew insects with tiny mutations attributable to their toxic environments; a book arguing that most important evolutionary developments had been symbiosis; a book on the politics of food; a book on the devastation of conventional agriculture; and many others.

¹⁵⁸ He recalled later, "I don't like it anymore. It's kind of ugly, but I'm very attached to my blankie. It's not a security blanket at all. In fact, I shuddered at it, but I said, Yeah, you come with me" (Chapela 10/21/2004).

I was very dictatorial on the symbolism there. The first day, Greenpeace and PANNA, all these NGOs showed up with all their flyers and all their posters and they started plastering the place with messages and symbolism and photos. I was diplomatic to them because many of them are my friends, but as soon as they left I just pulled everything off and made it disappear. I did not want any symbology more than what I could control. And that was a hard one (Chapela 10/21/2004).

This careful control of staging revealed the depth to which Chapela understood the performance as his opportunity to communicate his own message. Although perhaps in harmony with the NGO activists in terms of political stance on GMOs, Chapela sought to preserve his identity as a professor. This mix of activism and adherence to scientific ideals reflects the strategy of dissident science.

Narrative claims

Any effort to distill five days and nights of conversations, presentations, and lectures down to several narrative claims surely ignores a great deal of the complexity of the event, but with that in mind, I suggest four themes that emerged.

1. *University procedures, especially around tenure and promotion, lacked the transparency necessary for public accountability.*

Chapela wrote in his announcement:

To the extent that reason can assess, I do not know of any other academic information on the case that might suggest that a negative decision should be reached. Yet as of tonight, well over a year into the part of the process conducted in secret in California Hall, no decision has been made, as far as I am aware. I must therefore conclude that there is another set of criteria that counterweigh the strength of the case, but that such information cannot be publically [sic] shared (Chapela 2003b).

2. *Scientific inquiry of agbiotech required a mix of multiple disciplines including biology, economics, sociology, and ecology.*

On 30 June, the last day of his Open Office Hours, Chapela delivered a lecture to a group of high school students whose teacher had brought them to the Berkeley campus

to hear Chapela speak. He began discussing the biology of genetic modification, and quickly jumped to policy analysis. He described a perverse system of policies and subsidies that drove Mexicans off their agricultural land to work as agricultural laborers in the U.S. Chapela highlighted both the cultural and biological costs of such a situation, and implored the students to recognize the complexity of the relationships among politics, economics, sociology and biology.

3. *Chapela would not fight his tenure battle in either the mode of the respectful professor nor of the bitter and angry activist.*

A number of Chapela's allies within the university expressed extreme concern both before and during the event at his choice of action, suggesting that conducting such a public and protest-like event would only provide ammunition to the forces within the university that wished to deny him tenure.¹⁵⁹ Chapela maintained a different perspective, motivated less by strategic analysis to achieve tenure and more to take the opportunity to make statements about campus politics and the shrinking space for research that challenged biotechnology. This approach fit neither the storyline of naïve academic making a procedural appeal nor of a strategic campaign of activism.

Chapela recognized that the substantial delay in his tenure evaluation had the potential to slowly erode his academic capital – he had been less able to concentrate on conducting research and publishing papers, and he had suspended admitting any new graduate students. He explained that the Open Office Hours were “strategically intended

¹⁵⁹ Faculty members communicated these concerns directly to Chapela and also to me, in hopes that my access to him might provide a way to convince him to do otherwise.

to stop what they [the administration] were doing” in order to mark the fact that he had a strong academic case prior to the unreasonable delay (Chapela 8/7/2003).

Chapela had several requests of the university (seeing the complete file on his tenure case and minutes from associated meetings, accessing his FBI file), but had not composed a precise list of demands. He felt that his substantive demands were beyond what the university could supply (e.g., “cleaning house” to rid the university of industrial influence, hiring more faculty with a contrarian outlook) and was not even sure what it would take to make him “go inside” before the completion of the vigil (Chapela 6/25/2003).

Chapela began his email announcement with a quote from Robert Haas: “We asked the captain what course of action he proposed to take toward a beast so large, terrifying, and unpredictable. He hesitated to answer, and then said judiciously: ‘I think I shall praise it’” (Chapela 2003b). For Chapela, this quote had tremendous significance as he agonized over what strategy to take in the face of overwhelming resistance to his research and professional credibility. He later recalled in an interview:

With twenty minutes, I just dashed to the library...I went and pulled this book, and it was the first page that I turned...And it had to do with confronting the monster, with confronting the unfrontable, with dealing with the overwhelming, with facing the unbeatable. And the really interesting thing about that, is that it turns the page on what happened in the 60s. Which was you just confront it, you just go head on and break your head on it, and make sure you get a picture of it so then you bear witness. And I think part of the problem today, is that that is a very beaten model that doesn't do anything...So bearing witness doesn't do anything. If anything, it just brings in silence...Okay, so what do we do now? The answer is very equivocal...He says, “Here comes the monster. What do we do with the monster, Mr. Captain?” [The captain replies,] “Well um, hmm, okay, um. Where's my wisdom, where's my wit? Okay, let's praise it! Let's do something different!”...And it was very clear that I could have just gone into a hunger strike and do the witnessing thing. Right? Look at me dying and then someone will come and pick me up. And I know it won't happen. You know (laughs), I'm not into that. So then I took courage from that, to say well, what I'm going to do is I'm just going to praise what I have while I have it. I'm going to teach, I'm going to have office hours...In a way it's a very defeatist point of view; that says okay, I just cannot nurse the situation anymore; the

patient is dead. But in another light it is a very optimistic one. Where you say, yeah, this is what we can do; let's do it and see what happens (Chapela 10/21/04).

'Praising the beast' thus helped define Chapela's story of what the Open Office Hours meant and what strategy it reflected. He was not running a political campaign (at least in the traditional sense), nor was he satisfied to wait 'off-stage' in hopes of convincing those with power in the tenure review process that he could play the role of respectful and responsible professor. Instead, Chapela constructed a narrative that evoked a novel form of response as a dissident scientist.

4. *The turmoil and controversy surrounding Chapela's research and tenure case confirmed the failure of key public institutions to maintain independence from corporate interests.*

The narratives emerging from the Open Office Hours transcended the particular battles and controversies directly surrounding Chapela. In his announcement, Chapela wrote:

At least one person has said that I should be banned from the academic system, implying that my work harms the public role of the university as a hothouse for the agbiotech industry. Indeed I have long stood against the folly of planting 100 million acres with transgenic crops each year, without knowing even the simplest consequences of such a massive intervention in the biosphere...It would seem rational that our university - and the public - should strive to keep an independent source of advice on the wisdom of supporting such an industry. Rationality, however, must take a back seat when the university becomes grafted to a specific industry. Such has increasingly been the case at Berkeley and at other universities (Chapela 2003b).

Chapela thus weaved a complex and historical critique of university-industry relations into the Open Office Hours. This critique added power to the defense of his credibility with respect to his tenure review, but it also functioned to challenge broad issues of public science, industrial interests, and technological governance. These challenges embodied the dissident flavor of the Open Office Hours.

Audience

Chapela focused heavily on the need to create an audience for this action. When asked about what he had anticipated going wrong, Chapela replied:

Just standing here and just being ostracized, and not having anybody come. Just be there in the public plaza basically by myself. I really expected some friends to come and stand in solidarity and so on, with a little vigil, and then all my colleagues just not coming or doing anything; or like silence, which is I think the way we deal in academia with dissent. And for five days I would have had to just stick it through, and it would have been really painful, and that was really scary to think about (Chapela 6/30/2003).

Instead, over the five days, hundreds of individuals occupied the transparent office for intellectual conversations, presentations, and social interactions. The number and diversity of the attendees surprised even Chapela. From my perspective as a participant-observer, I witnessed a number of tenured faculty, and even the dean of his college, stop by to offer support, even with some reluctance in condoning the tactic. On the other hand, individuals with a more marginal connection with the university or stronger connections to activist groups spent many hours there and even took on roles of answering Chapela's cell phone, bringing him food, and staying up with him during the nights.

It is impossible to know the extent of the Open Office Hours audience, but it would be fair to say that it went well beyond those who physically attended. Chapela's announcement went out by email and was forwarded to numerous listserves, and the event received some limited media coverage (e.g., independent radio recordings by Maria Gilardin, Wittmeyer 2003). Chronologically, this represented Chapela's first public action designed to expose extensive audiences to his stories and his perspective – later events built on the themes and the momentum of this action.

While Chapela denied conducting the open office hours in order to effect a response from the administration, the administration clearly became a key audience for

the action. Not only did Chapela's statement implicate the administration in watching over a corrupt tenure review, but his choice of location on campus put him in plain view of the entrance to California Hall. On the morning of the first day, a high level administrator handed Chapela a letter confirming a one-year extension of his contract. This letter was presented on 26 June at 7:00 A.M., but was dated 19 June without explanation for the delay in delivery. While it remains an open question whether the Open Office Hours affected the content of the letter (i.e. pressuring the administration to offer a contract extension), it was clear by the timing of the delivery and the accompanying short conversation that the administration had become a quick audience to the event and hoped the notice of an extended contract would convince Chapela to end the Open Office Hours as soon as they had begun. Even some of Chapela's most loyal supporters were disheartened when he explained that the letter was insufficient to convince him to end the Open Office Hours.

Wayne Getz, a professor in Chapela's department and a member of the campus-wide ad-hoc faculty committee that reviewed Chapela's tenure application, passed by the Open Office Hours on the first morning on his way for coffee. According to Chapela, someone explained the situation to Getz, which motivated him to return to his office and write a letter to the campus administration (Chapela 1/13/2004). In this letter to Vice-Chancellor Paul Gray, dated 26 June, Getz broke confidentiality by disclosing his participation on the anonymous ad-hoc committee, but did so out of deep concern that the balance of powers between the faculty and administration had somehow been severely disrupted, as evidenced by the disconnect between his committee's recommendation and the administration's apparent response (Getz 2/13/2004). He wrote:

Question 1: Is the reason for the delay that the Budget Committee is seeking opinions of others outside of the framework of the usual tenure evaluation process?

Question 2: If the answer to Question 1 is affirmative, is the Budget Committee's action a result of one or more members of the committee holding opinions that are at variance with the opinion of the Ad-Hoc Committee on which I served?

Question 3: If the answer to Question 2 is affirmative, what relative weighting does your administration give to the opinion of one or two members of the Budget Committee that is at variance with the opinion of a carefully selected 5-person Ad-Hoc Committee of peer faculty? (Getz 2003).

This letter became pivotal evidence in *Nature's* coverage of Chapela's tenure denial later that year: an article entitled, "Berkeley accused of biotech bias as ecologist is denied tenure" (Dalton 2003).

Despite these two examples, the Open Office Hours had a more participatory feeling with regard to audience. The physical and intellectual environment created by Chapela and his closest allies encouraged interaction and discussion more than voyeurism or observation. Chez Panisse (a famous restaurant featuring local and organic food) catered a well-attended lunch one day, and Rex Dalton arranged for *Nature* (the scientific journal) to pay for a pizza dinner delivered on Saturday evening. The atmosphere was often jovial and relaxed – an invitation for visitors to direct conversations to their own areas of interest and expertise. Throughout, Chapela emphasized his desire for every visitor to sign his guest book. Answering a question about what the guest book meant to him on the last day of the Open Office Hours, he commented:

I think it means many different things. I haven't read it, but I know a couple of things. I just realize that I'm very apprehensive about it now. If it runs away I'm going to cry. It represents what this space has represented. I really don't know because I don't know what people are saying. But if it is anything like what I'm hearing people say, it's simply a space where all of a sudden people could say what they wanted to say, without fear of being graded, not using the right jargon. A space to share something they feel strongly about, which is what's been happening in this space. It's really one of the amazing surprises – that you realize how much people are not saying what they want to say (Chapela 6/30/2003).

Thus, while Chapela asserted a high degree of control over the framing of the event and much of the symbolism present, he created ample space for his diverse audiences to create the experiential content of the Open Office Hours.

11.3 Pulse of Scientific Freedom in the Age of the Biotech Industry¹⁶⁰

This section captures the theme of scientific dissent as performance in a very literal sense by focusing on an event during which four scientists, including Chapela, Losey, and Pusztai, actually appeared on a stage in a University of California, Berkeley auditorium. Their discussion, entitled “The Pulse of Scientific Freedom in the Age of the Biotech Industry”¹⁶¹ (hereafter, the Pulse Event) attracted a live audience of nearly five hundred, and the event was webcast around the world in real-time. The four scientists had published research that challenged the health or environmental safety of products commercialized by the agbiotech industry, and each told his story of attempts by industry and government to suppress or discredit his results.

