Constructing Quality in Academic Science: How Basic Scientists Respond to Canadian Market-Oriented Science Policy – A Bourdieusian Approach

by

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A thesis submitted in conformity with the requirements for the degree of Doctor of Philosophy

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Abstract

Canadian science policy has increasingly linked the value of academic knowledge to its contribution to economic competitiveness. A market vision of scientific quality is embedded in new funding criteria which encourage academic scientists to collaborate with industry, generate intellectual property, and found companies. While the “Mode 2” thesis advanced by Gibbons and Nowotny asserts that quality criteria in science are changing to incorporate economic relevance, there is little empirical evidence to either refute or substantiate this claim. Using Bourdieu’s theory of practice, this study explores the responses of basic health scientists to market-oriented funding criteria. The goal of the study was to understand how scientists, occupying different positions of power in the scientific field, defined “good science” and pursued scientific prestige. Twenty semi-structured interviews were carried out with 11 scientists trained before and 9 trained after the rise of market-oriented science policy. Data derived from Curriculum Vitae and Background Information Forms were used to estimate the type and volume of capital each participant held. Scientific capital, as reflected in peer-reviewed publications and grants, was perceived as the dominant form of
recognition of scientific quality. However, “entrepreneurial capital”, as reflected in patents, licenses, industry funding and company spin-offs, functioned as a new form of power in accessing resources. Study participants adopted different positions in a symbolic struggle over competing visions of “good science” and used different strategies to acquire scientific prestige. Some pursued a traditional strategy of accumulation of scientific capital, while others sought to accumulate and convert entrepreneurial capital into scientific capital. Findings suggest that there is no longer a single symbolic order in the scientific field, but that the field is stratified according to a scientific and market logic. Hence, support is provided for both continuity with “Mode 1” and change towards “Mode 2” evaluation of academic quality.
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<th>Full Form</th>
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<tbody>
<tr>
<td>CFI</td>
<td>Canadian Foundation for Innovation</td>
</tr>
<tr>
<td>CIHR</td>
<td>Canadian Institutes of Health Research</td>
</tr>
<tr>
<td>CRC</td>
<td>Canada Research Chairs</td>
</tr>
<tr>
<td>NCE</td>
<td>Networks of Centres of Excellence</td>
</tr>
<tr>
<td>NSERC</td>
<td>Natural Sciences and Engineering Research Council</td>
</tr>
<tr>
<td>OMRI</td>
<td>Ontario Ministry of Research and Innovation</td>
</tr>
<tr>
<td>OIT</td>
<td>Ontario Innovation Trust</td>
</tr>
<tr>
<td>ORDCF</td>
<td>Ontario Research Development Challenge Fund</td>
</tr>
<tr>
<td>POP</td>
<td>Proof of Principle</td>
</tr>
<tr>
<td>SSHRC</td>
<td>Social Science and Humanities Research Council</td>
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Chapter 1
Introduction

1.1. The Transformation of Academic Knowledge Production

The relationships between academic scientists and industry have undergone widespread change over the past two decades. Industry collaboration has become an accepted practice as increasing numbers of basic scientists rely on industry funding, found their own companies, patent their research findings and engage in product development. Canadian science policy supports and facilitates these practices as a part of a broader policy agenda to stimulate Canada’s economic competitiveness in the knowledge-based economy. In the late 1990s, the federal government both increased its investments in research funding and tied these investments to market-oriented evaluation criteria (Snowdon 2005; Wolfe 2002). The Medical Research Council was restructured to become the Canadian Institutes for Health Research with a mandate to improve knowledge and technology transfer among basic and applied health researchers. New funding organizations, including the Canadian Foundation for Innovation and Genome Canada, and increased funding for the Networks of Centres of Excellence program required applicants to meet both scientific and market-oriented criteria, such as the justification of economic relevance, matched industry funding, engagement in new product development, and the generation of intellectual property and spin-off companies.

Canadian science policy is not unique, as many other nations have also introduced economic goals into their science and technology policies (Albert and Laberge 2007; Jansen 2002; Kharbanda 2011; Mowery and Sampat 2005; OECD 2005; Suttmeier, Cao and Simon 2006). These policies can be seen as promoting the transformation of traditional academic science from a “Mode 1” to a “Mode 2” system of knowledge production. Specifically, some argue that traditional academic knowledge production – “Mode 1” science – is being transformed into a “Mode 2” system of knowledge production that is better able to serve the needs of the knowledge-based society (Gibbons, Limoges, Nowotny et al. 1994; Nowotny, Scott, and Gibbons 2001). Knowledge is no longer produced within closed university settings in disciplinary based units, but in multiple intersecting sites by transdisciplinary collaborators who must meet social and economic quality criteria as well as scientific rigour. Boundaries between
academic science, the market and the state are, according to the Mode 2 thesis, eroding as new patterns of interaction shape the production and use of knowledge.

While policymakers actively seek to transform academic science, the extent to which knowledge production is changing and the nature of these changes are disputed among science and technology studies scholars. Some scholars have emphasized continuity and stability in knowledge production as scientists and academic institutions reassert traditional academic values and quality criteria (Albert 2003; Calvert 2006; Gulbrandsen and Langfeldt 2004; Henkel 2005; Morris and Rip 2006; Tuunainen 2005). However, others claim that knowledge production is being transformed and that the boundaries between science, the state and the market are eroding (Owen-Smith 2003, 2005; Slaughter and Leslie 1997; Vallas and Kleinman 2008). In the latter group, there is disagreement over whether this change is compatible with or a threat to scientific productivity and traditional academic norms of openness and objectivity. While the patterns of interaction between university and industry have undoubtedly changed, it is unclear whether evaluations of scientific quality have also changed to reflect the new kinds of market practices scientists engage in and the market products they produce (Hessels and van Lente 2008).

The science and technology studies literature has tended to focus on direct interactions between science and the market as both the cause and effect of change in academic knowledge production. A less common approach has been to focus on broader changes in the culture and distribution of rewards in science (Owen-Smith 2003, 2005; Vallas and Kleinman 2008). An implicit theme running through the literature is that scientists and academic institutions are unequally positioned to resist or benefit from market-oriented practices (Kinchy and Kleinman 2003; Henkel 2005; Owen-Smith 2003, 2005; Slaughter and Leslie 1997). Yet few studies have explored how the distribution of power in science is influenced by market evaluation criteria or how differences in power shape the perceptions and strategies scientists use to negotiate market opportunities and pressures. This study explores how a group of basic health scientists in a Canadian university respond to market-oriented funding criteria and how these responses are shaped by their positions of power.
1.2. A Bourdieusian Approach

In this thesis, I use Pierre Bourdieu’s theory of practice to understand how interactions between individual behaviour and social structures produce the social order in science. A Bourdieusian approach is both a way of understanding how the social world works, through a particular set of conceptual tools – an ontology – and a way of knowing and doing research – an epistemology.

The use of Bourdieu’s work as a way of knowing requires a “metanoia, a mental revolution, a transformation of one’s whole vision of the social world” (Bourdieu and Wacquant 1992:251). The adoption of this new sociological gaze requires the researcher to break with objectivist and subjectivist sociological research traditions in order to take a relational understanding of the social world and a relational approach to knowing. This relational understanding is aided by Bourdieu’s concepts of field, capital and habitus.

Ontologically, Bourdieu conceptualized the social world as being made up of multiple competitive social spaces (fields) in which members compete for specific forms of power (capital) using strategies that are structured by dispositions formed under past conditions (habitus). Social practices are understood as the outcome of interactions between habitus and field among individuals who possess varying levels of capital. The structure of a field is the outcome of symbolic struggles over the value of different forms of capital which are produced according to the logic of each specific field. How can this theoretical approach help us to understand the relationships between market-oriented science policy and scientific practice?

Critically, it suggests that change in the logic of the scientific field and the meaning of “good science” will alter the forms or value of capital associated with that logic and lead to a restructuring of the strategies scientists use to achieve success. Market-oriented science policy seeks to alter the logic of the scientific field and the meaning of “good science” to incorporate criteria for economic value in order to change scientific practice. Yet a change in the meaning of “good science” in the policy field does not automatically change the logic of the scientific field or change it in the ways that policymakers intend. If change occurs, it will be an outcome of struggles among scientists seeking to maintain or improve their capital within the scientific field. As scientists who occupy different positions of power resist or adapt to a market-oriented vision of science, they will contribute to the reproduction or transformation of the symbolic
order – the dominant vision of “good science” and the forms and value of capital associated with that vision.

To explore whether category change in the policy field has had an effect on scientific practice, I explore and compare the responses of scientists trained before and after the rise of market-oriented science policy. I draw on Bourdieu’s concept of hysteresis as a mechanism for understanding change. When new symbolic categories differ radically from traditional categories, the ordinary rules of the game may be suspended, as expectations of success based on traditional categories no longer correspond with objective chances of success. Bourdieu (1990a) called this gap between the categories of perception that embody past conditions (habitus) and new conditions (field) the “hysteresis of habitus” (p. 59). Hysteresis can produce intense symbolic struggle among members of a field who previously shared similar perceptions and strategies, as they reposition themselves according to new symbolic categories and rewards. It is usually resolved in time through ongoing adaptations between habitus and field until a new equilibrium is achieved.

If the scientific field has remained stable, despite efforts by policymakers to steer knowledge production towards a “Mode 2” system, we would expect scientists trained before and after the rise of market-oriented policy to retain a traditional scientific vision and seek scientific recognition or capital according to a traditional scientific logic. They would experience harmony between their perceptions of “good science” based on a traditional scientific habitus and their objective chances of success. This does not imply consensus over the meaning of “good science”, but that symbolic struggles would play themselves out within the parameters of a scientific logic. If, however, academic science is changing in a Mode 2 direction, scientists’ perceptions and strategies to achieve “good science” based on a traditional scientific habitus may no longer be aligned with their objective chances of success according to market-oriented funding criteria. We would expect scientists trained before and after the rise of market-oriented science policy to have different perceptions and strategies for acquiring resources and recognition. Those trained in the market-oriented science policy era may be less likely to experience hysteresis as they would acquire a scientific habitus better adapted to the new conditions of the field. Symbolic struggles would play themselves out among and between
scientists trained in these two policy eras over the legitimacy of competing visions of “good science” based on a market and scientific logic.

Epistemologically, Bourdieu advanced the position that the social world can only be understood by exploring the relationships between the positions members occupy in a field, based on their overall type and volume of capital, and their position-takings – their perceptions and strategies to accumulate capital. Symbolic struggles are, in part, discursive struggles over meaning, but the reasons for and outcomes of these struggles cannot be fully understood discursively. Position-takings can only be understood in relation to the positions which produce and are produced by them. In addition to the use of a relational ontology to conceptualize how the social world works, I also adopt a relational methodology to produce an account of how scientists participate in the reproduction or transformation of the social world. A relational methodology demands that the researcher empirically examine the relationship between the “space of positions” – the objective positions individuals occupy in a given field – and the “space of position-takings” – the perceptions and strategies they use to maintain or improve their position (Emirbayer and Johnson 2008). This involves identifying the forms of capital that scientists perceive as having value, analyzing the type and volume of they possess, and relating these to their perceptions and strategies to achieve recognition. By using a relational methodology, I seek to develop an account of how scientists respond to a new vision of science imposed by policymakers, how their responses are shaped by differences in power, and how they contribute towards the reproduction or transformation of the symbolic order in science.

While Bourdieu’s theory of practice provides the dominant framework for this study, I also draw on Jennifer Todd’s (2005) framework for exploring relationships between collective identity change and broader social and political change. Bourdieu’s theory has emphasized the unconscious dimensions of practice that lead to the reproduction of the social order. During periods of stability, the strategies individuals use to maintain or improve their position in the field are largely unconscious as the expectations of habitus are well-adjusted to the conditions of the field. However, when hysteresis occurs, the taken-for-granted rules of play may be temporarily suspended, leading to greater awareness of the rules and the potential for conscious choice. According to Todd, intentionality plays a role when individuals and groups alter their collective identities and practices in response to macro-level change. Furthermore, they don’t
always take a stance that is clearly for or against changes in symbolic categories but may act both for and against competing categories, as they seek to retain traditional identities and values while adapting to new social conditions and opportunities. I use Todd’s framework to strengthen Bourdieu’s theory of practice by bringing these dimensions to light.

1.3. Organization of Thesis
In Chapter 2, I provide an overview of the changes in science policy and the innovation agenda linked to funding changes in Canada in the late 1990s, the dominant frameworks for understanding change in science, the state and the market, and a critical review of the literature on scientists’ perceptions of and strategies to navigate market-oriented science. In Chapter 3, I describe Bourdieu’s core concepts of field, capital and habitus, the concept of hysteresis, and Todd’s (2005) framework for exploring types and direction of identity change. In Chapter 4, I describe the methods through which I constructed the study sample and the two spaces of positions and position-takings and the strategies used to ensure rigour. The study findings are presented in Chapter 5, the space of positions, and 6, the space of position-takings. In Chapter 5, I provide a description of the characteristics of the sample, identify the forms of capital participants perceived as having value, and map the location of each participant in social space according to the type and volume of capital they possessed. In Chapter 6, I explore the position-takings of established and early stage scientists, respectively, towards market-oriented science and the relationship between positions and position-takings. The reasons for variations in position-takings and implications of position-takings for the structure of the scientific field are discussed in Chapter 7, which concludes with a review of the study strengths and limitations and implications for future research.
Chapter 2
Background and Literature Review

2.1. Introduction

Academic science has taken on an increasingly prominent role in Canadian science and economic policy over the past two decades. Since 1988, the federal government has implemented a variety of policies and funding strategies designed to promote knowledge translation, technology transfer and commercialization. Academic knowledge has become a key resource for stimulating Canada’s economic competitiveness in the knowledge-based economy. Since the late 1980s and 1990s, the prevalence of market involvement of academic scientists has grown steadily in the US and Canada (Mowery, Nelson, Sampat and Ziedonis 2001; Owen-Smith and Powell 2003; Owen-Smith 2005; Statistics Canada 1999, 2010). This period marks a distinct change in the role of academic knowledge, scientific practice, and the relationship between universities, the market and the state since the post-World War II era. In this chapter I will review the Canadian policy background, dominant approaches to understanding change, and the literature on market involvement of academic scientists.

The chapter is divided into three sections. In the first section, I provide an overview of the history of science funding in Canada and describe the evolution of the innovation and economic competitiveness agenda between 1988 and 2006. I describe the new funding organizations and programs which have incorporated market-oriented funding criteria alongside scientific criteria during this period. In section two, I review dominant approaches to understanding changing relationships between academic science, the market and the state, including the Mode 2 thesis, Systems of Innovation, the Triple Helix and Academic Capitalism. Two of these approaches – Mode 2 and Systems of Innovation – were used to inform Canadian science policy in the late 1980s and 1990s. Finally, in the third section, I critically analyze the empirical literature on the market involvement of academic faculty. I argue that the existing research tells us very little about why scientists adopt particular strategies towards market-oriented pressures and opportunities, and how differences in power shape the strategies they use to compete for resources and scientific recognition.
2.2. Policy Context: The Role of Science in the Knowledge-Based Economy

Canadian science funding has undergone a number of shifts over its history. The first national funding body, the National Research Council (NRC), was founded in 1916 and remained the only federal research funding agency until 1960 when the Medical Research Council (MRC) was established (Fisher, Atkinson-Grosjean and House 2001). The NRC and MRC provided funding for university researchers from the end of the Second World War until the late 1970s when the funding system was restructured to fund the social sciences, humanities, engineering and applied sciences in addition to the basic natural and medical sciences (Fisher et al. 2001). In 1977, the MRC continued to fund medical research but the Social Science and Humanities Research Council (SSHRC) and the National Science and Engineering Research Council (NSERC) were established to fund other disciplines. In the post-Second World War period up until the 1980s, Canada’s science policy focused on broadening and strengthening Canada’s knowledge base. From the 1980s onward, the science policy agenda began to shift to increasing the utility and use of academic knowledge in order to promote economic competitiveness and innovation. Key policy and funding initiatives and events from 1988 to 2006 are summarized in Table 1.

Table 1. Key Funding Initiatives and Events from 1988-2006

<table>
<thead>
<tr>
<th>Date</th>
<th>Key Funding Initiatives and Events</th>
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<tbody>
<tr>
<td>1988</td>
<td>NCE program founded</td>
</tr>
<tr>
<td>1997</td>
<td>Federal CFI and provincial ORDCF established</td>
</tr>
<tr>
<td>1999</td>
<td>Provincial OIT established</td>
</tr>
<tr>
<td>2000</td>
<td>MRC becomes CIHR; Genome Canada, CRC program established</td>
</tr>
<tr>
<td>2002</td>
<td>National Innovation Strategy launched and indirect costs of research funded</td>
</tr>
<tr>
<td>2006</td>
<td>CIHR operating grant success rate falls to all-time low</td>
</tr>
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</table>

The innovation policy agenda was initially driven by the federal Conservative government, who established the Networks of Centres of Excellence funding program in 1988 to promote university-industry collaboration (Fisher, Rubenson, Clift, et al. 2005). The federal Liberals took up the banner in 1993 in its Red Book, a strategy for increasing federal investments in
research and development (Wolfe 2002). However, funding for research and development fell in the mid-1990s as the government’s priorities shifted to deficit reduction and debt management. As the government’s fiscal position improved in the late 1990s, it returned to its previous priority of increasing global economic competitiveness through the development of a national system of innovation. Between 1997 and 2002 a cluster of new policy and funding initiatives were implemented to translate science policy into strategies to support, facilitate and accelerate university-industry collaboration and the commercialization of academic research (Cameron 2004; Fisher et al. 2001, 2005; Polster 2002; Snowdon 2005; Wolfe 2002).

The Networks of Centres of Excellence (NCE) program, the first Canadian university research funding program linking academic research with the government’s economic strategy, was introduced in 1988 by the Mulroney Conservatives (Fisher et al. 2001). This funding program did not have a separate organizational infrastructure, but was overseen by Industry Canada and administered through the three main federal granting agencies, MRC, NSERC and SSHRC. As the program evolved, it became increasingly focused on promoting industry-relevant research, university-industry collaboration and company spin-offs, bringing these criteria to the forefront of funding decisions. The goals of the NCE were to create a culture of innovation and commercialization among traditional scientists and to build a national infrastructure to support these goals. In the 2002 evaluation of the NCE program conducted by KPMG (2002), 22 NCEs were found to have generated 80 spin-off companies, 120 patents and 189 licenses. Between 1995 and 1998, NCEs had leveraged slightly more funding from public and private sector partners ($182.8 million) than had been provided by the federal government ($178 million). In their analysis of the creation and development of NCEs, Fisher and colleagues (2001) describe the program as “one of the flagship initiatives in a Federal policy framework promoting the commercialization of academic science and academy-industry partnerships” (p. 322). They argue that NCEs have succeeded in making the boundaries between basic and applied research and universities and industry more permeable.

After a period of decline in research funding during the 1990s, a rash of new funding initiatives were implemented by the Chrétien Liberals between 1997 and 2002. Under Chrétien, science and technology were a central part of the federal government’s economic strategy. New initiatives included the establishment of Canadian Foundation for Innovation (CFI), increased
stable funding for NCEs, the restructuring of the MRC to become the Canadian Institutes for Health Research (CIHR), the foundation of Genome Canada and the Canada Research Chairs (CRCs) program, and federal funding for the indirect costs of research. In 1997, the CFI provided an infusion of funding for research infrastructure, including modernization of research facilities, state-of-the-art-equipment, computer networks and research databases in health, science, engineering and environmental science (Wolfe 2002). Unlike traditional peer-reviewed funding programs, applications for CFI grants could only be put forward by institutions and in accordance with an institutional strategic plan and were based on a matched funding model. CFI grants provided applicants with 40% of the total research costs and the remaining funds had to be matched from other sources.

While the matched funding design was originally intended to leverage funding from the private sector, the “dot.com implosion” led to a decline in private sector investment in science after 2000 (Snowdon 2005). Furthermore, regional variations led to inequities in accessing the funding, particularly in Atlantic Canada. Since CFI was founded, most matched funding has been provided by other levels of government, universities or research institutions. The Ontario government developed two new grant programs, the Ontario Research Development Challenge Fund (ORDCF) in 1997 and the Ontario Innovation Trust (OIT) in 1999, specifically to match CFI grants (Snowdon 2005). By 2003, through its matched funding design, $3.65 billion in federal investments were leveraged into $9 billion in improvements in Canadian research infrastructure (Cameron 2004). The program also steered research towards applied and market-oriented projects, through its funding criteria. Applicants were required to meet three criteria: demonstrated research quality and need for infrastructure, contribution to higher capacity for innovation, and social and economic benefits to Canada (Polster 2002). Following on the heels of the establishment of CFI, the federal government announced that it would provide the NCE program with stable annual funding and increase funding levels to $47.4 million per year (Wolfe 2002; Fisher et al. 2001).

Three years later, the federal government continued to increase its investment in science while steering academic research towards economic relevance. In 2000, the MRC was restructured to become the CIHR, with a doubling of funds from $233 million to $474 million in its first year
(Fisher et al. 2005). The new research council was made up of 13 health research institutes which provided targeted funding for specific health problems. It was given a broader health research mandate, including greater research funding for population health and health services research, and an explicit focus on knowledge and technology transfer.

While health research has made revolutionary strides over the last 50 years, there remains an unacceptable lag time between discovery and the realization of health and economic benefits from applying the knowledge generated through research. Thus, countries and societies face the common challenge of how best to mobilize research to bridge the gap between what we know and what we do (CIHR 2004b:6).

Despite the shift towards knowledge translation and targeted research, CIHR’s largest single grant program continued to be the Operating Grant Program which funded investigator-driven research through open, peer-reviewed competitions (CIHR 2004c).

That same year (2000), the federal government also introduced Genome Canada and the CRCs. The purpose of Genome Canada was to provide targeted funding to large-scale genomics and proteomics research (KPMG 2009). Like NCEs and CFI, Genome Canada was based on an industry collaboration and matched funding model. It had an explicit mandate to promote the commercialization of research and 50% of project costs were required to come from other sources such as industry, other government programs and research institutions (KPMG 2009). As of 2008, funded research had generated 53 patents, 19 licenses, 32 copyrights, $3 million in licensing and royalty fees and 15 company spin-offs (KPMG 2009). The CRC program served a very different purpose. It was set up to retain high quality faculty and replace retiring professors through a one-time salary investment to create 2000 professor positions across Canadian universities (Cameron 2004). The positions were equally divided into Tier 1 chairs for new professors and Tier 2 chairs for senior professors. Like CFI, CRCs were to be awarded to research institutions rather than individual investigators.

1 Funding for NSERC and SSHRC increased as well, but only by one fifth and one third, respectively. Medical and health-related research has historically been one of the federal government’s dominant policy priorities, as reflected in the higher levels of funding provided to the CIHR (and the Medical Research Council which preceded CIHR) than to SSHRC and NSERC.

Additional investments were made in 2002, when the federal government produced its National Innovation Strategy (NIS) (Fisher et al. 2005). The NIS was presented in two papers, the first – *Achieving Excellence* – focusing on harnessing knowledge for innovation (Industry Canada 2001) and the second – *Knowledge Matters* – focusing on training people to contribute to the knowledge-based economy (HRDC 2002). This excerpt, taken from the first paper, captures the increased prominence given to knowledge in Canada’s economic strategy:

*Achieving Excellence: Investing in People, Knowledge and Opportunity* recognizes the need to consider knowledge as a strategic national asset. It focuses on how to strengthen our science and research capacity and on how to ensure that this knowledge contributes to building an innovative economy that benefits all Canadians (Industry Canada 2001: iv).

The NIS set an ambitious goal of making Canada among the top five innovative nations in the world by 2010. As part of this strategy, the federal government promised to double its R&D funding by 2010 and made a commitment to cover the indirect costs of research (Cameron 2004; Industry Canada 2001). The NIS informed science policy up until the mid-2000s when this study was carried out.

The increased focus on science and technology among federal policymakers has been accompanied by a steady increase in federal research spending. From the mid-1990s to the mid-2000s, there was a 30% increase in real levels of research funding to universities, from $900 million in 1997-98 to $2.5 billion in 2003-04 (Snowdon 2005). At the same time, the value of scientific knowledge has increasingly been framed by policymakers in terms of its capacity to foster innovation and stimulate economic development in the global knowledge-based economy. New funding was often tied to new funding criteria and processes, such as targeted and matched funding (Polster 2002). As a result of matched funding programs and alternate sources of funding, the proportion of research funded by the government declined by 20% between 1982 and 2002 (Fisher et al. 2005). Despite the overall growth in funding, academic scientists have experienced increasing difficulty in acquiring research grants. There has been a growing gap between the number of high ranking grants submitted and the number funded by CIHR. The proportion of unfunded grants with a ranking of 3.5 out of 5 or higher rose from 18% in 1999 (177 out of 986 applications) to 33% in 2003 (501 out of 1,517 applications).

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3 Indirect costs include overhead such as administration, heating, lighting, etc., that are not covered by research grants and are estimated to be 40% or more of the direct costs (Cameron 2004).
Increased levels of funding have been matched and exceeded by growth in the number of grant applicants.

Furthermore, at the time this study was carried out, CIHR had experienced its lowest ever success rates in the Operating Grant Competition (CIHR 2004c). Between 1995 and 2003 the average national success rate ranged between 28 and 31% (CIHR 2004c). The success rate for the September 2006 competition was only 16.4% (CIHR 2009a). In a message from CIHR’s President in January 2007, the low success rate was explained as the result on an increase in the number of applicants, an increase in the size of grants awarded, and a freeze in the base budget by the federal government (CIHR 2007). With an increase in the base budget, the success rate for the March 2007 competition rebounded to 26.5% (CIHR 2009a). However, the bulk of the data collection for this study occurred right after the September 2006 competition results were announced when scientists were very concerned about low success rates.

The rise in prominence of the role of science and technology as a key component of the federal government’s economic policy has been accompanied by an increase in commercial practices among academic scientists including patenting, licensing and spinning off companies (Statistics Canada 1999, 2010). Debates have arisen over the impact of the market involvement of academic scientists on the nature, quality and access to publicly funded knowledge continue among scientists and science and technology studies scholars. In the following section, I examine the dominant approaches to increasing interactions between science and industry, followed by a critical review of the literature on these interactions and their implications for academic knowledge production.

2.3. Dominant Approaches to Understanding Changing Relationships between Academic Science and the Market

A number of approaches have attempted to describe or explain the changing academic practices and relationships between universities, the market and the state. Dominant among them include Mode 2 knowledge production (Gibbons et al. 1994; Nowotny et al. 2001), Systems of Innovation (Edquist 1997, 2001), Triple Helix (Etzkowitz and Leydesdorff 1998, 2000; Etzkowitz 2003) and Academic Capitalism (Slaughter and Leslie 1997). Of these approaches,

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4 Seventeen interviews were carried out between March and August 2007; the remaining three were carried out in 2008.
the Mode 2 thesis has been the most widely disseminated and taken up among policymakers and scholars (Hessels and van Lente 2008). Mode 2 thought played a significant role in early Canadian science policy to promote research commercialization. One of the core authors of the Mode 2 thesis, Michael Gibbons, served as a science policy advisor to Industry Canada during the design of the NCE program (Fisher et al. 2001). According to this thesis, a new mode of knowledge production (Mode 2) has emerged which is different from the traditional university-based, disciplinary, peer-reviewed mode (Mode 1) (Gibbons et al. 1994; Nowotny et al. 2001). The authors describe Mode 2 as different in almost every way from Mode 1. They identify five key differences which are summarized in Table 2.

Table 2. Differences between Mode 1 and Mode 2 Knowledge Production

<table>
<thead>
<tr>
<th>Mode 1</th>
<th>Mode 2</th>
</tr>
</thead>
<tbody>
<tr>
<td>Academic Context</td>
<td>Context of application</td>
</tr>
<tr>
<td>Disciplinary</td>
<td>Transdisciplinary</td>
</tr>
<tr>
<td>Homogeneous</td>
<td>Heterogeneous</td>
</tr>
<tr>
<td>Hierarchical</td>
<td>Heterarchical</td>
</tr>
<tr>
<td>Peer review</td>
<td>Social accountability</td>
</tr>
</tbody>
</table>

Source: Gibbons et al. 1994

In Mode 1 science, research problems and agendas are determined within an academic context based on what is most interesting and relevant scientifically; it is organized and carried out within specific disciplines; the individuals involved in research are homogenous – they belong to the same research community and the same types of institutions (i.e. academic); the academic community is organized hierarchically; and the quality of academic science is determined by the scientific community through peer review. In contrast, in Mode 2 science, research problems and methods are developed within contexts of application that involve close relationships between scientists and non-scientists; research is carried out by fluid, transdisciplinary teams who bring a heterogeneous set of skills to different stages of research; because research is carried out by teams or networks that are loosely institutionalized and fluid, the structures are heterarchical rather than hierarchical, and power is shared among researchers; the quality of science is evaluated according to a broad set of criteria, including social
accountability, and by a broader range of actors from within and outside the scientific community. According to Gibbons and colleagues, Mode 2 knowledge production is not replacing Mode 1 but exists alongside and supplements it.

Like the Mode 2 thesis, the Systems of Innovation (SI) approach has been used as a framework to guide empirical research and science policy (Edquist 2001). SI seeks to understand the determinants of the development, diffusion and use of innovation and to promote the development of local, regional and national systems of innovation. According to the SI approach, innovation occurs through interactions between institutions, with universities playing a central role (Edquist 1997). Innovation policy based on SI supports the growth of interactive networks which include science and industry working together to produce innovations that stimulate regional and national development. SI has been widely adopted by government agencies, including the Organisation for Economic Cooperation and Development (OECD), the European Commission, the Government of Canada, the Government of New Zealand, and the Government of Flanders (Albert and Laberge 2007). Under the OECD, a set of policy guidelines for implementation and indicators for measuring innovation outcomes were developed which makes it attractive to policy makers and civil servants (Miettinen 2002; Sharif 2006). Unlike the Mode 2 thesis, SI is explicitly prescriptive and contains very little description of what a successful innovation system looks like or how it functions.

Like SI, the Triple Helix model also places innovation at the centre of economic competitiveness and views the interactions between different spheres as the source of innovation in a knowledge-based society (Etzkowitz and Leydesdorff 1998, 2000; Etzkowitz 2003). According to Etzkowitz (2003), most modern nations are trying to develop Triple Helix relationships between university, industry and government that stimulate innovation. He identifies two kinds of Triple Helix relationships – statist and laissez faire – that have failed to provide the best conditions for innovation. In the statist model (Triple Helix I), often found in socialist countries, the state controls universities and industry and their interactions. In the laissez faire model (Triple Helix II), typically found in social democracies and liberal countries, the state, universities and industry are separate spheres with strong boundaries between them and few interactions across boundaries. Etzkowitz argues that a third Triple Helix is emerging, in which the university has an equal role to industry and government in promoting innovation.
In the Triple Helix III, the boundaries between spheres are broken down and innovation is produced through interactions between overlapping spheres. In these overlapping relationships, institutions take on the roles of each other and hybrid organizations form (Etzkowitz and Leydesdorff 2000). As institutional spheres overlap, they also undergo transformations; universities become more entrepreneurial while industry and government become more knowledge-oriented. Knowledge and innovation are produced in the interactions between institutions, through the development of networks, incubators, clusters and science parks. The traditional flow of knowledge from the university to society becomes an interactive exchange. While the university takes on a central role in economic growth, it retains its core function as an institution for the preservation and transmission of knowledge.

Unlike Mode 2, SI and Triple Helix approaches, which largely describe and prescribe change, Academic Capitalism seeks to explain change in relation to broader political and economic forces (Slaughter and Leslie 1997). The Academic Capitalism thesis locates the sources of change in the university structure in changes in the relationship between capital and labour, as national industrial economies reposition themselves to become more competitive in the global knowledge economy. In the knowledge economy, the university takes on a heightened importance as a producer of highly qualified personnel, who create new technologies, and as a direct source of new technologies that can be brought to the market. Universities are increasingly pressured to reduce costs, generate their own revenue by adopting market-like behaviour, and contribute to the market economy through commercialization and intellectual property protection strategies. Proponents of the Academic Capitalism thesis see the encroachment of the market into the university as producing benefits for some and costs for others. Not all institutions or scientists are able to take advantage of market opportunities leading to uneven movement toward the market. Slaughter and Leslie (1997) suggest that changes in universities have created tensions between competing goals and struggles between academic autonomy and managerial control.

Despite their differences, there is a consensus across all approaches that a shift is occurring from investigator-driven, discovery-based science to problem-based, strategic, “relevant” science with an accompanying intensification of interactions between academic scientists and other societal actors from industry and government. Yet, as we shall see in the following
section, the empirical literature is fraught with contradictions about the nature and effects of these changes on academic norms, identity and knowledge production.

2.4. Critical Analysis of the Empirical Literature: What is known about the Market Involvement of Academic Faculty?

In this critical review of the literature, I will analyze the prevalence of market involvement of academic faculty, the threats and opportunities it provides to scientists, and the strategies scientists use to navigate these threats and opportunities. Academic scientists use a variety of strategies to resist or minimize market involvement or mitigate the negative impacts and take advantage of the opportunities market involvement can provide. Overall, the literature suggests that market and scientific goals are complementary under some conditions, and for some scientists, and contradictory under others. However, the existing research tells us very little about why scientists adopt particular strategies and how differences in power shape these strategies and their outcomes.

2.4.1. Prevalence of Faculty-Industry Relationships and Market Practices

The rise in university-industry collaboration and the involvement of academic faculty in market-oriented practices are well-documented. Growth in patenting and licensing activities by faculty and universities, the rise in number of technology transfer offices in universities, and an increase in the number of spin-off companies founded by academic scientists have all been reported (Mowery et al. 2001; Owen-Smith and Powell 2003; Owen-Smith 2005). For example, in an analysis of patenting and licensing in US universities, Mowery and colleagues (2001) found that the number of patents issued to US universities more than doubled, from 551 in 1984 to 1,228 in 1989, and then doubled again to 2,436 in 1997. Between 1980 and 1990, the number of universities with technology transfer offices increased from 25 to 200 and licensing revenues increased from US$222 million in 1991 to US$698 million in 1997. Most university patents and the revenue generated by them come from the life sciences. By 1998, half of all patents issued to US universities were based on biotechnology innovations (Owen-Smith and Powell 2003). A similar picture emerges from data on the growth in market practices in Canadian universities (see Table 3). Statistics Canada began collecting data on indicators of commercialization in universities in 1998. Between 1998 and 2008, the number of patents issued to Canadian universities tripled, from 1,826 to 5,908; the number of licenses rose fourfold (from 788 to
3,343); the royalties generated from licences tripled (from CAN$15.7 million to CAN$53.2 million); and the number of company spin-offs rose from 454 (the total number of spin-offs historically founded by Canadian universities) to 1,242.

Table 3. Market Practices of Canadian Universities from 1998 to 2008

<table>
<thead>
<tr>
<th>Commercial Activities</th>
<th>1998</th>
<th>2008</th>
</tr>
</thead>
<tbody>
<tr>
<td>Patents</td>
<td>1,826</td>
<td>5,908</td>
</tr>
<tr>
<td>Licenses</td>
<td>788</td>
<td>3,343</td>
</tr>
<tr>
<td>Royalties</td>
<td>$15.7M</td>
<td>$53.2M</td>
</tr>
<tr>
<td>Spin-offs</td>
<td>454</td>
<td>1,242</td>
</tr>
</tbody>
</table>

Source: Statistics Canada 1999, 2010

While there has been a steady increase in patenting, licensing and firm-founding activities among academic scientists since the 1980s, this is not necessarily true of industry funding. Three surveys carried out over three decades explored the prevalence and magnitude of industry sponsorship of US life sciences faculty (Blumenthal, Gluck, Louis et al. 1986; Blumenthal, Campbell, Causino et al. 1996; Zinner, Bolcic-Jankovic, Clarridge et al. 2009). Fewer faculty reported receiving industry funding in the mid-2000s than in the mid-1980s. While a similar methodology was used across studies, the samples were not the same. The first survey of 1,238 faculty at 40 US universities found that 23% of life sciences faculty reported receiving industry support in 1984 (Blumenthal et al. 1986). In 1994 and 1995, a follow-up survey of 2,052 life sciences faculty at 50 U.S. universities was carried out (Blumenthal et al. 1996). The proportion of life scientists who received industry support had risen to 28%. A third survey of 2,168 life scientists in 50 US universities was carried out in 2006-7 (Zinner et al. 2009). This study found that the proportion of faculty receiving industry support had declined to 20%.

2.4.2. Market-Involvement as Opportunity or Threat to Academic Science

Faculty cite additional resources as the primary benefit they gain from market practices. Potential drawbacks include reduced openness of academic knowledge, steering of research towards applied topics and the substitution of commercial practices for scientific practices. Industry-involved faculty have reported greater access to financial resources and advancement of basic research (Blumenthal et al. 1986; Glaser and Bero 2005; Lee 2000; Owen-Smith and
Powell 2001); employment opportunities for graduate students (Blumenthal et al. 1986); protection of academic freedom; and ability to generate revenue (Lee 2000; Owen-Smith and Powell 2001). Owen-Smith and Powell (2001) found that faculty patented for a variety of reasons: to protect scientific knowledge and inventions, to leverage federal grants, industry sponsorship and venture capital, to profit from business ventures, to serve the public good with the development of new products and treatments, to gain prestige, and to advance scientific thinking. Findings were based on interviews with 69 physical and life sciences faculty, administrators and technology transfer staff in two US universities. They found that the emphasis on academic freedom and revenue generation differed between physical and life sciences faculty. Physical scientists used patenting primarily as a means to protect and maintain their scientific freedom, while life scientists were more interested in using patents to leverage investments for new product development leading to eventual profits.

Financial gain and advancement of research were also reported as the dominant benefits of industry collaboration in a study by Lee (2000). In his 1997 survey of 671 science and engineering faculty across 40 US universities, he found that over two thirds of respondents reported financially benefiting from their industry relationships and advancing their own research. Greater benefits were reported from longer-term projects and more intense industry interactions. Lee also found that Assistant Professors were more motivated to acquire industry funding than Associate or Full Professors. Other studies have also found non-tenured professors to have more favourable attitudes than tenured faculty towards industry involvement. Crowe and Goldberger (2009) conducted a national survey to explore faculty perceptions of university-industry collaboration in the agricultural sciences field in the US. The survey was distributed to a random sample of 1,963 faculty across 21 agricultural disciplines in 53 universities for a final sample of 946 faculty. They found that pre-tenured faculty were much more open towards industry involvement than tenured faculty. Interestingly, in one of the few studies to explore differences in perceptions based on gender, they found that female faculty were no more likely to have negative perceptions of university-industry collaboration than male faculty. They were, however, less likely to report financially benefiting from these relationships.

A study carried out in the UK also found that pre-tenured faculty had more positive perceptions of industry collaboration than tenured faculty (Ambos, Mäkelä, Birkinshaw and D’Este 2008).
Data were drawn from a survey of the principal investigators of 207 UK research projects funded through a government program that required applicants to engage in industry collaboration. The respondents carried out research in science and engineering. The authors suggest that young, high achieving scientists are more ambidextrous in achieving the dual market and scientific goals and may in fact be driving the transformation of the science. The more positive perceptions of scientists in the early stages of their careers may reflect greater acceptance of the pursuit of both academic and commercial goals among new entrants or it may reflect the competitive nature of the current funding climate, in which new faculty must seek out all potential sources of funding (Crowe and Goldberger 2009; Lee 2000).

Research on market-involvement of academic faculty has explored a number of contradictions between market and scientific values and practices that may pose threats to science. Non-industry-involved faculty were significantly more concerned about the negative impact of market involvement on academic science than industry-involved faculty (Blumenthal et al. 1986; Campbell and Slaughter 1999; Glaser and Bero 2005). These threats can be summarized as the “secrecy” effect, the “substitution” effect and the “skewing” effect. The “secrecy” effect suggests that market involvement will lead to a decline in the open sharing of knowledge as knowledge shifts from being a public good to a private good through university intellectual property strategies (Heller and Eisenberg 1998; Murray and Stern 2007). In relation to the “substitution effect”, the fear is that market involvement will lead to a decline in academic productivity as market practices are substituted for scientific practices (Gulbrandsen and Smeby 2005). Finally, market involvement may lead to a skewing of research agendas towards applied and commercially relevant research topics (Florida and Cohen 1999). Efforts to measure these effects have produced mixed results.

The “Secrecy” Effect

One of the core values of traditional science is the open sharing of knowledge, which Merton called “communism” (Merton 1973). Granting intellectual property rights to scientists has the potential to restrict access to knowledge and transfer it to the private sphere, restricting its free circulation as an input to the advancement of new knowledge (Heller and Eisenberg 1998; Murray and Stern 2007). There is mixed evidence for the “secrecy” effect. In 1984, Blumenthal and colleagues (1986) carried out a survey of 1,238 life science, chemistry and engineering
faculty at 40 top-funded US universities. In the first large-scale study of faculty involvement in industry relationships, industry support was perceived as a risk to the free exchange of intellectual ideas. The majority of non-industry involved life sciences faculty (68%) expressed a concern with secrecy resulting from industry involvement, compared with only 44% of those who were industry-involved. Yet industry-involved faculty were four times more likely to report trade secrets than non-involved faculty (12% compared with 3%). The authors defined trade secrecy as “information kept secret to protect its proprietary value” (p. 1364).

Follow-up surveys conducted in the mid-1990s and mid-2000s found that self-reported trade secrecy has remained stable among industry-involved life sciences faculty (Blumenthal et al. 1986, 1996; Zinner et al. 2009). In a comparison of survey data collected across three decades, Zinner and colleagues (2009) found that trade secrecy among industry-involved faculty hovered between 11 and 12% in 1986 and 2006. Surprisingly, trade secrecy had risen among non-industry-involved faculty – from 3% in 1986 to 7.3% in 2006. However, industry involved faculty were twice as likely as non-involved to report publication delays of six months or longer (13.4% compared with 6.1%). Even short term publication delays can impede the spread of knowledge to other scientists and slow down the advancement of knowledge. Vallas and Kleinman (2008) also found that secrecy was reported by industry-involved and non-involved faculty alike. Based on data from a qualitative study of US university and firm scientists, the authors found that even non-industry involved faculty in the academic sphere experienced pressure to conform to a commercial logic, including a focus on revenue generation, framing research in terms of utility and viewing knowledge as a commodity. They argue that growing competitiveness resulting from the convergence of academic and commercial cultures is responsible for this trend of data withholding.

There is some evidence suggesting that the patenting of academic knowledge might lead to reduced use in future research over time. Two studies using patent and publication data to measure the impact of intellectual property rights and patenting strategies on the supply of public knowledge found a decline in the citation rate of publications based on a patented discovery (Murray and Stern 2007; Huang and Murray 2009). Murray and Stern (2007) examined 340 publication-patent pairs in biotechnology research, exploring publication citation rates in the pre- and post-patent granting period. If the patenting of a scientific discovery limits
the ability of scientists to build on that discovery (e.g., through licensing fees and materials transfer agreements), the authors hypothesized that intellectual property grants may lead to reduced citations to publications of the discovery. They found a 10 to 20% decline in citation rate in the four years after the patent was granted. Using methods developed in the first study, Huang and Murray (2009) examined 1,279 patent-paper pairs in human genetics research from 1988 to 2006. This study found a 5% decline in publication citation rate in the post-patent period. The decline in publication citation rate was strongly associated with private ownership of gene patents. Also, patents that were disease specific, had a broad scope, and were in areas where multiple patents were owned by multiple owners were more likely to be associated with lower citation rates. This implies that certain kinds of research associated with drug targets are more likely to be removed from the public sphere as companies vie with one another for the right to develop and profit from new treatments. This, in turn, may restrict academic scientists from carrying out fundamental research that falls within the scope of the patent even if this research has implications for a range of diseases and treatments outside of the patent’s scope.

The “Substitution” Effect

A second potential threat to academic science is the substitution of market practices for scientific practices leading to an increased teaching and administrative burden on non-involved faculty or a decline in scientific productivity. Campbell and Slaughter (1999) conducted a survey of 407 administrators and faculty drawn from the social sciences and fine arts, science and engineering and business in US universities. They found significant tensions between the industry-involved and uninvolved respondents in their attitudes towards university-industry relationships. Industry-involved faculty were in favour of reducing their teaching and service time to meet industry commitments while non-industry-involved faculty did not want to be forced to take on additional teaching and service hours. Moreover, non-industry-involved faculty did not view profit-making activities of universities and faculty as being in the public’s interest.

A number of studies have explored whether patenting behaviour reduces scientific productivity. Using patent and publication data, these studies have demonstrated that industry-involved scientists, inventors and firm founders are more productive both scientifically and commercially than non-industry-involved faculty (Blumenthal et al. 1986, 1996; Gulbrandsen and Smeby
2005; Meyer 2006; Owen-Smith 2003; Van Looy, Callaert and Debeckere 2006; Van Looy, Ranga, Callaert, Debackere and Zimmerman 2004; Zinner et al. 2009). These studies provide support for the Mode 2 thesis, suggesting that a new mode of knowledge production can advance science while producing socially useful knowledge. In a series of surveys of faculty-industry relationships carried out over three decades, Blumenthal and colleagues consistently found that industry-involved faculty were at least as productive academically and more productive commercially than uninvolved faculty (Blumenthal et al. 1986, 1996; Zinner et al. 2009). However, faculty who received more than two-thirds of their funding from industry reported lower productivity, both scientifically and commercially. In a survey of all faculty (N=1,967) in Norway in 2001, Gulbrandsen and Smeby (2005) found that industry-funded faculty produced more entrepreneurial outputs (patents, commercial products, firms, and consulting contracts) and published more than non-industry funded faculty. Findings from these surveys are limited by the fact that productivity was self-reported.

However, other studies that have examined the relationship between patents and publications using objective output measures have arrived at similar conclusions. For example, Van Looy and colleagues (Van Looy et al. 2004, 2006) used publication and patent databases to track the relationship between commercial and scientific activity over time in a Belgium university. They found that industry-funded scientists published more than non-industry funded scientists and that the difference in publication output grew wider over time (Van Looy et al. 2004). Patenting scientists also produced more scientific publications than non-patenting scientists (Van Looy et al. 2006). The authors conclude that ‘star scientists’ are more likely to choose to be involved in market practices or to be selected by industry as desirable collaborators. High performing scientists were able to leverage their market involvement into higher scientific productivity and prestige.

Using time series data for a random selection of patent-holder/control pairs (n = 16), Van Looy and colleagues (2006) mapped publication output before and after the year of the faculty member’s first patent. Inventor-scientists were more productive than their peers before they patented and the difference in publication output grew wider after they patented. On average, the difference in publication volume between patent holders and non-patent holders doubled after filing their first patent. The authors suggest that the convergence of a market and scientific
logic has altered the reward system in science, creating what Owen-Smith (2003) calls a hybrid system of stratification. In traditional science, recognition is disproportionately distributed to scientists who have already acquired a scientific reputation, something that Merton (1968) famously called the “Matthew effect”. Van Looy and colleagues (2004) suggest that the positive impact of commercial activities on scientific reputation can be considered a “compounded Matthew-effect” (p. 15). As scientific success becomes linked with market success, scientists are able to reap increasing returns from both practices. However, some scientists may be more likely to benefit from market involvement than others. At the individual level, some inventor scientists who were equally or less productive than non-inventor scientists before patenting remained equally or less productive in the period following patenting.

*The “Skewing” Effect*

The “skewing effect” refers to shifts in the balance of applied and basic research, with more research being carried out in applied areas at the expense of the advancement of fundamental knowledge (Florida and Cohen 1999). Despite the fact that many scientists believe, or fear, that science is being steered in a more applied direction (Glaser and Bero 2005), the evidence is mixed. Market-involved faculty are more likely to report having an applied research agenda (Blumenthal et al. 1986; Gulbrandsen and Smeby 2005). However, this is not reflected, for the most part, in the type of research publications they produce (Gulbrandsen and Smeby 2005; Ranga, Debackere and Tunzelmann 2003). In the 1985 survey carried out by Blumenthal and colleagues, both industry-involved (70%) and non-involved (78%) biotechnology faculty expressed concern that industry involvement would shift too much emphasis to applied research. Industry-involved faculty were three times more likely than non-industry-involved to report altering their research topics in directions with greater potential for applications. Similarly, in a survey of all tenure-track faculty (1,967) in Norway, industry-involved faculty were more likely to report being involved in applied research than non-industry involved faculty (Gulbrandsen and Smeby 2005). Nearly half of industry-funded faculty considered their research applied compared with one quarter of non-involved faculty. However, there were no differences in the type of publications produced – journal articles versus popular science publications – between industry-involved and non-involved.
A longitudinal study of the type of scientific publications carried out at a Belgian university does not support a shift towards applied publications among industry-involved scientists (Ranga et al. 2003). Ranga and colleagues tracked changes over a fifteen year period (1985-2000) in publications classified using the SCI database as “very applied”, “applied”, “basic” and “very basic”. They found a similar growth rate in applied and basic publications among industry-involved faculty, with a slightly faster rate for basic research publications. However, another study carried out at the same Belgian university by Van Looy and colleagues (2004) found the opposite: industry-involved scientists published more applied publications than non-involved faculty and at an increasing rate over a nine year period (1991-2000). This shift, they argue, did not come at an expense to basic research, as industry-involved scientists also published more basic publications than non-industry involved scientists. These findings were based on data drawn from faculty belonging to applied and natural science research divisions which were developed specifically to support industry collaboration and technology transfer. Findings may reflect the culture of these research divisions rather than the university as a whole.

In summary, the dominant reason scientists engage in market practices is to gain resources to invest in the advancement of scientific research. There is evidence that scientists can combine market and scientific practices without a significant cost to scientific productivity. In fact, scientists may be able to leverage their commercial success to increase their scientific prestige. Industry-involved and pre-tenured faculty are more likely to have a positive perception of market involvement. Some adverse effects of market involvement on the openness and skewing of research continue to be found. This literature has provided a top-down view on how market-oriented practices are perceived by scientists and shaping knowledge production. However, it tells us little about how academic faculty experience and respond to pressures and opportunities to engage in market-oriented science. In the following section, I examine studies of the strategies that scientists use to navigate the threats and opportunities provided by market-oriented science.

### 2.4.3. Strategies to Navigate Threats and Opportunities

A number of studies have explored how academic scientists respond to changes in science funding and academic culture and how they negotiate relationships with industry. These identify some of the conditions under which scientific and market practices are complementary.
and those under which contradictions emerge. The kinds of strategies scientists used are divided into two categories: 1) those to minimize market involvement and the impact of market pressures on scientific practice; and 2) those to negotiate the contradictions between scientific and market goals and practices. Both kinds of strategies involved boundary work (Gieryn 1983, 1999). Scientists used traditional and market-oriented visions of “good science” to defend, redraw or work across boundaries between science and the market.

**Strategies to Minimize Market Involvement**

Some scholars argue that knowledge production continues to follow Mode 1 patterns as recognition of scientific worth continues to be based on traditional epistemological criteria, embedded in traditional academic values and identities (Gulbrandsen and Langfeldt 2004; Henkel 2005). At the same time, scientists draw on both traditional and market-oriented visions of science to defend traditional scientific values and practices and minimize market involvement (Calvert 2006; Kinchy and Kleinman 2003; Morris and Rip 2006). In a large-scale study of academic responses to research policy change in the UK and Europe in the mid-1990s, Henkel (2005) found little change in academic identities and values. Interviews were carried with 165 natural and social scientists across seven disciplines, eleven universities and three countries (England, Norway and Sweden). While science policies increasingly sought to steer research agendas towards strategic research and hold scientists accountable to broad political and economic goals and stakeholders, scientists continued to identify with discipline-based academic communities and to value scientific autonomy.

The majority of respondents reported overt compliance with policy changes but minimal change in practice. Even when scientists took advantage of targeted funding or joined research networks with industry involvement, they retained a focus on the advancement of scientific knowledge rather than commercial goals. Gulbrandsen and Langfeldt (2004) also found that, despite pressures to engage in university-government-industry partnerships, social and natural scientists in Norway retained a traditional vision of scientific excellence. Based on interviews with 64 scientists in universities, research institutes and industry, they found that persistent differences across sectors were consistent with traditional criteria for evaluating research. University researchers continued to value theoretical originality while researchers based in institutes and industry valued relevance.
Scientists who retain and defend a traditional vision of “good science” are engaged in boundary work to maintain their credibility in the face of increasing pressures to engage in applied, relevant research. Gieryn (1983, 1999) first coined the term boundary work to describe the strategies scientists use to demarcate “science” from “non-science” in order to defend their credibility against competing claims. Boundary strategies were identified by Morris and Rip (2006) in their study of the responses of scientists to changes in science policy and funding in the late 1990s. Seventy-four industry and non-industry-involved life sciences faculty in four UK universities were interviewed. They found that scientists used a traditional definition of “Science” to maintain their professional autonomy and to defend scientific control over the evaluation of scientific worth, through the system of peer-review. Scientists “selectively appropriated” policies and practices that were most compatible with this definition. Because scientists did not depend on a sole source of funding, they were able to select from funders that allowed them to pursue their own research agenda.

Scientists may also use a market-oriented vision of science to acquire market-oriented resources while continuing to engage in traditional scientific practices (Calvert 2004, 2006; Kinchy and Kleinman 2003). In a study of how scientists define and use the term “basic research”, Calvert (2004, 2006) found that the term has multiple, ambiguous meanings and is used strategically at the boundary between science and policy or science and industry to garner resources. Calvert interviewed twenty-four physical and life scientists in the US and UK. In response to increasing pressures from government funders to produce applications, respondents described “tailoring” their research to make it appear applied without altering their research practices. In other situations, they defined their research as “basic” to rebuff unwanted commercial interest or avoid evaluation according to utility-oriented criteria. Along a similar line of thought, Kinchy and Kleinman (2003) argue that scientists commonly draw on “historically resonant discourses” of both purity and utility to maintain credibility (p. 870). These discourses have become institutionalized and taken for granted through prolonged use in the scientific community and structure practice. Kinchy and Kleinman found that ecological scientists who acted in political advisory roles maintained their scientific and political credibility by carrying out dual boundary work. Scientists used boundary strategies to both demarcate science from politics and ecological activism and to construct ecology as a useful science for solving environmental problems such as global climate change. These studies illustrate how scientists resist changing
their research agendas and practices in response to changes in policy and funding. Among scientists who seek to take advantage of market-oriented opportunities, a different set of strategies is used to navigate the contradictions that arise between scientific and market goals.

*Strategies to Negotiate Market Relationships*

Scientists experience numerous conflicts and contradictions when they try to accomplish both scientific and market goals. These tensions revolve around differences in norms, values and practices between the academic and commercial worlds. Conflicts arise between the proprietary culture of industry and the open norms of science (Slaughter et al. 2004), and between the demands of scientific research versus technology and product development (Tuunainen 2005; Ylijoki 2003). In order to access industry resources that can be used to advance scientific research, scientists must engage in a set of practices that their scientific careers have not prepared them for. In the process, they have developed new competencies and strategies to move between distinctly different worlds and produce different types of products. According to Packer and Webster (1996), scientists must develop a new set of “socio-technical competencies” to acquire intellectual property. In a qualitative study exploring how academic scientists acquire and use patents, Packer and Webster carried out in-depth interviews with 35 patenting scientists across 10 US universities. They found that scientists engaged in two types of work to acquire patents: “articulation” work and “production” work. Articulation work involved the work carried out in the scientific lab to demonstrate the feasibility of an invention within an applied context. Production work involved producing texts that make sense within the patenting system. Both of these types of work required the investment of time to acquire new skills.

Packer and Webster (1996) argue that patents themselves become a new form of credibility which can be invested in the achievement of scientific credibility. Yet, commercial credibility may not be easily translated into academic forms of credibility because the forms of investments and types of skills needed to secure these rewards are so different from those in academic research. They found that scientists used patents for two main purposes: to establish credibility within academic institutions and among potential industry sponsors and as a tool to protect academic freedom. In academic institutions that valued research commercialization patents provided credibility. Among industry representatives, they served as a tool to gain
industry sponsorship and acted as a means of repayment for sponsorship. While they were not perceived as having any direct scientific value, scientists used patents as a defensive strategy to keep knowledge in the public sphere and out of the proprietary hands of industry. Patents rarely led to direct lines of revenue or the transfer of technology into new marketable products.

Even as scientists engage in market-oriented practices they reinterpret and use them to achieve traditional scientific goals. Like Packer and Webster, Murray (forthcoming) also found that scientists used patenting to protect their academic freedom. This is illustrated in Murray’s case study of the “Oncomouse” patenting controversy in the US. She explores the behaviour of a group of life scientists who organized to oppose the patent rights of a large pharmaceutical company while increasingly engaging in patenting themselves. The controversy ensued when a genetically engineered mouse was patented and exclusively licensed to a US pharmaceutical company, placing severe limits on research involving the “Oncomouse”. The intellectual property rights gave the company rights to royalties on any products developed using the Oncomouse, the right to oversee publications based on research using the Oncomouse, and restricted informal breeding and sharing of mice. After a prolonged struggle between scientists and the company, the company agreed to loosen its restrictions among academic scientists to allow scientific research to proceed unimpeded.

While they resisted the company’s patent rights, the same community of scientists began to engage in patenting their own research. Murray (forthcoming) explains this behaviour in terms of the two alternative systems of logic and the difference in meaning of patenting by the academic and business community. According to the market logic, patents are the basis for financial exchange according to specific, contractual rules. However, scientists used patenting according to a scientific logic of prestige, as a new means for gaining recognition and as a strategy to protect science from industry’s attempt to profit from academic work. Scientists adopted this new logic of exchange, but modified it to fit with academic values and practices. Patents “expanded the calculus of credit and reputation as scientists used patents to disclose innovations, have them commercialized and gain recognition” (Murray forthcoming:33). Thus, while a market logic pervades the academic world, it is interpreted and modified through the existing lens and institutional structures of that world.
Because of the differences in the logic and content of commercial and scientific products, academic patenting has not lead to the degree of secrecy that was initially feared (Blumenthal et al. 1986). Scientists have developed strategies that allow them to both patent and publish their research findings. In a qualitative study of the strategies used by scientists and engineers to negotiate industry relationships, Slaughter, Archerd and Campbell (2004) found that industry-involved faculty used strategies of “sequencing” and “sanitation” to both publish and patent. The authors carried out interviews with 38 industry-involved science, medical and engineering faculty in 14 US universities. Sixty percent owned patents and 40% were involved in spin-off companies. While faculty reported few difficulties accommodating both publishing and patenting activities, they were often required to delay publication and withhold data. They “sequenced” their practices by patenting before publishing and “sanitized” data that were deemed confidential by the industry sponsor. Patent applications delayed publications for up to 18 months; this was not perceived as a problem by tenured faculty, but was for newer faculty and graduate students. Scientists were able to publish their findings without contravening pre-publication and non-disclosure agreements even if this meant withholding some of the data.

Some scientists have been able to effectively coordinate distinctly different academic and market practices, while others continue to come up against differences in commercial and scientific expectations and rewards that challenge their ability to move easily between worlds. Based on an empirical study of academic firm founders in France, Shinn and Lamy (2006) identified three types of entrepreneurs based on three modes of coordination between academic and commercial worlds. The “Academics” used a strategic mode of coordination, subordinating the spin-off to their academic activities while using the firm strategically to acquire resources. The “Janus” group used a sequential mode of coordination, alternating between a complete commitment to science and a complete commitment to the firm. The “Pioneers” used an imitation mode of coordination, placing commercial goals and values ahead of science. The “Pioneers” were the least successful of the three groups at achieving either their academic or commercial goals and experienced the greatest tension between their positions as academics and entrepreneurs. This is consistent with Blumenthal and colleagues’ (1986, 1996) findings that too much industry involvement is associated with lower scientific productivity. Both the Academics and Janus scientists experienced little tension between the two logics but the Janus scientists were more successful both academically and commercially.
When clear boundaries are not maintained, commercial success can threaten academic goals. Tuunainen (2005) and Ylijoki (2003) both found that commercially successful hybrid research groups in Finnish universities faced insurmountable tensions and contradictions between their academic and commercial goals. Hybrid organizations are formed at the interface between science and industry and have accountability towards both worlds (Etzkowitz 2003). Tuunainen (2005) conducted a case study of a hybrid firm founded by a group of agricultural scientists in a Finnish university. The research group’s goal was to produce a commercially viable virus-resistant potato and advance knowledge about the mechanisms and processes of virus resistance. To reach these goals, scientists needed to theoretically understand the biology of virus-resistance mechanisms and develop new research materials, tools and methods to develop a genetically modified potato. While the firm’s work was both scientifically and commercially fruitful, conflicts over ownership of IP, ownership and sharing of materials, and time spent outside of academic work on commercial work led to the removal of the firm from the university into a nearby science park. The hybrid organization failed, leading to the erection of clear boundaries between academic knowledge production and commercial development.

Similarly, Ylijoki (2003) found that the commercial success of a hybrid applied physics research group undermined its academic success. This research group had a history of market involvement, had created two spin-off firms and was involved in the ongoing industry collaboration to develop new laser technologies. Scientists became increasingly dissatisfied with their ability to succeed according to economic and academic criteria. Not only did the commercial orientation of the lab prevent scientists from investing sufficient time to achieve the level of scientific recognition they desired, but they also earned much less financially than their colleagues who had left academia to join the spin-off firms. This created a problem for the lab in retaining staff with the required levels of skill and knowledge. Both of these hybrid groups shared some of the characteristics of Shinn and Lamy’s (2006) “Pioneers”, as commercial goals and practices came to overshadow scientific goals and practices.

Using a multiple case study methodology, Ylijoki (2003) compared the responses of the applied physics research group to market pressures and opportunities with faculty in a traditional history department and an applied social sciences research centre. While the commercialization strategies of the applied physics lab resembled Shinn and Lamy’s (2006) imitation mode of
coordination, the history department used a strategic mode of coordination and the applied sciences research centre used a sequential mode of coordination. Differences in strategies were associated with differences in ability to maintain or leverage greater scientific recognition. The history department had undergone a shift from individualist orientation and internal funding to a collective orientation and external funding. History faculty successfully adapted to market pressures in order to access external sources of funding, but continued to maintain individual-based research practices. They subordinated the demands of the market to the practices associated with the production of legitimate knowledge to maintain their scientific prestige. The applied social science research group, on the other hand, had a history of depending on external funding, externally driven research agendas and low scientific prestige. As a result of changes in the funding context, they no longer had to justify their applied external orientation as it had become the academic norm. In fact, social science faculty had been able to leverage their applied practices and external funding to improve their scientific productivity and prestige. Using a sequential mode of coordination, faculty alternated between carrying out externally-relevant research and pursuing their own academic interests. However, they experienced increasing pressure and difficulties juggling these dual demands.

Contrary to the line of thought that only “star scientists” can leverage commercial success into scientific recognition (Van Looy et al. 2004, 2006), these studies suggest that, under some circumstances, non-elite scientists or research groups may also be able to do this. Owen-Smith (2003) provides compelling evidence at the organizational level that low to moderate performers may be able to use their commercial productivity to raise their scientific prestige. In a longitudinal study (1981-1998) of patent-publication data from 89 research-intensive US universities, Owen-Smith traced the trajectories of universities that he classified according to their initial patent and publication performance: the “patenting elite”, “scientific elite”, “founding elite” and “mobile” group. Over time, the patenting elite, which initially had a high level of patents but low scientific performance, experienced a decline in both their patenting success and publication impact. The scientific elite experienced a dramatic increase in patenting performance while maintaining a high publication impact. The founding elite, who started out as high performers in both realms, maintained their dominant position in both patenting and publishing. The mobile group began with lower performance in both patenting and publishing
but dramatically increased their patenting output with a corresponding increase in publication impact.