The Pulse Event represented a conceptual starting point for this dissertation. Although I had been collecting data about the controversies for several years prior to December 2003, the organizing question for my thesis emerged within the context of this event: namely, “How did we get here?” More specifically, why were four scientists on stage at the University of California discussing attempts at suppressing and discrediting their research rather than explicitly defending the validity of their results? What forces brought them there and how could we, as scholars of the scientific process, understand

¹⁶⁰ Portions of this section were originally presented at the 2004 annual meeting of the Society for Social Studies of Science (4S) in Paris, France (Delborne 2004).

¹⁶¹ Full webcast available at <http://pulseofscience.org/events/posevent> (Accessed 17 August 2005).

the significance of their behavior? The analysis below sheds light upon these questions and establishes a particular context for understanding the spectrum of scientific dissent from agonistic engagement to dissident science.

Stage management – Avoiding debate, showcasing discussion

Upon hearing of Arpad Pusztai's planned visit to the U.S., Chapela conceived the idea of holding a public event to discuss the pattern of suppression among scientists who had challenged agbiotech. Although a bench scientist, Chapela had significant experience in the worlds of politics and the arts, as suggested by the events described above.

Shunning the familiar pattern of debates between agbiotech promoters and critics,¹⁶² he envisioned the Pulse Event as an opportunity for showcasing a conversation. In an interview after the Pulse Event, Chapela commented:

I feel proud because I feel I've been learning how to put that on my frontal lobe about image, and about message, about theater. Because really it was a piece of theater. In my mind that's what I was doing, from the beginning, the conceptualization of it, the staging of it. Making it look as a television talk show. The size of the screen, everything was thought about. The flowers, the whole thing. I mean, it was horrible to get it to work because it was all off my cell phone by myself with students, just whoever decided to volunteer, as you know. But at the same time, within that framework of theatrics, I felt that we were also touching on something very important. That we were doing it in a way that is not usual, in that this openness of discourse, I think, is not usual in academia. The same thing that happened with the [open] office hours. I feel that we were doing what people believe the university does...I feel it worked really really well (Chapela 1/14/2004).

It was important to him that there not be too many people on stage and that the stage had a “living room” feel. He rented comfortable furniture, a rug, and flower arrangements to

¹⁶² Chapela explained his impatience with debates during an interview with the author. Chapela argued that debates are performances that remove scientists from a social context even further than they already are. He saw a debate as a poor point of intervention – one which purports to provide more information (as if that were always good) and conceals deeper questions and assumptions in the framing of the issue. He advocated for a process to engage people along the way rather than confronting them with something two opponents “regurgitate” in a predictable performance (Chapela 11/05/2004).

adorn the set. Complementing this homey, intimate presentation, Chapela also arranged for a complex internet component: 1) a website was created to advertise the event and provide a simultaneous webcast to groups around the world, 2) questions for the panel would be accepted via email, and 3) John Losey would participate from his Cornell office via webcam, with his image projected on a large screen on stage.

Chapela put significant energy into obtaining sponsorship for the event. Official sponsors included: The Knight Center for Science and Environmental Journalism at UC Berkeley, UC Berkeley's Bancroft Library Oral History of Biotechnology Project, Chapela's home department (Environmental Science, Policy and Management), UC Berkeley's Center for Sustainable Resource Development, the Berkeley Art Museum's Gene(sis) Project, the Council for Responsible Genetics, and The Jenifer Altman Foundation. Aside from some financial support, these sponsors greatly added to the credibility of the event. Michael Pollan, science writer and member of the faculty of UC Berkeley's journalism school, commented that having the "imprimatur" of the university boosted the Pulse Event's legitimacy: "People [are] encouraged to look at this as a bigger phenomenon, and not just four cranks" (Pollan 2003).

Characters – "The Dixie Chicks of the Life Sciences"

Although the characters of Chapela, Losey, and Pusztai have received significant attention in other sections of this thesis, it is useful to see how they performed their characters in this venue. First, however, I introduce the two journalists who helped frame the event. Subsequently, I introduce the panelists, including Tyrone Hayes, a contrarian

scientist who experienced severe resistance to his research on the toxic effects of a popular herbicide.

Michael Pollan, a recent addition to UC Berkeley's journalism faculty and a well-known *New York Times Magazine* science journalist, introduced the Pulse Event. Chapela originally approached him to moderate, but Pollan accepted what he perceived as the lesser role of introducing the panel. In an interview before the Pulse Event, Pollan explained his interest in the links between science and journalism:

Science journalism is kind of where political journalism was pre-Watergate. It is very dependent on authorities and institutions to validate truth claims...We go to scientific institutions...to tell us what's important...We have a lot of trouble independently assessing claims. So what do we do when something like *Nature* unpublishes something we've already published? We get all screwed up. We fall essentially silent...There's nowhere to stand and say Ignacio Chapela got unfairly attacked here...these charges are false...There's nowhere to stand...Frankly most scientific journalists punt (Pollan 2003).

As someone who had covered biotech as a journalist (Pollan 1998, 2001b) and popular author (Pollan 2001a), Pollan played the role of bridging the gap between the four scientists (main characters) and the audience. His introduction, discussed below as contributing to the narrative of the Pulse Event, contextualized the discussion as belonging to the larger issue of growing intolerance of dissent and diversity in the American political context. He described how a miniseries mildly critical of Ronald Reagan was cancelled by the CBS network; a politician who joked of the need for "regime change" was all but charged with treason by the White House; and the Dixie Chicks (a band) were excoriated for criticizing President Bush. By rhetorically linking the dissenting scientists with unreasonably punished political dissent, Pollan guided the audience both to sympathize with the panelists and to view their scientific controversies in a broader political context. In great theatrical style, Pollan introduced the scientists as

the “Dixie Chicks of the Life Sciences,” lending an additional degree of ‘hip’ credibility to their characters.

Mark Dowie, a freelance journalist on technology and the environment and lecturer at UC Berkeley in science journalism, served as the moderator. His short introduction made it clear that he sympathized with the plight of the four scientists, remarking that their work had been challenged not in the scientific spirit but because of the economic implications of their research. He later wrote an article for the *San Francisco Chronicle* that reinforced these sentiments:

Between 1999 and 2001, unbeknownst to the others, each made a simple but dramatic discovery that challenged the catechism of the same powerful industry -- biotechnology -- that by then had become the handmaiden of industrial agriculture and the darling of venture capitalists, who are still hoping they have invested their most recent billions in “the next big thing” (Dowie 2004).

On the other hand, Dowie performed the role of sifting through questions to pose to the panel, and did not shy away from a question submitted through the internet that challenged the scientists’ claims of suppression, given the high level of notoriety they had achieved. For the most part, Dowie functioned as a moderator, explaining how the evening would progress and inviting each panelist to introduce himself before questions and discussion.

Arpad Pusztai went first, and portrayed himself as a “conventional scientist” who had done nothing extraordinary.

[N]othing actually prepared me for what followed after my very simple announcement in 1998. This was a little television broadcast: 150 seconds of it, 12 sentences, in which because our money for the research which we did came from the British taxpayers, including myself, I thought that it was quite right for me to report back to them that what is the potential problem with GM food.

He emphasized that he had no prior commitment to the results he obtained:

We were really perplexed. Quite truly, I tell you. I was very perplexed. Because we had taken the gene and the gene product after 6 ½ years of studies for *not seeing* the things which we were seeing. And we couldn't understand it...[I]nsertion into the genome did cause some unpredictable changes with potentially harmful consequences.

Effectively heading off the accusation of bias, Pusztai positioned himself as a naïve scientist both in terms of the results obtained by his team and the political reaction to the findings.

So I naïvely reported this to the British public and the consequence was quite unbelievable. I never thought that as a conventional scientist, I'd have reporters parking and living on my drive-in to try to get my views on anything (laughs). I'm a very polite and humble person and I couldn't really understand how this happened...I reported something which was science and there were all these implications. So I leave this to you to ponder on: how does a straightforward, simple scientist get into this field of major international controversy?

Thus, despite Pusztai's extensive 'education' about the politics of science through personal experience and clear knowledge and opinion about issues such as scientific suppression, academic freedom, and corporate influence of research (see, for example, Pusztai 1998, 2001), he chose to present himself firstly as a naïve, unsuspecting, conventional scientist. To the extent that Pusztai remained within this strategy, his choices reflected well the practice of boundary-work to place his scientific personae back within the fold of legitimate science, mirroring the ideal of the objective, disinterested, and focused scientist.

Following Pusztai, Tyrone Hayes, Associate Professor of developmental endocrinology in the Department of Integrative Biology, UC Berkeley, introduced himself. He began by echoing Pusztai's claim of naiveté:

I have to say that the one thing we certainly have in common is that I was very naïve. In the sense that I was asked by a company to evaluate a chemical. At the time I certainly didn't think about the implications, about the compound. It was just simple science that I was getting into.

Hayes maintained his posture of a simple scientist as he told his story of his contrarian research and the resistance to it. Specifically, he framed the narrative as a series of disappointments, suggesting that he had held higher expectations of scientific, regulatory, and corporate institutions. He explained that in 1998 he joined a private research consulting group, Ecorisk, which was regularly hired by Syngenta (formerly Novartis). In this role, Hayes discovered that extremely low levels of atrazine, an herbicide manufactured by Syngenta, caused malformations in frog sex organs and demasculinization of male frogs. His first ‘disappointment’ was that Syngenta/Ecorisk encouraged him to repeat the work,¹⁶³ but more slowly, and had not disclosed data that suggested a mechanism for atrazine to function as a mammalian carcinogen through endocrine disruption. Hayes ended his relationship with Syngenta/Ecorisk and repeated the work on his own. Subsequently, Syngenta encouraged Hayes to submit a proposal to do the same experiments under its institutional umbrella, a move Hayes considered an attempt to purchase research he had already completed. When Hayes refused, Syngenta arranged a meeting on the UC Berkeley campus to attempt to intimidate Hayes and discredit his team’s results. Hayes ended the meeting, repeated the experiments on his own, and published the findings in the *Proceedings of the National Academy of Science (PNAS)* (Hayes, Collins et al. 2002). He recalled: “So I repeated the work. I just did it – did what my older peers that I looked up to told me to do. Do the science. Stick to the science. Stay out of the politics. So I did just that. I didn’t say a word.” Hayes thus

¹⁶³ Hayes was careful to say that he had no objection to repeating the study, especially given the surprising nature of the results (measurable effects at contamination levels lower than that allowed in human drinking water). He expressed disapproval, however, on being encouraged to slow down.

presented himself as a scientist striving for objectivity with a naïve trust of industry's ability to interface properly with academic science.

Hayes's second disappointment came from the EPA's reaction to his work. After he published a follow-up study showing similar effects on amphibians in the wild (Hayes, Haston et al. 2002), the EPA emailed him several times to warn him not to "provoke the company" since the company was "big" and would try to ruin his career. Indeed, according to Hayes, Syngenta sponsored numerous studies to 'replicate' his research, but all such research was conducted with extremely inappropriate methodology (e.g., a study in which 90% of the animals died). Hayes noted the tragedy that some doctoral students earned their degrees by doing nothing more than repeating his experiments "very poorly." In extending his concern to the educational experience of young scientists – even those who had produced work to invalidate his atrazine studies – Hayes claimed the high ground of a devoted academic. Simultaneously, Hayes' disclosure of the EPA's behavior in discouraging him from continuing his contrarian science portrayed him as committed to the public interest. Caring more for the public's 'right to know' the results of research on toxic effects of atrazine than his own professional security demonstrated his courage in comparison to the EPA, an agency with presumed institutional power and mandate to oppose "big" business in the name of the public interest.

Hayes' third disappointment again surrounded the EPA's apparent coziness with industry and lack of adherence to principles of integrity. According to Hayes, someone at the EPA sent him a research proposal submitted by a bioscreening firm for expert review. The proposal extensively plagiarized Hayes' own publications, which he announced to the EPA. The EPA responded by offering to contract some of the work to Hayes and also

explaining that the rules of the “ivory tower” did not “function in the real world.” The company denied having seen the publications, but Hayes clarified that he had Federal Express receipts of the studies he sent them. The company then denied having seen the raw data, but Hayes faxed the EPA copies of the raw data with handwritten comments by company representatives. The EPA finally notified Hayes that this indeed was a violation and they would prosecute the company. Hayes then learned that the company had arranged a meeting with the EPA, which apparently halted any disciplinary action, and was the last Hayes “heard of the case” (for additional details of Hayes’ experience, see Blumestyk 2003; Pierce 2004). Hayes thus presented himself as a dutiful, persistent, courageous, principled scientist with an impeccable research program. His careful description certainly revealed his critique of the role of corporations in influencing science and regulatory agencies, but he maintained a professional distance from those inquiries, making it clear that his life revolved around making ‘good’ science in the face of institutional pressures to do otherwise.