According to Owen-Smith (2003), mobile organizations were able to take advantage of a window of opportunity to raise their status relative to other universities. They were then able to maintain this higher status through an ongoing positive feedback loop between publishing and patenting. Owen Smith argues that the reward system in science is changing, as two previously separate stratification systems – scientific and market – become integrated into a single hybrid stratification system. Academic rewards are stratified according to scientific competence and recognition whereas commercial rewards are stratified by a patenting and licensing competencies and recognition. In a hybrid stratification system, universities that develop effective scientific and market strategies will gain an advantage relative to universities that do not. They will experience a scientific and commercial “Matthew effect” (Merton 1968) as success in one realm feeds into success in the other. Just as Packer and Webster (1996) attribute patenting success at the individual level to the development of a new set of competencies, Owen-Smith attributes organizational patenting success to organizational learning and development of organizational capacity to effectively patent and license academic knowledge.

To summarize, this literature identifies processes through which scientists resist, accommodate and take advantage of market pressures and opportunities. It deepens our understanding of how industry-involved scientists navigate contradictions between the norms of the market and norms of science and the challenges of working across the boundaries between these two worlds. Three general conclusions can be drawn. First, scientists position their research as basic or applied in order to gain access to material and symbolic resources and credibility. Symbolic strategies may or may not correspond with changes in practice. Second, scientists tend to interpret and use market practices within the framework of traditional science. Even when scientists engage in market practices, such as the protection of intellectual property, they may do so to achieve academic rather than commercial ends. These strategies mitigate some of the negatively anticipated effects of market involvement, such as the “secrecy” and “skewing” effect. Third, scientists are not equally able to take advantage of and benefit from market involvement, and even those who succeed face ongoing challenges. Scientists are more likely to succeed in meeting dual goals if they learn a new set of market-oriented competencies and
maintain clear boundaries between scientific and commercial practices. Contrary to patent-publication outcome studies, which suggest there is no “substitution effect”, studies of scientific practice suggest that commercial practices may, under some conditions, replace or override scientific practices. This seems most likely to occur when scientists are unable to develop market-oriented competencies and fail to maintain boundaries between their scientific and commercial worlds.

In summary, market involvement provides opportunities to academic scientists and can, under some conditions, be complementary with scientific practices. The adverse effects initially predicted have failed to manifest themselves, at least to the extent initially feared, as scientists have learned a new set of competencies and developed strategies to accomplish dual goals. However, the contradictions between market and scientific goals and practices continue to pose challenges. University-industry relationships have often been interpreted through a lens of stability or transformation (Gulbrandsen and Langfeldt 2004; Gulbrandsen and Smeby 2005; Henkel 2005; Hessels and Van Lente 2008; Morris and Rip 2006; Tuunainen 2005; Ylijoki 2003). From a stability perspective, scientists continue to be valued and rewarded for producing “good science” as defined by traditional academic criteria (i.e. peer-reviewed publications). They actively resist, minimize, subordinate and reinterpret market practices according to a traditional Mode 1 academic logic. From a transformation perspective, a shift is occurring towards a new mode of knowledge production that can be perceived as either compatible with or a threat to traditional science. Scientists can simultaneously produce commercial and scientific products and leverage success in one form into success in the other. However, scientists may not be equally able to benefit from market opportunities. Furthermore, contradictions and conflicts between a market and scientific logic can, under some conditions, overburden scientists, undermine scientific productivity, change the nature of scientific research and threaten open intellectual exchange.

Both stability and transformation arguments, I believe, miss the point. They tend to focus on the direct effects of industry involvement rather than broader changes in the culture and reward system in science (Owen-Smith 2003, 2005; Vallas and Kleinman 2008). Scientific practice is motivated by the achievement of prestige and recognition (Bourdieu 1990a; Merton 1968). The current literature illustrates that scientists use market logic and market practices to acquire new
forms of credibility and resources that they invest in the pursuit of academic recognition. It suggests that the achievement of academic recognition may be increasingly dependent on the ability of scientists to achieve dual goals (Owen-Smith 2003), but that scientists are unevenly equipped to conform to and benefit from market involvement. This disruption of the traditional academic rewards system may lead to a restructuring of the distribution of rewards in science and the strategies that scientists use to achieve scientific recognition.

While the existing literature provides valuable information about the extent, nature and impact of the market involvement of academic faculty, significant gaps remain. Most studies focus on the perceptions or practices or outcomes associated with market-involvement but few (if any) explore the relationships between perceptions, practices and outcomes. These studies produce partial interpretations and contradictions that are difficult to resolve. They fail to explain why scientists make sense of and respond to market-oriented policies and practices the way they do and how their strategies, and the outcome of these strategies, are shaped by scientific and other forms of power.

This study seeks to address these knowledge gaps by exploring the following question: How does scientific power shape, and how is its distribution shaped by, the type of strategies of resistance or accommodation that scientists engage in? By focusing explicitly on the relationship between subjective meanings and objective positions, this study will begin to identify the reasons underlying particular position-takings and the implications for change in the structure of the scientific field. If the processes of knowledge production are changing as the Mode 2 thesis claims, it is likely that these changes are being driven by changes in the distribution of recognition and rewards in science which extend beyond those scientists who are market-involved (Owen-Smith 2003, 2005; Vallas and Kleinman 2008).

2.5. Conclusion

In this chapter, I have laid out the policy context for this study, explored dominant frameworks for understanding the impact of policy change on science, and reviewed the empirical research exploring the perceptions and practices of scientists in response to policy change. Canada, like many other nations, has adopted a policy discourse of promoting economic competitiveness in the global knowledge economy. Investments in academic research since the mid-1990s reflect
this discourse and tie research funding to the achievement of political and economic goals. Dominant frameworks for understanding the effects of broad political and economic change have failed to explain or provide a framework for empirically exploring what is actually happening in the system of science. Empirical studies point to the need for further research to explore changes in the culture and distribution of rewards in science. The following chapter describes the study’s theoretical framework guiding this exploration.
3.1. Introduction

In the science and technology studies (STS) literature that examines changes in the relationship between science, the market and the state, Bourdieu’s theoretical approach has played a minor role (for exceptions, see Albert 2003; Kinchy and Kleinman 2003). A more dominant approach has been the use of Thomas Gieryn’s (1983, 1999) concept of boundary work, the related concept of boundary objects (Star and Griesemer 1989), and, to a lesser degree, boundary organizations (Guston 1999, 2001). Boundary work has proven to be a useful conceptual tool for describing the symbolic distinctions that scientists make between “science” and “non-science” in their pursuit of credibility. In other fields outside of STS, boundary studies have also explored strategies to alter the social boundaries that produce unequal distributions of symbolic and material resources (Lamont and Molnar 2002; Pachucki, Pendergrass and Lamont 2007). However, the boundary work literature within and outside of STS has not, thus far, been very successful in explaining relationships between symbolic boundaries and social structures (Lamont and Molnar 2002).

In this study, I use Bourdieu’s theory of practice to explore the responses of basic scientists to a market vision of science which is embedded in market-oriented funding criteria. One aspect of Bourdieu’s theory of practice focuses on the symbolic strategies individuals use to defend or redraw symbolic boundaries around the legitimate vision of what is at stake in a particular social arena – in the case of science, the legitimate definition of “good science”. The second aspect of Bourdieu’s theory of practice focuses on the forms and distributions of power that structure, and are structured by, individual action. Taken separately, each of these aspects fits within a boundary work framework. However, boundary work studies have only alluded to the ways in which symbolic boundaries reinforce, normalize, reframe, contest or dissolve objectified social differences; as a result, they are unable to tell us why, and under what conditions, particular boundary strategies are adopted and are more or less effective (Albert, Laberge and Hodges 2009; Lamont and Molnar 2002; Kinchy and Kleinman 2003). In contrast, a Bourdieusian approach takes the relationship between systems of meaning and material distributions of power as the central object of study and may, therefore, be able to overcome...
this identified weakness. By analyzing the symbolic strategies that scientists used in relation to their objective positions of power, I hope to be able to explain why scientists adopted the strategies they did, and how these strategies reproduced or transformed the structure of distribution of power within the scientific field.

In this chapter, I first provide an overview of the concepts associated with the dominant framework of boundary work. I then introduce Bourdieu’s sociology and define field, capital and habitus, the core concepts of Bourdieu’s theory of social practice. From there, I move on to elaborate on the notion of “hysteresis” – the dissonance between individual’s subjective experiences and understandings of the social world (habitus) and the objective conditions of this world (field). Bourdieu’s (1984, 1990a, 2000) concept of hysteresis provides a useful conceptual tool for exploring the strategies of basic scientists who are confronted with a disruption in the definition of legitimate science and new criteria for attaining research funding. An underdeveloped area of Bourdieu’s work is, however, an understanding of how individuals respond at the level of collective identity and, at times, with conscious intent to changes in their social world. To strengthen this aspect of my theoretical framework, I draw on Jennifer Todd’s (2005) Bourdieusian-informed typology of collective identity change which I use to map the type and direction of change in habitus in response to changes in the field.

3.2. Boundary Work: The Dominant Framework

Boundary work studies in science and technology studies have focused on the strategies scientists use to construct symbolic boundaries around the legitimate vision of science and boundary crossing as scientists engage in more intensive relationships across the boundaries between science, the market and the state. Thomas Gieryn (1983, 1999) developed the term boundary work to describe the strategies scientists use to demarcate “science” from “non-science” in order to achieve professional goals. According to Gieryn, there are no essential characteristics of science but a multitude of different characteristics allowing scientists to present different depictions of what science is in order to achieve different professional goals. Scientists engage in boundary work to expand their own authority or expertise into new fields, to acquire a monopoly of expertise over a particular field, or to defend the autonomy of their work from political or economic intrusions. Science and technology boundary studies have explored how scientists use symbolic categories, such as “basic” and “applied” research
(Calvert 2004, 2006) or discourses of “purity” and “utility” (Kinchy and Kleinman 2003), to defend the autonomy of science from political and market forces, acquire resources, and gain credibility among different audiences.

Another approach to the study of boundaries has been to explore the stabilizing role of boundary objects (Star and Griesemer 1989) and boundary organizations (Guston 1999, 2001) in response to challenges to traditional boundaries between science, politics and the market. Unlike boundary strategies of demarcation which create walls between worlds, boundary objects and boundary organizations create bridges across different social worlds. Star and Griesemer (1989) developed the concept of the boundary object to understand how individuals from different social worlds collaborate in producing shared products that bridge the differences across social worlds by providing benefits to both worlds. Boundary objects are inscribed with multiple and potentially conflicting viewpoints from each world which makes them stable enough to share a common meaning across worlds and flexible enough to be interpreted and used differently in each world.

Boundary organizations are, according to Guston (2001), more stable and durable forms of boundary objects. They provide stability to individuals working across the boundaries between different social worlds when three criteria are met. They must: 1) involve the voluntary participation of actors on both sides of the boundary, as well as mediators; 2) have dual accountability to each world; and 3) provide opportunities and incentives for individuals to create and use boundary objects. When individuals with different visions of “reality” are brought together in new patterns of relationships, boundary objects and organizations can provide stability without requiring either group to change or concede its own vision.

The concepts of boundary objects and boundary organizations have been used in the science and technology studies literature to explore how scientific and non-scientific actors collaborate across previously impermeable boundaries. Guston (1999), for example, has identified technology transfer organizations (TTOs) as boundary organizations while Polyvas and Cowell (1996) describe them as “boundary-spanning units” (p. 312). TTOs enable collaboration between scientists and industry representatives on mutually beneficial projects while simultaneously meeting goals specific to their own social worlds. Guston (1999) found that
invention disclosures and patents, which were co-produced by scientists and technology transfer staff, functioned as boundary objects. In the business world, they were used as mechanisms of technology transfer and the development of profitable inventions. In the scientific world, they were used as indicators of scientific productivity in order to sustain public investments in research and TTO activities.

These studies provide a rich body of research about the strategies scientists to defend their authority and expertise while taking advantage of new opportunities arising from university-industry collaboration (for example, see Calvert 2006; Morris and Rip 2006; Murray forthcoming). However, they do not explore whether or how the reward system in science has been affected by expectations to produce value according to an economic vision of science. They have been unable to explain variations in boundary strategies, such as why some scientists use strategies of demarcation to rebuild the boundaries between science and the market while others use boundary organizations to support them in boundary crossing (Albert et al. 2009; Kinchy and Kleinman 2003; Lamont and Molnar 2002; Pachucki et al. 2007). In a recent review of the boundary literature, Lamont and Molnar (2002) make a conceptual distinction between symbolic and social boundaries, where symbolic boundaries are the “conceptual distinctions made by social actors to categorize objects, people, practices” and social boundaries are the “objectified forms of social differences manifested in unequal access to and unequal distribution of resources (material and nonmaterial) and social opportunities” (p. 169). While symbolic boundaries separate people into social groups, social boundaries pattern interactions between groups by constraining and enabling access of different groups to different forms of resources. Symbolic boundaries may uphold and normalize or contest and reframe social boundaries. The authors of this review argue that the most important theoretical advancement of boundary work will come from research that explicitly examines the configurations between symbolic and social boundaries. In this dissertation, I use the theoretical concepts developed by Bourdieu to conceptualize and explore how scientific practice is shaped by, and shapes, the structure of the scientific field. Because it takes the relationships between subjective systems of meaning and objective positions of power as the direct object of study, Bourdieu’s theory may be able to provide a way through the theoretical impasse that has been identified in the boundary work literature.
3.3. Bourdieu’s Central Concepts: Habitus, Capital, Field

It could be said that the central project of Bourdieu’s work has been to develop an epistemological approach to an understanding of the social world works that overcomes the objectivism of structuralism and the subjectivism of phenomenology (Jenkins 1992). He developed his unique epistemological approach out of frustration with these two dominant sociological and anthropological traditions of his time. In the subjectivist tradition, subjects’ accounts of their lives and their lived experience are taken to reveal the truth of their social world. In the objectivist tradition, sociologists and anthropologists seek to understand social life through the rules, norms and structures that organize social life. In Bourdieu’s own research experience, the truth of the social world could not be found solely in subjects’ accounts nor could it be found solely in the structures governing social life. While subjects have a practical knowledge of how their social world works, they also accept their world as natural or self-evident; they do not know the conditions that make it possible for the world to exist as it does. Therefore, the accounts that research subjects produce of their interactions can themselves only reveal a part of the truth (Bourdieu 1989). Yet it is through these interactions that the social structure is produced. Abstract academic theories of social rules and norms that do not consider how these are interpreted, acted upon, challenged and reproduced by individuals can also only produce partial knowledge of the social world. Bourdieu did not discard either of these traditions but rescued their strengths by linking them together in a new epistemological approach that overcomes the objectivist / subjectivist divide. This approach is fundamentally relational, in that social structure and individual behaviour are seen as mutually constitutive, and can only be understood in relationship to one another. Bourdieu’s theory of practice brings together the subjective and objective to explain how the social world is constructed and reproduced.

Bourdieu frequently used the metaphor of “the game” to illustrate social behavior. People play games, such as soccer or chess, within a bounded social space, with clearly agreed upon rules, yet with unlimited opportunities to strategize and improvise while playing. The act of playing itself does not allow for conscious reflection during play, but demands spontaneous action and reaction based on an embodied competence, or know-how, that is acquired through practice and repetition. To succeed in playing their particular position, players must be able to anticipate the
probable success of different moves and react instantaneously during the dynamic unfolding of the game. Through this metaphor, it is apparent that practice is constrained by the rules of the game and the differentials level of competence of the players, and is also free to unfold in unpredictable directions with unforeseen outcomes. Practice is generated through the interplay between habitus and the social field, or social context in which it is enacted and mediated by the type and volume of power – capital – individuals hold as resources for action. These core concepts of Bourdieu’s theory of practice are elaborated below.

3.3.1. Field and Capital
Bourdieu conceptualized the social world as composed of multiple social fields in which participants compete for the acquisition of specific resources (capital), and for power to define the principles on which these resources are distributed (Bourdieu 1975). Based on his empirical work in a number of different types of social fields – for example, art, education, media (Bourdieu and Passeron 1990; Bourdieu 1996a, 1998) – Bourdieu identified properties that are common to all social fields in advanced, highly differentiated, complex societies, as well as properties that are unique to each field. Fields exist in relationships of relative autonomy and dependence on one another and are structured according to two opposing “principles of differentiation”: the possession of economic or cultural capital (Bourdieu 1996b:5). Economic and cultural capital can take an infinite number of forms. While each field is dominated by one or the other of these two forms of capital, each field also produces its own unique forms of capital.

Bourdieu identified four types of capital that function as resources and forms of power: economic, social, cultural and symbolic (Bourdieu 1984, 1986, 1989). Economic capital is the capital that corresponds to material wealth. Cultural capital includes educational or intellectual resources associated with class membership and may include other cultural resources associated with art, music or literature which convey social status. Social capital refers to social networks and group memberships that enable members to gain access to other forms of capital. Symbolic capital is the form of capital taken by all other forms when they are perceived as socially legitimate and confer an authority on its holders. In a field where economic capital is dominant, for example, it will also act as a form of symbolic capital, giving social legitimacy to those who
possess material wealth and providing the justification for the distribution of wealth that makes it appear normal and inevitable.

While social field tends to be dominated by one of these dominant forms of capital, each field has a unique logic that differentiates it from other social fields and produces its own specific forms of capital. The ability of members of a particular field to establish their own unique vision of what practices are legitimate and what products have value depends on their degree of autonomy from other fields, and in particular, from the field of power. In The State Nobility (1996b), Bourdieu describes the field of power as the field where the economically and culturally elite across many fields struggle for the dominance of their own preferred form of capital or, in other words, for the “dominant principle of domination” (Bourdieu 1984:125). Economic capital is the dominant form of capital in the field of power, but the economically powerful also depend on cultural capital to legitimize social and material divisions. In the past, the church performed this legitimizing function but in modern secular societies education has taken over this role and functions as the dominant form of cultural capital. In order to provide a legitimizing function, a relative degree of autonomy between cultural power and economic is required. It is this necessity for relative autonomy that has given the academic field a seemingly high level of autonomy from the field of power. Academic science has been able to determine what is considered legitimate science and the processes for evaluating and awarding scientific success (Bourdieu 1990b).

While fields are located in relation to one another according to their degree of autonomy, individuals and groups are located within fields according to the types and total volume of capital that they hold. It is these patterns of distribution of capital that give each field its structure. To grasp the structure of the field, and how it is reproduced or transformed, we must first grasp the positions of individuals within the field and their social trajectories. In Distinction (1984), Bourdieu describes three dimensions of social space. The position individuals occupy depends on their total volume of capital, the relative weight of different forms of capital (their asset structure), and the evolution of the volume and composition of capital over time (trajectory). Upward mobility in the status structure may be achieved through the accumulation of capital or the conversion of capital from a lesser form into a more valued form, either moving vertically within a field or horizontally between fields. Bourdieu describes
how farmers, small shopkeepers and craftsman who found their small businesses in decline reconverted their economic capital into the cultural capital of teachers, technicians and health care personnel (p. 137).

The accumulation of capital requires an investment of time and labour. The volume and type of capital individuals accumulate relative to other members of the field structures their objective chances of success, constraining and enabling particular strategies. As individuals seek to accumulate, reconvert, or raise the value of their capital, they engage in symbolic struggle over what is “at stake” in the field. In this struggle, they seek to impose their own vision of the world, their own system of classifying social difference, and their own legitimizing rationale for this system of distributing symbolic and material power. Through strategies of conservation or subversion they seek to preserve or transform the forms and value of capital and the “exchange rate” between these different forms (Bourdieu 1984:125). The accumulated actions of individuals and groups engaged in ongoing struggle lead to the reproduction or transformation of the structure of the field. Most often this struggle results in reproduction rather than transformation due to the advantage held by those who possess higher volumes of capital or capital of a higher value.

Capital, which, in its objectified or embodied forms, takes time to accumulate and which, as a potential capacity to produce profits and to reproduce itself in identical or expanded form, contains a tendency to persist in its being, is a force inscribed in the objectivity of things so that everything is not equally possible or impossible. And the structure of the distribution of the different types and subtypes of capital at a given moment in time represents the immanent structure of the social world, i.e., the set of constraints, inscribed in the very reality of that world, which govern its functioning in a durable way, determining the chances of success for practices (Bourdieu 1986:241).

Change in the form, value or exchange rate between forms of capital, opens up the possibilities for new strategies of conservation (reproduction) or subversion (transformation).

Any change in either the instruments of reproduction or the state of the capital to be reproduced therefore leads to a restructuring of the system of reproduction strategies. The reconstitution of capital held in one form to another more accessible, more profitable or more legitimate form tends to induce a transformation of the asset structure (Bourdieu 1984:125-6).

Yet changes in the forms and value of capital do not necessarily lead to changes in the system of stratification. The overall volume of capital an individual holds may remain the same even as the asset structure – e.g., the ratio of economic to cultural capital – changes. Symbolic struggles
to transform the forms and value of different types of capital are not necessarily struggles to transform the distribution or resources and recognition, but may be strategies of conservation aimed at “transforming so as to conserve” (Bourdieu 1984:156). In the permanent struggle for status described by Bourdieu in *Distinction*, the strategies of individuals who occupy lower positions in the social hierarchy to profit from new forms of capital are most often cancelled out by the strategies of individuals who occupy higher positions. In this perpetual struggle, there is ongoing change in the substance of capital even as relational differences in the distribution of capital are conserved (Bourdieu 1984:163). In this process of social reproduction “permanence can be ensured by change and structure perpetuated by movement” (Bourdieu 1984:163). As I explore later in this chapter, transformation is more likely to occur when hysteresis occurs. First, to understand how these actions are generated in relation to the structures of the field, I turn to the concept of habitus.

### 3.3.2. Habitus

Habitus provides a conceptual tool for understanding how individuals act to reproduce social divisions and classifications, even when these divisions are not in their own interest. Habitus is both the embodiment of the classificatory schemes embedded in social structures at the time of its formation, and generative of new classificatory schemes and practices; it is differentiated, but also differentiating; structured but also structuring (Bourdieu 1990a). I draw on the work of David Swartz (2002) to describe these “two faces of habitus” (p.635). According to Swartz, one “face” of habitus (its structured dimension) is the internalization of dispositions, both cognitive and corporeal, that give agents a sense of their “place” in the social world and a competence to recognize and act in accordance with that “place”. Habitus is internalized primarily through childhood socialization but may change over one’s life, and new dispositions may be layered onto the primary habitus (through immersion in a career, for example). Different forms of capital are embodied in habits of “taste”, ways of walking, talking, thinking and being. Individuals who occupy similar positions in the social structure are, therefore, also likely to have similar dispositions as their perceptions and categories of thinking have been structured by similar social conditions.
Habitus gives individuals a sense of her or his position within the field and provides a practical sense or practical knowledge, largely below the level of conscious thought, of how to “play the game”. It gives rise to particular expectations and probable trajectories in the social world.

The habitus…adjusts itself to a probable future which it anticipates and helps to bring about because it reads it directly in the present of the presumed world, the only one it can ever know. It is thus…a realistic relation to what is possible, founded on and therefore limited by power. This disposition, always marked by its (social) conditions of acquisition and realization, tends to adjust to the objective chances of satisfying need or desire, inclining agents to ‘cut their coats according to their cloth’, and so to become the accomplices of the processes that tend to make the probable a reality (Bourdieu 1990a:64-65).

This leads to strategies that are “reasonable without being the product of a reasoned design… intelligible and coherent without springing from an intention of coherence and a deliberate decision” (Bourdieu 1990a:50-51).

The second “face” of habitus (its structuring element) is its generative capacity to produce new systems of classification and new forms of action, as past conditions of habitus meet new conditions in the social world. While habitus tends to produce actions that reproduce the social structure of its formation, under some circumstances, it produces new actions that may lead to radical transformation. Habitus is not solely determined by past social structures, as action plays out in interaction with social fields which offer infinite opportunities for improvisation, as members jostle and jockey for position.

…practices cannot be deduced either from the present conditions which may seem to have provoked them or from the past conditions which have produced the habitus, the durable principle of their production. They can therefore only be accounted for by the social conditions in which the habitus that generated them was constituted, to the social conditions in which it is implemented (Bourdieu 1990a:56).

It is through the interrelationship between fields, capital and habitus that we can understand how social structures are reproduced and transformed. The field creates the present context for action and both enables and constrains practice, while capital functions as a capacity or resource for action (Swartz 2002). Habitus – mediated by capital – generates action in relation to fields, embodying past structures which enable and constrain present action.

Bourdieu’s concepts of habitus, capital and field are often applied to explain the reproduction of social structures and the participation of dominated groups in reproducing the conditions of
their own domination. They have less often been used to explain social transformation. In the following section I will explore “hysteresis” – the mismatch between habitus and field – as a key concept for understanding social transformation in the scientific field.

### 3.4. Hysteresis and Collective Identity Change

The reproduction of social structures and social categories, according to Bourdieu (1990a), is the result of the unconscious ongoing harmonization that occurs between habitus and field. When the conditions of formation of the habitus and the structures of the field are in harmony, individuals and groups play their roles in reproducing social structure without conscious awareness even when they reproduce the conditions of their own domination. However, as social fields evolve, either through their internal dynamics or in interaction with other social fields, mismatches between habitus and field can lead to unexpected changes in practice and the structure of the field. The time lag that occurs between change in social conditions and changes in the categories of perception of these conditions is what Bourdieu called the “hysteresis of habitus” (Bourdieu 1990a:59). This mismatch is usually resolved over time, as agents’ habitus eventually adjust to the new conditions of the field, reassert the former conditions, or withdraw from the field. Before this resolution occurs, there is a suspension of the ordinary rules of the game.

Hysteresis is most clearly illustrated in Bourdieu’s (1984) description of the crisis of faith that occurred in the educational system in France in *Distinction*. The mass entry of formerly excluded lower and middle classes into higher education led to the devaluation of educational credentials. Members of the middle and upper middle classes who expected their educational investments to yield certain occupational results found themselves squeezed out of the job market, leading to disillusionment and questioning of the logic of the educational system. Similarly, in his study of the conflict between academic faculty during the riots of May 1968 in France, Bourdieu (1990b) found that changes in academic recruitment criteria disrupted the “order of succession” in academic careers. This disrupted the “probable trajectories” of academics who conformed to the old order but failed to reap the rewards of their intense educational investments, while those conforming to the new order were able to “seize the opportunity created by the critical break in the ordinary order, to advance their own claims or defend their interests” (Bourdieu 1990b:175).
When social categories or structures are disrupted, the possibility for change in unexpected directions emerges.

Everything suggests that an abrupt slump in objective chances relative to subjective aspirations is likely to produce a break in the tacit acceptance which the dominated classes – now abruptly excluded from the race, objectively and subjectively – previously granted to the dominant goals, and so to make possible a genuine inversion of the table of values (Bourdieu 1984:168).

While habitus can shift and adapt to the slow evolution of the social field, it usually lacks the capacity to adapt to more radical change (Bourdieu 2000). In situations of sudden change in social categories or structures, the dissonance between habitus and the social field can create a crisis for members of the field. When following taken-for-granted rules ceases to produce the expected outcomes, what was perceived as the natural order of things may be revealed as arbitrary and undesirable, creating a “margin of freedom” during which individuals or groups struggle to bring about or prevent alternative futures (Bourdieu 2000:234). Under these circumstances, individuals may adopt non-routine strategies to take advantage of new opportunities or minimize potential losses that changes in the field present.

Bourdieu, however, tells us little about how symbolic categories and practices change in novel and unexpected ways. This gap may result from what some have called Bourdieu’s overemphasis on deterministic aspects of habitus and underemphasis on the possibilities for intentionality in social change (Jenkins 1992; King 2000). While Bourdieu acknowledges the possibility of lucidity and conscious choice during periods of disruption, he provides little theoretical guidance for exploring how these conscious practices emerge alongside and in contrast to routine, unconscious practices. I draw on Jennifer Todd’s (2005) framework on collective identity change to augment this gap.

Although Todd (2005) does not explicitly refer to Bourdieu’s concept of hysteresis, it is implicit throughout her framework, in which dissonance between habitus and field is seen as one of the core sources of social transformation. Todd has explored both gradual and sudden change in collective identity patterns in response to political change using Bourdieu’s concepts of habitus, capital and field. She developed a conceptual framework for exploring the relationship between changes in collective identity, at the micro-level, and macro-level changes in political and economic policies and programs. Todd situates her analysis of collective
(social) identity within a Bourdieusian framework, distinguishing between the unconscious properties of the habitus and the more conscious and adaptable elements of social identity. Collective identity, in a Bourdieusian sense, is not a personal or psychological trait, but a social location, or one’s “social being” (Bourdieu 2000:237). Like habitus, it is both an outcome of practice – the ongoing adaptation between habitus and field – and a compass that guides action. Todd conceptualizes collective identity as a component of habitus from which individuals can continuously arrange and rearrange how they make sense of themselves in relation to the social world. Identity is always in a process of construction and reconstruction that involves some degree of self-awareness and intentionality. New meanings and practices may emerge in response to change in the social world, but only to the extent that the building blocks already exist within the habitus.

Identity formation and change is thus a continuous process that involves a considerable degree of intentionality. It takes place by the incorporation of new elements of embodied meaning and value, or the rearrangement of old. New elements may be created, not ex nihilo but by the choice to foreground particular practices and relations rather than others, so that over time the meanings embedded in these practices become an integral part of the self, while others fade (Todd 2005:437).

Todd also distinguishes between change in identity category and change in the meaning of identity. For example, in her research on ethnic identity in Ireland following the Good Friday Agreement in 1998, the binary opposition between being Irish or being English was retained, for some, while the meaning of being Irish shifted to a less oppositional identity in which some aspects of “Britishness” were embraced. Identity change is uneven, depending on the symbolic and material resources individuals or groups possess or can access.

Todd argues that tendencies towards collective identity change are patterned according to the availability of symbolic and material resources and the “logically possible responses, at the level of identity, to socio-political change” (p. 432). She identifies six logically possible responses: reaffirmation, assimilation, conversion, adaptation, ritual appropriation and privatization (Table 4). These responses vary in the degree and direction of identity change: the degree of change ranges from none and partial to total change, while the direction of change is either towards coherence and transparency or towards ambiguity and tension between identity and practice.
Table 4. Direction and Degree of Change in Collective Identity Categories

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<th>No change</th>
<th>Partial change</th>
<th>Total change</th>
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<tbody>
<tr>
<td>Transparence and coherence between practice and category</td>
<td>Reaffirmation</td>
<td>Assimilation</td>
<td>Conversion</td>
</tr>
<tr>
<td>Ambiguity and tension between practice and category</td>
<td>Adaptation</td>
<td>Ritual appropriation</td>
<td>Privatization</td>
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Reaffirmation of identity occurs when neither the available categories of identity, nor the practices associated with these categories, are challenged. While the categories themselves are retained, as well as the coherence between identity and practice, new experiences of “winning” or “losing” may result in changes in the meaning ascribed to these categories. Similarly, adaptation to new social conditions can occur without a change in identity. Unlike reaffirmation, adaptation usually leads to tensions between identity and practice. Partial identity change occurs when agents assimilate to the new social order by bringing new elements of their identity to the foreground and adopting new identities and practices. When assimilation occurs, agents retain a sense of coherence between their identity and practice. A collective form of assimilation, which Todd calls ritual appropriation, occurs when the symbolic content of traditional categories is retained, ritually, while the practical significance of that content is replaced. For example, in Great Britain the royal family continues to play a symbolic role but not a practical role in ruling. Conversion and privatization lead to total identity change, when the former categories of identity are no longer considered relevant within the new social conditions. This may occur after a long period of increasing dissonance between the social conditions and practice; the practices and identities associated with the old order are no longer sustainable, either practically or ideologically. Conversion occurs when a new set of identity categories are adopted and the tension between identity and practice is resolved. Privatization is the complete loss of identity that occurs when agents cannot accept the new categories and withdraw from the field. In the following section, I apply Bourdieu’s concepts to the scientific
field and illustrate how these concepts and Todd’s classifications can help us to understand the responses of basic scientists to market-oriented science policy.

3.5. Application to the Scientific Field

In two key works, *Homo Academicus* and the *Science of Science and Reflexivity*, Bourdieu (1990b; 2004) applied and enhanced his concepts of field, capital and habitus in the study of science. As described in *Homo Academicus*, academic science has traditionally had a high level of autonomy from the field of power. The basic or natural sciences have been structured around cultural capital, and in particular the symbolic power of scientific legitimacy, or scientific capital. As a power of recognition, scientific capital can only function among agents who possess the categories of perception necessary to know it and recognize it (Bourdieu 1991, 2004). Recognition of good science and new potentialities for advancing science depends on the ability of scientists to perceive and appreciate distinctions in academic practice and products; it is achieved and bestowed through the habitus. New scientists acquire a scientific habitus through extensive socialization and training under the mentorship of senior scientists. Through this training, the perceptual categories of recognition of good science are internalized and embodied in the scientific habitus which gives them a practical sense of how to “play the game” (Bourdieu 2004). Bourdieu describes the “craft” of the scientist as something that cannot be easily articulated or understood. Unlike other “crafts” or practices, such as art or sports, scientific practice is based on the accumulation of theoretical constructs and instruments and the embodiment or incorporation of these in the scientific habitus: “the scientific habitus is a realized, embodied theory” (Bourdieu 2004:40).

Scientists are permanently engaged in a struggle, both social and scientific, for the accumulation of scientific capital and the power to define what counts as legitimate science. What scientists consider to be a legitimate scientific topic, tool, technique, interpretation or result is perpetually contested, as individuals try to change the meaning of legitimate science to bring it more in line of what they are capable of achieving (Bourdieu 1991). The strategies scientists use depend on both their dispositions, which enable them to recognize potential opportunities for advancement, and their objective chance of success based on the position they occupy. The playing out of these strategies, in turn, determines the structure (distribution of power) of the field.
There is no scientific choice – choice of area of research, choice of methods, choice of a publication outlet...that does not constitute, in one or other of its aspects, a social strategy of investment aimed at maximizing the specific profit, inseparably political and scientific, provided by the field, and that could not be understood as a product of the relation between a position in the field and the dispositions (habitus) of its occupant (Bourdieu 1991: 9-10).

Within the traditional symbolic order, objectified forms of scientific capital include peer-reviewed publications, tenure, promotion and various scientific awards (Bourdieu 1990b). While scientists may deploy any number of strategies to accumulate scientific capital, and convert one form of scientific capital to another (e.g., publications into tenure), these strategies conform to a scientific logic, as opposed to, for example, an artistic, theological, political or economic logic.

Before the rise of market-oriented science policy, the system of peer-review used to evaluate scientific quality by funding organizations was, like the publication system, based on a scientific logic (Rip 1994). The dominant form of scientific capital – publications – could be easily converted into another form of scientific capital – grants – which was then reinvested in the accumulation of more scientific capital. Changes in science policy and funding criteria in the mid-1990s introduced a new logic and a new vision of legitimate science based on the criteria of utility and economic relevance (Cameron 2004; Fisher et al. 2005; Wolfe 2002). Symbolically, an economic vision of science challenges the traditional symbolic order. It redefines scientific value according to an economic model, holding science accountable to political and economic goals of national economic competitiveness. Structurally, market-oriented funding introduces new funding criteria that reward new research practices, such as industry-collaboration, and research products, such as patents.

From a Bourdieusian perspective, we would expect a change in the meaning and criteria to evaluate legitimate science in the political field to impact scientific practice if it: 1) alters the rules of reproduction of the dominant forms of capital; 2) produces new forms of capital; or 3) alters the value of capital (Bourdieu 1984). Change in the form and value of capital provides possibilities for new strategies of reproduction (conservation) or transformation (subversion). Todd’s framework identifies six possible responses to science policy change that include acting with, acting against, and acting both with and against a market-oriented vision of science. This
framework is used to describe and explain processes of identity (re)construction and how these contribute to the structure of the field.

3.6. Conclusion

In this chapter, I have focused on how a Bourdieusian framework can be used to understand changes in the relationship between science and the field of power, the breakdown between the expectations of scientists trained within the traditional symbolic order and new conditions resulting from the rise of the global knowledge-based economy, and the processes of collective identity change and change in practice that are reproducing or transforming the structure of the scientific field. If, through the imposition of an economic vision of science, the definition of scientific quality is changing, we would expect this to manifest itself in changes in the form or value of capital and new scientific identities and practices. A Bourdieusian framework focuses the researcher’s attention on changes in the definition of scientific value – changes in the symbolic order – and changes in the distribution of scientific and other forms capital – the material or structural order. It also draws the researcher to examine the relationships between the actions and strategies of individuals and broad structural forces, where social action is understood as both structured and generative through the interactions between habitus and field.

Using Bourdieu’s concepts of habitus, capital and field, and Todd’s categories of collective identity change, this study explores the perceptions and responses of scientists towards market-oriented science policy. It investigates how scientists’ perceptions and practices are shaped by the interactions between their dispositions – their accumulated embodied capital, categories of perception and expectations of success based on past structures of the field – and their positions in the field – their accumulated objectified forms of capital. If changes in funding criteria have altered the forms or value of capital, we would expect scientists trained in the pre-market-oriented science era to experience hysteresis – dissonance between their expectations and objective changes of successfully acquiring research funding and/or recognition. For some, this dissonance may induce changes in perceptions and practices in order to take advantage of new opportunities provided by market-oriented funding, while others may experience new conditions as a threat or constraint. If changes in capital have become institutionalized, we would expect to see manifestations of a new scientific disposition among scientists trained in the market-oriented science policy era. The responses of scientists to new field conditions will,
in turn, contribute to the construction of the dominant symbolic order. Based on this ontological understanding of the social world, in which social structure and individual behaviour are understood as mutually constitute, this study:

1. Identifies scientists’ perceptions of the types of capital that functioned as scientific recognition and as resources for the pursuit of recognition;
2. Maps the structural location of scientists according to the type and volume of capital held;
3. Explores the position-takings of scientists towards market-oriented funding criteria and their strategies to acquire scientific recognition and resources; and

In the following chapter, I describe how this relational ontology translates into a relational methodology.
Chapter 4
Constructing the Research Object – A Relational Methodology

4.1. Introduction
The most crucial act of research is, according to Bourdieu, the construction of the research object (Bourdieu and Wacquant 1992). It begins with the research design and continues throughout the research process. The construction of the object requires the researcher to make a series of “epistemological breaks” with the preconstructions of common sense, with scholastic constructions, and with the researcher’s own viewpoint. What these epistemological breaks accomplish for the researcher is a paradigm shift, or what Bourdieu called “a metanoia, a mental revolution, a transformation of one’s whole vision of the social world” (Bourdieu and Wacquant 1992:251). This metanoia is a conversion towards a relational epistemology.

There is a vast amount of research taking up Bourdieu’s theoretical concepts themselves, but much less attention has been given to a relational methodology of doing research. Emirbayer and Johnson (2008) describe how researchers can think and practice relationally by focusing on both the space of positions and the space of position-takings:

The first implication of this mandate is that researchers should attend both to the objective indicators of positions…and to the indicators of position-takings…The ultimate aim of such a dual approach is synthesis of these two spaces – those of positions and of position-takings – into a map of the field or fields in question (p. 33).

These two spaces are constructed during the study in a back and forth process of gathering data and testing theory. The initial design of this study was based on theoretically-informed ideas about who the members of a shared field would be and what kinds of indicators of power might function in the field. The study design reflects the study’s theoretical framework and seeks to generate an account that can make sense of both the structures of the scientific field that constrain or enable scientific practice – the space of positions – and the strategies of scientists that reproduce or transform these structures – the space of position-takings. The strategies themselves are understood to emerge from the scientists’ habitus which embody past structures, creating the potential for harmony or dissonance between habitus and field. Attention to the co-constitution of structure and meaning infused each stage of the study design and data analysis.

I used a case study design as this is a particularly useful approach for studying the interactions between habitus and field (Grenfell and James 1998). The case study allows for the study of the
habitus of particular individuals who occupy particular positions within a hierarchically structured field. The case study can also produce general understandings that have relevance beyond the unique situation observed: “a particular case that is well-constructed ceases to be particular” (Bourdieu and Wacquant 1992:57). In this chapter, I will describe how the case was constructed, the sources and types of data that were collected, strategies for data analysis and reflexivity as a strategy to ensure rigour.

4.2. Constructing the Case

The case was defined to include a group of basic scientists who were members of a shared scientific community, and who were engaged in biotechnology-related research, at a single research-intensive university. A research-intensive university was selected as an appropriate setting as participants in this type of setting are more likely to be impacted by research funding criteria. This university was also located in close proximity to a cluster of biotechnology companies, which provided academic scientists with opportunities for industry collaboration and commercialization of academic research. Biotechnology-related research was selected as it is one of the core targets of federal and provincial policies which seek to increase the use academic research in its broader goals of increasing national economic competitiveness (BCO 2002; Industry Canada 2001). Scientists in this location conducting this type of research were likely to be subject to significant external pressures and opportunities for commercialization of their research and would likely have had to articulate a response to funding criteria pushing them towards market involvement.

Inclusion in the study was restricted to basic scientists involved in the discovery stages of research. Discovery-oriented research seeks to uncover fundamental mechanisms and processes that advance basic understanding of how things work (e.g., biologically, biochemically, genetically). Clinical and applied scientists, in contrast, seek to develop knowledge that will directly influence or change some aspect of human life. For example, basic research may discover that a particular gene is related to a type of cancer. This gene becomes a drug target. Potential treatments are then developed, through applied research, and their efficiency and effectiveness are tested in clinical trials. Unlike clinicians and applied scientists, basic scientists have not traditionally been involved in the production of the kind of directly useful, applicable knowledge that market-oriented science policy promotes. I anticipated that basic scientists
engaged in discovery-oriented science would experience greater incoherence between their scientific values and market-oriented funding criteria than clinician scientists and scientists working in applied research. I therefore saw the basic sciences as a richer area to explore symbolic struggles over the legitimate vision of "good science" and the impact of funding criteria on the logic and structure of the field.

**Sample and Recruitment**

Within the boundaries of the case, I sought the perspectives of two distinctly different groups of scientists: established scientists trained in the pre-market-oriented science era and early stage scientists trained during the market-oriented policy era. Based on the theoretical framework, perceptions of market-oriented funding were taken to embody structures of the scientific habitus which are laid down during the prolonged period of scientific training. Differences in perceptions and strategies of these two groups might therefore reveal differences in the forms and value of capital between these two policy eras. The first federal policy changes to promote the commercialization of basic research dated back to the early 1990s with the formation of the Networks of Centres of Excellence (NCE) and steadily rose to the top of the science policy agenda by the late 1990s. In the late 1990s and early 2000s a number of changes were made to the leading government funding agencies and a variety of new initiatives were introduced based on the new policy direction (Snowdon 2005; Wolfe 2002). To create a clear demarcation between these two policy eras, 1992 was used as the cut-off point for the end of the pre-market-oriented policy era and 1999 as the cut-off point for the beginning of the market-oriented policy era. Established scientists began their academic careers in 1992 or earlier (fifteen or more years’ experience), before the NCE program was fully established. Early stage scientists began their academic career in 1999 or later, after the restructuring of the Medical Research Council into the Canadian Institutes for Health Research and the establishment of the Canadian Foundation for Innovation and Genome Canada. Most early stage scientists were pre-tenure, although tenure was not a criterion for exclusion.

A theoretical sampling strategy was used to generate a sample that had been exposed to two different policy regimes. Publicly available data from the university website were used to identify potential study participants. Scientists carrying out basic research with potential links to biotechnology were identified within the Faculty of Medicine, Faculty of Dentistry, Faculty
of Arts and Science, and Faculty of Pharmacy. In March 2007, a recruitment email with a study description (see Appendix A) was sent out to all members of two departments within the Faculty of Medicine (n=156) and the Faculty of Arts and Science (n=159). Twenty scientists emailed back their willingness to participate. I followed up with them by phone to ensure they met inclusion criteria and to set up an interview time. Between March and August 2007, I interviewed seventeen participants, 11 established scientists and 6 early stage scientists. After analyzing the first seventeen interviews, it became apparent that there was insufficient representation of scientists in the early career stage. In the spring of 2008, I recruited and interviewed an additional 3 early stage scientists from the Faculty of Medicine. The same recruitment strategy was used as in the first round of recruitment with the additional criterion that only scientists who began their academic position after 1999 were eligible to participate. In total, the study included 20 participants.

4.3. Data Generation
The data for this study were generated from semi-structured interviews and two types of documents: curriculum vitae and a one-page Background Information Form (see Appendix D). Participants were assured confidentiality and signed a consent form to participate at the time of the interview (see Appendix B).

4.3.1. Interviews
Semi-structured interviews were used to gather data on participant perceptions and stances towards market-oriented science and their strategies to acquire funding. The interview guide included three broad questions: 1) how would you define scientific excellence; 2) what market-oriented criteria have you encountered from funding organizations; and 3) what is the impact of market-oriented funding criteria on your scientific practice and the quality of science overall (see Appendix C). Before each interview, I reviewed the participant’s CV and used this information to tailor questions about their funding experience to the specific types of funding they held. Interviews ranged in length from 45 minutes to 2.5 hours. They were held at the participants’ location of choice which was most often their office located in or near their lab. This gave me an opportunity to view the participants’ labs which provided a sense of the size, number of staff, and equipment used. I also recorded field notes after each interview which contained immediate impressions of the interview.
All but one participant consented to have the interview recorded. This early stage participant chose to be interviewed outside the building on a park bench without a recorder. I took notes during the interview and detailed notes immediately after the interview to capture as much of the conversation as possible. This participant was initially hesitant to participate in the study due to the fact that he lacked any experience or interest in collaborating with industry or generating intellectual property. I assured him that this perspective was valid and important to my study. His reluctance to be audio-recorded may have reflected his discomfort speaking out against what has become a taken-for-granted discourse among scientists who began their career in the market-oriented science policy era (see Chapter 6). Nineteen recorded interviews were transcribed verbatim by a professional transcriber according to guidelines I developed for this study; identifying information, including proper names of people and departments or companies, was removed during transcription.

4.3.2. Documents
Participants emailed me their curriculum vitae before the interview and completed Background Information Forms during the interview. Curriculum vitae were gathered to generate data on potential indicators of capital scientists held. Data on academic rank, awards, grants and publications were gathered as potential indicators of scientific capital. CV data were augmented with data collected during interviews from a short Background Information Form (see Appendix D). The form was organized in three sections, academic background, commercial background and personal background, and was intended, in conjunction with interview and CV data, to serve a number of purposes. The academic background section was used to gather information about career length, discipline and volume of scientific capital. It provided data that were not always included on the CV such as the date the PhD degree was awarded, tenure status, the number of staff and students employed in the lab, the number of students supervised, and, in the opinion of the respondents, the most prestigious awards and journals. This is reported in the data analysis section. The commercial background section was used to identify the types of entrepreneurial practices the participant was involved in. Participant CVs inconsistently reported data on patents, industry funding and positions held in private companies. The Background Information Form ensured that these types of data were systematically collected.
The third section on personal background was intended to identify the participant’s class origins. This section asked participants to report their date of birth and father and mother’s occupation and level of education. Based on the theoretical framework, I anticipated that class origin might be transfigured into a particular scientific habitus and particular stances towards market-oriented research funding. However, I had difficulties in collecting the data and responding to the surprise and discomfort some participants expressed when completing the form. Some occupations, such as “businessman” did not provide a clear indication of class origin. On more than one form, “retired” was reported as the occupation. When questioned, I justified the question in terms of it providing “general demographic information” but felt uncomfortable with this explanation. Due to gaps in the data, I dropped this line of inquiry and did not use data from this segment of the form.

4.4. Data Management and Analysis

The initial forms of data generated included interview transcripts, field notes, CVs and Background Information Forms. During the data analysis process, I generated new forms of analyzed data, including interview summaries, CV and Background Information Form summaries, analytic memos, chunks of coded data, visual maps of concepts, and tables of indicators of capital. I used Microsoft Word, Excel and QSR NVIVO to manage data and aid data analysis. QSR NVIVO was helpful in the early stages of the study to organize and retrieve data according to largely descriptive categories. The bulk of analysis was carried out through extensive writing in Microsoft Word as I tested, compared, and elaborated upon ideas.

Throughout the study, I used several data analysis strategies that are common to many qualitative research studies, including data reduction, constant comparison, exploring contradictions, and testing rival explanations (Miles and Huberman 1994; Seale 2004; Yin 2003). In the initial stages of analysis, I immersed myself in the data and carried out basic descriptive analysis (Sandelowski 2000). I used several data reduction strategies including interview summaries, interview coding, and extraction of data from CVs and Background Information Forms (Miles and Huberman 1994). I extracted potential indicators of capital from...

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5 Scientists with a lower class habitus, who selected a career in the basic sciences because it is remote from the market, and who have achieved upward class mobility from this career choice, may be less inclined to accept a shift towards the market compared to those with higher class origins (Bourdieu 1990b).
CVs and Background Information Forms and entered into a standardized template for each participant (Appendix E). I developed excel spreadsheets to summarize and display the data.

The categories I used to reduce interview data during this stage were derived from the study questions and theoretical framework. For example, the headings used to organize interview summaries followed directly from the interview guide: Funds Held, Criteria Encountered, Good Science, and Impact of Funding on Science. Subheadings were used for data that did not clearly fall into these categories but seemed potentially important. For example, in several interviews Career Satisfaction was a subheading of Impact of Funding on Science. Although the study did not set out to explore scientists’ career satisfaction, several participants reported that it was affected by the funding context. During this process of organizing and immersing myself in the data, I began the work of constructing the space of positions and the space of position-takings.

4.4.1. Constructing the Space of Positions

In order to map the space of positions, I needed to first identify the forms of capital that participants perceived as resources for the pursuit of scientific recognition and as recognition itself. To be effective as a form of capital, a resource had to in some way help scientists acquire recognition or other resources that could be converted into recognition. These “effective resources” (i.e. capital) are not equally distributed. They have an objective reality that cannot be understood through the perceptions and interactions of the individuals who possess them.

How can we concretely grasp these objective relations which are irreducible to the interactions by which they manifest themselves? These objective relations are the relations between positions occupied within the distributions of the resources which are or may become active, effective, like aces in a game of cards, in the competition for the appropriation of scarce goods of which this social universe is the site (Bourdieu 1989:16-17).

The difficulty in identifying forms of capital is that what constitutes capital and the value of capital is always an object of struggle itself. Members of a field are permanently engaged in symbolic struggles to raise the value of the types of capital they hold relative to other forms of capital. Because of this constant struggle, research “discovers and reproduces uncertainties which are inherent in reality itself” (Bourdieu 1990a:77).

Analytically, I separated the types of capital that functioned as resources and recognition of scientific worth from the struggle over the value of different types of capital. Identifying these
forms of capital involved the initial identification of potential indicators which were then tested in relation to scientists’ perceptions of what was effective in improving or maintaining their positions within the field.

Early intuitions about the principles of division operative within a field are put to the empirical test and gradually refined until they yield an objective space, defined perhaps according to criteria quite different from those that had originally guided the study (Emirbayer and Johnson 2008:8).

My early intuitions were based on Bourdieu’s research on the academic field, the literature on changes in the scientific field and my own experiences as a researcher in the health science.

In *Homo Academicus* (Bourdieu 1990b), an in-depth study of the academic field in France in 1968, Bourdieu found that the academic field was structured around the opposition between cultural and economic capital, in an inverse relationship to the structure of the field of power. Cultural capital, in the form of scientific capital, dominated over economic capital as the principal of stratification. This opposition was especially pronounced in academic sectors that were furthest and most autonomous from the field of power, such as the natural sciences and humanities. In the professions, such as law and medicine, Bourdieu noted a convergence between economic and cultural capital, as members of professions competed for both the economic rewards of their profession and the scientific rewards of the academic field.

Based on my experience with health funding organizations as a doctoral student, and the current literature on changes in the academic field, I suspected that this convergence between economic and cultural capital may have been occurring in academic sectors that were previously considered to be far from the market, in what Owen-Smith (2003) called a hybrid system of stratification. I was familiar with funder demands to justify the relevance of my research in terms of its applicability and utility. However, as someone who could be considered to be an applied social scientist, I did not know whether this market-oriented logic was objectified in new forms of capital in the basic sciences which have traditionally been considered to be further from the market but which have experienced the greatest growth in direct market involvement (Blumenthal et al. 1986, 1996; Zinner et al. 2009).

Unlike Bourdieu’s (1990b) study of the academic field in France, my purpose was not to identify an exhaustive range of all the types of capital that scientists might possess, but to test
this hunch about the convergence between economic and cultural forms of capital. I found, as described in Chapter 5, that some scientists with certain types of market experience that were objectified in measurable ways, such as number of patents, licenses, industry funding and company spin-offs, identified these commercial products as providing a competitive advantage in certain grant competitions and as a direct source of income to fund research. While scientific capital in the form of peer-reviewed publications remained the dominant principal of stratification, a new form of economic capital, which I called “entrepreneurial capital”, appeared to form a secondary principal of stratification. These two forms of capital corresponded to two distinct logics, the logic of traditional science and the logic of market-oriented science promoted by science policymakers and embedded in new funding programs.

Participants perceived research grants as both an economic and scientific form of capital, sometimes conforming to a scientific logic, at others to a market logic, and at times to both, depending on the type of grant. There was, in fact, a great deal of uncertainty about the relationship between funding criteria and scientific worth, which is discussed in Chapter 6 where the strategies scientists used to acquire particular forms of capital and convert one form into another are examined.

*Indicators of Scientific and Entrepreneurial Capital*

Peer-reviewed publications were unanimously recognized as the ultimate form of recognition of scientific worth, in terms of access and promotion within the field and recognition among peers. Research grants were reported as the second most valued form of scientific capital. Because the scientists in this study were members of a tightly defined scientific community with common standards, I was able to use publication data to develop indicators of scientific capital. The homogeneity of this community was clearly demonstrated when all participants identified the same three journals, *Science, Nature* and *Cell*, as the most prestigious journals in their field. Publication volume (the total lifetime number of peer-reviewed publications), productivity (the number of peer-reviewed publications per year within the five years preceding the study) and prestige (publications in tier 1 and tier 2 journals) based on data collected in participant CVs were used to rank the overall volume of publication-based scientific capital each participant possessed. Grant-based scientific capital was estimated based the total value of grants held. Data collected in the commercial background section of the Background Information Form,
including the number of patents and licenses held, companies founded, and funding received from industry, were used as indicators of entrepreneurial capital. The specific criteria used to rank each participant according to these two types of capital are described in Chapter 5.

Figure 1. Conceptual Map of the Space of Positions

After ranking the overall volume of each type of capital participants held, I created a visual representation of the space of positions along the dimensions of scientific and entrepreneurial capital and mapped the location of each participant’s position according to the volume of each type of capital held. Figure 1 illustrates this conceptual representation of social space. This two-dimensional representation illustrates both the volume and the composition or asset structure of opposing types of capital. The volume of scientific capital is represented along the vertical axis; entrepreneurial capital is located along the horizontal axis. The space of the upper left and upper right quadrants is the same in total volume of scientific capital, but has a different asset structure. The ratio of scientific to entrepreneurial capital is high:high in the upper right quadrant and high:low in the upper left quadrant. The space of the upper right and lower left quadrant are opposite in both volume of capital and asset structure – high scientific:high
entrepreneurial capital in the upper right and low scientific:low entrepreneurial capital in the lower left. Similarly, the upper left and lower right quadrants are opposite in volume of capital and asset structure (high entrepreneurial:high scientific capital and low entrepreneurial:low scientific capital) while the lower left and right quadrants are the same in volume of scientific capital but have a different asset structure (low entrepreneurial:low scientific capital and low scientific:high entrepreneurial capital).

4.4.2. Constructing the Space of Position-Takings

In constructing the space of position-takings, scientists’ perceptions of “good science” and the impact of market-oriented funding on the quality of science were understood as position-takings in a symbolic struggle over the legitimate definition of science and the legitimate principal of differentiation structuring the distribution of rewards within the field. It is through these struggles that the structure of the distribution of power is reproduced or transformed. My analysis of the space of position-takings began with the differentiation between points of view on market-oriented funding and the underlying structures of habitus that generated particular perceptions and strategies. I analyzed the responses of established scientists and early stage scientists separately, grouping position-takings according to the six categories of collective identity change developed by Todd (2005), including reaffirmation, ritual appropriation, privatization, adaptation, assimilation and conversion. Three of Todd’s categories – reaffirmation, adaptation and assimilation – were found to correspond to differences in views towards market-oriented science and to reflect differences in scientific habitus among participants in this study. I mapped these position-takings on the space of positions in order to explore scientists’ strategies to accumulate particular types of capital. This part of the analysis was guided by the following types of questions:

- How do scientists define “good science” and the impact of market-oriented funding on the quality of science?
- What underlying structures are embedded within different categories of perception of scientific quality?
- Why do some scientists with a traditional scientific habitus adapt to market-oriented practices while others do not?
- What is the relationship between scientists’ perceptions of market-oriented funding (habitus) and the type and volume of capital they possess (position in the field)?
- What strategies do scientists use to accumulate different types of capital and covert capital from a lower to higher form?
• Why do scientists who occupy similar positions have different responses towards market-oriented funding and those who occupy different positions have similar responses towards market-oriented funding?
• What are the similarities and differences in perceptions and responses between early and established scientists?

Through this iterative process of relating positions, dispositions and position-takings to one another, I produced an account of how this particular group of scientists’ responses to market-oriented science contributed to the construction of quality criteria that were embodied in dispositions and institutionalized in the objective structures of the field.

4.5. Reflexivity and Rigour

The strategies I used to ensure rigour are in keeping with the theoretical and epistemological assumptions of this study. I endeavoured to produce a “realist construction” (Bourdieu 1999:618), one that does not pretend to be neutral or objective (as in the tradition of positivism) and is therefore an account produced from a particular point of view. However, this approach attempts to minimize the effects of this viewpoint on the researcher’s understanding of the social world. The “truth” of a realist account can only be evaluated in relation to the position it is spoken from, even as the researcher attempts to minimize its impact on the object of study: “there is no object that does not imply a viewpoint, even if it is an object produced with the intention of abolishing one’s viewpoint” (Bourdieu 1990b:6). The researcher’s own viewpoint is minimized through “epistemological vigilance” which requires one to “make explicit, in a methodological way, the problematic and principals of object construction” (Bourdieu 1991:38). In other words, I had to expose both the strategies I used to produce this construction and the impact of my own position in the construction of the research object and the written account.

The crucial difference is not between a science that effects a construction and one that does not, but between a science that does this without knowing it and one that, being aware of work of construction, strives to discover and master as completely as possible the nature of its inevitable acts of construction and the equally inevitable effects those acts produce (Bourdieu 1999:608).

Throughout the research process, I was constantly aware of my own position in the scientific field and how my choices would affect the account I produced. I found myself identifying with the frustration expressed by scientists who opposed market-oriented criteria and with their
perceptions of scientific quality. I was surprised to find that basic scientists were much more theory-driven than data or methods-driven. They spoke about following the research into the unknown and constructing a good story to explain the data. This resonated with my own experience as a qualitative social scientist. From these scientists’ perspectives, as well as my own, the emphasis of funders on utility and predictability was seen to undermine the discovery process and the creative processes that lead to the production of the highest quality science.

My awareness of this identification also led to a dilemma over whether I should take an explicitly (or implicitly) political and normative stance against market-oriented policy – in effect, adopt a position in the symbolic struggle over the value of scientific capital vis a vis economic capital in the scientific field – or whether I should produce an account of the struggle itself. In other words, should I try to use science as an “instrument of power” or contribute to a “science of power” (Bourdieu 1990b:16)? This is a particularly difficult task for the sociologist who studies to academic field while occupying a position within that field.

The sociology of science is so difficult only because the sociologist has a stake in the game he undertakes to describe…and because he cannot objectify what is at stake, and the corresponding strategies, unless he takes as his object not simply the strategies of his scientific rivals but the game as such, which governs his own strategies too and is always liable to exert an insidious influence on his sociology (Bourdieu 1975:40-41).

In grappling with this dilemma, I came to understand the meaning of the kind of reflexivity that Bourdieu espouses that involves objectifying my own position in order to minimize its impact on the account of the social world that I produce. This kind of reflexivity requires the researcher to go beyond what Gingras (2010) calls the “epistemic reflexivity” of making the construction of the account visible towards a “sociological reflexivity” which requires the researcher to make her own social position and its impact on the construction of the research object visible. In this process, I came to clarify my own beliefs about epistemology and power. I have been drawn to a realist epistemology because I believe in the emancipatory power of truth. The most political of acts is to bring to light the things that are hidden in plain sight; to say the things that no one wants to know (Gingras 2010).

How then did I use my awareness of my own position as a sociologist and my own strategies to gain recognition within the academic field to strengthen the rigour of my analysis? By taking seriously my objective of analyzing the struggle itself over the definition of legitimate science, I
was able to maintain a distance from my own judgments about the impact of market-oriented science on scientific quality during the process of analysis. This lead, I believe, to closer attention to the assimilation strategies of early stage scientists and the ways in which they embodied both a market and scientific logic. The purpose of maintaining this distance was not to avoid taking a stance altogether, but to ensure that any stance taken would be based on a more accurate construction of reality, given the limitations inherent in the “fuzzy, woolly” nature of reality and the tools of sociology to understand it (Bourdieu and Wacquant 1992:23).

It also led me to be explicit about how I constructed the space of positions and position-takings and how theory and method were intertwined in order to allow the reader to understand how I produce this particular account (Bourdieu 1999).

It seems to me imperative to make explicit the intentions and procedural principles that we put into practice in the research project whose findings we present here. The reader will thus be able to reproduce in the reading of the texts the work of both construction and understanding that produced them (P. 607).

The strength of the methodology used in this study is its focus on the relationship between symbolic constructions of legitimate science and the objective relations of power of members of the scientific field. This relational methodology overcomes one of the weaknesses in the existing literature on change in academic quality criteria. Studies that explicitly or implicitly explore the maintenance or erosion of boundaries between science and the market frequently describe symbolic struggles over social categories without linking these to objective distributions of power. Detailed empirical attention to the forms of capital that are more or less effective and the dispositions that are more or less likely to succeed in response to new field conditions can identify tendencies towards conservation or change in both symbolic categories and material relations of power. This is an intellectually demanding approach that requires the researcher to transform her vision of the social world without much guidance in how to best do this. Bourdieu’s own writings on method are scattered and obscure, which may partially explain why many scholars take up his theoretical concepts without using a relational methodology. Studies often use habitus or field as separate explanatory concepts without empirically exploring their relationship to one another (Emirbayer and Johnson 2008). By this, I mean that data are gathered at one level – either the level of habitus (perceptions of reality) or field (structural conditions) and inferred at the other, rather than basing the analysis on data gathered at both levels.
One of the dangers of trying to gather data and link the symbolic with the material is that one will do justice to neither. A greater danger may be in not being able to find scholarly audiences that are receptive to thinking outside of subjectivist or objectivist boxes. As a strategy for an aspiring scholar to accumulate recognition, the use of a relational methodology may be questionable. On the one hand, Bourdieu is one of the most widely recognized and cited sociologists of the late 20th century (Swartz 2003). On the other hand, he describes his own style of research as dangerous on a number of grounds: a) he is radically sceptical of the rules of the academic game which often lead to producing theory for theories’ sake (Bourdieu 2000; Jenkins 1992); b) the kind of knowledge produced using this approach may be knowledge that “no one wants to know” (Gingras 2010:10) and that may at the same time appear obvious due to a “naïve reading” (Bourdieu 1990b:23); and c) the practice of reflexivity leads to the production of accounts that lack the appearance of “scientificity” (Bourdieu 1990b:28).

Despite these difficulties and dangers, this approach has the potential to offer insights that other approaches cannot. In particular, it may be more likely to identify “the conditions that are likely to provoke change…and the conditions that make it likely to proceed in one direction rather than another” (Todd 2005:433). I did not anticipate that a single exploratory study itself could provide definitive answers to the broader question of change in quality criteria in science, but anticipated that one of the most significant outcomes of this study would be to open up questions that could provide a more fruitful line of inquiry than dominant approaches allow.
Chapter 5: Results Part 1 – Mapping the Social Space of Positions

5.1. Introduction

The purpose of this chapter is to provide an overview of the characteristics of the sample and to map out the structural positions respondents occupy in relation to the type and volume of valued capital they hold. The chapter is divided into four sections. In the first section, I describe the career stage, faculty and research setting, disciplinary background, commercialization experience and funding of study participants. In the second section, I present the forms of capital scientists perceived as legitimate, as recognition of “good science” and as resources to carry out “good science”. Based on these recognized forms of capital, in the third section I rank study participants as having low, moderate or high volumes of capital, and map the distribution of positions in social space in section four. This map of the structural positions provides the foundation with which to explore relationships between positions and position-takings towards market-oriented science in Chapter 6. In these chapters, pseudonyms are used to protect the identity of participants.