Losey echoed Pusztai and Hayes’ sense of being unprepared. He first described how he had not even intended to study *Bt* corn’s effect on monarch butterflies, but that he and his students happened to notice the pollen dusting milkweed leaves, which led to the laboratory experiment. This narrative placed him within the role of the archetypal inquisitive scientist, who allows his observations (rather than his values or biases) to lead his inquiry. Second, Losey testified to the spiraling controversy initiated by his *Nature* publication (Losey, Rayor et al. 1999): “The maelstrom of press that surrounded the publication of the article was just like nothing I have ever seen before. I had never dealt with the press before that happened. So it was overwhelming.” Although he did not use

the term “naïve,” he implied that his scientific training had not prepared him for the task of engaging political issues around his research. On the other hand, Losey explained that he had disagreed with Novartis’ urge to conduct more research before announcing any details of his monarch studies – mainly because the GM corn had already been released on twenty million acres. This perspective revealed sensitivity to the political context of his research, but it fell within the boundaries of ‘public-interest’ science. Losey never implied a position on a moratorium or ban on GM corn, rather simply advocated for further research to know the ‘true’ environmental costs and benefits of the technology.

Losey made two interesting moves that further clarified his personae at the Pulse Event. First, he read the following passage and asked the audience to guess which panelist the text addressed: “The publisher might want to reconsider its plans to publish this piece, especially in view of the inaccurate and disparaging statements about our products.” Losey explained that it had been a “trick question” as this had been written about Rachel Carson’s *Silent Spring* (1962), although it could have been written about any of the panelists and had a similar tone to the reactions to scientists first exposing the harm of cigarettes or the growing ozone hole. Losey thus aligned himself and the other panelists with historical scientific dissenters who courageously engaged science to expose public health threats by vested corporate interests. His related move, however, was more complex. He described the frustration of having environmental groups over-interpret his findings, which led to industry bashing him for things he never said. He explained how the mixture of corporate, scientific, media, and environmental actors fanned the “flames of controversy” and degraded the potential for needed scientific discourse. Losey thus

positioned himself as a scientist reluctantly engaged in a political controversy, but advocating for a return to de-politicized science to inform social and political action.

In contrast to the other speakers, Chapela announced that he would avoid talking about the specifics of his work, instead trying to “transcend” these stories. Nor did Chapela discuss the details of the resistance to his work or the peculiarities of his tenure denial. He explicitly differentiated himself from the other three scientists on stage:

I feel that I am different. I was not naïve when I got into this. I was kind of prepared. I'm a mutt, an academic mutt: I've worked for industry, I've worked for international organizations, I've worked for NGOs, I've worked for indigenous communities in different parts (especially in Latin America). And it was with that background that I ended up in this place that is called the Department of Environmental Science, Policy, and Management. The guys recruited me and hired me, and I was really elated, because this is a place where people are actually proposing that you can do science, and you can do serious thinking about that higher level of problems, that politics are part of science, that policy is something we can think rationally about.

Chapela's self-description stood in opposition to the previous three characterizations and challenged the archetypal view of proper scientists. Instead of framing his professional personae as insulated from politics (ideally, or striving to be so, at any rate) and focused upon strictly 'scientific' inquiry, Chapela offered himself as a hybrid. He did so with reference to some institutional legitimacy – his understanding of the stated objective of his interdisciplinary department – and with a nod to positivism (policy as “something we can think rationally about”). Nevertheless, this portrayal focused on shifting the frame of conversation away from personal stories of suppression and toward the historical and political context for suppression.

One might argue, however, that this stated strategy only thinly veiled Chapela's intention to draw more attention and sympathy from a mostly friendly crowd. The audience largely knew Chapela (an outspoken and well-known faculty member on the Berkeley campus and the hub of all efforts to publicize the event), and Chapela did paint

himself as a courageous young faculty member destined to have disrupted the smooth approval of the UC Berkeley-Novartis Agreement – because he knew “too much” and believed the faculty would have had concerns if they had known more about it. Chapela also took the opportunity to announce publicly that the administration had officially denied his tenure (bringing an audible hiss among the live audience). Coincidentally, *Nature* had just released their online version of the next day’s journal that included a news article extremely critical of Chapela’s tenure review (Dalton 2003).¹⁶⁴ Chapela arranged for copies of the article, entitled “Berkeley Accused of Biotech Bias as Ecologist Is Denied Tenure,” to be handed out to the Pulse Event’s audience. *Nature*’s report connected the controversy around the UC Berkeley-Novartis Agreement and the Chapela Maize controversy to his tenure denial. The article quoted Professor Wayne Getz accusing the tenure review process as having been “hijacked.” Getz, who served as one of five members of the confidential ad-hoc expert committee that reviewed Chapela’s tenure case, broke confidentiality to accuse the administration of watching over a corrupt process. He noted that the ad-hoc committee had unanimously recommended tenure, but that the chairperson had subsequently resigned under mysterious circumstances and that somehow the decision was overturned at a higher level.

While it is difficult to imagine how Chapela could have completely eliminated his personal story from the event, he did effectively background those details in favor of foregrounding a set of comments about the changing institutions of science, the academy, and the public. From a dramaturgical point of view, gathering the four characters with

¹⁶⁴ The Pulse Event was scheduled too far in advance to have intentionally occurred on the date of this publication.

their stories of suppression (ironically) gave them the legitimacy to discuss broader issues of academic freedom and the politics of science (areas outside of their professionally credentialed domains of expertise).

Narrative claims – dissent, biology and the public university

The scientists on the Berkeley stage sought legitimacy for their research in a public venue, but it was not an evening of fact-building in the traditional sense. Nor was it an evening of like-minded, marginalized scientists discussing their current work, comparing notes, and recommending important directions for future research in assessing the risks of agbiotech. Instead, by focusing on the broad notion of scientific freedom, the Pulse Event argued that the political and economic momentum of the agbiotech industry had suppressed and discouraged contrarian science. This assertion offered an alternative explanation for the harsh treatment of the four scientists' research other than as proper policing of the boundaries of legitimate science. As a self-conscious strategy to showcase patterns of scientific suppression, the Pulse Event argued that powerful social forces had interfered with the 'normal' practice of science, nearly producing scientific martyrs. While we cannot outright dismiss the possibility that standards of scientific excellence justified much of the resistance to the research conducted by Hayes, Losey, Pusztai, and Chapela, to accept this hypothesis at face-value would be nothing more than playing audience to mainstream boundary-work. The sociological value of paying attention to scientific dissent does not depend upon the dissenters being 'right'.

Michael Pollan's introduction contextualized the claim of scientific suppression. He framed the panel as a signal of a much deeper problem in contemporary science:

What you see before you here tonight are some pretty rare birds [Dixie Chicks], exceptional scientists willing to speak out, willing to go public. What we don't know is how many others have been silenced or intimidated into simply not asking troublesome questions, not designing controversial experiments...My fear...based on my own experience as a journalist...is that the population of such scientists is large and growing.

This 'tip-of-the-iceberg' rhetoric not only countered the exclusion boundary-work that attempted to marginalize these dissenting scientists, but also glorified them as scientists possessing great courage – elite by virtue of their visibility. Pollan explained the tragic irony that while political controversy has bred journalistic inquiry and analysis, scientific controversy has left journalists “completely flummoxed” and they “fall silent and decide to concentrate instead on the latest diet.” This has created a context wherein actors with an economic or political interest in a particular scientific outcome merely need to “muddy the waters” to discredit a piece of research – in other words, to offer some technical criticisms or produce a study with opposite conclusions in order to create the appearance of a lack of scientific consensus. The media silence - and therefore public inattention – has exerted no pressure for more science to be done. Pollan's introduction thus made the narrative claim that the public's typical window on scientific dissent was warped by institutional realities of journalism and the ability of corporate interests to shut down the path of investigation without even directly suppressing contrarian science. This claim not only encouraged the audience to interpret what they knew about these cases in a different light, but also implied the need for a different kind of 'scientific' response other than simply producing more contrarian data.

In his effort to de-personalize the event and not focus upon issues of his tenure denial and resistance to his own research, Chapela described two historical developments as defining the context for the Pulse Event conversation. He referred to them as two

“tragedies”, the first of which dealt with his perception of the changing discipline of biology:

We are watching drying on the vine - the dying of an extraordinary domain of human inquiry, something that we used to call biology. A natural science, a natural philosophy, a way of looking at the world that is being narrowed, channeled down to just one way of looking at the world...

Chapela compared the state of biology to the recent plight of physics as it entered the nuclear age, during which only one branch of physics became the dominant and legitimate domain of inquiry. Left unsaid was that nuclear physics led to atom bombs and associated nuclear technologies with enormous destructive power. Thus Chapela implied that the narrowing of biology would have consequences not just for intellectuals wanting the benefits of broad and diverse inquiry, but also for society which would inherit the technologies produced by a science restricted by technique rather than by wisdom. This critique of the trajectory of biology hinted at Chapela’s normative project of wanting to redefine science.

Chapela’s second ‘tragedy’ addressed the withering of the public university as an institution that harnessed science for the public good:

We are watching right before our eyes, right here in this space, the loss of an intellectual commons. A public sphere, that used to be, that has been paid by the public for many many years for generations to build these brick and mortar walls. (I’m very happy to deal with virtual worlds and so on, but we need these labs, we need these pipes, we need this glassware, and so on to run experiments.) And the public has worked hard at getting this, at securing this, and we’re watching it disappear.

Borrowing from the *Nature* article that detailed the break of confidentiality in his tenure case (see above), Chapela asked how a place such as UC Berkeley could become “hijacked so easily?” He referred to it as a “jewel of a university” and a “ship of truth seeking...that is being looted and pirated left and right.” Chapela gave the example of a

quote from Ed Penhoet, former dean of the school of public health and a CEO of a local biotech company:

I think that [the biotech] industry, with more than 4000 companies and now all the pharma companies, have done an extraordinary job of extracting *all the value* from the basic science that gets conducted in universities, funded by NIH. And the notion that there is a lot of extremely valuable science left untapped in this country and that needs somehow an outlet different from the biotech companies, seems to me simply incorrect [emphasis by Chapela when spoken].¹⁶⁵

Continuing with the ship metaphor, Chapela then asked whether the university had been adrift, vulnerable to pirating, or just in need of repair. He rejected these passive explanations and offered:

I would claim that this is a ship that has been purposefully and consciously captained and steered into the shoals where it can be pirated. I would posit, and I know that this is uncomfortable for many of us, but we have been too much of a willing crew. I think it's really easy to go after Novartis and Monsanto, and we should go after them because they deserve it. But there is a cultural opportunity that we give, that we make possible for them. It's not Monsanto, not Novartis that took my tenure away. It's us. There's something intrinsic in the institution that we really need to think about.

This passage and Chapela's subsequent call for a public "rudder" for this "ship" captured the complexity of his argument. Chapela made it clear that the cause of the loss of scientific freedom was not simply improper external intervention by Novartis and Monsanto. He argued instead that we—the professors, the students, and the public—had neglected our duty to hold science accountable.¹⁶⁶

¹⁶⁵ Chapela retrieved this quote from an article posted on the UC Berkeley News website: "Biotech panel marks 50th anniversary of DNA double helix discovery by contemplating both the past and the future," by Robert Sanders, Media Relations, 16 October 2003. http://www.berkeley.edu/news/media/releases/2003/10/16_biotech.shtml. Accessed 22 August 2005.

¹⁶⁶ The article in the *San Francisco Chronicle* picked up on this theme: "The sad part is that the academies and other allegedly independent institutions that once defended scientific freedom and protected employees like Hayes, Chapela, Losey and Pusztai are abandoning them to the wolves of commerce, the brands of which are being engraved over the entrances to a disturbing number of university labs" (Dowie 2004).

Audience

Approximately five hundred persons was an impressive turnout for a mid-December evening – campus faculty and students were preparing for finals and there was heavy rain, which tends to discourage weather-phobic Bay Area residents. It is not possible to know precisely how many virtual viewers watched the webcast live or have seen it since, but some data suggest that the audience stretched significantly beyond the confines of the university: 1) the web coordinator during the event received nine separate questions via email for the panelists, representing at least three countries, 2) the Pulse of Science website that served as a central communication point for the event had received over five thousand hits by early January, representing one thousand servers (indicating high geographic and institutional diversity) and tens of countries (Chapela 1/13/2004). In addition, both the *San Francisco Chronicle* and *Nature* covered the event, greatly expanding the media audience (Dowie 2004; Knight 2003).

Because of a variety of intentional choices of stage management, the Pulse Event drew in the audience to participate in the discussion. Chapela staged the conversation in such a way that the audience (or viewer) felt a part of the ‘living room’ in which the scientists were conversing. Chapela also went to great pains to incorporate audience questions and comments into the performance. He distributed blank cards for questions throughout the live audience and encouraged virtual viewers to send emails. A team of graduate students from the Berkeley Biotechnology Working Group, including myself, read through the questions and did an initial round of prioritizing and sorting. We then gave this organized stack to the moderator, who used his discretion in choosing questions to pose to the panel in the last third of the evening. This process created an intentional

role for the audience to become engaged beyond the passive role of attending a lecture or debate. It is interesting, however, to note that Chapela made no arrangements for the audience to participate in a conventional political action such as signing petitions, conducting a letter writing-campaign, or even signing up for a mailing list for future actions. Instead, Chapela relied upon the website to serve as his ongoing link with this audience, a much more passive strategy than those just mentioned.¹⁶⁷

Aside from the procedural decisions, the narrative components also engaged the Pulse audience. Chapela ended his remarks with a discussion of the term, ‘time of useful consciousness (TUC)’, borrowed from a radio program with the same name and referring originally to the Navy’s terminology for the brief moment when a fighter jet is going so fast that the pilot is about to pass out and has just enough time to take action to save the plane. Chapela urged the audience to see themselves in this moment with respect to the loss of biology and the loss of the intellectual commons of the public university. Likewise, Pusztai implored the audience, “all these clever people,” to figure out some solution to the problem of the disjoint between corporate science and the public interest, specifically asking, “Please, all the economists come rescue us, for all our sake!” While this plea may have struck the audience as a narrow appeal, Pusztai’s intention was clearly to expand the concern of science beyond the conventional scientific community.

Nature’s news coverage of the Pulse Event raised important questions about the construction of audience. Two weeks after the event, they published a short piece

¹⁶⁷ The *Pulse of Science* website (<http://pulseofscience.org/>, accessed 23 August 2005) is a complete overhaul of Chapela’s original website to advertise the Pulse Event. This reformulation explicitly attempts to create an online community for discussion and dissemination of information. The *Pulse of Science Fund*, a follow-up initiative by Chapela that was launched in November 2004 attempted to raise money to assist Chapela with his tenure battle and also begin to create institutional space for critical thinking and research about biotechnology, scientific freedom, contrarian science, public universities, and corporate science.

entitled, “Scientists Attack Industrial Influence” (Knight 2003). The article characterized the event as a “rally,” using that word—and no other—three times in just over 200 words. This upset Chapela and others who had helped with the event, including Mark Dowie (the moderator) who described it as an “unfair characterization” (Dowie 2003). Although the audience showed tremendous support for the panelists, the event lacked the character of a rally: the discussion lasted nearly two hours, no signs or slogans were used, and there was no identifiable target or action that attendees were asked to take. Chapela contacted Jonathan Knight, the author of the article, to question his use of the descriptor. Knight responded that he had not, in fact, used the word “rally” in his submission to his editor (Chapela 1/13/2004). I verified this in an interview with Knight, who admitted that he was shocked at the word choice and knew instantly that it would upset readers who supported Chapela or attended the Pulse Event.¹⁶⁸ Knight explained that the short turnaround time of publication prevented the standard practice of an author seeing a copy of the edited article before publication, and he hypothesized that his editor had simply made a misinformed choice. He emphatically defended the integrity of *Nature*’s news division and said that he could not imagine that his editor was pressured (Knight 2004). Upon my request, Knight put me in touch with the responsible editor, who did not respond to my requests for an interview or email exchange (email to Colin Macilwain from author, 16 June 2004). One might wonder whether the same intellectual and political stance within *Nature* that resulted in the unprecedented withdrawal of support of the Quist and Chapela publication could explain this word change. Without a doubt,

¹⁶⁸ Knight explained in an email: “The word rally does not appear anywhere in the story I filed. I used 3 terms in various places for the event: 'event', 'gathering' and 'forum' or 'public forum'” (email from Jonathan Knight to author, 2 June 2004).

classifying the Pulse event as a “rally” did tremendous discursive work to disrupt the event’s intended construction of an audience engaged in serious intellectual work and reflection.

Whether considered part of the audience of the Pulse Event or one of its targets, the UC Berkeley administration became embroiled in the discussion. Not only did Chapela announce his tenure denial with reference to accusations of bias and misconduct (Dalton 2003), but also a virtual audience member submitted a question that directly challenged the Chancellor’s ability to impartially oversee Chapela’s tenure case. David Noble, Professor at York University in Toronto, submitted the following email question/comment during the Pulse Event:

SUBJECT: foxes in the chicken coop
QUESTION/COMMENT: Corporate influence on academic science comes from within as well as from outside the university. How does the fact that Chancellor Robert Berdahl sits on the board of directors of the multinational Lam Research Corporation, for which he receive roughly \$40,000 annual compensation plus benefits, affect his decisions on the careers of people like Professor Chapela?¹⁶⁹

The moderator read an abbreviated version of this question to conclude the Pulse Event, addressing it to the Chancellor (who was not physically present). Thus, regardless of the intention by campus administrators to participate as audience to the event, they were constructed as part of this audience – as perceived by actual viewers and in all likelihood

¹⁶⁹ Berdahl is listed as a member of Lam Research Corporation’s Board of Directors (http://www.lamrc.com/main.cfm?section=1&subsection=2&subsubsection=2&subnav=subnav_1.cfm&contenturl=company_2_2.htm, Accessed 23 August 2005). According to Lam’s website they are “a leading supplier of wafer fabrication equipment and services to the worldwide semiconductor industry” (http://www.lamrc.com/main.cfm?section=1&subsection=1&subsubsection=0&subnav=subnav_1.cfm&contenturl=company_1.htm, Accessed 23 August 2005). The lack of a clear connection between wafer fabrication and biological science makes Noble’s accusation less severe than it sounded during the event, but the larger question about the appropriateness of a university chancellor serving on *any* board of directors for a for-profit corporation still stands.

dragged into paying attention to this accusation against the Chancellor as it was made in public.

11.4 Summary

The performances described above shed light upon what I have come to call dissident science. Unlike agonistic engagement, which contains dissent within accepted rules and norms of scientific conduct, dissident science explodes beyond conventional discourse. Some may argue that this phenomenon is nothing more than ‘activist science’, terminology that is invoked only to de-legitimize scientists by implying that their values guide their inquiry, interpretations, and conclusions irrespective of evidence. As such, ‘activist science’ does little but to reveal attempts at severe boundary work on the part of the speaker.

I also differentiate dissident science from activism that engages science. Activist organizations such as the ETC Group, Greenpeace, and Food First used scientific results produced by Chapela, Losey, and Pusztai to justify their policy recommendations, but these groups primarily hold research at arms length. They are not involved in attempting to produce scientific ‘facts’ that hold credibility within communities defined by scientific norms. Exceptions may exist when such organizations do conduct primary research to bolster a campaign, but these organizations tend to maintain very weak ties to discourses within the scientific community.

By contrast, dissident science represents a strategic attempt to expand scientific discourse beyond the conventional realms of fact-building and maintaining boundaries of legitimacy. Dissident science incorporates the politics of science as well as the larger

context of policy and governance beyond what is normally considered the scientific realm. It is an approach that challenges the practices of conventional science and boundary-work, but without abandoning the vision of science as a special realm of knowledge-making.

Chapter 12 From Agonistic Engagement to Dissident Science

While the previous chapter focused on performances of (mostly) dissident science, this chapter analyzes the strategies inherent in such performances. The performances were tactics (enactments of strategies), and were implemented with varying degrees of success. Black Canvas, for example, impressed the art community but likely alienated many of the audience members who physically attended the panel discussion. Open Office Hours brought media attention to Chapela's professional struggle, created an informal network of supporters that participated in subsequent actions to protest UC Berkeley's handling of his tenure case, and motivated the campus administration to deliver a contract extension long-awaited by Chapela. The Pulse Event successfully transcended the particular controversies of the participating scientists to establish a discourse about academic freedom and intellectual suppression, but *Nature's* characterization of the event as a "rally" lessened this achievement in the eyes of its readers. In other words, the dissident performances had diverse consequences on a variety of audiences.

This chapter shifts attention from tactics (performances) to strategies (approaches to responding to resistance). I draw heavily from the three events and other examples from the case studies to present six underlying strategies of dissident science: exposing the resistance, making the politics explicit, expanding the network beyond the scientific community, conducting anti-autonomous boundary work (bringing in the public), mimicking a social movement, and shifting the intellectual terrain of debate. In contrast

to agonistic engagement, these strategies not only operate within the discourse of their respective controversies (at the level of critique), but also challenge conventional relationships among science, politics, and publics (at the level of meta-critique). As such, dissident science represents both a functional approach to responding to resistance and an institutional intervention that carries political and rhetorical risk. These are the stakes of scientific dissent.

12.1 Expose the Resistance

The first dissident strategy involves intentionally exposing the resistance to contrarian science. Creating a narrative that discredits resistance on the basis of its origin, character, timing, or severity differs greatly from the expectation that scientists would respond to attack with yet harder ‘facts.’ At the Pulse Event, for example, Losey described how Novartis representatives discouraged him from publishing his study, implying that their self-interest guided their behavior rather than credible scientific critique. He went on to associate himself and the other panelists with Rachel Carson, as if to transplant what we all ‘know’ about the unjust and economically-motivated resistance she endured to the stories of each of the panelists.

In a similar move, Pusztai published an article in *The Ecologist* entitled, “Academic Freedom: Is It Dying Out?” (Pusztai 2000). The first part of the article told his story with particular emphasis on the unjust resistance he faced. The following excerpts demonstrate the exasperated tone Pusztai adopted:

Whatever his precise motivation, the Rowett's director swung round, in a matter of hours, from publicly supporting my work to publicly attacking my very integrity... Perhaps, however, even these comments were too much for the sensitive biotechnology industry, for at this point, events took a Kafkaesque turn...

To my mind, the entire point of the audit was to create a 'show trial'. None of the many nutritionists at the Rowett were appointed to the audit committee set up to judge my work, no proper statistical analyses of my findings were carried out by the committee, and I was given no opportunity to explain my work and the director's mistakes. The whole audit was over in less than 10 hours...

THE WITCH-HUNT CONTINUES

Astonishingly, this was not the end of the matter. It was apparently felt that I had not been slandered and discredited enough by the scientific and political establishment. The House of Commons Science and Technology Committee, Royal Society (RS), Committee on Toxicology, Advisory Committee on Novel Food Processes (ACNFP), and the Nuffield Foundation Bioethics Committee all produced virtually identical, and equally damning, reports on my work within the space of a few days...None of these reports included any direct input from me...Most tellingly, none of these institutions would disclose whether they, or those preparing the reports, had any links with the biotechnology industry (Pusztai 2000).

This description, with frequent implications of corruption and bias, framed the entire controversy around Pusztai's study as unworthy of scientific attention. By exposing the character of resistance, Pusztai shifted attention away from the technical details of the critiques and highlighted the apparent political tension surrounding the science as the key to understanding the controversy.

Chapela invoked this strategy in the Open Office Hours and the Pulse Event. The metaphor of the 'open office' served to expose the closed, secret, and presumably illegitimate resistance to his tenure. At the Pulse Event, although Chapela did not dwell on the particulars of resistance to his research or his tenure, the act of gathering the four panelists on one stage – linked primarily by the implication that similar forces had resisted each of their work – followed the strategy of exposing resistance. As mentioned above, he also took the opportunity to feature the *Nature* article that accused UC Berkeley of bias in a tenure review process that had been "hijacked" (Dalton 2003).

In one sense, this rhetorical strategy differs little from attacks on contrarian scientists that focus on their identities and affiliations rather than the quality of their scientific claims. Dissident scientists employing this approach thus risk losing their

public position ‘above’ the temptation to engage in mudslinging. More poignantly, exposing the resistance reveals a willingness to mix the politics surrounding research with the validity of research – shaky ground in a cultural context in which the imagined barrier between science and politics provides much credibility to the domain of research. The strategy holds promise, however, by appealing to an audience beyond the scientific community with a discourse of justice and fair play. Revelations that taint the motive of resistance or character of resisters not only reduce the demand for technical responses to particular criticisms, but also begin to frame the context for science as so ‘stacked against’ contrarian science that the entire field of resistance becomes discredited – despite protests from particular resisters who wish to claim that *their* critiques come from an appropriate and unbiased perspective (see, for example, Kaplinsky 2002).

12.2 Make Politics Explicit

A related strategy involves making the political context of science more explicit, without necessarily focusing on particular examples of resistance. This carries a slightly lower risk of creating the impression of a scientist resorting to politics in self defense.