5.2. Participant Characteristics

Twenty basic scientists participated in the study, nine early stage and eleven established scientists (see Table 5). All participants held a faculty appointment with the university’s Faculty of Medicine (n=16) or Faculty of Arts and Science (n=4). Fourteen held a primary (salaried) appointment with the university while the remaining four were based in hospital research institutes, a short walk from the university. Most participants had a similar disciplinary background, the majority having trained in Biology or Biochemistry; other disciplines represented included Chemistry, Biophysics, Physiology and Experimental Medicine. They

<table>
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<tr>
<th>Career Stage</th>
<th>Faculty</th>
<th>Research Setting</th>
<th>Disciplinary Background</th>
</tr>
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<tbody>
<tr>
<td>Early</td>
<td>Medicine</td>
<td>University</td>
<td>Biology/Biochemistry</td>
</tr>
<tr>
<td>Late</td>
<td>Arts and Science</td>
<td>Hospital Institute</td>
<td>Bioinformatics</td>
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Table 5. Research Stage, Setting and Disciplinary Background
carried out research on a wide variety of topics using fruit flies, yeast, worms, or fish as model organisms. All participants could articulate the potential relevance of their research to a wide variety of diseases\(^6\). While they identified as members of a common scientific community, each also belonged to a scientific sub-community depending on their area of specialization.

**Early stage scientists:** Early stage scientists had an average of four years academic experience, ranging from less than one to eight years. While all early stage scientists were seeking to establish a scientific reputation, some were further along this trajectory than others. They were in varying stages of gaining grants, establishing a lab and recruiting students. Nathan, the most recent hire, was still unpacking boxes, writing grant applications and setting up his lab. He did not yet hold any grants. Vincent, who had just recently attained tenure, had a very large well-staffed lab and many grants. Two-thirds (n=6) of early stage participants either owned or had previously filed for a patent based on their doctoral, post-doctoral or currently active research. Only two, Trevor and Vincent, had been actively involved in patenting and development since beginning their tenure-track position. At the time of the study, Vincent continued to sit on the Board of Directors of a company he had co-founded and pursue patenting opportunities, while Trevor had withdrawn from his development activities to focus on his research and publications.

**Established Scientists:** Established scientists had between eleven and thirty-four years experience, with an average of 25 years. Four were nearing retirement age while the remaining seven were active in maintaining or building their scientific reputations. Four established scientists had no commercial experience at all, while seven had filed for a patent at some point in their academic career. Of these, five had also received industry funding and two were company founders. Sheldon and Robert had the highest volume of entrepreneurial experience, each holding 18 patents, multiple licenses to industry and past experience as company founders.

**Funding:** Scientists held a wide range of grants (Table 6). Grants are divided into two categories: investigator-driven and market-oriented. Investigator-driven grants are those grant

\(^6\) Details about the specific disease relevance of participants’ research are given in broad terms to ensure their anonymity as some participants were leading researchers in their field and had been in the media for disease-specific discoveries.
Table 6. Type of Funding Held Across Career Stage

<table>
<thead>
<tr>
<th>Investigator-Driven Grants</th>
<th>Market-Oriented Grants</th>
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<tbody>
<tr>
<td><strong>CIHR</strong> Operating</td>
<td><strong>NSERC</strong> Operating</td>
</tr>
<tr>
<td>Established</td>
<td></td>
</tr>
<tr>
<td>Thomas</td>
<td>X</td>
</tr>
<tr>
<td>Dirk</td>
<td>X</td>
</tr>
<tr>
<td>Robert</td>
<td>X</td>
</tr>
<tr>
<td>Sheldon</td>
<td>X</td>
</tr>
<tr>
<td>Ivan</td>
<td>X</td>
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<tr>
<td>Sarah</td>
<td>X</td>
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<tr>
<td>Simon</td>
<td>X</td>
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<tr>
<td>Jack</td>
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<td>Gerald</td>
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<tr>
<td>Marjory</td>
<td>X</td>
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<tr>
<td>Jeremy</td>
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<tr>
<td>Early Stage</td>
<td></td>
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<tr>
<td>Trevor</td>
<td>X</td>
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<tr>
<td>Damien</td>
<td>X</td>
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<tr>
<td>Vincent</td>
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<td>Kate</td>
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<td>Mike</td>
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<td>David</td>
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<td>Nathan</td>
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rewarded based on scientific criteria as evaluated through peer-review. They are distributed through two federal granting agencies, CIHR and NSERC, as well as research foundations as the Heart and Stroke Foundation of Ontario. Market-oriented grants are distributed through new funding organizations and programs that require applicants to demonstrate both scientific excellence and social or economic relevance. As seen in Table 6, all scientists held operating funds from NSERC or CIHR, with the exception of Nathan who had submitted applications but did not yet hold any research grants. Seven of 20 scientists held an NSERC operating grant, while 17 of 20 held a CIHR operating grant and four held both NSERC and CIHR operating grants. Established scientists held a wide variety of types of market-oriented grants, while early stage scientists tended to hold market-oriented grants targeting new investigators, from the Canadian Foundation for Innovation (CFI) and the Ontario Ministry of Research and Innovation (OMRI). Three also held Genome Canada grants.

In summary, participants in this study belonged to a common scientific community engaged in health-related research. They were at different stages of their career development, had a range of entrepreneurial experience and varying types of funding, both investigator-driven and market-oriented. In the following section, I explore the forms of capital that these scientists perceived as legitimate.

5.3. Forms of Capital: Resource and Recognition
According to this study’s theoretical framework, the structure of the distribution of power in science is reproduced or transformed in the daily practices of scientists seeking to maintain or improve their positions in the field. This is accomplished through ongoing struggles to define legitimate capital. Different fields produce different forms of capital according to the logic of the specific field (Bourdieu 1986). Capital is both a capacity and resource for action, and a recognition of past action that exists in an objectified or embodied state (Swartz 2002). The Mode 2 thesis suggests that scientists are being evaluated according to new criteria which would give rise to new forms of capital and symbolic struggles over scientific worth (Gibbons et al. 1994). Given the lack of clear research evidence supporting change in quality criteria (Hessels and van Lente 2008), this study asked participants how “good science” is recognized and rewarded, and what new evaluation criteria they encountered from
funders. I do not attempt to document all available forms of capital that scientists use to pursue success, but focus on the kinds of capital scientists identified as the dominant forms of recognition of “good science” and the dominant forms needed to acquire research funding. Peer-reviewed publications and research grants were perceived as the dominant forms of scientific capital. A new kind of entrepreneurial capital linking scientific worth to an economic logic was identified as a new resource necessary for accessing certain types of grants and funding research.

5.3.1. Recognition of “Good Science”: Scientific Logic

Participants consistently identified peer-reviewed publications as the overriding indicator of scientific recognition which determined access to academic positions, tenure, promotions and grants. Publications were the most powerful form of scientific capital scientists in this study sought regardless of research setting and career stage.

Well, peer review publications are the be-all and end-all of science. Without it you don’t get any funding and it’s really the only way you get recognized by your peers so publishing in good journals is the main point, right. It’s always been that way. Sheldon, Established Scientist

Publications are, as Marjory put it, the road to “glory” for scientists which outweighs wealth as the measure of success.

We are certainly not in it for the money because there’s no money in it. I mean, I am not complaining because my husband and I together we’re quite comfortable, but it’s not like we’re millionaires or billionaires or ever will become. So what we’re in it for is the glory and if you can’t publish your work there certainly is no glory, right? Marjory, Established Scientist

It is a well-established fact in science studies that scientists with an established reputation are more likely to have the legitimacy of their research recognized than those who have yet to establish their reputation; recognition breeds greater recognition in what Merton (1968) called the Matthew effect. Simon, a mid-career scientist, describes how his reputation “snowballed” based on a few “cool discoveries” and “good publications”, enhancing his likelihood of future success.

You get a reputation based on publications and based on being seen talking about your work. Unfortunately there’s salesmanship, a lot of it actually, so how you are perceived by the peers who review your papers and review your grants is determined by their interactions with you at these meetings. And so much of it snowballs based on sometimes a little bit of luck, in the beginning. I am at a stage now where we’ve been building on some really cool discoveries and we’ve been getting some good publications and I am now developing a reputation so that people are more likely to accept more what I am writing and saying it’s true. You know, two people could write
the very same paper and if one person’s unknown and the other person everybody knew, it would be a completely different outcome. Simon, Established Scientist

According to the Matthew effect (Merton 1968), scientists who have already acquired a reputation based on publications receive an exaggerated degree of credit for their quality of their work giving them an accumulative advantage in securing funds and future publications.

Participants in this study unanimously identified with a broad scientific community rather than a specific disciplinary community, ranking Science, Nature and Cell as the top tier journals. Peer-review was generally viewed as a fair way to evaluate scientific merit, but several participants noted that there is a “human element” to judgment. Top tier journals were seen as faddish and sometimes publishing papers based on how “hot” the topic was rather than on the quality of the study.

Nature is the pipe that every scientist loves their work to go because, like I say to students, it’s the Vogue of the world of fashion. No more or no less than that. Just because it appears in Vogue doesn’t mean that it’s better than any other type of fashion. Trevor, Early stage scientist

After publications, grants were said to be the second most important indicator of scientific value, enabling access to academic positions and promotions. Grants also functioned as a material resource to invest in science. Traditionally, grants and publications have been easily interchangeable, as each was invested and converted into the other in a reciprocal relationship.

It’s a circular thing. We’re rewarded for basically publications, quality and number. And to get those you need money. To get the money, you need to bulk up your publications. Simon, Established Scientist

However, participants reported two changes in the funding context that made it more difficult to convert publication success into grant success. First, steep competition for investigator-driven grants made these grants more difficult to acquire, requiring scientists to demonstrate higher productivity to maintain the same level of grant success. Second, market-oriented grant criteria required applicants to demonstrate value according to an economic logic.

In the first case, at the time that this study was carried out, having a successful publication record and high ranking scores in grant competitions no longer ensured expected levels of operating grant success. In 2007, CIHR operating grant success rates had plummeted to an all-time low of 16% since the formation of CIHR in 1999
(CIHR 2009b), compared to an average of 25 to 30% (CIHR 2004c). Many scientists with a high volume of scientific capital and high ranking scores in grant competitions had their applications rejected. The gap between expectations of success and actual success led to great frustration over time wasted rewriting and resubmitting unsuccessful grants and looking for alternative funding sources. For example, Thomas had a very strong publication record, with 98 publications in total, an average of 5 per year over the previous 5 years and had several articles published in tier 1 journals. Yet he compared operating grant competitions to gambling.

Because you know it’s gambling. If you have success rates in the teens, you know that a lot of good grants get turned down. There are just too many good people sending in too many good grants. There are now decisions being made on the second decimal. I want to see who can judge the quality of a grant whether it is 4.18 or 4.19. 

Thomas, Established Scientist

According to Thomas, grants as criteria for rewarding tenure may need to change if grant success rates remain very low. Otherwise, “good scientists” will not be awarded tenure.

The criteria will have to change if nothing changes in the environment. Because you can’t evaluate somebody based on whether they get a grant or not. It used to be that if you don’t have a grant, you don’t get tenure. If the system isn’t going to change, that rule will go. 

Thomas, Established Scientist

In other words, operating grants, while highly competitive, continued to be awarded based on a scientific logic. As I describe in the next section, however, market-oriented grants held scientists accountable to an economic logic, introducing new criteria in addition to scientific capital and producing a new form of entrepreneurial capital.

5.3.2. Recognition of “Good Science”: Economic Logic

Participants identified publications as necessary but insufficient to ensure success in market-oriented grant competitions. Four participants reported having grant applications rejected based on financial or relevance criteria despite being ranked very highly on the scientific component of their application. Participants encountered economic and social relevance criteria in their applications to the CFI, OMRI, NSERC Strategic Grants, CIHR Strategic Grants, CIHR Team Grants, Canada

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7 A variety of OMRI grants were based on a co-funding model, including the Early Research Award (ERA), Premier’s Research Excellence Award (PREA), Ontario Innovation Trust (OIT) Ontario Research Development Challenge Fund (ORDCF) and the Ontario Research Fund (ORF) (OMRI 2006).
Research Chairs (CRC), and the Networks of Centres of Excellence (NCE). They had universally experienced pressure to justify their research in terms of its economic or social relevance. The most common relevance criterion noted was the Benefits to Canada section of applications which was a part of several provincial and federal grant programs, including CFI, OMRI, NCE and Genome Canada. Applicants were required to complete a section on the benefits of their proposed research to Canada, in terms of job creation, training of qualified personnel and contribution to social and economic well-being.

The OMRI Early Research Award plus the CFI are really the only two where that has been an awful lot of paper work involved in how was this going to benefit Canada, how is it going to benefit the economic sector, the business sector, biotech sector, how are we going to train highly qualified people, all of those kinds of things. They have affected the things that we include with the application. They don’t really substantially affect the science. *Kate, Early stage scientist*

The Benefits to Canada section of these forms did not exclusively refer to the economic benefits of research.

It's not only commercialization that could be very beneficial to Canada so the CFI grant you need to write a whole set page or two pages on benefits to Canada. Commercialization is one aspect. But the fact that we're doing medical research, it could have a huge impact in a number of ways including reduction of health care costs or things that might actually affect care directly. *David, Early stage scientist*

Because all scientists in this study carried out health-related research, none were excluded from funding programs based on criteria to justify relevance. However, other types of market-oriented funding restricted applicants to particular research areas or to those able to access industry funding or produce commercial products.

A more restrictive mechanism used to ensure relevance was targeted funding programs. Targeted funding was earmarked for specific often disease-focused research problems determined by policymakers, laypeople, scientists and/or industry representatives. Targeted funding was distributed through CIHR and NSERC’s strategic grant programs, OMRI grant programs, CRCs and Genome Canada. Genome Canada, by its organizational mandate, only provided funding targeting genomics and proteomics research projects. Scientists who spend years specializing in a particular area may benefit from targeted funding but cannot steer their research into the
direction that policymakers decide to target even if they could anticipate the next “hot spot”.

The bull’s eye keeps shifting every few years, depending on what’s politically hot. For example, the Ontario Research Foundation, last year they were looking at health related issues. This year, their priority is green technology, new alternate sources of energy and that sort of thing. Simon, Established Scientist

Most respondents were unaware of how the types of research problems to be funded were selected or who was involved. Many participants also referred to the lack of transparency in funding decisions for strategic and targeted grants.

Money is being poured into some of these huge things like Genome Canada and these mysterious one time huge donations of money to particular institutions. There is no transparency about how those places were picked. This happened very recently by the Harper government and now huge amounts of money are going to particular places. The impression was that these places had lobbied for money and received money. Kate, Early stage scientist

Targeted programs were perceived as politically motivated and not necessarily based on an open competition where success is determined by peer review.

Other market-oriented grants required applicants to demonstrate commercial viability or produce commercial outputs. Scientists encountered commercialization criteria when applying to Genome Canada, NCEs, and CIHR Proof of Principle (POP) and Drug and Product Development (Rx&D) programs and a variety of grants provided by the OMRI. These grants required applicants to secure industry funding, own or generate intellectual property or demonstrate the potential to develop products or company spin-offs. The CIHR POP was only available to scientists who already held a patent and was used to advance product development.

There are other agencies, other grants that are specifically directed towards generating IP. They will only give you money if you already have intellectual property like the CIHR Proof of Principle grant, which we applied for. Gerald, Established Scientist

Other programs such as Genome Canada, CFI, NCEs and OMRI, which required scientists to raise matched funding, sought to increase collaboration between scientists and industry representatives in order to stimulate new product development. Co-funding tied scientists to new types of relationships and practices, such as writing business plans, conducting market analyses, establishing relationships with venture capitalists and industry executives and founding companies. However, due to the
small industrial base in Canada and the difficulties scientists faced in raising matched
funding from industry, early co-funding criteria were softened to allow matched
funding from non-industry sources and non-cash matches, such as the donation of
equipment.

They quickly realized that you can’t milk an industry that you are supposed
to support. I think it was a provincial grant, ORDCF that had in its mission
statement, “These grants are supposed to support a fledgling biotech
industry in Ontario. And by the way, we want this tiny little industry to
match those millions of dollars.” Whoever has a clear mind and reads this,
obviously it was ridiculous. So they relaxed their rules. Thomas,
Established Scientist

Most of the 60% in matched funding raised for CFI grants came from the provincial
level of government and the universities themselves rather than private industry.
Many participants in the study who held CFI grants used their university start-up
funds and awards from the OMRI as matched funding. A series of co-funding
programs developed by the OMRI were used to match CFI grants, including the now
defunct Ontario Research Development Challenge Fund (ORDCF), Ontario
Innovation Trust (OIT), and Premier’s Research Excellence Award (PREA), as well
as the currently active Early Researcher Awards (ERA) (OMRI 2006). While co-
funding criteria of these programs were often met through other co-funding programs,
rather than industry funding, they also held applicants accountable to relevance
criteria.

Participants made clear distinctions between the formal requirements of investigator-
driven grants provided by the major federal funding organizations (CIHR and
NSERC) and the market-oriented funding organizations and programs. However, they
also expressed uncertainty about the extent to which market-oriented criteria were
subtly permeating scientific culture. Even in the absence of formal relevance criteria,
participants questioned whether an economic logic was influencing the evaluation of
scientific worth in investigator-driven grant review processes and institutional
performance evaluations.

5.3.3. Uncertainty over Criteria to Recognize “Good Science”

While most scientists perceived CIHR operating grants as functioning according to a
traditional scientific logic, some saw a subtle bias towards research that had clear
potential for applications, even in investigator-driven competitions.
CIHR wants proof that all this money is going towards something. It used to be enough to say, “We don’t know how bone is being dissolved. Let’s just go find out.” Now you have to write your grants, saying, “If we could stop bone from being dissolved we can create this drug to prevent osteoporosis.” *Gerald, Established Scientist*

Even when novelty is the defining criteria of “good science”, what makes a scientific problem novel and “interesting” may have shifted to encompass the potential for application.

“Interesting” is pretty hard to define, but there’s certainly an aspect of application and impact that comes into it and things that have higher potential for application and impact in science and society are probably rated as more interesting. Whereas classically, true science, people would say, you don’t need to have any application. It’s just a search for knowledge. *David, Early Scientist*

How much this thinking had influenced funding decisions was open to interpretation. One participant expressed uncertainty about whether she would have shift the direction of her research to make it more clinically relevant in order to receive funding.

What I don’t have a sense of is whether grant panels are really shifting their focus toward insisting that you have something really translational or have a model system in your lab that’s more clinically relevant. I don’t know how many people have actually moved in that direction. But it’s certainly crossed the minds of lots of people I know to wonder whether we are going to have to change the direction of our research. *Kate, Early stage scientist*

The uncertainty about how far an economic logic had penetrated funding organizations and academic institutions was also expressed with regards to intellectual property.

Several participants noted that the on-line application process for CIHR required applicants to complete a section on intellectual property that appeared directly underneath “publications” in the CV module of the application. Most participants did not perceive patents as having a significant impact on operating grants success, but some thought it may exert a subtle effect by making up for a weak publication record or adding credibility to an application. Sheldon, a senior scientist with a strong publication and patenting record, thought that the investment of industry or technology transfer offices in patenting research may have added credibility in operating grant competitions.

I think it enhances a proposal when you say that this work was considered patentable by the hospital and they sent the money to patent it. So if the
hospital thinks it’s worthwhile, it adds legitimacy. Definitely. Sheldon, Established Scientist

While some had not noticed the patent checkbox on CIHR online applications, others had discussed its influence with colleagues. Regardless of whether or not it influenced grant success rates, Gerald thought it exerted an effect on scientists’ perceptions and behaviour.

CIHR Common CV Modules have a box. NCE CV modules, all the CV modules I know of all have boxes now, saying “Do you have patents?” And I remember I was sitting around one committee and someone mentioned the fact that that everyone’s got patents now and someone else said, “That’s because there’s a box, everyone feels they have to put something in the box or they’re inadequate.” Everyone’s applying for patents so they have something to put in the box. Gerald, Established Scientist

Most hospital-based scientists who participated in this study included a section on their CV for patents, even if they had none. Unlike university-based scientists, hospital-based scientists were formally evaluated on the number of patents they held. Kate remarked that she always had to report to the hospital on her intellectual property and included a section on her CV for patents where she had written “None Noted”.

They ask about that. When we go for promotions, for grants, they always want to know if you have patents. I don’t know how long people have been asking me that. Ever since I have been here (6 years), that’s been one of the things I have to mark on everything. There are people at this hospital who have patents that have made a fair amount of money for the hospital. The hospital would, I am sure, love us all to have patents. Kate, Early stage scientist

Even though intellectual property was a formal criterion in performance reviews, it was not perceived as having a very significant impact, compared with publishing good scientific papers. Robert, a senior investigator in a hospital research institute, did not perceive patents or the commercial potential of research as having an impact on hiring decisions.

We hired new investigators in the last couple of years. They have excellent papers, Nature, Science, whatever, and if they have a patent that’s, as far as our consideration goes, that’s down on the list. We’re not really worried about it one way or another. Their funding is secure just by them publishing really good papers. That’s really the criteria, not patents. Robert, Established Scientist
Therefore, the formal measurement and evaluation of patenting activity combined with its low status relative to the importance of publications gave patenting an ambiguous status in the hospital setting.

The same could be said for the university setting, According to Damien, an early-stage scientist, “They always ask if we have patents but I’ve never got the impression that it makes a big difference.” Particularly for early stage scientists, publications far outweigh patents in importance for attaining tenure.

As a young investigator, I have far more important things like publishing papers. If I did have to produce patents so that the university granted me tenure, then I probably would pursue it. *Trevor, Early stage scientist*

However, another participant perceived his patent activity as potentially useful in his bid for tenure. Gerald, an established scientist who had just undergone the tenure review process, submitted his patent applications in the package.

There isn’t a box saying do you have any intellectual property, but when I wrote it up, I always put that in. I don’t hold any patents. I filled out the forms for the University of Toronto for intellectual property rights. And now we’re applying for patents. So no, the tenure review didn’t specifically ask but I made a point of including it. *Gerald, Established Scientist*

Although scientists in both institutional settings did not perceive patents as a significant source of prestige in the evaluation of their performance, they did perceive them as a potential source of credibility which could augment their status and reputation within academic and granting institution. Like patents, the ability to frame their research in terms of its applicability was also a resource that scientists could use to accumulate grants and prestige. What they seemed to be unsure of was the extent to which they might need to alter their practices in order to demonstrate clinical relevance or produce commercial products in order to acquire a higher degree of economic credibility with funding organizations and academic institutions.

In summary, scientific capital as reflected in peer-reviewed publications and research grants was perceived as the dominant form of capital within the scientific field. Yet scientists reported being held accountable to a market logic in order to acquire the resources necessary to produce “good science”. The salience of this market logic was heightened by the low success rates for investigator-driven grants and the

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8 Gerald had worked as a scientist in hospital research institutes for 10 years before moving into a tenure-track position in the Faculty of Medicine. Therefore, although he was an established scientist, he had just attained tenure.
proliferation of market-oriented funding programs as alternate funding sources. Some market-oriented programs (e.g., CFI, CIHR Strategic Grants) required a minimal investment of time and labour as their criteria could be met using rhetorical strategies. However, others were contingent on the demonstration of commercial outputs, including patents, marketable products, industry funding and company spin-offs. These indicators, reflecting a market logic, could be termed entrepreneurial capital. In the following section, I rank participants according to the volume and type of capital (scientific and entrepreneurial) they held.

5.4. The Space of Positions in the Scientific Field
Bourdieu (1985) defined a social field as a structured space of positions in which the positions individuals occupy depend on their overall volume and composition of capital. In the scientific field, scientific capital has traditionally been the dominant form of symbolic capital (Bourdieu 1990b). Grants and publications were two kinds of scientific capital that conformed to a scientific logic and were easily converted from one form into the other (Rip 1994). However, market-oriented science policy, by producing a new form of entrepreneurial capital, appears to have disrupted scientists’ expectations about the relationship between publications and grants, as a strong publication record no longer ensured grant success. Entrepreneurial capital is based on an economic logic that is in polar opposition to the logic of scientific capital. However, it can be invested and converted into scientific capital.

In the following section, I analyze the volume of scientific and entrepreneurial capital of each study participant in order to map their positions in the scientific field. A wide variety of potential indicators of capital were initially identified and gathered through participant CVs and Background Information Forms. Based on the preceding analysis of interview data on the dominant forms of capital, I selected specific indicators of scientific and entrepreneurial capital and used these indicators to rank the total volume of each form of capital. My purpose in ranking participants’ volume of capital was to identify relative differences in volume and type of power as the basis for exploring whether and how these differences related to scientists’ responses to market-oriented science policy.
5.4.1. Volume of Scientific Capital

Based on scientists’ perceptions of “good science”, I have divided scientific capital into two forms, grant-based and publication-based. Grant and publication data derived from interviews and Curriculum Vitae were used to rank participants according to these two types of scientific capital.

Grant-Based Scientific Capital

The volume of scientific capital associated with grants was estimated from the total value of grants held. These data were initially collected from participant CVs. Based on inconsistencies between CV data and grants reported in interviews, a complete search of three on-line granting databases was used to gather data on research funding received from NSERC (2008), CIHR (2008) and CFI (2008). When inconsistencies in grant values were found, grant databases were used. Participants were ranked as having a high volume of grant-based scientific capital if they held over $5 million in research funding; a moderate volume of grant-based scientific capital if they held $3 to $5 million; and a low volume if they held less than $3 million (see Table 7). The values associated with low, moderate and high scores were determined by graphing the responses of each participant according to the total value of grants held and observing the naturally-occurring breaks in the rank ordered data.

Table 7. Ranking of Grant-Based Scientific Capital: Early and Established Scientists

<table>
<thead>
<tr>
<th>Rank</th>
<th>Total Grant Volume</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>&gt;$5 Million</td>
</tr>
<tr>
<td>Moderate</td>
<td>$3-$5 Million</td>
</tr>
<tr>
<td>Low</td>
<td>&lt;$3 Million</td>
</tr>
</tbody>
</table>

Table 8 summarizes the total value of grants participants actively held at the time of the study, the value of investigator-driven and market-oriented grants and the proportion of grants that was market-oriented. While early stage scientists with low grant-based scientific capital had a significantly lower volume of grants than established scientists, creating separate criteria to reflect this would not have changed their overall rank. Established scientists, on average, held four times the volume of research funding as early stage scientists, ranging in value from $1.5 million to $45.5 million. Most early stage scientists had a relatively small funding base (less than $2 million) with the exception of three who were co-investigators on multi-investigator, multimillion dollar grants. The total value of grants held by early stage scientists
(those who held grants – one had just begun to apply) ranged from $900,000 to $31.6 million. Five of the scientists with a high volume of grant-based scientific capital received a high proportion of funding from market-oriented grants (87-100%). However, four of the eleven scientists with a low volume of grant-based scientific capital also received over two thirds (66-87%) of their funding from market-oriented grants. Market-oriented funding did not, therefore, guarantee a higher overall volume of grants.

Table 8. Volume of Grant-Based Scientific Capital

<table>
<thead>
<tr>
<th>Participant</th>
<th>Total value of grants *</th>
<th>Total value of investigator-driven grants *</th>
<th>Total value of market-oriented grants *</th>
<th>Proportion of grant $ market-oriented</th>
<th>Grant-based Scientific Capital</th>
</tr>
</thead>
<tbody>
<tr>
<td>Established</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Robert</td>
<td>$45.50</td>
<td>$3.50</td>
<td>$42.00</td>
<td>92%</td>
<td>High</td>
</tr>
<tr>
<td>Thomas</td>
<td>$13.70</td>
<td>$1.80</td>
<td>$11.90</td>
<td>87%</td>
<td>High</td>
</tr>
<tr>
<td>Jack</td>
<td>$8.70</td>
<td>$7.10</td>
<td>$1.60</td>
<td>18%</td>
<td>High</td>
</tr>
<tr>
<td>Simon</td>
<td>$5.70</td>
<td>$2.40</td>
<td>$3.30</td>
<td>58%</td>
<td>Moderate</td>
</tr>
<tr>
<td>Marjory</td>
<td>$3.90</td>
<td>$0.60</td>
<td>$3.30</td>
<td>85%</td>
<td>Moderate</td>
</tr>
<tr>
<td>Sarah</td>
<td>$2.90</td>
<td>$1.60</td>
<td>$1.30</td>
<td>45%</td>
<td>Moderate</td>
</tr>
<tr>
<td>Ivan</td>
<td>$4.40</td>
<td>$4.10</td>
<td>$0.40</td>
<td>9%</td>
<td>Low</td>
</tr>
<tr>
<td>Sheldon</td>
<td>$4.20</td>
<td>$1.80</td>
<td>$2.40</td>
<td>57%</td>
<td>Low</td>
</tr>
<tr>
<td>Gerald</td>
<td>$3.80</td>
<td>$1.30</td>
<td>$2.50</td>
<td>66%</td>
<td>Low</td>
</tr>
<tr>
<td>Dirk</td>
<td>$1.90</td>
<td>$1.60</td>
<td>$0.30</td>
<td>16%</td>
<td>Low</td>
</tr>
<tr>
<td>Jeremy</td>
<td>$1.50</td>
<td>$0.20</td>
<td>$1.30</td>
<td>87%</td>
<td>Low</td>
</tr>
<tr>
<td>Early Stage</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alex</td>
<td>$31.60</td>
<td>$0.30</td>
<td>$31.30</td>
<td>99%</td>
<td>High</td>
</tr>
<tr>
<td>Vincent</td>
<td>$30.00</td>
<td>$1.50</td>
<td>$28.50</td>
<td>95%</td>
<td>High</td>
</tr>
<tr>
<td>David</td>
<td>$27.20</td>
<td>$0.10</td>
<td>$27.10</td>
<td>100%</td>
<td>High</td>
</tr>
<tr>
<td>Ralph</td>
<td>$1.80</td>
<td>$0.50</td>
<td>$1.30</td>
<td>72%</td>
<td>Low</td>
</tr>
<tr>
<td>Mike</td>
<td>$1.60</td>
<td>$1.30</td>
<td>$0.40</td>
<td>25%</td>
<td>Low</td>
</tr>
<tr>
<td>Damien</td>
<td>$1.60</td>
<td>$1.60</td>
<td>$0.00</td>
<td>0%</td>
<td>Low</td>
</tr>
<tr>
<td>Trevor</td>
<td>$1.10</td>
<td>$0.60</td>
<td>$0.50</td>
<td>45%</td>
<td>Low</td>
</tr>
<tr>
<td>Kate</td>
<td>$0.90</td>
<td>$0.90</td>
<td>$0.00</td>
<td>0%</td>
<td>Low</td>
</tr>
<tr>
<td>Nathan</td>
<td>$0.00</td>
<td>$0.00</td>
<td>$0.00</td>
<td>0%</td>
<td>Low</td>
</tr>
</tbody>
</table>

* millions of dollars

Publication-Based Scientific Capital

I estimated the volume of publication-based scientific capital based on publication volume, productivity and the journal prestige. Volume was measured by the total lifetime number of peer-reviewed publications and productivity by the number of
peer-reviewed publications per year within the five years preceding the study. A five year period was selected to reflect their most recent level of productivity as this was anticipated to be related to their most recent strategies to maintain or improve their positions in the scientific field. Prestige was measured by adding the total number of articles published in tier 1 journals with the number of articles published in tier 2 journals in the five years preceding the study. All top tier journal articles were included due to the high level of prestige they bestow; a five year time period was chosen for the same reasons as mentioned above.