At the Pulse Event, Losey described the unwanted influence of industry money on research that investigated the safety of GM crops. Rather than focusing on an egregious instance of a corporation funding a biased study, Losey described the benefits of an institutional scenario in which parties from multiple sectors organized a research initiative. He spoke about how industry and regulatory agencies provided funds for follow-up studies on the relationship between monarch butterflies and *Bt* corn, but that a consortium of industry, regulatory, academic, and NGO scientists prioritized the research

proposals. Losey described this as a “brief, shining, Camelot-like period” and contrasted it with the norm of relying upon industry to fund safety studies which are sparse and rarely peer-reviewed. Losey thus managed to make a very critical and political point about the involvement of industry in science-making, but with a degree of personal distance from the claim.

Black Canvas included many similar references to the corrupting influence of the biotech industry on the science produced inside and outside of industrial laboratories. Chapela made the additional jump to claim that this influence had even permeated a local police force:

Earlier this year in St Louis Missouri, Monsanto’s town, I sat in a panel at the Biodevastation conference next to an empty chair. Sarah Bentz, who should have been there to speak, had been picked up - together with more than twenty others - by the City Police on her way to the conference, eventually charged for carrying her vitamin C in her pocket. May the canvas keep her absence present, together with all the others’ (Chapela 2003c).

This echoed a passage Chapela had earlier included in his email announcement of his Open Office Hours: “Fear is justified when even the president of the country equates with criminal acts any questioning of the wisdom of deploying transgenic crops” (Chapela 2003b). Statements such as these suggest an overwhelming climate against contrarian science in agbiotech without directly attacking resistance to particular research. Nevertheless, it remains a dissident move by framing science as circumscribed and permeated by political concerns.

12.3 Expand the Network beyond the Scientific Community

While recruiting additional allies with scientific credibility remains an agonistic move, the case studies also include examples of scientific dissenters reaching beyond the

scientific community for support. From one perspective, this strategy represents nothing more than building power through an extended network. More critically, however, such moves challenge the narrow definition of scientific legitimacy, as defined in conventional scientific discourse, by incorporating other sources of expertise to build credibility.

Chapela frequently expanded his network of support beyond the scientific community. He announced his Open Office Hours at a dinner hosted by the International Forum on Globalization (IFG) as part of an activist-led protest to the USDA's ministerial meeting on agricultural technology in Sacramento in June 2003. His written announcement, emailed later that night, represented a collective process of writing with colleagues and friends in the social sciences and humanities, what Chapela called "the social part of the commitment" (Chapela 6/30/2003). When asked toward the end of the Open Office Hours to recall moments that exemplified the event, Chapela described a lunch of Mexican mole brought by a friend and California cuisine catered by Chez Panisse, attended by a "very interesting mix of people: everything from a homeless person to senior professors, from scientists to social scientists, to activists, [and] the media" (Chapela 6/30/2003). These details suggest an appreciation for the role of non-scientists in supporting his science.

In an interview the month before the Pulse Event, Chapela described the support network he had cultivated:

This event that I'm putting together – the fact that these organizations are coming out to provide precisely an umbrella to be able to speak under – tells me that, yeah, there is something like that [mentoring support] if you know how to tap into it...This is really important, I think it would be interesting to study, which is: As you're developing your career, what contacts, what networks, what support networks do you need to establish that would allow you to stick your neck out and survive? Because the common assumption is that you stick your neck out and you die. Right? And I think you have case studies where you can see that. Most whistleblowers die, but not all. I do see a very big difference between myself and people like Pusztai or even John Losey. I think that's

going to be really interesting to explore. We've never been together to talk about these things. But what we have, I think we have a difference, and the difference is that I've been active in the world of NGOs, in the world of activism, outside academia, in such a way that when I become a whistleblower, if you want to call me that, within academia, I have a network to fall back onto without dying, where I don't become dead morally... You can be killed if you don't have roots anywhere else. Whereas I feel that I did have those roots, that I did have that network, and that has been really my sustaining force. And then, yeah, of course, the colleagues inside the university and so on who are not vested in this other story have been really supportive, but they've been supportive only secondarily. If I was relying only on my colleagues here for moral support, I don't know where I would be. I would probably be dead, or have given up and gone somewhere else (Chapela 11/6/2003).

Chapela went on to recommend that scientists should work outside of academia to “roam the world” and make other contacts, and then come back with relationships that prevent them from working in a “system of validation [that] becomes five people.” Thus, Chapela drew a connection between the ability to be an effective dissenter (or whistleblower, in his terms) and a support network that transcended the conventional scientific community.

On the other hand, Chapela maintained his own boundary for his network, which he enforced with varying success. As described above, Chapela refused to allow activists from NGOs to define the message of his Open Office Hours – taking down their signs after they left. He also reported the discomfort of having a large group of young people who “got all touchy-feely,” brought candles and incense, and almost made it a “religious thing.” Near midnight, when Chapela was due to end his vigil, they pushed him under a blanket and began burning palo santo to create a kind of “personal sweat lodge.” Chapela, exhausted from the five day event, just began pushing his cart of possessions away at the strike of midnight, yelling “goodbye, goodbye” and instructing them to clean up the flowers and symbols they had brought with them (Chapela 10/21/2004). Extending networks of support beyond scientists does not translate into throwing the gates wide open, but managing a new boundary requires effort.

Pusztai also described the tension of maintaining networks of support beyond the scientific community. He recognized the advantage to being seen as an “independent scientist” rather than associated with any NGOs or “pressure groups.” But he also lamented the failure of the NGO community to truly support him in his fight against the Rowett – he lacked sufficient legal funds to break his gag order and knew that organizations such as Greenpeace or Friends of the Earth could have provided this backing.¹⁷⁰ In retrospect, Pusztai commented that “perhaps it was good that it happened this way...[because] any alignment with the NGOs is a double-edged sword.” On the other hand, he recognized that his claims had resonance with the views and perspectives of the NGOs and he viewed his role as supplying them with “ammunition” (Pusztai 2003).

This tension over unconventional networks of support for dissenting scientists has the potential to create a positive feedback loop. If resistance originates within the legitimate scientific community, and dissenters respond by recruiting non-traditional allies, those new connections can serve as further evidence or fuel for resistance *based on those affiliations*. For example, in responding to charges of conflict of interest, Kaplinsky brought up Chapela’s position as a board member of the Pesticide Action Network-North America (PANNA) as an indication of potential bias (Kaplinsky 2002). If such accusations further marginalize the dissenter, he may seek more protection and support from realms outside of the scientific community. In this way, dissident behavior can become self-reinforcing, a pattern especially apparent in the Chapela Maize case.

¹⁷⁰ Pusztai thought that the NGOs had believed the propaganda discrediting his work, and that they had not wanted to risk their reputation and resources on sloppy science (Pusztai 2003).

12.4 Anti-Autonomous Boundary Work (Involve the Public)

Gieryn's (1999) typology of boundary-work includes the practice of scientists defending their autonomy to define realms of inquiry as belonging to the domain of science, thus positioning themselves as the key experts to define questions, conduct research, and draw conclusions from evidence to produce 'truth'. Indeed, many scientific institutions reinforce this boundary between scientists and laypersons that keeps the public in a dependent, non-participatory relationship with science (e.g., tenure processes, scientific advisory boards, peer review, governmental funding requirements and patterns of research). In light of the previous discussion of the move to expand networks of support, the potential emerges for a dissident move to weaken the boundary of scientific autonomy to involve the public more fully in scientific judgment.

At the Pulse Event, Pusztai described his decision first to appear on television in a manner that stretched the conventional understanding of scientific peers: "As a simple scientist, I reported this fact to my peers, which were the British taxpayers, and told them there are problems." On one level, Pusztai emphasized the duty of publicly-funded scientists to make their results known to the public as the patron of their work, but his choice of the word "peers" communicated much more. Within the scientific community, 'peer' confers legitimacy in judgment and promotion and thus holds tremendous power (e.g. peer review). By equating taxpayers to scientific peers, Pusztai deconstructed the boundary that excluded the public from science-making, implying that television viewers were capable of understanding his preliminary research and interpreting the significance appropriately. While this rhetorical move at the Pulse Event might signify an *ex post facto* justification of discussing research on television before publishing in a scientific

forum (something for which he was criticized), Pusztai's choice signified a dissident act even if it did not provide an accurate window into his motivations in 1998.

Chapela's response to a question about the meaning of democratizing science revealed the complexity of this boundary-work around the autonomy of scientists in relation to the public:

[Democratizing science] certainly is not doing science by consensus or public voting...Obviously that's not conducive to anything good in science. So it's not a popular science making...[I]f you see yourself as 1) part of a community and 2) a representative of that community, then the type of questions that you ask, the field in which you position yourself, and what experiments you decide to run changes entirely. So if I think of anything around the word 'democratizing science', I would turn it around a little and say it's more about incorporating science and scientists into the operation of society. We have worked for many years in exactly the opposite direction...and building this myth into the training of a scientist, that says that whatever crosses your imagination is fine and that is what should be done, and the idea that those things that cross your imagination have nothing to do with your life experience, your state in life, and your connection to other people (Chapela 11/5/2004).

Chapela thus challenged the mythical notion of scientific freedom of scientists unfettered by connection with or responsibility to anything other than their curiosity (see Polanyi 2000 [1962]). In effect, he argued for re-embedding scientists in a social context – *reducing* their autonomy from the public – in order both to counterbalance the growing connections between corporations and scientists and to align the practice of inquiry with an identity that emerges from community. At a deeper level, this approach denied the *possibility* of scientific autonomy in the Polanyian sense and argued that the relevant question was not primarily the degree of absolute autonomy but rather *which* connections scientists had to *which* communities.

In November 2004, Chapela held a fundraising event to launch the Pulse of Science Fund. Although certainly worthy of full description as a performance of scientific dissent, two components illustrated Chapela's desire to integrate aspects of the

public into his science. First, much like the environment he created at the Open Office Hours, he aimed for a participatory experience rather than coming “to get something, or just give money” (Chapela 12/3/2004). The program allowed for extensive conversation, pads of paper were displayed for the contribution of ideas, two videos played in separate rooms, and Chapela made his tenure case file available for unsupervised examination. Second, Chapela created poster displays that showcased the various events he had organized, including the original ‘Black Canvas’ with an explanatory note, pictures from his Open Office Hours, and newspaper clippings from his involvement with the Novartis controversy and initiatives to ban the planting of GM crops in Mendocino. He recalled:

What I wanted to achieve was to be able to show a track record...I think people started getting the feeling that, oh, so that little thing he did with the canvas thing, and that little thing he did with the brochures there, and sitting outside the chancellor's office, are not disarticulated. They're actually part of -- it's becoming like a track record of publications. It's part of my vitae. So putting my vitae on display was part of my goal. For people to realize, well if I'm going to put my interest and my money into this, it's not a new initiative (Chapela 12/3/2004).

In considering his public actions as part of his professional curriculum vitae, Chapela revealed deep assumptions about the role and judgments of the public in providing legitimacy to his science. The boundary of scientific autonomy would make such a presentation not only irrelevant but irreverent, by supposing that laypersons had the capability and right to engage with the practice of science-making.

12.5 Mimic a Social Movement

Social movements addressing biotechnology, food issues, environmental issues, corporate control of research, and the university-industry complex all took up the stories of Chapela, Losey, and Pusztai in their campaigns. Losey demonstrated the least enthusiasm for this interaction, even commenting at the Pulse Event that environmental

groups had blown out of proportion his findings from the monarch study, which led to industry groups attacking him for things he never said personally. Losey's ambivalence about participating in the Pulse Event, which led to his inability to appear in person, also showed his reluctance to align his science with an event with some flavor of a social movement. Pusztai maintained a more ambivalent relationship with NGOs such as Greenpeace and Friends of the Earth – as noted above he lamented their lack of critical support, but appreciated, at least retrospectively, that his lack of direct affiliation with any activist group protected his credibility as a scientist. Nevertheless, Pusztai set up a website that resembled a campaign information website¹⁷¹ and he participated in numerous forums and debates worldwide, including the Pulse Event. His publication of an article with ActionBioScience.org provided further support of his collegial orientation toward engaging his science with social movements (Pusztai 2001).

Further along the spectrum, many of Chapela's actions had great resonance with a social movement. His Open Office Hours, although framed as 'office hours', resembled an occupation, sit-in, and protest. The *Daily Californian's* headline to describe the event certainly echoed this perspective: "Professor Camps Out in Front of California Hall in Protest" (Wittmeyer 2003). Black Canvas also reflected the strategy of protest – refusing to participate as expected and instead arranging for a performance that critiqued the entire exhibit as a corrupted context for conversation.

The following year, with his tenure officially denied, his contract extension ending at the close of the fall semester, and awaiting the results of an appeal of the tenure decision, Chapela taught his presumed last class as a UC Berkeley professor on 9

¹⁷¹ <http://www.freenetpages.co.uk/hp/a.pusztai/>. Accessed 1 September 2005.