Scientists were asked on the Background Information Form to report tier 1 and tier 2 journals in their field. Regardless of research setting or career stage, participants identified Science, Nature and Cell as the top tier scientific journals. These journals have impact factors of 26 to 30 (Thomson Reuters 2008). Second tier journals were field specific and varied across participants. Because participants had some difficulty recalling and identifying tier 2 journals (“wait a minute, was it JBC or JCB?”), I decided to use impact factor over self-identification of journals as a means to capture 2nd tier journals. As all self-reported tier 2 journals had an impact factor of 3 or more, I used an impact factor of 3 as the cut-off point.

In Tables 9 and 10, I provide a ranking scheme for established and early stage scientists, respectively. Established scientists were ranked as having a high volume of publication-based scientific capital if they ranked high on two out of three indicators: over 150 publications in total (volume), four or more per year (productivity), and/or ten or more in tiers 1 and 2 journals (prestige). Established scientists scored a moderate volume of scientific capital if they ranked moderate on two of three indicators: between 50 and 150 publications (volume), two to three publications per year (productivity) and/or five to nine publications in tiers 1 and 2 journals (prestige). They were also assigned a moderate score if they ranked low, moderate and high across the three indicators. Established scientists with low scientific capital ranked low on two or three out of three indicators: less than 50 publications (volume), one publication per year (productivity) and/or zero to four publications in tiers 1 and 2 journals (prestige). As with grant-based scientific capital, the values associated with low, moderate and high scores were determined by graphing the responses of each

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participant according to each indicator and observing the naturally-occurring breaks in the rank ordered data.

In one exception, I assigned a high ranking of publication-based scientific capital to an established scientist with moderate volume and productivity. Jack held a ten-year term as Director of a CIHR research institute which reduced his research commitment by half, explaining why his productivity in the five years preceding the study was only 2 articles per year. This position itself reflects a high level of scientific prestige. As well, he was the only participant to have hold the University Professor designation, the highest ranking academic position. In the five-year period before he began the Director position (1995-2000), he had published six journal articles per year. He also had the highest volume of tier 1 and tier 2 journal articles (18) of all participants, many of which were published in tier 1 journals.

Table 9. Ranking of Publication-Based Scientific Capital: Established Scientists

<table>
<thead>
<tr>
<th>Rank</th>
<th>Volume</th>
<th>Productivity</th>
<th>Prestige</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>&gt;150</td>
<td>4 or more per year</td>
<td>10 or more in tiers 1 and 2 journals</td>
</tr>
<tr>
<td>Moderate</td>
<td>50-150</td>
<td>2-3 per year</td>
<td>5-9 in tiers 1 and 2 journals</td>
</tr>
<tr>
<td>Low</td>
<td>&lt;50</td>
<td>1 per year</td>
<td>0-4 in tiers 1 and 2 journals</td>
</tr>
</tbody>
</table>

As seen in Table 9, a separate ranking scheme was used for early stage participants to take into account the shorter length of their careers. Early stage scientists scored high in volume of publication-based scientific capital ranked high on two of three indicators: over 25 publications in total (volume), three or more publications per year (productivity) and/or six or more publications in tiers 1 and 2 journals (prestige). Those with moderate volumes of publication-based scientific capital ranked moderate on two of three indicators: between 15 and 25 publications in total (volume), two publications per year (productivity) and/or three to five publications in tiers 1 and 2 journals (prestige). They also received a moderate score if they ranked low, moderate and high across the three indicators or low volume and productivity but high prestige. Those with a low overall volume of publication-based scientific capital ranked low on two of three indicators: less than fifteen publications in total (volume), one publication per year (productivity) and/or zero to two publications in tiers 1 and 2 journals (prestige).

The ranking schemes in Tables 9 and 10 are applied to the data in Table 11 to give
Table 10. Ranking of Publication-Based Scientific Capital: Early Stage Scientists

<table>
<thead>
<tr>
<th>Rank</th>
<th>Volume</th>
<th>Productivity</th>
<th>Prestige</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>&gt;25</td>
<td>3 or more per year</td>
<td>6 or more in tier 1 or 2 journals</td>
</tr>
<tr>
<td>Moderate</td>
<td>15-25</td>
<td>2 per year</td>
<td>3-5 in tier 1 or 2 journals</td>
</tr>
<tr>
<td>Low</td>
<td>&lt;15</td>
<td>1 per year</td>
<td>0-2 in tier 1 or 2 journals</td>
</tr>
</tbody>
</table>

each participant an overall ranking. Established scientists published between 25 and 200 publications over their career while early career stage participants published between 13 and 30 publications over their career. The most productive established scientists published from 4 to 6 publications per year within the past five years, similar to the most productive scientists in the early stage of their career, (4 publications per year).

Table 11. Volume of Publication-Based Scientific Capital

<table>
<thead>
<tr>
<th>Participant</th>
<th>Length of Career (years)</th>
<th>Peer-reviewed publications (total)</th>
<th>Peer-reviewed publications per year (past 5 years)</th>
<th>Peer-reviewed publications in tiers 1 and 2 journals (total)</th>
<th>Publication-Based Scientific Capital</th>
</tr>
</thead>
<tbody>
<tr>
<td>Established</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ivan</td>
<td>34</td>
<td>200</td>
<td>6</td>
<td>9</td>
<td>High</td>
</tr>
<tr>
<td>Sheldon</td>
<td>24</td>
<td>183</td>
<td>5</td>
<td>7</td>
<td>High</td>
</tr>
<tr>
<td>Robert</td>
<td>24</td>
<td>128</td>
<td>4</td>
<td>15</td>
<td>High</td>
</tr>
<tr>
<td>Thomas</td>
<td>24</td>
<td>98</td>
<td>5</td>
<td>10</td>
<td>High</td>
</tr>
<tr>
<td>Jack</td>
<td>30</td>
<td>90</td>
<td>2</td>
<td>18</td>
<td>High*</td>
</tr>
<tr>
<td>Sarah</td>
<td>16</td>
<td>49</td>
<td>3</td>
<td>16</td>
<td>Moderate</td>
</tr>
<tr>
<td>Jeremy</td>
<td>11</td>
<td>36</td>
<td>3</td>
<td>8</td>
<td>Moderate</td>
</tr>
<tr>
<td>Simon</td>
<td>19</td>
<td>34</td>
<td>1</td>
<td>6</td>
<td>Low</td>
</tr>
<tr>
<td>Marjory</td>
<td>19</td>
<td>42</td>
<td>2</td>
<td>1</td>
<td>Low</td>
</tr>
<tr>
<td>Gerald</td>
<td>13</td>
<td>34</td>
<td>1</td>
<td>3</td>
<td>Low</td>
</tr>
<tr>
<td>Dirk</td>
<td>15</td>
<td>25</td>
<td>1</td>
<td>3</td>
<td>Low</td>
</tr>
<tr>
<td>Early Stage</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alex</td>
<td>3</td>
<td>30</td>
<td>2</td>
<td>16</td>
<td>High</td>
</tr>
<tr>
<td>Vincent</td>
<td>8</td>
<td>27</td>
<td>4</td>
<td>4</td>
<td>High</td>
</tr>
<tr>
<td>David</td>
<td>1</td>
<td>21</td>
<td>4</td>
<td>18</td>
<td>High</td>
</tr>
<tr>
<td>Nathan</td>
<td>1</td>
<td>14</td>
<td>2</td>
<td>7</td>
<td>Moderate</td>
</tr>
<tr>
<td>Damien</td>
<td>5</td>
<td>14</td>
<td>1</td>
<td>6</td>
<td>Moderate</td>
</tr>
<tr>
<td>Mike</td>
<td>1</td>
<td>10</td>
<td>1</td>
<td>6</td>
<td>Moderate</td>
</tr>
<tr>
<td>Trevor</td>
<td>3</td>
<td>18</td>
<td>1</td>
<td>0</td>
<td>Low</td>
</tr>
<tr>
<td>Ralph</td>
<td>4</td>
<td>17</td>
<td>1</td>
<td>2</td>
<td>Low</td>
</tr>
<tr>
<td>Kate</td>
<td>6</td>
<td>13</td>
<td>1</td>
<td>4</td>
<td>Low</td>
</tr>
</tbody>
</table>
While early stage participants had, overall, a lower publication volume than their more established peers, the highest ranking early stage scientists had a higher volume and higher level of productivity than the lowest ranking late stage participants. It is interesting to note that the early stage scientists with the greatest grant and publication success (Alex, Vincent and David) were based out of what the university promoted as a “cutting edge” interdisciplinary research centre, rather than a traditional university department or hospital research institute. This may suggest that new scientists perceived as having star potential were more likely to be attracted and recruited to a centre that was founded on market-oriented grants and that has a distinct culture that fosters commercialization of research.

5.4.2. Volume of Entrepreneurial Capital

Indicators of entrepreneurial capital included patents held or pending, patents licensed to industry, industry funding, active involvement in product development and founding a company spin-off. Data on patents and licenses, industry funding and development of commercial products were derived from the Background Information Form while data on company spin-offs were drawn from interview transcripts. The number of patents held or pending was also verified using Canadian, US and Worldwide patent databases (CIPO 2008; USPTO 2008; WIPO 2008). As with scientific capital, a ranking scheme was devised to combine indicators into a single rank of low, moderate or high (see Table 12). The same scheme was used for early and established scientists as there were minimal differences between the two groups.

<table>
<thead>
<tr>
<th>Rank</th>
<th>Patents Held / Pending</th>
<th>Licenses</th>
<th>Industry Funding</th>
<th>Product Development</th>
<th>Company Founder</th>
</tr>
</thead>
<tbody>
<tr>
<td>High</td>
<td>&gt;3</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Moderate</td>
<td>1-3</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
<td>N/A</td>
</tr>
<tr>
<td>Low</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>N</td>
</tr>
</tbody>
</table>

Cut-off points for number of patents were determined using the same method used for grants and publications. A simple yes/no was used for the following indicators: licenses, industry funding, product development and company founder. Those assigned a “yes” received a high rank, while those assigned a “no” received a low rank on that indicator.
Participants were assigned a high ranking if they had more than three patents, held industry funding and had either a) licensed their patent to industry; b) were actively involved in product development; or c) had founded a spin-off company. They received a moderate ranking if they had between one and three patents. As the additional four indicators were based on a yes/no evaluation, they were not used to assign a moderate ranking. Those who did not meet any entrepreneurial criteria were ranked as low in entrepreneurial capital. The ranking scheme is applied to the data in Table 13 which summarizes the type and volume of entrepreneurial capital held by each participant.

**Table 13. Volume of Entrepreneurial Capital**

<table>
<thead>
<tr>
<th>Participants</th>
<th>Patents Held / Pending</th>
<th>Licenses</th>
<th>Industry Funding</th>
<th>Product Development</th>
<th>Company Founder</th>
<th>Entrepreneurial Capital</th>
</tr>
</thead>
<tbody>
<tr>
<td>Late Stage</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Sheldon</td>
<td>18</td>
<td>5</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>High</td>
</tr>
<tr>
<td>Robert</td>
<td>18</td>
<td>4</td>
<td>Y</td>
<td>N</td>
<td>Y</td>
<td>High</td>
</tr>
<tr>
<td>Thomas</td>
<td>4</td>
<td>0</td>
<td>Y</td>
<td>Y</td>
<td>N</td>
<td>High</td>
</tr>
<tr>
<td>Simon</td>
<td>2</td>
<td>0</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Gerald</td>
<td>2</td>
<td>0</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Jeremy</td>
<td>1</td>
<td>0</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Sarah</td>
<td>1</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Ivan</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Jack</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Marjory</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Dirk</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Early Stage</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Vincent</td>
<td>2</td>
<td>0</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>High</td>
</tr>
<tr>
<td>Trevor</td>
<td>1</td>
<td>0</td>
<td>Y</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Ralph</td>
<td>1</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>David</td>
<td>3</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Moderate</td>
</tr>
<tr>
<td>Alex</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Kate</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Mike</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Damien</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
<tr>
<td>Nathan</td>
<td>0</td>
<td>0</td>
<td>N</td>
<td>N</td>
<td>N</td>
<td>Low</td>
</tr>
</tbody>
</table>

Just over half of participants had moderate to high volumes of entrepreneurial capital (n=11). Late stage scientists, overall, were more likely to hold multiple patents, licenses, and industry funding. Seven established scientists held a total of 47 patents. Four early stage scientists held a total of 7 patents, although only two of these had been actively involved in generating intellectual property since beginning their
academic careers. One of the most entrepreneurial participants in the study, Vincent, was an early stage scientist. As a company founder actively involved in generating intellectual property, he was the only early stage scientist who had a high volume of entrepreneurial capital. The most entrepreneurial scientists, across career stages, were based in hospital research institutes or the university-based interdisciplinary research units rather than traditional academic departments.

In this section I have developed indicators and ranked scientists according to their volume of scientific and entrepreneurial capital. In the following section, I use these rankings to map the distribution of positions scientists held in social space.

5.5. Distribution of Positions in Social Space

Based on the ranking of participants with a low, moderate or high volume of scientific and entrepreneurial capital, I developed a conceptual representation of the distribution of positions in a field polarized according to competing visions of “good science”. I wrestled with the task of combining grant-based and publication-based capital into a global ranking. However, grants were not perceived as equivalent to publications as an indicator of “good science”. Because so many grants had criteria related to an economic as well as a scientific logic, research grants can be seen as a hybrid form of capital that may embody both a scientific and an economic logic. In order to make a clear distinction between positions based on capital conforming to a scientific logic and capital conforming to an economic logic, I therefore used publication-based scientific capital to represent the overall volume of scientific capital participants possessed.

Figure 2 illustrates the position each participant occupied relative to the other participants in the study based on the volume of scientific and entrepreneurial capital. Scientific capital (publication-based) is represented on the vertical axis and entrepreneurial capital on the horizontal axis. Scientists located in the upper right quadrant had high volumes of scientific and entrepreneurial capital. Those in the upper left quadrant had high scientific and low entrepreneurial capital. The lower left quadrant included scientists with low scientific and low entrepreneurial capital. None of the scientists in this study were located in the lower right quadrant, representing low scientific and high entrepreneurial capital. This is
consistent with the literature on academic entrepreneurs, which demonstrates that high performing scientists are more likely to be engaged in commercial activities (Van Looy et al. 2004). Scientists who had moderate levels of scientific or entrepreneurial capital are located along the horizontal or vertical axis. Although grant-based scientific capital was excluded from this conceptual representation, it is integrated into the analysis in the following chapter as a significant resource scientists drew upon in their strategies to navigate market-oriented funding.

While mapping of positions in social space is similar in appearance to the correspondence maps Bourdieu (1984; 1990b) used in *Distinction* and *Homo Academicus*, it is not based on a statistical analysis. Rather, it is a visual representation of the estimated volume of scientific and entrepreneurial capital participants held. Furthermore, it is not the only possible way of depicting social space, but serves the specific purpose of this study, which is to explore scientists’ perceptions and responses towards market-oriented science in relation to their positions in the scientific field. For example, a study exploring the attitudes of faculty towards diversity policies might depict social space along the dimensions of bodily or...
gender capital and scientific capital and explore relationships between positions and position-takings based on these different forms of capital. Therefore, while this map is a construction which has an interpretive function, it is a “realist construction” (Bourdieu 1999:618) which seeks to represent the objective reality of participants as accurately as possible by incorporating scientists’ perceptions of what functioned as legitimate capital and objective indicators of those forms of capital.

5.6. Conclusion

In this chapter, I have described the characteristics of study participants and mapped their objective positions of power in terms of the volume and type of capital held. I found that scientists identified two types of capital that conformed to two distinctly different logics: the logic of traditional science, as defined by scientists, and the logic of the market, as defined by policymakers and industry representatives. While scientific capital continued to function as the dominant form of recognition, entrepreneurial capital provided scientists with a new type of resource to compete for scientific recognition. The market logic embedded in funding programs created uncertainty in how scientists interpreted the value of publication-based scientific capital as a resource to gain grants and recognition according to institutional performance criteria.

In the final section of this chapter, I mapped the position each participant occupied in the scientific field along the dimensions of scientific and entrepreneurial capital. Through this process, I found that the participants in the study occupied a range of structural positions which, in keeping with the study’s theoretical framework, ought to be associated with a range of position-takings. Bourdieu (1990b) contrasts a sample that is structurally representative with a random sample:

In contrast to random sampling, which would dissolve the structures (especially since a structurally determining position can be represented by a very small number of people and sometimes...by a single person), this mode of selection enables us to characterize the positions of power through the properties and the powers of their holders. (P.76).

The sample is not meant to be representative of the whole population of basic scientists, but to enable an analysis of the relationships between the practices of scientists who occupy a range of positions and the structure of the scientific field. The goal of this type of analysis is to identify processes and mechanisms of social
reproduction and change within a particular context that may have broader applicability to other settings. Unlike many studies which focus solely on the perceptions and strategies of scientists who are industry-involved (e.g., Slaughter et al. 2004; Shinn and Lamy 2006; Tuunainen 2005), I also sought to capture symbolic struggles that may be occurring between scientists involved in industry or not. Including participants with a range of entrepreneurial experience enabled this exploration.

In the following chapter, I explore the space of position-takings – the strategies scientists used to impose their own definitions of “good science” and to acquire different forms of capital – in relation to the space of positions. I use the categories developed by Jennifer Todd (2005) to classify scientists’ responses to the market-oriented vision of science embedded in market-oriented funding and the dispositions (embodied capital) that generated these responses. I pay particular attention to harmony and dissonance between participants’ expectations of what it takes to be a “good scientist” and relationships between their volume and composition of capital and the strategies they used respond to market-oriented funding.
Chapter 6
Results Part 2 – Position-Takings towards Market-Oriented Science

6.1. Introduction

In this chapter, I analyze the perceptions of scientists towards market-oriented science and their strategies to achieve research funding and recognition. The chapter is divided into two parts. In the first, I present the position-takings of scientists trained before the rise of market-oriented science policy (established) and, in the second, those of participants trained after these policy changes (early stage). While Todd identified six logically possible responses at the level of collective identity to broad social or political change, the responses of the participants in this study fell into only three of her categories: reaffirmation, adaptation and assimilation (see Table 14). One group of scientists reaffirmed the identity category and practices of the traditional basic scientist and acted against market-oriented science. They resisted changes in the definition of legitimate science and in the boundaries between science and the market.

A second group of scientists retained a basic science identity but adapted their practices to successfully compete for funding and scientific capital, acting both with and against market-oriented science. While they continued to express a traditional scientific identity, they emphasized the applied dimensions of their research and the compatibility of fundamental science with their new market practices. Finally, a third group of scientists assimilated elements of the new vision into their identities and practices, as willing participants in market-oriented science. They valued both fundamental and applied dimensions of their research and the production of scientific and market products.

Table 14. Responses to Market-Oriented Science Policy and Programs

<table>
<thead>
<tr>
<th>Relationship between identity and practice</th>
<th>Partial Change (identity/practice)</th>
<th>No Change (identity/practice)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Acting against market-oriented science</td>
<td>Reaffirmation of traditional scientific identity and practices</td>
<td>Transperrancy and coherence</td>
</tr>
<tr>
<td>Acting with and against market-oriented science</td>
<td>Retention of traditional scientific identity</td>
<td>Adaptation of practices</td>
</tr>
<tr>
<td>Acting with market-oriented science</td>
<td>Assimilation of market-oriented identity and practices</td>
<td>Transperrancy and coherence</td>
</tr>
</tbody>
</table>

6.2. Position-Takings: Established Scientists

In Chapter 5, I sketched a conceptual representation of all the possible positions participants could occupy in social space based on their scientific and entrepreneurial capital. Figure 3 illustrates the position-takings of established scientists in relation to the positions established scientists occupied. The volume of scientific capital is represented on the vertical axis, moving from low at the bottom of the axis to high at the top. The volume of entrepreneurial capital is represented along the horizontal axis, moving from low on the left side of the axis to high on the right. Each circle in Figure 3 represents an individual scientist and the colour of the circle represents her or his position-taking towards market-oriented science. These position-takings represent different stances in a symbolic struggle over the legitimate definition of “good science”.

Figure 3. Positions and Position-Takings of Established Scientists

From this figure we can see that four established scientists reaffirmed traditional science, four adapted, and three assimilated to market-oriented science. Each of these position-takings will be discussed in turn.
6.2.1. Reaffirmation: Resistance and Rhetorical Strategies

Despite varying levels of entrepreneurial and scientific capital, Marjory, Dirk, Ivan and Robert expressed opposition to market-oriented science policy. Robert, a hospital-based cancer gene scientist, had a high volume of entrepreneurial and scientific capital. Holding 18 patents, 4 licenses, industry funding and a company co-founder, Robert was one of the most entrepreneurial participants in this study. He had a total volume of 128 publications and an annual productivity of 4 publications per year in the five years preceding the study. Robert also held the largest total volume of grants ($45.5 million). Ivan had a high volume of scientific capital and a low volume of entrepreneurial capital. A biochemist, Ivan carried out bone research in the Faculty of Medicine and Faculty of Dentistry. One of the most senior participants in this study with 34 years experience, Ivan had the highest total volume of publications (200) and the highest annual productivity (6 publications per year). While he had some limited industry experience, this had not translated into the accumulation of entrepreneurial capital. Marjory and Dirk had low scientific and low entrepreneurial capital. Both were also located in the Faculty of Arts and Sciences, had fewer than 50 publications in total and published one to two publications per year. Neither had any entrepreneurial capital. Marjory was a molecular biologist who studied reproductive health and Dirk was a developmental biologist who studied cell differentiation processes.

Despite differences in type and volume of capital, entrepreneurial experience and institutional setting, these scientists experienced market-oriented science as a threat to the traditional symbolic order of science and their own location in the scientific field. In the symbolic struggle between competing definitions of “good science”, these scientists reaffirmed traditional science by rejecting a market-oriented vision of science and by pursuing a traditional path of accumulation of scientific capital. They upheld a traditional vision which holds that discovery is unpredictable, must be carried out using rigorous scientific methods, and can only be evaluated by experts through the process of peer review.

One argument scientists used to uphold this traditional vision of science was the “serendipity argument”. Scientific discovery, it is claimed, is fundamentally unpredictable and the most successful strategy for producing discoveries that lead to
useful innovations is to invest in high quality science as determined through the peer review process. Moreover, the premise of market-oriented policy and funding is flawed because it requires scientists to predict where the most important and useful discoveries will come from. This common perception is reflected in this quote from Marjory:

The big problem with this whole policy is that it’s being driven by people who do not understand fundamental sciences and one of the fundamental things about science is that it’s completely of an exploratory nature. We’re trying to find out things we don’t know. How are we supposed to predict, if we’re going off into the unknown, what kinds of commercialization might come from that?

Interestingly, these participants almost always framed their arguments around public investment in science according to an economic definition of the “public good”. The language of economic competitiveness and innovation permeated their defence of traditional basic science. They argued that basic science is more likely than market-oriented science to lead to innovative research with social and economic applications, referring to the discovery of penicillin, the cloning of Dolly the sheep, and the discovery of RNA1. It is important to note, however, that they did not see basic science itself as incompatible with utility. As Dirk noted:

I mean I wouldn’t mind if my work would be useful (chuckle). I wouldn’t be opposed to that. But you cannot go directly sometimes, in a certain kind of direction. Very often you have to find out some basic things and you don’t see where it leads.

Therefore traditional scientific mechanisms of investigator-driven operating grants and traditional peer-review evaluation were defended as the best mechanisms for promoting the scientific discoveries that drive innovation.

Only one participant in this study – Marjory – used a non-economic argument to oppose market-oriented science. She saw industry-driven research as a potential threat to public safety. Marjory’s research, on reproduction and fertility, had great potential for application in a highly profitable field. Yet, she argued that profit-motivated science is bad science, and puts the health and safety of the public at risk.

In my personal opinion, the vast vault of the research in the assisted reproductive technologies field is what I would consider to be bad science. Bad science, because almost all of it is being conducted in humans and therefore you cannot do the proper controlled experiments. So it gets back to what’s good science and what’s bad science. On the good science, you have to be able to perform your experiment in a controlled way. But with assisted reproductive technologies, you can’t do those controlled
experiments because it’s with humans. So a lot of it is, in my opinion, very bad science.

According to Marjory, a fertility treatment developed by industry had been found to put pregnant women at risk of a life-threatening condition. Not only did market-oriented funding require scientists to meet criteria at odds with the logic of science, it was also perceived as diverting funding away from “good scientists” to those who fit a non-scientific set of criteria. This perspective is reflected in the following quote from Ivan, who argued that targeted and disease-specific funding were taking money away from high quality basic science and diverting it into poorer quality targeted and applied science.

Because of how things are being set up, a lot of money is going to people that wouldn't ordinarily get the grant. So there's a shift towards people that are not as strong scientifically, but they fit the paradigm. It's funding people that probably wouldn't have been successful in the individual (operating grants) committees. If you're starving your best scientists to do this kind of thing, is this the best way to go? I don't think so.

Robert expressed a similar perspective that science is being rewarded on the basis of illegitimate criteria.

There is more money being diverted into what they call RFAs (requests for applications) which are targeted. And that sometimes takes the process to a non-peer reviewed kind of level, because there are few groups that can actually answer the RFA. It’s almost as though the money’s being directed at some groups.

According to this argument, the long-run impact of the diversion of funding to translational research is a reduction in both the volume and quality of basic knowledge. Market-oriented funding programs were perceived as not only steering research into particular topics, but also as eroding the process of peer-review. Without peer-review to ensure a high quality of science, market-oriented funding programs may not produce “good science”. Surprisingly, Robert, one of the most entrepreneurial participants in the study, took one of the strongest stances against industry collaboration and targeted and matched funding.

Especially when industry gets involved, that can work the whole review process. Some of these panels have industry and government representatives. Genome Canada is very big on bean counting, not just the money, but counting, how many assays you have done. The reporting is very severe. You’re reporting every three months. It's almost as though they don't care what the result is. They just want to make sure that somebody is turning the crank somewhere. And so these things sound a bit like too much
government intervention to me. Science doesn't work very well that way. *I think we need to scream and yell that we're on a bad course right here.* There's nothing wrong with the review that traditionally has been done by grants panels for NCI (National Cancer Institute), for all the other sorts of science areas in NSERC and MRC in the old days. That whole format, there's nothing wrong with that at all. It's the only way you can do it.

Symbolically, through their active resistance to a market-oriented vision of science, these scientists defended a traditional vision of “good science”. They engaged in boundary work to demarcate “science” from “non-science” and “good science” from “bad science” (Gieryn 1983, 1999), differentiating between research that is exploratory, unpredictable, theory-driven, subject to peer-review and therefore “scientific”, and research that is developmental, application-oriented, assessed by politicians and business people and therefore “non-scientific”. By distinguishing, symbolically, between a scientific vision of legitimate science and a market-oriented vision, they sought to uphold the traditional symbolic order and to defend their own position within the scientific field. The strategies they used to acquire funding and scientific prestige arose from a traditional scientific habitus and conformed to a traditional path of accumulation of scientific capital. At the same time, they recognized the reality of external pressures for relevance and the threat this posed to their own status. In response to the dissonance they experienced between their perceptions of scientific value, rooted in a traditional scientific habitus, and the criteria funders were using to evaluate scientific quality, they engaged in strategies of minimal compliance. They opted out of commercialization grants, “tailored” grant applications to meet criteria (Calvert 2006) or “selectively appropriated” grants with commercialization criteria that did not require them to change their research topics or practices (Morris and Rip 2006).

All four established scientists who reaffirmed a traditional scientific identity relied primarily on CIHR and NSERC investigator-driven grants to fund their research. They also held some forms of market-oriented funding. Marjory was the principal investigator on a multi-investigator CIHR Strategic Grant ($3.9 million). Dirk and Robert held CFI infrastructure grants. Robert held a CRC, was co-investigator on a multimillion dollar multi-investigator Genome Canada grant and had applied for a CIHR Proof of Principle grant. All had used what Calvert (2006) calls “tailoring” strategies to emphasize the applied potential of their research without altering their
research practices in order to acquire funding, while Marjory’s CIHR grant was targeted towards a specific health problem but did not require a commercial justification or business plan. It was relatively easy for her to present the potential applications associated with her research. She herself had no intention of producing commercial products even though patent applications were used as performance measures by this grant program\textsuperscript{10}. Although she did not hold a CFI grant, she described them as “nightmarish in terms of all these commercial connections you have to make with people who speak a totally foreign language”.

Dirk also tailored his research in order to acquire a CFI infrastructure grant which included a Benefits to Canada section. In his first draft he did not adequately link the cell differentiation process he was studying with potential disease processes. The grant application was vetted by the university research office and returned to him for revisions. In the second draft, he “made up” a concrete application. In his words, “It wasn’t invented, but it was really stretching it.” As he further elaborated:

This is in fact what we do and I have become very good at it, apparently, but it’s cheating in a way. In the end, I will do whatever comes up. Of course, right? This is just an exercise of writing down in detail about what I plan to do and then, when the first results which comes in which are different, it will be a different direction altogether. And nobody cares at the end.

The inability to gain funding for research without an obvious link to application made lead Dirk to carry out some of his own research in secret:

I did some research and found something I think is a very interesting deep problem and I want to follow it up but it's not in my new grant application because it cannot be funded. It's risky and nobody could justify that it is health related or has any benefit. So I will do that on the side. I just do it on my own with the money I have, and don't tell anyone, and if it's good nobody will care.

While the literature on the “secrecy effect” has focused on the threat of industry involvement to the open norms in science (Murray and Stern 2007; Slaughter et al. 2004), this suggests that a different kind of secrecy may arise among scientists who resist market involvement. In order to make their research fit market-oriented

\textsuperscript{10} “The evaluation of strategic activities of the CIHR Institute of Human Development, Child and Youth Health will include evaluation of the outcomes of this RFA. We shall seek evidence, from research programs funded through this RFA of measurements of scientific accomplishments (peer reviewed papers, presentations), evidence of new integration between health research pillars, evidence of successes of activities related to trainees, evidence of new patent applications, when appropriate, and/or recommendations for improvements to clinical practice or health policy guidelines” (CIHR 2010).
agendas, scientists may be pushed to carry out fundamental research in secret. This may in fact be one way that scientists resist having their research agendas “skewed” or steered towards applied and market-relevant topics.

Both Dirk and Marjory carried out research that could be considered “close to the market”. They used mammal models, which tend to be viewed by funding agencies as closer to the market than non-mammal models, such as yeast or fruit flies, and carried out biological research with direct implications for specific health problems. Yet neither perceived market-oriented practices as in the realm of the possible. They had never applied for grants that required them to produce commercial products, nor did they see this as something they could do. Marjory described herself as being “completely lost” if she had to attempt this.

So far I have not had to do that. But if, if there is some sort of policy change that requires us to build in some kind of commercial aspect to our research, I am going to be completely lost. I have no idea how I could possibly write something that is going to make any kind of sense to me. For basic scientists to have to reorder the way they think about things is very difficult. We’re not trained as businessmen.

For Dirk, the availability of grants that required matched industry funding or the development of therapeutics did not even appear on his radar as possible funding sources because of his orientation towards fundamental science. As he put it:

Pressure, no. I mean, if I would get pressured, I would say, this is the wrong person. I mean, I respect that kind of research, but this is a different kind of research, right? This needs different education and everything. I have no problem with that kind of thing but not everybody can do the same kind of thing, right? Some people have to find out basic things, basic science. And others who are more interested in applications, they should do that and get selected by engineers over there. Engineers do that kind of thing, take basic research findings and try to apply it. Which is a great thing. But it’s not me…I mean, if we are pressured to do good science, I think that should be good enough.

For Dirk, the shift towards an applied culture in science produced a sense of bewilderment.

I grew up in a tradition where science was something respectable in itself. So when you want to study the universe, you don't talk about applications. You just want to know, right? It's kind of a culture shock for me that everybody, including students, agencies, “What's the use of it? Why are you doing that? What can be cured?”
While Marjory and Dirk were unable to see themselves altering their practices to take advantage of market-oriented opportunities, they were aware that a shift in culture would put them at a disadvantage. They would be increasingly cut off from resources needed to carry out science. While she stopped short of saying that it would change the nature of scientific knowledge produced, Marjory described how a business culture would change the kind of scientist able to carry out scientific research:

I think when it comes to funding, if the way you are thinking as a scientist is not going to be successful in getting you the money to do the science you want to do, then what’s going to happen is that those people who do have that sort of business ethos will be the ones who get the money. I am not saying that those people would necessarily conduct their science in a different way. But it would certainly introduce a different culture.