December 2004. Supporters packed the room, announced plans to submit petitions and demands to the administration, and donned armbands of green and red material. Chapela explained that the colors represented hemoglobin and chlorophyll, “magical” proteins that regulated energy capture and release at the biochemical level, but also had significance at larger scales of concern (e.g. plants and humans, national flags, etc.). He also described how the salesperson at the fabric store had responded to his question about what the two colors made her think of: she answered, “A movement.” From the classroom, the mix of students, faculty, and concerned citizens and activists marched to the front of California Hall and rallied with signs, chants, and speakers protesting the impending loss of Chapela as a faculty member. Chapela did help to coordinate these actions and gave a speech at the rally that, in part, described how the administration had removed one of the door handles of California Hall during the Vietnam War era, which had the effect of physically sequestering the administration from organized protest. It was clear to participants in that event, hearing from the variety of speakers that included Chapela’s lawyer explaining their lawsuit against the university, that a campaign was being waged to overturn the denial of tenure (also see www.tenurejustice.org), and that Chapela was a central figure rather than simply the object of a social movement surrounding him.

While mimicking a social movement creates opportunities for building diverse sources of power that may influence the context and practice of science, this extreme departure from agonistic dissent carries high risks in terms of undermining legitimacy. Wayne Parrott, a scientist who submitted a formal critique of Quist and Chapela’s (2001)

Nature manuscript, accused Chapela of resorting to a political campaign because he could not defend his science properly:

Parrott believes the ruckus that followed publication of the paper was actually instigated by Chapela himself and raised to a fearful craze by the anti-GM community... "After all these letters went to *Nature*," says Parrott, "Chapela turned around and rallied the troops. They said we were mudslinging. They said we were in industry's pocket. They said that we were unethical, and that what we did was totally uncalled for. All that had to have come out of Chapela because anyone who publishes had better be ready to defend what they publish. And he published something that was trash and indefensible (Lepkowski 2002).

For Parrott, the existence of a social movement around Chapela implicated him as a sponsor of that campaign. Chapela's frequent appearance at and organization of activist events placed him in a position of vulnerability to such accusations. A related example involved *Nature*'s use of the word "rally" to describe the Pulse Event (Dalton 2003). As described above, Chapela had aimed for creating an event with a high degree of scientific and political legitimacy, a performance quite distinct from a rally or protest. An editorial decision – perhaps a misunderstanding, perhaps reflecting political bias, perhaps based upon historical knowledge of Chapela's actions – re-framed the event for a media audience as a component within a social movement. This upset Chapela, not because he would refuse to participate in a rally, but because he understood that the discourse of activism could undermine scientific credibility in certain contexts such as among the readers of *Nature*.

12.6 Shift the Intellectual Terrain of Debate

A key feature of dissident science involves shifting the intellectual terrain of debate from controversy over facts to a challenge of the very institutional boundaries and power structures that define scientific legitimacy. In this sense, dissident science is a

synthesis of intellectual struggle and social action. The cases under consideration demonstrate at least four moves that attempt to redefine the landscape of controversy.

The academic-industrial complex hinders the production and dissemination of contrarian science.

During the Pulse Event, Losey alluded to the lack of independent resources to fund GM safety research. Chapela drew consistent parallels between the treatment of his research and his tenure case with affiliations and commitments by scientific institutions with industrial organizations. Likewise, Pusztai wrote:

When scientists who apparently have no obvious financial connection with the biotech industry defend GM crops so blindly, and attack even the mildest critics, slandering their work and abilities in the process, we must ask ourselves what motivates them. And one possible motivation is that, with the rapid disappearance of the State patronage of science, many of these people are genuinely worried about the future funding of scientific research.

Perhaps they feel that the only chance for the survival of research in the 21st century is to set up an alliance with industry. So they may have to embrace this new creed wholeheartedly, warts and all, and throw their whole weight behind genetic manipulation, regardless of what they may individually think about its merits...

But this is a very dangerous attitude. By accepting money from an industry which has aggressively set out to dominate many aspects of life and society, science and scientists are becoming servants of multinational concerns whose motives are at best questionable and at worst positively detrimental (Pusztai 2000).

Drawing attention to the institutional context of science as an explanatory factor in the production of knowledge challenges the view of science as immune from pressure and improper bias. Thus, for scientists to make such claims, they leave themselves open to the challenge that *they* are the ones vulnerable to influence and incapable of objective research while the rest of the scientific community remains trustworthy.

Creating space for contrarian science is an issue of academic freedom.

Both Pusztai and Chapela used the term “academic freedom” in relation to their perception of improper resistance to contrarian research. This rhetorical move shifted attention away from the questions around the technical quality of their work to the patterns of resistance across the field of contrarian science in agbiotech. Chapela’s framing of the discussion among Losey, Pusztai, Hayes, and himself as “The Pulse of Scientific Freedom in the Age of the Biotech Industry” revealed a strategy to move away from a martyr discourse (focusing on narratives of mistreatment and personal tragedy) to a social discourse about the relationship among citizens, scientists, public universities, and corporations. The risk of this strategic shift involves appearing to be unwilling to engage in the technical details and stand up to the ‘organized skepticism’ we expect in the scientific community.

The politics surrounding the context of agbiotech cannot be disentangled from the practice of science.

Losey’s reading of the letter discouraging the publication of *Silent Spring* implied that contrarian science always faces opposition on the basis of the political implications of its claims. Likewise, the Pulse Event as a whole brought forward not only the climate for contrarian science in agbiotech but also the national trend of intolerance of dissent as relevant for understanding the resistance to the four scientists’ work. As described above, this dissident claim runs the risk of upending the essential status of science as a method of knowing that can be relied upon in the face of economic or political pressure. Those

who advocate for this non-essentialist view thus risk appearing to advocate for science wholly dependent on institutional context.

The boundary separating the public from science-making has become problematic, both from a political point of view and from the perspective of the quality of knowledge production.

Chapela's performances, especially, worked to draw the public or segments of non-credentialed scientists into his work. He emphasized this theme in his critique of the public university as adrift because of a lack of public accountability. Beyond complaining, however, Chapela came to advocate for safe spaces for the public and science to intimately connect. Beginning with the symbolic and discursive space of his Open Office Hours, developing further during the Pulse Event, and culminating in the launch of the Pulse of Science Fund, Chapela envisioned ways for the practice of science to occur within the context of communities:

Pods, cocoons and other places. [bold in original]

On November 21st, I will announce the creation of a space of support, quite simply, of uncompromising questions and their questioners (that which some of us understand as Science). In the absence of a university able to confront a time of catastrophic loss in diversity, I want to weigh in not by advocating reform, but by helping build safe spaces where inquiry could take place, to inform the present, but more importantly to help define what options (not necessarily what outcomes) we can and desire to leave behind for those coming in the future. Thinking of biological processes as developmental ones, and thinking of humanity in a trajectory unavoidably entwined with that of the rest of the world, I want to bring my work to bear on the developmental spaces that will allow futures that we might not only survive, but also desire. The obvious analogies are those of seed-pods, cocoons and the uterus, spaces where development is possible, capsules in a journey through time and inimical environment, into a future where such development will be necessary (Chapela 2004b).

Chapela thus attempted to create new institutional spaces for his vision of science connected to oft-excluded publics. Furthermore, this passage showed how his attempt to connect with a 'lost' understanding of science pushes for a reconfiguration of the

scientific process in his current environment. He pulled his audience into his science, but recognized that doing so in a traditional sense of an aggressive social movement would likely lead to obliteration rather than development.

12.7 Discussion

In Part Three, I have begun the project of articulating the heterogeneity of scientific dissent as existing on a spectrum from agonistic engagement to dissidence. Agonistic engagement respects conventional norms of scientific discourse and involves producing additional facts, assembling the support of scientific actors and institutions, and emphasizing the importance of disciplinary boundaries in assigning expertise. Dissident science integrates struggles over scientific authority with social action to reconfigure relationships among science, politics, and publics. Table 4 summarizes some of the key differences that emerge from this framework.

Table 4: Agonistic Dissent vs. Dissident Science

	Agonistic Dissent	Dissident Science
Source of epistemic authority	Adherence to norms of scientific community	Ability to expose biased assumptions of promotional science and convince a diverse community of scientists and laypersons of the significance and credibility of contrarian research
Negotiating the boundary of scientific credibility	Respects the boundary where it is, attempts to demonstrate that contrarian science belongs within that boundary	Challenges the enforcement of the boundary as corrupt, although does not challenge the need for such a boundary. Attempts to introduce other factors to determine credibility beyond technical measures (e.g. institutional context, affiliation)
View of public	Eventual consumer of science	Force of accountability, participant in setting priorities, jury for politicized controversies

	Agonistic Dissent	Dissident Science
View of politics	Politics should consume science	Politics subsumes science
View of scientific freedom	Classic. Scientists should be free from contextual constraints as they choose, enact, and interpret their research. Applying this norm widely uncovers interest-based science which is often the source of resistance to contrarian science.	Complex. The quest shifts from eliminating 'outside' interests to embedding science in institutions organized to promote the public interest. Transparency and participation become paramount.
Impact of intellectual suppression	Force of marginalization that must be countered with facts	Fuel to the fire of controversy that must be countered with stories and facts
Role of activism	Detracts from scientific credibility	A social force that can and should be integrated with science-making, especially as a counter-balance to corporate interests.

The case studies demonstrate that scientists enjoy a degree of flexibility in drawing from these two ideal types of dissent. The limitations of this analysis do not permit a theorizing of what conditions produce which types of dissent, but a number of hypotheses emerge from the data:

- The more that dissenting scientists are protected by buffers of legitimate scientific authority (e.g., tenure, support by expert colleagues) the more likely they will remain within agonistic discourse.
- Familiarity with goals and tactics of advocacy (politics, activism, marketing) increases tendencies and capacities for dissident science.
- Dissenting scientists adjust their strategy according to targeted audiences.
- The greater the intensity of popular debates around the political implications of a scientific arena, the more attractive dissident strategies will be.

- The more developed the advocacy network around a scientific arena, the more dissenting scientists will remain in an agonistic mode (relying on other actors to politicize their work).
- The more scientists come to understand resistance to their work as arising in extra-scientific contexts, the more likely they will pursue dissident strategies.

These hypotheses not only address the motivation for pursuing different strategies of scientific dissent, but also raise provocative questions about efficacy in different contexts and the ability to move back and forth between divergent strategies within a particular episode of controversy.

While I have presented scientific dissent as a spectrum of behaviors that range from agonistic to dissident, a great deal of boundary work occurs within controversies to segregate dissenting strategies into proper (e.g. ‘rational’, ‘normal’, ‘fact-based’) and improper (e.g., ‘activist’, ‘bought-and-paid-for’, ‘party-line’, ‘ideological’). The location of this boundary is a matter of social construction and contestation in the same way that actors attempt to differentiate modes of resistance as acceptable or egregious depending on contextual understandings and strategic goals. Rhetorically, all parties tend to essentialize science to some degree as a trusted path to knowledge, seeking to insulate scientists from corrupting influences (whether activist, ideological, or industrial). What differs, however, is the treatment of ‘dissidence’, whether named as such or implied. For example, supporters and opponents of Chapela have categorized him as dissident (as having infused his science with politics). For his supporters, this is socially responsible as it exposes the corruption of the current academic-industrial complex – dissidence being

required to overthrow the regime.¹⁷² For his opponents, however, dissidence is scientific treason – a departure from scientific discourse for political ends that undermines the authority of science broadly.

A finer degree of boundary work occurs in the struggles to separate science from politics, and scientists from publics. These are the boundaries at stake in the move from agonistic engagement to dissident science. The dissident perspective claims the political as deeply entwined with the practice of science – respecting the political context in which science making occurs, the ‘internal’ politics of negotiating legitimacy among scientific actors and institutions, and the political implications of scientific results (e.g., knowledge, ignorance, technology). Likewise, dissident science appeals to public involvement, altering boundaries between experts and laypersons in the production of knowledge. The agonistic perspective fears this as a pollution of expertise, but dissident science envisions expertise less as a container to be protected than as a fiery social process of integrating ‘fact’ and ‘value’ struggles.

¹⁷² In his work on water fluoridation controversies, Martin shows the flipside of this narrative. Pro-fluoridationists, who represent the mainstream and consider anti-fluoridation science a ruse for ideologues, accept and promote the duty of pro-fluoridation scientists to engage in political campaigns. They justify this apparently paradoxical stance by referencing the existence of anti-fluoridation campaigns that demand a political, not just scientific, response. “The scientific part, they [the pro-fluoridationists] believe, consists of scientific findings which contain no basis for opposing fluoridation. This is the foundation for the claim that there is no scientific debate. The political part of the issue arises from the existence of opponents who are motivated for nonscientific reasons. This political opposition must be countered, and thus many of the proponents counsel the waging of a political struggle for fluoridation” (Martin 1991, 62).