To hold their ground and successfully compete for operating grants, and continue to opt out of larger market-oriented grants, they experienced heightened pressure to publish, described by Dirk as “cruel”.

Like Marjory and Dirk, Ivan and Robert both restricted their reliance on market-oriented funding. However, because both had very high volumes of publication-based scientific capital, they were in a better position to compete for investigator-driven grants. Ivan, a biochemist affiliated with the Faculty of Medicine, carried out research with direct implications for dental and bone health and most of his grants came from CIHR. He attributed his funding success to his ability to “structure his research so that it sells”. Over his career he had witnessed a shift towards disease-specific funding and team-based grants that required basic scientists to collaborate with clinicians and to meet milestones established by non-scientist stakeholders. Only one year away from retirement, he was not concerned about funding his own research.

I can sort of stand back for this 'cause I mean I'm basically one year from normal retirement. So, as long as I have my grants or a grant, I can continue as long as I want. I don't have to worry too much about this. I have a grant for five years, so I plan to – as long as I am interested in doing it and able to do it, I'll keep going. But if we have problems getting funding for a team grant or something like that, it's not going to really impact on me.

Robert, a cancer geneticist, also had few concerns with funding his research. Although Robert held market-oriented grants, he refused to alter his research agenda or practices to bring them into line with market-oriented funding: “I can get money from other places that don't have those strings.” Because he worked in a “hot topic”
area (cancer genetics) with other industry-involved scientists, he was a co-investigator on a large Genome Canada grant. Although Genome Canada grants have specific commercialization criteria, Robert limited his contribution towards its scientific rather than commercial components. He had also filed an application for a CIHR Proof of Principal (POP) grant that would fund the geno-typing of a large patient population. He described the CIHR POP grant was “a perfect fit” for a project he already intended to do. Robert both “tailored” (Calvert 2006) his research and used a “selective appropriation” strategy (Morris and Rip 2006). Scientists who selectively appropriate funding programs only apply to those that require minimal change in practice and avoid others with criteria that would be more difficult to meet. Robert was able to use his accumulated entrepreneurial capital to access certain types of market-oriented grants without engaging in market-oriented practices.

Unlike Marjory and Dirk, who could not conceive of themselves engaging in market-oriented practices, Ivan and Robert both had past industry experience dating back to the mid-1990s. Ivan was motivated to collaborate with industry when he made a discovery from his basic research program that had a clear health application. Ivan tried to patent a technique for the early identification of a certain dental disease. The patent application failed due to similarities with another patent, but a company that was interested in the technology invested a small amount of money in the research to bring the product to the market. In the end, as Ivan noted, the investment did not advance the science or produce a product:

They did provide some money, I think it was like $30,000, but that's peanuts. And so I was spending a lot of time trying to come up with innovative ideas of developing different techniques that could be used in a dentist office. After about a year or so, could be eighteen months, I mean every time we'd meet with the company they'd say, "Well, how is it going?" We would say, "Well, we've made progress". "Can you give us a date when you're going to have this done?" I said, "No, because this is research, we can't". But they want a date...and I was getting increasingly annoyed with that because here they were, and I understand their point of view, they want to get a product to make money. But they weren't giving us a lot to do this with. We were doing it mostly on our own time. And at a point I got so totally frustrated, I just backed out of it. It actually soured me a lot to getting involved in those other things because it took a lot of time. And either to the company or to myself, in terms of academic development and any monetary reward, nothing came out of it.
The contradictions Ivan experienced between the demands of the market and the demands of science reinforced his traditional scientific identity. His investment of time and effort did not result in any successful patents, licenses or a significant investment of industry funding that helped him to advance his own basic research agenda. Nor did he acquire entrepreneurial capital that could be leveraged into acquiring market-oriented funding. However, because he had a very successful academic career, neither his unproductive experience with industry nor his choice to opt out of market-oriented funding had negative consequences for his own status. His objections to market-oriented funding were not, as he states in the following quote, based on “sour grapes”:

In some ways I can say this because I've been successful. If I hadn't been successful, a lot of what I'm saying could be like bitterness: because I wasn't successful I think the system's wrong. But I can't say that for me because, in terms of grants, I've been very successful. But it doesn't mean to say that I think that the granting system is the way it should be.

Robert, in contrast, because he had invested and lost much more in his market experience, described himself as “jaded” by the experience. Robert had the highest volume of entrepreneurial capital relative to other study participants, with 18 patents, 4 licenses and extensive experience as a co-founder of a spin-off company. As a cancer gene researcher, Robert and his colleagues made a discovery that had high potential for the development of a new cancer drug. Together, they raised several million dollars from venture capitalists and founded a biotechnology company to develop the treatment and also carry out further basic research. He left his academic position to manage the company, describing the early optimism he felt:

Oh, it was very exciting. We thought we were close to having something practical to treat cancer, so that's kind of a Florence Nightingale kind of feeling. It's just one of those things you think, “God, this has got to be perfect” (laugh). Well, nothing's perfect. It was for a short time, it was! It was wonderful. But then reality stepped in.

Over time, this optimism waned as conflicts arose between the goals of scientists and the goals of industry. When this happened, he described the business agenda as taking over the scientific agenda.

Looking back on it now, I probably wouldn't have done those things left up to my own devices. It seemed to be just turning the crank. It wasn't very innovative. It would never get in Science or Nature, let me tell you. Money was being spent and efforts were being made and it wasn't really my agenda. My agenda is to discover and to reveal new things about genes and
proteins and systems that relate to these diseases. It's not to test drugs. I don't really want to test drugs unless the drug is something that's a real tool for us that can help us uncover something. You have to separate discovery research from technology. Technology is applying the discovery. Everybody knows how to do that. It’s not science.

Out of frustration, Robert left the company. A short time later, a new CEO was hired to manage the company but his “real agenda”, according to Robert, was to dissolve the company and disperse the money. The new CEO sold $23 million in assets and then sold the company. The scientists who formed the company did not see any of this money: “everybody else lined their pockets and then disappeared”. Robert lost his own personal financial investment in the company but, even worse, was the time and scientific status he sacrificed to the company. Because he had withdrawn from the academic sphere and invested so much in the company, it took time for him to re-establish his grants and research productivity when he returned to his academic position.

Robert followed what Shinn and Lamy (2006) call an “imitation mode of coordination” (p. 1470); he adopted the values and practices of business and spent more time away from the lab than in the lab, failing to maintain clear boundaries between his scientific and market-oriented practices. In a study of academic firm founders in France, Shinn and Lamy found that scientist-entrepreneurs who adopted an imitation mode of coordination experienced a sharp decline in their scientific productivity after founding their firm. Scientist-entrepreneurs who subordinated commercial goals to scientific goals, in a strategic mode of coordination, or alternated equally between science and the market, in a sequential mode of coordination, maintained or improved their productivity both scientifically and commercially. Despite his negative industry experience, Robert was not opposed to market-oriented practices: “I think there's nothing wrong with patenting. It really does give you an opportunity at some point to commercialise or benefit from that. Patenting is fine.” What he opposed was having science funding contingent upon industry collaboration. He worried that industry-driven funding programs at CIHR and NSERC that required scientists to engage in what he called “technology-type exercises” may reduce the freedom of scientists to carry out legitimate science. Like Dirk, Robert experienced a general sense of being devalued for doing the work he most valued. In reference to what gets recognized by the Canadian public and the media, he said:
I'm not looking for accolades in that way. I mean, we get our approval from our peers and there's awards and international recognition. It's not a question of that, but it would be nice if science was recognized for what it is. And that's the engine that generates ideas for technology. It's not generating the technology itself. It generates the ideas for it and in that way you can advance technology. But in itself it's its own pursuit, right?

Because of his high volume of entrepreneurial capital, it appears unlikely that Robert will be able to sustain a coherent stance against market-oriented science. In fact, he was already moving towards a more adaptive stance, drawing on the knowledge he gained from his past experience and was in the planning stages of developing a new “virtual company”. However, to maintain scientific control over the project, he intended to do it without investors.

I don't want them to tell me again what to do (laughs). I'm just not on. I was the vice-president for a while at (name of company spin-off), but I might as well have been asleep on the floor. There's science and then there's business, and business always wins in the sense of what the agenda is.

In summary, scientists who reaffirmed their traditional scientific identity and practice experienced misalignment between their scientific habitus and the conditions of the field. They responded with symbolic strategies that attempted to reassert earlier field conditions, rather than to bring their own practices into alignment with changing expectations. Marjory and Dirk were unable to mould themselves to the new market logic, while Ivan and Robert found the contradictions between the market logic and scientific logic irresolvable. They experienced an internal coherence between their traditional scientific identities and practices, yet this came at a cost of feeling undervalued and having to hide the true nature of their research. Scientists who adapted to changes in the conditions of the field, on the other hand, were able to use market-oriented funding as a resource to maintain or improve their position as funding conditions changed. While they retained a traditional basic science identity, they brought their practices into greater alignment with the objective conditions of the field through a hybrid strategy of accumulation and conversion between scientific and entrepreneurial capital. It is to this group that we now turn.

6.2.2. Adaptation: Contradictions, Complementarities and Conversions

Four of eleven established scientists, Thomas, Sarah, Simon and Gerald, adopted the position-taking of adaptation. Like the previous group, scientists who adapted to market-oriented science occupied a range of positions in the scientific status hierarchy
(see Figure 3). However, all had moderate to high levels of entrepreneurial capital, reflecting their involvement in market-oriented practices. Thomas possessed a high volume of both scientific and entrepreneurial capital; Sarah held a moderate volume of both scientific and entrepreneurial capital; and Simon and Gerald each had low scientific and moderate entrepreneurial capital. Thomas, a hospital-based biochemist who studied protein structures related to infectious diseases, had just under 100 publications in total and an annual productivity rate of 5 publications per year. He held four patents and was intensely involved in industry collaboration to develop a promising new drug treatment. He also held the second highest volume of total funding ($13.7 million), over three quarters of which came from market-oriented grants.

Sarah carried out biochemical research on proteins related to infectious diseases that can also cause cancer, in the Faculty of Medicine. In contrast with other scientists who adapted to market-oriented science, Sarah derived less than half of her research funding ($2.9 million) from market-oriented grants and none from industry funding. In fact, the only market-oriented grant she held was a CRC. She did hold one patent at the time of the study.

Simon and Gerald, both molecular biologists, had published 34 peer-reviewed articles in total and an average of one publication per year in the five years preceding the study; each held 2 patents and had received industry funding. Simon was affiliated with the Faculty of Medicine, but his lab was located in the prestigious university interdisciplinary research centre where he studied gene families in fruit flies and zebra fish. He held seven grants worth $5.7 million. Over half of this funding came from CIHR grants with commercialization criteria. Gerald worked in the Faculty of Dentistry (and was cross-appointed to the Faculty of Medicine) where he carried out research in the area of bone biochemistry. He held nine research grants worth $3.8 million, two-thirds of which came from grants with commercialization criteria.

These four “adapters” viewed market-oriented funding as both a resource and a barrier to “good science”. They shared many of the same objections to market-oriented funding criteria as scientists who reaffirmed traditional science and carried out boundary work to demarcate “science” from “non-science” and “good science” from “bad science” (Gieryn 1983, 1999). Like “reaffirmers”, adapters also defended a
vision of “good science” based on serendipity, methodological rigour and peer-review. Even those who benefited from market-oriented funding, perceived it as diverting money away from “good science”, as reflected in this quote from Simon:

> Most of our huge findings today have come from very basic research. It couldn’t have been foretold. And yet what the government’s doing now is taking money away from basic research, and ours isn’t even that basic, and trying to put it on top of these strategic things. Down the line, we’re going to starve the system. What the problem with the applied stuff and the attention it’s getting, is that they are sucking money away from the people doing the important basic research and trying to redirect it.

One way market-oriented funding was perceived as reducing the quality of science was by targeting research towards disease-specific areas. Non-specific or general research that seeks to understand fundamental biological mechanisms may end up having relevance for a variety of diseases that could not have been anticipated (Calvert 2006). This perception is reflected in the following quote from Sarah:

> The thing is you don't know where those important discoveries are going to come from. Let's say, I'm going to put this pool of money for breast cancer. But somebody working in a basic area may make the observation that ends up being key for breast cancer. And if they're really good, they'll pay attention to that. They'll think of the implications and what that might mean. They may not have been intentionally working on cancer, but they might find themselves with a big discovery that all of a sudden helps. Many of the very important medical discoveries have been made completely accidentally by good people who really pay attention and recognize when they've got something.

Even when it was distributed through peer-review processes, research funded through targeted competitions was considered to be of lower scientific quality. Because fewer scientists are eligible to apply for targeted funding than for operating grants, the success rates in targeted grant competitions tend to be significantly higher. Sarah expressed what she considered to be a common objection to targeted funding among her peers:

> I think I certainly can speak for most of my colleagues in saying that in general we're quite against targeted funding. Because in an open competition, the best possible science wins. What happens if you start to sort of pigeon-hole money and topics, first of all you're taking that money out of the general flow, so less excellent grants get funded. Researchers who are struggling and can't get funding will tend to put in a grant on the targeted topic because they know it's much easier to get funding. The cut-off is not the same. I just don't agree with that.
A common problem identified with matched funding programs was the time wasted procuring matches in order to meet funding criteria even when scientists and industry supporters lacked a genuine shared research interest. Scientists used their industry relationships to gain access to grants, but often the matches made were in-kind and may not have had the impact on industry collaboration that funders intended. According to Thomas, there were legitimate matches, where scientists and industry shared a common research question and engaged in genuine collaboration, and matches that were what he called a “public relations exercise”. Knowledge-based industries increased their credibility by making contributions to scientific research, while scientists gained access to resources, without really engaging in collaboration.

The question is, whether we might not see a lot of…window dressing? A lot of matching stuff, they’re now in-kind. How do you evaluate in-kind? Yes, I know, there are the NSERC rules. But for a large part, and we’ve gone through that with the ORDCF, it’s a negotiation. Whether that’s a real match or not is a different thing.

The need to constantly invest in the appearance of being applied and industry-involved was something that scientists universally opposed as it took time away from doing the actual research.

Like “reaffirmers”, by emphasizing their commitment to advancing scientific knowledge and resisting market-oriented funding criteria that impeded their scientific goals, “adapters” reinforced their traditional science identity. Unlike “reaffirmers”, they used a hybrid strategy of accumulation of both scientific and entrepreneurial capital. This group of participants acquired skills in developing a business case, networked with clinicians, industry representatives, technology transfer staff and venture capitalists, applied for patents, and altered their research topics and research methods. They actively struggled to learn a new set of market-oriented skills and competencies (Packer and Webster 1996). “Adapters” became involved in market practices through a variety of routes, including informal networks with colleagues who were market-involved, serendipitous discoveries with obvious potential for application, frustration with lack of resources through traditional funding channels, and membership in formal networks and funding programs designed to encourage industry collaboration. They also had varying levels of success in gaining access to market-oriented resources and navigating the contradictions between market and scientific practices. While Simon, Gerald and Thomas had all made significant
changes in their research agendas and practices, only Simon perceived himself as raising himself up to a new level of success.

For Simon, direct industry involvement preceded his involvement in market-oriented funding. Frustration with not having the resources to carry out the kind of large-scale experiments he envisioned led him to connect with industry funders. Although he was a full Professor and a member of a prestigious interdisciplinary research institute, he could not carry out the kind of research he wanted to (and the recognition that came with it) by pursuing traditional funding channels. Changes in the nature of genomics research itself demanded a new style of research funding which, according to Simon, was more likely to be provided through industry and new funding programs with commercialization criteria.

The granting agencies now, it’s been harder and harder to get the funding levels needed to do anything like this. Most of the funding here, a hundred, a hundred and fifty thousand dollars at a time, will only let you do work on a few genes at a time. And what we found, that’s amazing and productive, is looking at whole sets of genes. And the findings you can discern and where you go from that are exponentially greater. For two or three times as much money, we learn ten or a hundred times more and move much, much faster. But the grants aren’t really meant for that kind of work here. It’s an old style.

Through his informal social network and close working relationships with other industry-involved scientists, Simon made industry connections that led to his first patent application. These new relationships and practices required him to shift his perspective from thinking in terms of scientific value to thinking in terms of economic value. Simon struggled with being able to communicate the value of his scientific discoveries within a market logic. This is illustrated in his discussion of his difficulty in writing a business case for a CIHR Proof of Principle competition.

We did get a one year CIHR Proof of Principal grant eventually to help us move the research a little bit further along, to show what it could do and to help with patent costs. But the problem is that we are not business people and don’t speak “businessese”. I had to submit that Proof of Principal grant two or three times. And the science part would every time get in the 4.2 to 4.7 range out of 5. The business model would get a 3 point something out of 5 because we hadn’t written it out in a full detailed business plan form with the right terminology addressing the right things.

Despite these difficulties, Simon was enthusiastic about the possibilities that industry collaboration could offer in terms of advancing his scientific agenda. In fact, he was
in the early stages of exploring founding a company spin-off. He described the benefits of this spin-off as both contributing to the economy and advancing science.

I figure if we could spin off a company and get the kind of investment that goes into that, we could really move so much faster. There would be a double benefit. The findings that we could make in the health related area could actually help the local economy in many different ways. It would be innovation coming to Ontario and jobs and products and health benefits. Not only that, but we would take all the findings and the tools that we’ve used and do the science to understand how the pathways work genetically and molecularly. That, in turn, goes back to help the company develop things that have other uses. And so it’s just a win-win.

As a result of his increasing engagement with the market and as a strategy to acquire market-oriented resources, Simon had diverted a significant portion of his lab to developmental research. To bring his discoveries closer to the application stage, he shifted a portion of his research from a fruit fly to a zebra fish model organism. As a mammal model, zebra fish are closer to humans than fruit flies and, thus, his research moved one step closer to the clinical trial stage. An extensive investment of time was required to learn about the new organism and acquire the new infrastructure (e.g., aquariums) to support this new work.

Simon attributed the major advances in his research to the resources he gained from industry that allowed him to investigate clusters of genes rather than individual genes. Since receiving industry funding, he had published five papers in tier 1 journals, and anticipated publishing a rash more.

It was only getting the funding that we did from industry that allowed me to get to that level. It took us up to a whole new level of productivity and quality. We never would have gotten there with granting agencies the standard way. It’s only now starting to really come out, too. It took three or four years to use that funding to develop the infrastructure, the talent pool to start making discoveries. This year we should have a half dozen of these truly big papers. In previous years, it was one or two a year.

By adapting to the market opportunities, Simon was finally able to realize his scientific expectations and finally experience joy and satisfaction in his work.

I do get less time with the people in the lab, but as the lab grows, they start to provide the seniority and expertise. The quality of the people in the lab makes up for me not being in there. And I get to look at all of the data and synthesize the stories and provide the direction. Just looking at the results come out...the more people, the faster it comes, the more satisfying and rewarding it is, the more fun. I think I really only truly enjoyed my job
without frustration in the past five or fewer years. It’s being able to do stuff that I envisioned.

Like Simon, Thomas reported diverting a portion of his research to applied projects that took him further and further from his own field of expertise. Thomas held much larger and diverse kinds of market-oriented funding than the other adapters. He had more resources to invest in the accumulation of entrepreneurial capital as he oversaw two research labs, had a large technical staff including a full-time grant-writer and had many graduate students and post-doctoral fellows. Thomas did not feel forced or pressured into changing his practice to fund his research, but he described himself as tempted.

I’m kind looking for the right word. Enticed is too positive, forced is too negative, somewhere in between. I feel tempted, and not just because it’s interesting. Also tempted because I think I can secure funding down the road, or make sure I get it in the right direction, but also keep my hand in the money pot.

He believe that industry-involvement was neither good nor bad for one’s reputation as a scientist, but it did allow a scientist to access more resources than traditional funding sources.

No one is putting a gun to my head and saying, “You have to be more applied.” It’s just sources. Pots of money. Not evaluation so much. I don’t think that really goes in there. It used to be, it was bad if you go with industry. I don’t think you get punished anymore. But there’s just much more money in those pots. And some of them, at least in the past, were so easy to get. You know, you wrote 5 pages and got 20 million.

Thomas was in an advanced stage of translating a “serendipitous” discovery made in his lab into a potential therapeutic. He was collaborating with pharmacists, clinicians and pharmaceutical companies to move into the stage of clinical trials on a new treatment for a common infectious disease. Because of his extensive industry connections, it was easy for Thomas to make a phone call and have a company provide matched funding.

For matched funding I have $50,000 from (name of company). I asked Mike, I called him up, “Can you do me a favour?” Because we had collaborated on a small project, nothing very serious just a small project. And he said “Yeah. Alright.” And so I had 50 thousand more than the other people.
Through his applied and commercial research, Thomas felt that he was moving further and further away from his area of scientific expertise.

On the whole, scientists who engaged in identity reconstruction to foreground aspects of their dispositions that were compatible with market-oriented funding criteria were satisfied with the benefits they received. Yet contradictions between the demands of the market and the demands of science led to internal tensions and ambiguities that are best reflected by the words of Thomas.

Yes my research has changed. I’m not sure it would have changed if we had a different funding climate. I was really forced to think about this. Fortunately we got something (a potential drug). If we hadn’t, I would feel much worse. With that came like an admission that this might actually be fun up to a point. I’m not going to take it into a company, I have no time…but I’d be happy to sit on an advisory board to see my contribution go further with a limited time commitment. So I’ve changed my attitude a little. That may be the positive spin.

The negative aspects were that he did not get to have as much “fun” scientifically in his research. What happened to quote on the negative side?

While Simon and Thomas both expressed some degree of choice in adapting their practices, Gerald stated clearly that his survival as a research scientist depended on it. He described himself as “skating on thin ice” in terms of a career that left him no choice but to jump through whatever hoops the funding agencies required. As he put it:

My basic feeling is that there was a top-down decision to get people more into applied sciences. And it's been successful. It's half successful, because yes, in my lab we're doing applied science, which we never did before. We're getting money to get intellectual property, which I never did before. I'm talking to pharmaceutical companies, which I never did before. So the last five years I'm doing all this stuff that I never did before. And I was pushed into it. I didn't do it of my own volition, I did this because I was following the money and they made it quite clear: if you want the money, this is what you gotta do. And so I'm jumping through the hoop.

Gerald distinguished clearly between doing “good science” and generating intellectual property (IP), but he claimed that market-oriented funding did not detract from the quality of science he produced.

I don't care where the money comes from. I will get the money and I will do what I think is excellent science with it. If someone says, “We'll give you $50,000 if you can get some IP”, fine, I'll do whatever you want. Just give me the $50,000. We'll do good science and hopefully we'll get some IP
out of it. But my primary goal is to do good science. But if I have to write a grant saying I will attain intellectual property, I will do that.

As a member of a Network of Centre of Excellence (NCE) in his field of research, Gerald received formal support for negotiating industry relationships. The NCE provided networking opportunities between scientists and industry which led Gerald to gain access to industry funding. In fact, Gerald attributed his industry involvement to this funding program.

More than any individual or organization, (name of NCE) has been pushing me into commercialization. Because that is their mandate. The network tries to network basic scientists with industry, they’ve been working at this for 7 years now.

However, one challenge he faced as a result of this involvement arose from having to disclose his findings to industry sponsors and delay publication until a patent application was filed. While these generally had a minimal impact on his scientific goals, he reported one experience where the intellectual property potential of an industry-funded project impeded the goals of his graduate student. The student was required to present a poster at the end of her summer placement. Gerald suddenly realized that she could not present her research because it had been funded by a grant designed specifically to produce intellectual property. As he noted:

For my timeline, that’s fine, but not for a student’s timeline where they’re graduating and need something on their CV this year. After that coming down, I’m very hesitant having students working on those types of projects.

In the end, the student was able to present the poster by using a false name in place of the actual name of the protein studied. This is similar to the strategy of “sanitization” reported by Slaughter and colleagues (2004) where data were withheld until the patent application had been filed. While this provided a challenge to his ability to conform to a scientific and a market logic, Gerald saw this as part of his learning curve and something that he would be able to more successfully manage in the future. What Gerald opposed was the sheer number of competitions and the difficulty in managing so many grants with so many different conditions that took time away from his engagement in research and writing.

I would say, stop making so many competitions. It can drive you insane. I think it’s okay to tell us, “Be applied. Think about how you want to see your research being applied.” But decrease the number of competitions and increase the number of basic grants that the committees hand out. The bureaucracy to manage all these competitions is enormous.
Because of his low volume of scientific capital, Gerald could not fund his research without pursuing all grant opportunities that were available, whether investigator-driven or market-oriented.

In contrast with the market-oriented experience of Thomas, Gerald and Simon, Sarah was less successful in accumulating and investing entrepreneurial capital in her scientific research. Prior to her participation in this study, she had taken a sabbatical to manage a company founded by her colleagues in the Faculty of Medicine and funded by Genome Canada. The company put up millions of dollars of matched funding. It was a basic research company working on vaccine development and Sarah had the methodological expertise to oversee the research. Even though Genome Canada wanted to continue to fund the research, the company ran out of money and couldn’t put up the matched dollars. Sarah had a considerable amount of market experience, had developed market competencies, and worked in a research area with clear potential for application. However, the disease-relevance of her research was not of interest to industry due to its low potential for profit, as Sarah describes:

It is a drug target. But it's just that most companies don't have (name of virus) on their list of things they're interested in for a couple of reasons. One is that it's not considered to be a big enough health problem in rich countries. It's a very big health problem in Northern Africa and parts of south-east Asia, but they don't have money to buy drugs. So companies, their bottom line is can they recoup the money and how quickly. We went door to door talking to all kinds of investors and companies and basically they all came back saying if they were interested, we were the people they'd want to work with. But they weren't interested.

Without industry support, and with little support from the university’s technology transfer office, Sarah did not have the resources to patent her own discoveries.

I don't currently hold any patents but there had been some that I've held in the past. Patents are really expensive. We do a lot of stuff that one could patent but I don't have the money to patent it and the university doesn't usually pay for that. Unless you can find a partner like a company that's directly interested in using the patent, you just don't have the resources. I don't have $20,000 to pay patent fees (laughs). It costs a lot. We had an application with (pharmaceutical company), we had made some discoveries here that we thought were worth patenting. The company agreed to file the patent for us and we would end up licensing it to them. But they have gone out of business now so they dropped the patent.

Despite her limited ability to accumulate entrepreneurial capital and convert it into scientific capital, Sarah could see opportunities to benefit from market practices.
Although she did not gain financially from her industry involvement, she did gain in other ways, including developing new skills and competencies. She described how, when the company went bankrupt, she was given expensive materials that were just being thrown out.

You sort of benefit in that you learn other ways of doing things, because companies and academics operated very differently. They have different end points, they have different ways of operating entirely. It was educational for me to learn about how a company does things. And in the long run, after they shut down that project, all the reagents that we had made stayed there with the company. But when the company was shutting down, I asked them for everything from the project. I now have whatever we could find at that point, because it was going to be thrown in the garbage. Honestly, we grabbed it from people throwing it in the garbage.

Sarah was able to perceive and act on opportunities for market involvement without sacrificing her scientific productivity but also without significant gain.

To summarize, this group of scientists who adapted to changing field conditions had discovered and developed new capabilities and skills. They were not against the goals of market-oriented science policy, but they did oppose top-down imposition of criteria that stood in the way of carrying out “good science” and the erosion of scientific autonomy. While they retained a strong traditional basic scientist identity, the meaning of being a basic scientist had shifted to accommodate changes in their research practices. They emphasized the compatibility of their scientific goals with their market practices, subordinating the latter to the former. By investing in the accumulation of entrepreneurial capital, they sought to convert it into scientific capital, in order to maintain or improve their position in the scientific field. Yet they continued to oppose market-oriented funding criteria which contradicted their fundamental beliefs about what constituted “good science”. In contrast, scientists who assimilated market-oriented policy willingly embraced a market-oriented vision of science. In the next section, I explore the position-takings of this group.

6.2.3. Assimilation: Hybrid Identities and Practices

Three established scientists, Jack, Sheldon and Jeremy, assimilated a market-oriented vision of science. Sheldon possessed a high volume of scientific and entrepreneurial capital, Jack had high scientific capital and low entrepreneurial capital and Jeremy had a moderate volume of both scientific and entrepreneurial capital. Both Jack and Sheldon were hospital-based scientists approaching retirement. Sheldon, a cell
biologist involved in research with a variety of potential clinical applications, had the second highest total volume of publications (183) and had published 5 peer-reviewed articles per year in the five years preceding the study. He held 18 patents, five licenses, and industry funding, had co-founded two spin-off companies and was actively involved in the development of a potential drug treatment with industry collaborators. Jack had a career as a physician before he moved away from clinical work into academic science, where he carried out basic research in developmental biology. At the time of the study, Jack held a half-time ten-year (2000-2010) position as the Director one of the CIHR Institutes. He had a total of 90 publications and, compared to other study participants, the highest number of publications in top tier journals (18). Although Sheldon had a high volume of entrepreneurial capital and Jack had low, both derived three quarters of their research funding from investigator-driven grants.

Jeremy was an exception in this study as he did not carry out health-related research. A plant biologist in the Faculty of Arts and Science, his research explored biological processes in tree growth and development with applications related to environmental sustainability and global climate change. He was included in the study for comparative purposes, as comparing the experiences of one type of scientist with another may throw into relief the unique features of a group. Jeremy’s perceptions and strategies were not, on the whole, significantly different from other scientists who participated in this study. Like others, Jeremy carried out fundamental research with the potential for applications and competed for funding from many of the same funding organizations. CIHR, which only funds health-related research, was the only exception. Most of his research budget ($1.5 million) came from market-oriented funding.

Both Jack and Sheldon took it for granted that scientific research should contribute to economic growth. They did not see a contradiction between producing “good science” and producing “relevant science”, as defined by science policymakers. Both also had followed non-traditional career paths in science, which gave them different reasons for assimilating an economic logic alongside a traditional academic logic. As Director of a CIHR research institute, Jack occupied a unique position between the scientific and science policy fields. Throughout the interview he constantly juxtaposed the
views of basic scientists, who are committed to scientific discovery and, and those of policymakers, who want to harness biomedical knowledge to build a competitive biotechnology industry. He supported both views. As a basic scientist himself, Jack agreed with the “serendipity” argument, but saw discovery and application as inextricably entwined. He stated:

Basic science researchers, the vast majority in Canada, are not interested in commercialization activity. I think every basic scientist would love it if their work had some profound clinical impact. And there are some fields of science where it's inherent in the activity, for example cancer research. But it's really hard to predict the long term consequences of many basic discoveries. If you try to figure out what led to discoveries that improved survival, both streams of activity – clinical and basic – have a profound impact.

Unlike basic scientists who reaffirmed or adapted to market-oriented science, Jack was in favour of market-oriented funding programs. As reflected in the following quote, he had adopted the language of science policymakers who sought to bridge the gap between discovery and application in order to stimulate the biotechnology industry.

That’s the problem in Canada. You have got strong biomedical research and this campus is absolutely fabulous but we are not as good as actually we should be in developing biotech in this country. And it’s quite correct that any government should want to include that.

Moreover, Jack saw the NCE program and Genome Canada as highly successful programs at both advancing knowledge and promoting commercialization of knowledge.

However, new funding programs also created certain problems. Low CIHR success rates were not, in Jack’s view, the result of a political agenda to divert resources from basic, investigator-driven research to applied, problem-oriented research. Rather, they were the result of an imbalance in investments between salaries and infrastructure and operating costs stemming, in his perspective, from the absence of a coherent national science strategy. The low success rate created, he suggested, resentment among basic scientists towards CIHR’s stream of strategic funding, particularly as strategic grants are often funded with lower scores than operating grants, due to the smaller pool of applicants. Unlike the majority of established scientists, Jack did not see strategic grants as taking money away from basic research because the vast amount of new
money invested in CIHR since the late 1990s has gone to basic (biomedical) research. As he stated:

When CIHR was founded, there was more money. The most important fact which the basic science community continually forgets is that, by far, most of the new money went to basic science. It didn’t go to the other three pillars\(^{11}\). Now as a percent increase, the others hauled out bigger increases, but the absolute amount of money that basic science gets vastly exceeds the amount that clinical, health policy or population health research get.