Chapter 13 Conclusion

The public is increasingly aware of how conflicts of interest, the university-industrial complex, and the privatization of science have eroded the quality of and possibilities for science in the public interest. An underlying assumption to these narratives is that ‘bad science’ has been evolving to take advantage of every opportunity while ‘good science’ remains helpless and committed to ancient ideals of objective and disinterested inquiry. But what if the trends that make ‘science in the public interest’ more difficult also *open up opportunities* for forms of science that forge novel relationships among scientists, the public, politics, and public institutions?

As a whole, this dissertation attempts to answer that question by combining a theoretical framework (scientific dissent as pathway and performance), empirical data about the scientists who participated in that event, and tools and ideas from the social studies of science. Aside from a theoretical contribution to STS and a historical contribution to those who want to know more about the stories of Chapela, Losey, and Pusztai, this project also enters a conversation about the policy and practice of science and the governance of technology.

First, to repeat a mantra in STS, science is political. The USDA funded Losey’s initial study. Pusztai spoke about his research on television at least partially because he felt an obligation to warn the public about the possible health implications of his work. The Mexican moratorium on growing transgenic maize boosted the salience of Quist and Chapela’s research as a scientific claim *in a particular political context*. The rhetorical purification of science and politics, a tool deployed to construct a kind of untouchable

expertise, is a strategy of power, not of knowledge production. As the case studies show, contrarians, promoters, and dissenters all engage this rhetoric to achieve legitimacy, but they will do so only as long as society maintains the fiction of separating science and politics. While this vision has undoubtedly been co-constructed by scientists and laypersons, it also has the potential to be ‘co-deconstructed’ – taken apart by the cooperation of forces that created the vision in the first place. The aim is not to throw science out as a valuable institution, but to increase our attention to the ways that science and politics interact. To decide to insulate a particular aspect of science from the influence of politics *is itself a political decision* and one that we may want to make with some frequency. We should confront efforts to strengthen boundaries of scientific autonomy, however, not with a knowing nod of approval, but with vigorous questioning about what political purpose such a move would serve.

Second, there is social value in protecting a place for scientific dissent in fields of controversy with intense economic and political components. Regardless of how we judge the quality of the contrarian findings presented in this dissertation, they each represented a scientific question with extreme public relevance that was not being asked (or taken seriously) within networks of mainstream promotional science. Pusztai’s GM potato studies have not been repeated or his assessment protocol tested for validity. The type of research consortium of governmental, corporate, and university scientists that followed up on the Losey Monarch controversy has not been formalized as a strategy for pursuing valid, reliable, and relevant knowledge about GM technologies. Quist and Chapela’s second finding that questioned the stability of transgenes in ecological contexts has produced little but silence. These types of questions may only emerge as contrarian

ideas, and we must search for ways to subject these approaches to processes of political as well as scientific judgment. Critiques of the quality of contrarian science do matter, and they should factor in the social valuation of contrarian claims, but they should not be sufficient to push contrarian approaches completely out of view. Science has value not just for the knowledge it produces, but for the questions it generates. Judging the quality of knowledge may require significant expertise, but valuing questions should remain a joint project engaging scientists and publics.

Third, we should embrace the whole spectrum of scientific dissent, recognizing that different contexts require different responses that range from agonistic engagement to dissident science. Conventional norms of scientific communication do serve a purpose, and the performance of ‘organized skepticism’ may produce incrementally better knowledge in many contexts. Scientists who respond to resistance by producing more evidence, seeking expert allies, and drawing disciplinary distinctions support a highly rational process that can be accessible to publics to various degrees. The social support (institutions and resources) for these dissenters should be proportional to the value of the questions they are posing, a value determined not by experts in isolation but by more political and participatory processes.

At the other end of the spectrum, dissident science also serves an important social purpose. When the norms of scientific communication suppress politically valuable inquiry or when resistance from interested parties becomes too powerful, dissident scientists extend the controversy beyond the protective boundary of science. Their struggles for credibility violate the myth of the politically-insulated scientist, but that is exactly the point. Their work creates opportunities for publics to engage in science –

governing priorities, increasing accountability, and offering their own expertise as a resource in the quest for understanding. Chapela's dissident performances had value apart from their role in shoring up the credibility of his claims; they offered visions of alternative relationships among scientists, politics, and publics. In recognition of the increasing number of scientific controversies that operate in webs of political, economic, and cultural power, these visions are worth consideration.

13.1 Major Claims

A snapshot of scientific controversy might locate dissent as simply the minority opinion, but this definition severely restricts any effort to understand the practice and meaning of scientific dissent within controversy. In my view, a more illuminating and productive model (Figure 1: Scientific Dissent as Pathway and Performance, p. 8) requires a more comprehensive picture – one that respects the complexity of context, credibility struggles, strategic frames, and scientific/political assumptions.

The terrain of promotional science in agbiotech includes an interlocking set of institutions, actors, technologies, and practices. Together these create the landscape upon which scientific dissent must emerge. The analysis of the Gene Flow Conference and the Hort-Biotech Workshop demonstrated that mainstream science in the public and quasi-public spheres evinces and produces a consistent worldview that promotes the research, development, and deployment of agbiotech. This worldview not only contains assumptions about the technology (e.g., its promise, its safety), but also about the actors who might challenge those assumptions (e.g., anti-biotech activists as irrational).

The emergence of contrarian science thus occurs on a landscape stacked against technical claims that challenge agbiotech and deeply suspicious of the scientific credibility of anyone who would make such claims. Chapela Maize, Losey Monarch, and Pusztai Potato suggest that contrarian science begins with some hope of acceptance by the scientific community – ranging from extreme naiveté (Pusztai) to skeptical and prepared (Chapela). Choices made by contrarian scientists in terms of method, presentation of results, and communication of significance have tremendous repercussions for signaling vulnerabilities that will be attacked as the controversy matures. Although representing only the first sparks of scientific dissent, contrarian science challenges the promotional worldview in particular ways that foreshadow heated conflict.

While the scientific claims of the three case studies certainly drew a great deal of media attention from their very beginnings, the patterns of resistance that they faced cemented their place in popular discourse. My analytical approach of lumping diverse behaviors (e.g., critical comments by a journal referee, attempts by political opponents to delay or transform publications, personal threats) emphasizes resistance as something experienced (coherently) by contrarian scientists. Resistance thus creates the opportunity for active dissent.

When responding to resistance, scientists adhere to norms of scientific communication or violate such expectations of conduct to varying degrees. Regardless, dissent is an active process of responding to the flows of promotional science, contrarian science, and resistance. While I do not intend to valorize dissent unequivocally, any response other than withdrawal or silence represents significant effort and risk.

Conceptualizing dissent as a pathway presents scientific dissent as a social formation, which allows my analysis to transcend the framing of scientific controversy as a clash of ideas (a framing usually invoked by those with the upper hand). Dissent is a window not just into the political antics of scientists with different ideas, but of society's struggle to maintain a system of knowledge production – orderly enough to provide expertise, but disorderly enough so as not to suppress learning.

Complementing the conceptualization of scientific dissent as a pathway, I have invoked the dramaturgical lens to treat scientific dissent as a performance. Doing so has served a number of purposes.

First, as a participant-observer in/of scientific controversy, I have benefited from a framework that guided my attention to behavior and details that I otherwise might have missed (e.g., the role of a conference agenda in purifying science from politics, the self-presentation by contrarian scientists as 'naïve,' the narrative interpretations of scientific claims that transcend the text published in scientific journals, the political implications of constructing audiences as participants). It also has pushed me toward a more symmetrical analysis of diverse actors in controversy.

Second, the emphasis on performance has prevented my analysis from falling prey to the trope of the disinterested scientist playing only a passive role in the social formation of knowledge. Science is action. The political and institutional connections among scientists, government, industry, NGOs, and publics create a complex play of generating factual claims *and convincing audiences to accept and engage those claims*. The power of science to have influence in the world stems in part from its capacity to generate convincing performances. Scientific dissent is one genre of such performance –

one that involves recognizable patterns of character presentation, narrative development, stage management and audience construction. The brilliance of any single performance does not guarantee success, but the web of performances (promotional, contrarian, resisting, and dissenting) reveals the complexity of what is really at stake in scientific controversy – the power to produce an ongoing ‘show’ that continues to attract patrons and becomes a fixture in the culture.

Third, my extension of Hilgartner’s work to attend to the phenomenon of audience construction has provided insight into the performance of dissident science. Most evident in Chapela’s Open Office Hours and organization of the Pulse Event, dissident scientists construct participatory audiences, not simply as popular witnesses to improve transparency, but as part of a strategy to expand the boundaries of credibility and improve public accountability. As the ‘fourth wall’¹⁷³ disintegrates, the scientific performance loses some of its rhetorical purity and veneer of rational order. Simultaneously, new opportunities emerge for improvisational exchanges between experts and publics to improve the veracity and relevance of scientific knowledge and to understand better the social power of competing systems of knowledge production.

Fourth, the dramaturgical lens has demonstrated the heterogeneity of performances of scientific dissent. I have described a spectrum of behaviors and strategies, ranging from agonistic engagement (following the cultural norms of scientific communication) to dissident science (violating those norms). A single scientist or a single performance may exhibit qualities of both, implying that aspects of political opportunity

¹⁷³ In theater, the fourth wall is the imaginary wall between the stage and the audience, signifying the boundary that separates the parties but allows the audience to observe the play.

and context may impact strategic choices. While other metrics may also be useful (e.g., paying greater attention to the institutional or material targets of dissenting research; locating dissent more precisely within the cultures of academic disciplines or sub-fields, networks of laboratories, university or corporate campuses, or social movement networks), the agonistic-dissident spectrum provides particular insight into the capacity of different forms of dissent to forge new relationships among science, politics, and publics.

Dissident science does not erase the tension between expertise and democracy. Even as Chapela invites the public(s) into his controversial research and tenure struggle, he reserves a privileged role for science as a process of discovery and source of reliable knowledge. He does not endorse ‘truth by vote’ or the dismantling of research institutions as logical consequences of his deep critique of the field of biology and the status of the public university. Instead, his dissident performances envision new possibilities for expertise and democracy.

At the Open Office Hours, for example, Chapela shared his expertise (scientific and professional experiences – including his tenure controversy), but also *sought the expertise that became available by providing a forum for ideas to be taken seriously*. Informal conversations, the sign-in book, and organization of food, media, programs, logistical support – all offered opportunities for diverse sources of expertise to cooperate in social action. In this manner, dissident performances can provide access for a collective and de-centralized form of expertise that conventional science often overlooks (with its reliance on credentials).

With regard to democracy, the Pulse Event showcased the value to his audience of engagement over representation. Chapela did not organize a campaign to channel citizen dissatisfaction through established institutions of governance to re-align UC Berkeley with the public interest. Instead, he created a forum for conversation that connected the issues of the status of contemporary political dissent (e.g., the Dixie Chicks), the role of journalism in holding science accountable, the links between private and public institutions, the culture of biological science, and what I would call the pathway of scientific dissent. The success of that event (drawing a large physical audience, attracting media coverage, and foreshadowing the launch of the Pulse of Science Fund) suggests that the strategy of engagement had consequences. In this way, my analysis has shown that dissident science can contribute to the project of infusing scientific politics with public power.

13.2 Collision of Worldviews

In the process of studying the pathways to scientific dissent in agricultural biotechnology, I have come to understand the opposing worldviews of contrarian and promotional science. My analysis suggests that these worldviews not only have descriptive power (revealing internal coherencies and contradictions) but organizational power in terms of governing action in what has largely become a partisan debate (pro/anti-GMO). In his study of water fluoridation controversies, Martin (1991) describes a similar phenomenon – the pressure to conform to one worldview or the other (pro/anti-fluoridation) results in an exaggeration of partisanship and a constant clearing of the middle ground as each side attempts to suppress discourse that allows a questioning of its

position. Applied to agbiotech, this suggests that dissident actions (strong breaks from convention) would have the greatest potential to break through the partisan discourses, as more agonistic dissent would tend to be disciplined for the sake of presenting a unified front.

Table 5 draws on my experience carrying out research on agbiotech controversies since it expands beyond the empirically based sets of assumptions presented in Chapter 5 with the goal of providing a richer context for understanding.

Table 5: Comparison of Contrarian and Promotional Worldviews

	Contrarian	Promotional
Science		
Scientific practice	Skeptical, questioning of assumptions	Enthusiastic, promoting of possibilities
Scientific knowledge of genetics	Incomplete, dangerously simplified	Incomplete, but sufficient for technological success
Commercialization of science	Problematic because it introduces potential bias	Necessary, if not desired, to move technology from the laboratory to the field where it is useful
University-Industry partnerships	Problematic because corporate agendas hijack research direction; public scientists lose credibility in policy disputes; incentives only to pursue profitable research	Necessary because of dwindling state funding of research; a means of spurring innovation and keeping public scientists on the ‘cutting edge’
Future research	Should focus on ecological and health risks of GMOs	Should focus on next generation of technologies and improving precision/control/power.
Dissenting scientists	Heroes, the ‘true’ scientists who buck the corporate trend and risk institutionalized suppression	Marginal and motivated by ideology, prone to sloppy science

	Contrarian	Promotional
Policy		
U.S. regulatory regime of GM crops and food	Grossly inadequate; co-opted by industry; ‘substantial equivalence’ should be tested rather than assumed; should be process-based	Imperfect; overly bureaucratic and cautious; impediment to technological development; should be product-based
Global regulatory regime of GM crops and food	Too much emphasis on homogeneity, without respect for national/cultural differences	Needs greater homogenization to ensure free trade, which will help lift up developing nations from poverty
Intellectual property rights	Question empirical role in stimulating innovation; tool of biopiracy	Necessary to spur innovation, problematic because of the ‘patent thicket’
Public		
Cause of opinion	Moral and cultural evaluations of the role and potential risks/benefits of technology	Uneducated about science and technology; led astray by ‘fear-mongering’ activists
Significance of opinion	Citizens as offering alternative and situated expertise	Consumers as restricting commercial possibilities; ‘privileged’ consumers in the Global North preventing technology adoption in Global South
Role in policy formation	Engaged and integrated	Passive audience, separated from expert discussion
Role in guiding research	Rightful participation as taxpayer and citizen	Improper corruption of the pursuit of knowledge
Agriculture		
Modern – conventional	Problematic, in need of massive overhaul	Problematic, in need of improvement
Industrial model	Ecologically and socially destructive, especially in the Global South	Efficient and important to export economies of scale around the globe
Organic model	Promising, ecologically, socially and nutritionally	Unimportant, a distraction from the task at hand; cannot work at sufficient scale

	Contrarian	Promotional
Cause of hunger	Distribution of wealth; inequitable land tenure; misguided focus on cash crops in Global South	Supply of food; world price of food; over-population
Conventional breeding	Safe; tested through time; could benefit from biotech screening techniques	Imprecise; inefficient; limited by biological boundaries
Agricultural biotechnologies		
<i>Bt</i> crop performance	Spotty performance; uneconomical in many cases; insufficient to protect against many pests; prone to create insect resistance rapidly	Reduction of pesticide applications; economical; resistance can be managed with refuges
Herbicide tolerant crop performance	Increases herbicide application; decreases crop vigor and yield; changes nutrients in soybeans; creates herbicide resistance over time; a technological ploy to sell more herbicide	Reduction of most harmful herbicides; increases yield; allows growers more flexibility; uses an ecologically-friendly herbicide
Pharming (producing medicine in GE crops)	Dangerous due to gene flow and uncertainties of genetic engineering process; impossibility of reliable segregation	Promising; potential to make medicine available cheaply to world's poor
rBGH (bovine growth hormone)	Unnecessary (surplus milk production); dangerous and painful to cows; increases antibiotic exposure to human consumers of milk	Scientific way to increase milk production efficiency; no significant negative effects; labeling milk as rBGH-free violates truth-in-labeling requirements because no difference exists

	Contrarian	Promotional
Ecological impacts	Increased pesticides and herbicides; non-target effects on insects and soil biota; gene flow that causes super-weeds; gene flow that undermines biodiversity; preserves damaging commitment to monoculture; reduces global biodiversity by displacing local varieties	Less pesticides and dangerous herbicides; more no-till farming; less pressure to convert diverse ecosystems to agriculture because less land needed with increased efficiency; increase in biodiversity (new transgenic species)
Health impacts	Allergic reactions likely; limited but suggestive evidence that GE process introduces disturbing changes; lack of labeling and short duration of human exposure makes safety claims impossible	Allergic reactions possible but scientific and regulatory processes can protect adequately; lack of demonstrated health impacts in U.S. (exposed) population shows safety
Social impacts	Farmers lose right to save seed; biopiracy robs Global South of biological wealth; increased dependency on ever-fewer corporations for global food supply; lack of right to choose GE-free food because of lack of labeling and unwanted gene flow; directs scarce government and foundation funds away from sustainable agriculture research and development	Scale-neutral technology (seed) allows adoption by all farmers; revenue-sharing agreements have potential to enrich indigenous communities who are stewards of biodiversity; nutrition deficiencies addressed by simple technologies (e.g., golden rice)

There are undoubtedly scientists and other actors whose worldviews of agbiotech do not conform to the above dichotomy – a promising direction for exploring a nuanced version of scientific dissent. One could ask whether my model of scientific dissent as a pathway would hold *within* a particular community of like-minded actors. For example, what happens when a scientist embedded in professional and social networks dedicated to

organic agriculture or agroecology produces research that incorporates GM technology into a vision for sustainable farming? Do forms of resistance parallel those represented in this dissertation? Does dissent vary from agonistic engagement to dissident action? What is the impact of the existence of a separate scientific community (promotional science), which would derive great rhetorical benefit from opponents who break ranks? These questions emerge from the recognition that the scientific world is not monolithic, and that the boundaries created have consequences on knowledge production as a whole.

13.3 *Dissertation as Performance*

By foregrounding dissent, this project opened up some lines of inquiry and closed others. I asked about the emergence and management of ideas that challenged agbiotech rather than, for example, attempting to understand how the science of agricultural biotechnology responded to social critique. This focus gave dissenters a voice and a status that they tend not to enjoy in the pages of *Nature Biotechnology*. In theatrical terms, I brought the sideshow onto the main stage and turned on the expensive lights. My analysis thus risked giving dissenters more legitimacy (scientific or otherwise) than they ‘deserved.’ Of course making this judgment launches one directly into the scientific controversy itself, which suggests the necessity of taking the risk as a researcher to remain open to marginalized voices.

I began with promotional science rather than showing how promotional science was constructed in response to anti-biotech activism and consumer distrust of GM technologies. I attempted to emphasize that the model is iterative rather than sequential. Nonetheless, I had to begin somewhere, and my presentation staged promotional science

as the landscape upon which all else transpired. This framing prevented me from accessing the full complexity of promotional science as historically and intellectually situated. In a related move, my sequence of chapters privileged ‘resistance’ over ‘support.’ Networks and actions that supported contrarian science (or opposed resistance on behalf of dissenters) appeared as part of the story, but secondary to the actions of resistance or responses undertaken by dissenting scientists themselves. Further research could explore the role of support and sponsorship of contrarian science. Fruitful questions might include: In what ways do dissenting scientists become pawns in struggles between existing factions of organized actors and institutions? Under what circumstances can non-scientific sources of support for dissenting scientists offer assistance without undermining the scientific legitimacy of the researcher? Do supporters of contrarian science resist promotional science with similar or different strategies than those outlined in this dissertation’s section on resistance? Aside from the few examples in this dissertation, when do dissenting scientists reject networks of support (for example, Chapela’s refusal to let NGOs define the message of his Open Office Hours)?

My choice of research site (agbiotech) and definition of contrarian science resulted in a project that focused on dissenters whose science and politics aligned with left-wing, progressive, and radical causes. These cases (and characters) enabled a narrative with undertones of respect and sympathy for dissenters (given my own political leanings). Other cases of dissent would line up in very different ways, creating excellent opportunities to test the conceptual model of dissent as a pathway and performance. Two potential cases suggest the theoretical value of such work.

Peter Duesberg, a distinguished UC Berkeley virologist, believes that HIV does not cause AIDS. The *San Francisco Chronicle* reviewed his book, *Inventing the AIDS Virus* (1996):

Duesberg's heterodox thesis is uncompromising: The AIDS epidemic, he holds, is an artifice, if not an outright fraud, perpetrated by a vast conspiracy that links the federal Centers for Disease Control and Prevention and the National Institutes of Health to glory-fixated scientists grasping for government grants and to profit-hungry pharmaceutical companies peddling deadly wares on unsuspecting victims (Perlman 1996).

Duesberg is clearly a contrarian with respect to the dominant flow of intellectual work, economic activity, and political discourse that seeks to address the worldwide AIDS epidemic by reducing HIV infection rates. He has undoubtedly experienced resistance in many forms, and has become a full-fledged dissenter. Comparing his trajectory with those presented in this dissertation raises a fascinating question: In what ways do Duesberg's performances of dissent differ from those of Chapela, Losey, and Pusztai, given that existing social movements around HIV-AIDS would appear to counter the implications of his science?

Controversies around climate change would offer another valuable comparison. In an interview published in *The Planet*, the Sierra Club's bi-monthly activist newsletter, Dr. Stephen Schneider, a Stanford University professor of climatology for 35 years, discussed the issue of scientific uncertainty around climate change (Lesle 2005). He described a strong scientific consensus for the factual status of a global warming trend and human activity as a proximate cause. When asked who had responsibility for the apparent lack of consensus and uncertainty, Schneider responded:

First, the so-called contrarians. The group that will take any study that just comes off any press anywhere and if it has any element that slightly disagrees with the mainstream wisdom, they immediately declare the overall, basic, well-established consensus to be dead until this study is resolved. Now, that is not how science is done. Science is done on the basis of the large preponderance of evidence contained in hundreds--in this case

thousands--of studies. No one new study can come along and prove it right or prove it wrong. This is a deliberate manipulation (p. 4).

Schneider's language demonstrates strong boundary-work to exclude certain voices from science's umbrella of legitimacy.¹⁷⁴ From the perspective of Sierra Club members, who presumably advocate action to head off global warming, this rhetoric appears appropriate and necessary. But the same words, used to critique contrarians such as Chapela, Losey, and Pusztai would cause great alarm to agbiotech activists who would question why scientists producing data they 'agree with' should not be taken seriously. Possible research questions include: In what ways does the alignment between scientific dissent (questioning global warming) and corporate interests (fossil fuel related companies) affect the landscape of dissent? Do the media show different degrees of sympathy to contrarian scientists depending on the political implications of their work? Do scientific dissenters identify with different 'role models' (e.g. Galileo, Rachel Carson, Bruce Ames) depending upon their political stance? To what degree is dissident science performed differently in an arena with popular activism working for mainstream science?

The flames of scientific controversy are still burning around agbiotech, and scientists continue to tread the pathways of dissent. On 26 October 2005, Fred Kirschenmann, then Director of Iowa State University's Leopold Center for Sustainable

¹⁷⁴ According to Schneider, contrarians do exist, but they are essentially industry pawns who achieve credible status within the media only because of journalism's fixation with giving 'both sides' their fair chance to argue their opinion. Schneider observed: "[T]he media become a serious obstacle to good communication because of this belief that if they haven't gone and found some crackpot somewhere to say it ain't so, they haven't discharged their obligation. And my answer to them is: So every time NASA sends up a satellite, why don't they go get somebody from the Flat Earth Society to say it isn't true?" (Lesle 2005, 5). Setting aside the derogatory term of "crackpot," Schneider draws our attention to the media's role in choosing which controversies to respect and which controversies to ignore. In dramaturgical terms, this is the stage management work that controls scientific performance and constrains the possible narratives that can emerge.

Agriculture, received a letter from the dean of the College of Agriculture requesting his resignation and asking him to accept an appointment as “distinguished fellow” instead. The Leopold Center serves as a national resource for the critique of industrial agriculture and research on alternative food systems. It released two major studies in 2001 showing that Iowa farmers did not experience an economic benefit from planting GM soybeans or corn, and in 2002 its state funding was cut by \$1 million. In 2004, Kirschenmann participated in the release of the Union of Concerned Scientists’ report “Gone to Seed: Transgenic Contaminants in the Traditional Seed Supply.” Kirschenmann protested the request for his resignation, which he had not anticipated, but the dean indicated she had already named his replacement. Although she had served on the search committee that hired Kirschenmann in 2000, her support waned in the last two years as she charged the Leopold Center with neglecting “key stakeholders.” Kirschenmann reported that “she never really clarified who those stakeholders were,” but the well-established agribusiness interests in Iowa politics seemed the obvious answer. It thus appears likely that Kirschenmann’s removal resulted from his contrarian stance on agricultural issues including agbiotech (GM Watch 2005; Philpott 2005).

Kirschenmann’s decision to accept the distinguished fellow position and continue to work with the Leopold Center suggests a more agonistic pathway of dissent, but much of this performance remains to be seen. What matters most, perhaps, is not whether Kirschenmann takes on the role of dissident scientist, but that we, as scholars and citizens, pay attention to the dynamics of dissent – what assumptions he challenges, what interests resist his claims, which audiences are paying attention. The scientific facts in question (e.g., whether GM crops increase the net income to farmers) do have

significance for the management of agricultural biotechnologies, but the associated 'facts' of the performance and management of dissent may contribute just as much to the social challenge of choosing modes of governance, enacting strategies for knowledge production, and integrating democracy and expertise.

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