Jack simultaneously defended the CIHR’s emphasis on strategic and non-biomedical research to basic scientists and advocated to policymakers for a more equal balance between operating and other types of grants, on behalf of basic scientists.

While Jack took a strong stance in favour of the ability of academic science to achieve the dual goals of science and the market without a cost to scientific quality, he acknowledged that scientists who succeed at these two goals may attain a strategic advantage in science.

You can be a very successful scientist and never give a hoot about commercialization or founding a company. But there are some scientists who have an interest and if they have some funding which relates to their commercial inclination, that increases the number of people and resources in their whole enterprise which might just increase their ability to do any kind of research. It actually might work to the benefit of their whole research program.

Jack, himself, did not have this commercial inclination. However he can be seen as pursuing a hybrid path of accumulation of scientific and academic power (Bourdieu 1990b). In his study of the French academic world before the rise of market-oriented science, Bourdieu described the scientific field as polarized according to the dominant form of scientific capital and a subordinate form of temporal or academic capital. Academic capital was a form of political power that came to those who held institutional positions such as departmental Chair or senior administrator in a university or funding organization. Academic capital was awarded according to a different logic than scientific capital and required a large investment of time that took away from the accumulation of scientific capital. Jack, as a senior administrator in the largest federal medical granting program, had acquired a high volume of academic capital. The legitimacy of academic institutions and funding organizations, in the eyes

\(^{11}\) CIHR’s four pillars or research are: biomedical; clinical; health systems and services; and the social, cultural and environmental factors that affect the health of populations (CIHR 2004a).
of policymakers, increasingly relied on ability of these institutions to demonstrate accountability according to a market logic. Therefore, Jack’s assimilation of a market logic was consistent with his political, rather than entrepreneurial, ambitions.

Sheldon, on the other hand, had a strong commercial inclination. As someone who had held industry funding for many years, had multiple patents and licenses, and had founded a company, Sheldon was somewhat oblivious to changes in science policy and funding. As he said:

I've always had patents. I've always been, I don't know about leading, but certainly I have a lot of patents. I've always had a commercial interest. Now that's becoming more and more a requirement, but it's something that I've always done, so I probably haven’t noticed.

Because he had an entrepreneurial disposition before it was popular or widespread in science, changes in funding conditions did not challenge his core identity. Surprisingly, he did not welcome market-oriented funding programs even though he appeared well-positioned to succeed according to their entrepreneurial criteria. Apart from one CIHR Strategic grant, his remaining grants were investigator-driven operating grants from CIHR or research foundations. As he said, from his perspective, market-oriented grants had more strings attached to them than direct industry involvement:

To tell the truth, I get fed up with writing grant applications. I don't really want to write another grant application to do something that the company has essentially agreed to do anyway.

Most of his experience industry was not demanding and had little impact on his research agenda.

As a company founder, however, Sheldon ran into difficulties navigating contradictions between his scientific goals and those of the company. In the 1990s he founded a biotechnology company to develop a cancer treatment drug. The company raised a lot of money initially and, because he did not want to leave his scientific position to run the company, he hired a CEO but stayed on as the Scientific Director. Sheldon described entrepreneurial scientists as having two choices, to either manage the company and retain control, or hire someone else to manage it and risk losing control.

When a scientist gets into commercial areas there are two ways to go. Either he quits his daytime job and manages the company or he sits on the
sideline and let somebody else manage the company. I didn’t want to quit my job. I don’t want to be totally commercial. So we had a CEO and he arranged for takeover of several other companies, one of which crashed and needed refinancing just after 9/11. After that they had a bad clinical trial, so the company went down the tubes.

After the company went bankrupt, Sheldon was able to disentangle his intellectual property from the company and use it to found a second company. Because he did not leave his academic career or sacrifice too much of his research agenda and resources to the company, his loss was not as great as that experienced by Robert. However, like Robert, he did not intend to lose control over his research agenda a second time.

Before I sort of sat on the sidelines, not the sidelines, but I was not in the driving seat. Whereas now I am more in the driving seat. I don’t really want to do that, but because it ran away from me last time and went down the drain, I am taking a more proactive role.

Like Sheldon, Jeremy had a predisposition to engage in market-oriented science. However, one difference emerged between them from differences in their fields of research. Jeremy had been drawn to forestry research early in his career out of a commitment to environmental activism. His motivation to engage with the forestry industry was to provide knowledge that would help companies implement sustainable practices. After working overseas for several years, Jeremy had just returned to Canada in 2004. He provided the unique perspective of someone who was rejected by Genome Canada based on the financial audit. This was his first grant application upon his return to Canada. Jeremy and his colleagues were shocked to have their application rejected.

I was one of the many scientists across Canada that was stung by the Genome Canada co-funding debacle. Genome Canada had as their first tier of evaluation due diligence which was a financial audit conducted by KPMG. I headed up a group of twenty-two scientists where we had, with the co-funding, it was around a $30 million project. I coordinated it with a colleague out at the University of British Columbia who had been funded in the previous Genome Canada round. We thought we were golden. We thought everything was fine. We were sitting there with two other people from our project, including an economist, going through our management strategy and they said to us, “Tell us how your co-funding is going to work”. We told them and they said, “That’s not co-funding”. So, in an instant, the four of us sitting at the table, there was a collective jaw drop. You could hear the thump on the table as their jaws hit the table.
After this huge blow, Jeremy took two components from the Genome Canada project and resubmitted them to the NSERC Strategic grant competition and both were funded.

What does that tell you about the science that was underpinning the project? When I submitted two strategic grants everybody said you’re absolutely stupid to submit two strategic grants. It’s hard to get one funded and unheard of to have two funded in the same year.

Given his experience, it is not surprising that Jeremy had a negative opinion of Genome Canada, in terms of the way it funds science and the quality of science it funds. He described the organization as a “fishing exercise” and giant information database, in contrast with “good science” which is hypothesis-driven and asks legitimate biological questions.

Despite his negative experience with Genome Canada, he ultimately, believed that science should in some way be held accountable to social as well as scientific goals.

I think there need to be two levels of science. I think there’s got to be one that perhaps is accountable to a public agenda. And then another one that is more discovery-oriented. And they both have to be valued equally and they both have to be funded equally. And that’s not happening.

In summary, Sheldon, Jack and Jeremy all engaged in the redrawing of symbolic boundaries around “good science” to include dimensions of economic and social relevance, although each came to this position-taking through different routes. They willingly incorporated the values of market-oriented science as they coexisted harmoniously with their own identities and practices which, thus, acquired a hybrid form. However, acceptance of a market logic did not ensure a blanket acceptance of market-oriented funding programs, which were perceived as demanding, time-consuming, inefficient, and out of balance with basic operating grants. While the majority of established scientists retained a traditional scientific identity, even if they altered their practices, early stage scientists were split between a traditional and hybrid scientific identity. In the following section, I present the position-takings of this group.

6.3. Position-Takings: Early Stage Scientists

Scientists in the early stages of their careers were trained in a much different academic and funding environment than their senior colleagues. A cultural shift had reduced the stigma associated with industry involvement, while the pressure to bring
in external funding was more intense for new entrants than they had been for their senior peers. Yet, they were insulated from some of the pressures to apply for research funding with commercialization criteria, as funding programs geared specifically towards new investigators (e.g., new investigator awards and start-up grants) did not usually have such criteria. The latter granting programs were used by new scientists to establish their labs and develop their research programs. New scientists most often encountered commercialization criteria in the Benefits to Canada section of CFI grants. While many early stage scientists entered their academic careers with entrepreneurial experience and intellectual property, few remained actively engaged in entrepreneurial practices after beginning their academic positions.

Figure 4 maps the relationship between the positions of early stage participants and their position-takings towards market-oriented science. The volume of scientific capital is represented on the vertical axis, moving from low at the bottom of the axis to high at the top.

**Figure 4. Positions and Position-Takings of Early Stage Scientists**

The volume of entrepreneurial capital is represented along the horizontal axis, moving from low on the left side of the axis to high on the right. Each triangle in Figure 4 represents an individual early stage scientist and the colour of the circle represents her
or his position-taking towards market-oriented science. Four early stage scientists reaffirmed a traditional vision of science while five assimilated a market-oriented vision. Those who reaffirmed traditional science had low, moderate and high volumes of scientific capital and all had a low volume of entrepreneurial capital. They retained a traditional scientific identity and distanced themselves from market-oriented funding and practices. Assimilation of market-oriented science occurred among early stage scientists with low, moderate and high volumes of scientific and entrepreneurial capital. This group accepted relevance and applicability criteria as conditions for research funding. While they did experience contradictions between the logic of science and the logic of the market, they were confident that they could negotiate these contradictions. I first describe the perceptions and strategies of the “reaffirmers”, who maintained continuity with the traditional scientific order, and then turn to the group of “assimilaters” who exhibited different dispositions and values.

6.3.1. Reaffirmation: Continuity with the Traditional Order

Four of nine early stage scientists, Kate, Mike, Damien and Alex, reaffirmed a traditional vision of science. All had a low volume of entrepreneurial capital; Kate had low scientific capital, Mike and Damien had moderate scientific capital, and Alex had high scientific capital. A hospital-based stem cell biologist, Kate studied cell growth and cell differentiation in fruit flies. At $0.9 million, she had smallest research budget of all study participants, apart from one early stage scientist (Nathan) who had just accepted his tenure-track position and was in the process of grant-writing at the time of the study. All of her grants were investigator-driven. Mike and Damien, both university-based molecular biologists, studied a family of genes in yeast related to cancer and other diseases. They each had three investigator-driven and two market-oriented grants (CFI and OMRI). Damien was the only one of the four who had acquired exposure to commercialization practices during his graduate training. While his training environment had a strong commercialization culture, he himself had no direct involvement in market practices. Alex was a computational biologist located in the interdisciplinary research centre at the university. He had the highest volume of publications and grants of all early stage scientists. Because his research was in the “hot topic” area of genomics, a large proportion of Alex’s funding came from market-oriented sources.
Like established scientists who reaffirmed a traditional vision of science, these scientists took the position that traditional science is incompatible with market-oriented science on the basis that the latter restricts novelty and impedes both the discovery and innovation process. The strongest stance against market-oriented science was taken by Kate.

I have some fairly strong opinions (laughs) about whether it’s a good idea to be sort of mandating policy in terms of application when you are talking to basic science researchers because I think that there are a lot of serendipitous discoveries that wind up having huge impacts on health care, on biotechnology, on our ability to do all kinds of things, that you could never have predicted from what somebody set out to do at the outset. And I think mandating it isn’t going to make it happen any faster. In my opinion, and it might actually inhibit it if you don’t fund people who are creative.

She described market-oriented sources of funding as “distractions” that are not necessarily based on peer-review and that potentially waste resources on a poorer quality of science. For her, this made the funding environment appear “a little bleak”. None of the “reaffirmers” saw themselves as interested or able to engage in the kind of commercial practices that would lead to intellectual property or enable them to qualify for funding that required commercial outputs. As Damien stated, “I purposefully insulate myself. I have no interest.” Similarly, while Alex could identify potential commercial opportunities, he had not considered acting on them.

All my career I’ve been doing basic science research so I’m sure if I tried there would be opportunities for tech transfer or for starting my own company but I never even considered it. It never occurred to me. What I do, there is no immediate commercial application. Well there might be, I think, if I choose I could probably develop useful software for industry, it’s just not my interest.

Kate described herself as being “at the extreme end of not having something that I could imagine turning into a patent”. At the same time, she was also the only one who worried that rhetorical strategies might not be sufficient to ensure future funding. A US funding organization from which she had previously received funding had recently shifted its priorities to research using mammal models. After having an application rejected by that agency, she began to consider shifting her own research methods to meet funding criteria, stating:

It’s certainly crossed my mind and I am sure it’s crossed the minds of many of my colleagues who work in model organisms like fruit flies or worms or yeast. Maybe we should also have some aspect of research in our lab that’s going to be more fundable, that’s more closely related to the things granting
agencies want you to do. I am not convinced that we are going to actually learn anything more useful by doing that.

Kate then described the high cost involved in switching animal models and the low return in terms of advancing her science.

It’s difficult because I think that if your training is in medicine, it’s much easier to make those leaps. I know researchers who have moved their research into working with mice. Once they have discovered something in the fruit fly, they want to know is it also true in a mammal. But I think it’s harder unless you really have the background training to move your research into a completely different area from what you’ve been training to work on and think about and be most creative in.

While funding conditions had not yet altered her research practices, they had influenced her level of stress.

The dissonance between Kate’s traditional disposition which motivated her to explore fundamental mechanisms in a non-clinically-oriented model organism and the funding context pushed her to consider altering her practices, withdrawing from the field or risk being expelled from the field. During the interview, she discussed the possibility of losing her position with the hospital if she failed to fund her research, seeking a tenure-track position to gain job security, or working in an entirely different field as a science teacher. Leaving the scientific field altogether is akin to Todd’s (2005) category of “privatization” which she describes as a total loss of identity through withdrawal into an alternate sphere. For scientists who participated in this study, privatization was rarely considered as an option, as it would entail the loss of their livelihood in a profession they had invested years of training. Therefore, for Kate to even consider alternate careers suggests a high level of concern or dissatisfaction with the funding context.

While none of the other reaffirmers expressed the same level of concern as Kate, they were frustrated with market-oriented funding criteria. One of the criteria they were most likely to encounter was the requirement to justify their research in terms of its applicability or its Benefits to Canada. On the one hand, these scientists argued that even the weakest market-oriented criteria can reduce the quality of science when scientists frame their research problems in terms that are justifiable to non-scientists. Research projects that appear more clinically relevant may in fact be less novel, less creative, and ultimately, less useful. For example, Damien suggested that the kind of
major discoveries that have earned scientists the Nobel Prize would not be funded in 
the current Canadian funding environment.

Restricting areas of research to those that can be justified in terms of 
applicability can limit real novelty, because you can’t see the application in 
something so fundamental. For example, the research on RNA1 in a worm 
would not be funded here, yet that research was awarded the Nobel Prize 
and resulted in the development of techniques that are now routinely used 
in science.

On the other hand, it was not difficult for them to justify their research in terms of its 
medical relevance. Like established scientists, they engaged in “tailoring” practices 
(Calvert 2006) to make their research appear more relevant. Mike, who held funding 
from CFI and OMRI, described the rhetorical strategies he used to get funding.

I do very basic research. We don’t study people or anything close to people 
at all. We study yeast. But it’s quite easy to make it relevant to human 
biology. The molecules, the genes are related. The gene family that I study 
is known to have roles in cancer – oncogenesis. It’s kind of a new area, so 
using a model system to make very basic discoveries about this gene family 
that is known to have some mysterious role in cancer in humans, it’s very 
yeasy to spin that as being important to do.

Although it is easy to put a spin on a research project, it was not necessarily 
enjoyable. As Mike described writing the benefits to Canada section of his CFI 
application:

Oh man, that was terrible. Because you really had it piled higher and 
deeper. But whatever, I don’t care. It got me the lab.

Paradoxically, Alex received most of his research funding from market-oriented 
programs, yet was least aware of market-oriented funding criteria. At first, he spoke 
only about CIHR, which he saw as his primary funder:

I don’t think that economic outcomes are a mandate for CIHR. Maybe it is 
for NSERC or the provincial funding agencies. CIHR, it’s not what they are 
looking for, how many patents you can generate. It doesn’t affect us at all.

As I questioned him during the interview, he, in fact, denied that he had encountered 
market-oriented criteria and seemed puzzled by my research questions. Becoming 
increasingly puzzled myself, as I knew that most of his grants came from market-
oriented funding programs, I read a portion of his Curriculum Vitae to him which 
described the commercial products promised in the Genome Canada grant he held. He 
responded, somewhat sheepishly, by saying that those aspects of the grant were 
handled by other senior investigators with extensive market experience. While his
expertise as a computational biologist was necessary on the large multi-investigator project, he was insulated from, indeed, totally unaware of its commercial demands.

Mike and Damien, who relied largely on investigator-driven funding, were well aware of the discrepancy between their funding levels and the funding levels of scientists based out of the interdisciplinary centre. Despite similarities in the model organism (yeast) they used and the types of research problems they explored (the functioning of genes and families of genes in relation to particular disease processes), scientists in the traditional academic department experienced less coherence between their research agendas and market-oriented policy agendas. While the fundamental approach they adopted towards knowledge production shut them out of market-oriented funding opportunities, they defended this stance on the basis that they produced a superior quality of science. Mike described the interdisciplinary centre as a “temple of information” which members of his department used to generate “knowledge”:

We collaborate with a lab over there. I went and saw one of their post-docs and I saw some of the information he had generated. Then we took it here and turned it into knowledge. There are a lot of terabytes of information being generated. That’s not interesting. In the end, it’s gotta become a story, you gotta make some biology out of it. At least for me, yeah, I think most people see it that way. It’s easy to generate information. It’s not hard to do that. The hard part, the creative part, is doing something interesting with that information.

Compared with Kate, Mike, Damien and Alex had greater confidence in their abilities to succeed scientifically without conforming to a market-oriented vision of science. They all had better publications records than Kate but also worked in a different institutional environment. As tenure-track professors in a university department, they were less concerned with job security. Also, unlike hospital-based scientists, they were not formally evaluated on their patent portfolio. Yet, even with this higher level of confidence, Mike and Damien both asked during the interview, “Should I be worried?” This may have been an artefact of the interview itself, which asked them to reflect on the impact of market-oriented funding. Or, the interview may have revealed an insecurity hovering beneath the surface. While early stage scientists who reaffirmed a traditional identity resembled their established counterparts, those who assimilated exhibited somewhat different characteristics and strategies. Established scientists who assimilated had gone against the grain of traditional academic culture
while early stage “assimilators” embodied the new academic culture in which they trained.

6.3.2. Assimilation: Shifting Scientific Identities and Values
Five of nine early stage participants (Vincent, David, Ralph, Trevor and Nathan) assimilated a market-oriented vision of science. They had incorporated two sets of values in a hybrid scientific identity and shifted between them as the circumstances required, expressing a high level of confidence in their ability to negotiate contradictions that emerged between scientific and market goals and practices. All early stage assimilators had commercialization experience or had acquired intellectual property during their doctoral or post-doctoral training and had begun their academic careers with more commercialization experience than many of their senior colleagues.

Within this group, two distinct position-takings were identified, corresponding to different positions they occupied within the scientific field. A “pro-market” assimilationist stance was evident among the most highly entrepreneurial and successful early stage scientists. A more sceptical, or “pro-science” assimilationist stance, was more commonly expressed by early stage scientists with low to moderate levels of scientific and entrepreneurial capital. Despite these differences, both groups accepted relevance and applicability as legitimate goals of basic science.

“Pro-Market” Assimilation
“Pro-market” assimilationists expressed distinctly different beliefs from all of the other participants about how science should be valued and how money should be invested. Most participants who had adapted to or assimilated to market-oriented science tolerated externally imposed change as a means to an end. To have value in the scientific field, entrepreneurial had to be converted into scientific capital. This group, however, perceived entrepreneurial as being legitimate products in and of themselves; they could be converted into scientific capital but were also symbolic forms of capital in their own right.

Two early stage scientists, Vincent and David, actively endorsed the government’s goals of promoting commercialization and technology development within academic science. Vincent, a molecular biologist, and David, a computational biologist, were based in the highly commercial interdisciplinary research unit. Vincent had high
volumes of entrepreneurial and scientific capital and had published more peer-reviewed articles (27) in eight years than one established scientist had published (25) in fifteen years. Vincent held two patents and industry funding, was actively involved in product development, and was the co-founder of a successful spin-off company. David also had a high volume of scientific capital, having published more peer-reviewed articles (18) in top tier journals than all but one established scientist. However, he held only moderate entrepreneurial capital (2 patents) and had not engaged in market-oriented practices since beginning his tenure-track position. Both held Genome Canada grants, but David also held a range of other market-oriented grants.

Vincent and David both saw market-oriented funding as an opportunity for scientists to acquire resources and potential personal wealth, and for the government to achieve economic goals. Vincent pointed to the successes of other countries in linking economic growth with science.

I think there is nothing better for any government in the world, than to invest in science and technology. You look at the example of the Arabs. Around fifteen years ago the government decided to invest a tremendous amount of money in informatics and today, we hear they are the top country in the world for informatics. You look at Finland, in the case of their telecommunications. The government again, fifteen or twenty years ago, started to invest a lot of money for telecommunications. They had nothing. Now they are really the leading field of telecommunications. The lesson in technology – it certainly will be a good investment.

Unlike scientists who opposed market-oriented funding on the grounds that it would erode the quality of science, these scientists did not see this as a problem. According to Vincent, the diversion of funding away from “total basic research” and towards more applied, relevant research was justified.

I think it’s great because this money is coming from taxpayers so I think taxpayers should know where this money is going and how someone like me is going to use this money to address some important research questions. This type of project can only help people who are doing medically related research. Because there are some people who do total basic research. And we, they, will not be able to get all the money.

In making this statement, Vincent distanced himself from traditional basic scientists, first by identifying with the “we” that does basic research and then switching to “they”. This may illustrate the two facets of Vincent’s hybrid identity. While he saw himself as a basic scientist, he also saw himself as something different. Vincent was
the only scientist in this study who defended the diversion of funding away from “total basic research”. In doing so, he actively redrew the symbolic boundary around “good science” to include criteria of economic relevance.

Vincent had extensive entrepreneurial experience in Europe, before relocating to Canada to take up a tenure-track position. Like Simon, an established scientist who felt driven into commercial practices by poor funding through traditional channels, Vincent also resorted to spinning off his own company in order to generate revenue to invest in his own research. The company he co-founded with his European colleagues was very successful and, like Simon, this enabled him to raise his level of scientific productivity. However, it was not until his recruitment to an academic position in Canada that he was able to fully leverage his entrepreneurial success into scientific capital. Since his arrival, he was publishing four publications per year and was expecting several publications in top tier journals. As a co-investigator on a Genome Canada grant, his research was very well resourced.

Vincent’s experience of the Canadian funding environment, compared to his country of origin, was one of greater coherence between his expectations of success and his actual chance of meeting these expectations. This scientist accepted and used the market-oriented discourse to justify his success and access to multimillion dollar commercialization grants, giving full credit to the research climate in Canada. The competence to work within a market-oriented logic had already been honed from his extensive commercialization experience as the co-founder of a company. Early in his career, Vincent learned how to negotiate contradictions between doing science and doing business, participating just enough in the latter to leverage his entrepreneurial capital into scientific capital.

Even though Vincent attributed his own career success, in part, to his involvement in market practices, he experienced contradictions between the goals of the market and the goals of science. Therefore, while knew how to form and run a company, it was not an experience he would repeat.

I would never do that again. No. It was a great experience but so much work to do. Some people, it’s their own job and it’s all they do for a living and I had all these other things. I would work my normal working time for my group and the rest for the company. It was a lot of stress. I learned a lot. It was a great experience. I didn’t regret that I did it.
He was also selective in submitting applications to grant programs with market criteria. For example, he had withdrawn an application to the OMRI due to the demand to generate intellectual property.

I gave up because – the project I wanted to submit – we didn’t have a clear potential of where to develop intellectual property. They said that we will support your project as long as you show potential and I thought oh, so much for it. I really have enough funds.

Unlike other basic scientists whose sole purpose for participating in the commercialization of science was to acquire scientific capital, Vincent expressed hopes of personally profiting from the sale of his spin-off. He therefore sought to maximize both his symbolic capital, through recognition in top tier journals, and his material wealth, as a successful entrepreneur.

Like Vincent, David also promoted a hybrid vision of science. However, he did not support the diversion of basic research funding into applied research. His viewpoint, as exemplified in the following, was similar to Jack, the CIHR Scientific Director who advocated for an equal balance between basic and applied funding.

I think they (politicians) want to have the programs that help spur the economy in some way. *I think that’s good and that’s forward thinking by the government,* that they should be doing these types of things. That’s why I’m not just directly against the co-funding thing. But it’s much more important that operating grants match the contribution for infrastructure. I mean there are two ways of fixing an imbalance: cut back or increase, right? I’d always argue for increase because we’re just going backwards if you try and balance by cutting back. It’s actually harming things that you set in place already.

Vincent and David both perceived market-oriented science as providing scientists with opportunities for commercialization. As a computational biologist, David saw these opportunities as very limited. Under the right conditions, however, he was willing to invest in the commercialization process.

If there is some very clear opportunity, then I'd be tempted to become involved in it. I probably wouldn't be interested in spending the time to start a whole company myself because it's a huge amount of time and I'd much rather focus on research. It's just much more interesting to me. But sometimes the company situation may help you push your research. I've seen people do this where they start a company and they make a lot of money and they can hire researchers, and the company, it pushes their research forward. Those are nice successful little stories.

While Vincent had acquired his commercial experience through his own direct involvement in founding a company, David’s commercial experience was one step
removed, during his research training. Both his doctoral and post-doctoral supervisors were industry-involved or had founded a spin-off company. David was exposed to market practices which had been, for him, both a positive and negative experience. He described what happened at the research institute where he completed his post-doctoral fellowship when his supervisor took academic leave in order to found a company as follows:

> We lost our supervisor for a year because he took a sabbatical to go and work there to help start it up, and a leave of absence. So that was negative from the point of view of not getting as much time with the supervisor. And then it was positive in terms of they were interested in using the technology from all different students and researchers in the institute that were involved. Students would get asked to come and help, give a talk or something and we get paid. So that was our source of income that would have not otherwise been there. I have to say that was nice.

Both David and Vincent experienced tensions between time lost to commercial involvement and resources gained that could then be invested in academic practices. Even with this awareness of potential tradeoffs, they were optimistic about the benefits of commercial involvement, carefully evaluated potential opportunities, and made choices based on their own knowledge and past experience.

“Pro-Science” Assimilation

Three early stage scientists who had internalized a market vision of science, Nathan, Trevor and Ralph, subordinated the goals of the market to the goals of science. Nathan and Trevor were located in traditional academic departments in the Faculty of Medicine and Faculty of Arts and Science, respectively, while Ralph was affiliated with a hospital research institute. Nathan had low entrepreneurial capital and moderate scientific capital, while Trevor and Ralph each held a moderate volume of entrepreneurial capital (each had one patent) and a low volume of scientific capital. A biochemist who studied immune responses in infectious diseases, Nathan had just recently begun his tenure-track position in the Faculty of Medicine. He was in the grant-writing process and did not yet hold any investigator-driven or market-oriented grants. Apart from Vincent, Trevor was the only other early stage scientist who tried to engage in market-oriented practices after beginning his academic position. Yet he held only one market-oriented grant. Ralph, in contrast, received nearly three quarters of his funding from market-oriented grants.
Like Vincent and David, Ralph, Nathan and Trevor valued the production of knowledge that resulted in real world applications and had internalized this into their scientific identities. They clearly perceived the potential for their own research to lead to health diagnostics and treatments. Ralph, for example, described how he was drawn to his current position because of the opportunity to develop clinical applications from his research related to childhood cancers.

I really like to think of myself as someone who can help translate information from very fundamental research into applications that can be used to try and cure cancer in children. One of the reasons I took the job here was that there’s an open-mindedness about the Research Institute and translating information from basic science into clinical applications.

However, they differed from “pro-market” assimilators in two key ways. First, they perceived the cost of accumulating and converting entrepreneurial to scientific capital as higher and the benefits as lower. Second, they retained a more traditional vision of the “public good” which did not necessarily equate with economic growth and development.

There were a variety of reasons why Nathan, Trevor and Ralph found the accumulation and conversion costs higher than “pro-market” assimilators. None worked in the “hot topic” area of genomics research and all held much smaller volumes of funding ($1 to $2 million CIHR grants compared with $28 to $30 million Genome Canada grants). Nathan described the effort involved in translating basic discoveries into applications as follows:

To actually make it applicable is a lot of work. It requires collaboration and a huge amount of funding. The goal of the research we do is to design novel antibiotics, antimicrobials. If you actually want to implement something like that you have to go through a number of clinical trials, which cost in the order of billions of dollars. You need to have a drug company partner to do that. If you want to take your research to that level, you need to have collaborators.

Nathan had acquired commercialization knowledge and experience from his post-doctoral supervisor, who was industry-involved and used this involvement to leverage grants.

She did have a number of ties with drug companies that were developing novel antibiotics or therapeutics. So, after the drug company would screen a bunch of targets, she would get structures of the complexes of those and
then turn them over to the drug company and they could evolve their targets based on that.

His own post-doctoral research findings were identified by the university’s technology transfer office as potentially patentable. Nathan left it up to that office to decide whether to pursue the patent application. He saw himself as playing a supportive role in the long-term project of translating knowledge into applications. As someone just starting out on the tenure track, it was not a short-term priority, as he had yet to set up his lab and begin to generate a stream of research from which applications would emerge. However, as he stated, “It’s always in the back of your mind”.

While Nathan and Ralph saw future potential for applications arising from their research, Trevor had already made a basic discovery with exciting treatment potential. Yet, after extensive effort to bring this treatment potential to the application stage, he abandoned the project in order to focus on his publications. For two years, he met with university technology transfer staff only to have them reject his discovery as too “early stage”. In addition to the frustration he experienced with the technology transfer office, Trevor lacked access to mentors with commercialization experience in his department. Compared with applying for a research grant, he described the patent application process as “almost a secret process”.

With a grant application, I felt, well, everyone in my department has a grant, so let’s go ask them. “How does it work?” And so everybody can tell you, and everyone will give you a different story. But at least you are allowed to ask them information about it. Whereas the number of people who have patents is very few in this department. I don’t know anyone here except a colleague across the hall, and he was quite helpful. But it was almost a secret process. I just kept asking, “What is this?” And then I was never sure who to actually deal with and why was I calling this technology transfer person. What is she doing for me? I know she sent me this form, but where does she go with that form? Is she just a paper pusher or does she decide on this? It was all very unclear and very frustrating.

As a pre-tenure scientist, he also described himself as having little incentive to pursue the application.

You probably won’t get a significant publication out of it, but then what happens from my understanding, is that the tech transfer office cashes in as well as some commercial entity, cashing in on your hard work, and you are really left with, as far as I can understand, minimal amounts of payback. So, in a nutshell, there is really very minimal incentive for a basic researcher to move forward. Maybe if you are more senior and you have
got the grants and you’ve got a really established lab and you’re not worried about tenure… then you could probably devote the time to this.

Just as Trevor’s commercialization experience had muted his early enthusiasm, so too had Ralph experienced the down side of market involvement. For Ralph, this came during his post-doctoral training when a basic discovery led to the development of a potentially marketable technology. He and his supervisor entered into negotiations to license the new technology to a biotechnology company. However, after the scientists shared their information with the company, they were “shocked and dumbfounded” to find out that the company simply reproduced it in their own lab and failed to pay the agreed upon patenting and licensing fees. Like established scientists in this study who had negative industry experiences, Ralph expressed concerns about the science agenda being overshadowed by the business agenda.

I think if companies have too much of a stake in academic research, they're going to direct academic research in ways that will destroy academic research. This may be an extreme way to put it, but I think if we’re being told how to do work and what to study, I think that's wrong. That's not the pursuit of knowledge.

However, this awareness of the contradictory goals of science and industry did not deter Ralph from adopting entrepreneurial practices and strategies. He had three projects with commercial potential and expected to file for a patent with two years. Even though he self-identified as a “translational” scientist – a scientist committed to the translation of basic discoveries into applications – Ralph drew a clear line between academic science and private science.

My main focus is really just contributing to science. It's the difference between us, those of us in the academic world and I think the people who are in the corporate world. I think it’s important we protect our inventions, that anything that comes from it gets put back into the academic enterprise. Because that's the whole purpose, right?

The second key difference between “pro-market” and “pro-science” assimilators was in the way they defined the benefits of their research in terms of the “public good”. Vincent and David adopted the language of science policymakers and focused on the potential economic benefits both to their research projects and to the public, in terms of job and economic stimulation. Nathan, Trevor and Ralph, however, justified their
compliance with a market logic in terms of producing health applications that would reduce suffering and improve human welfare.

These scientists were passionate about the potential of their research to relieve human suffering. For this reason, despite the difficulties he encountered, Trevor did not lose hope in eventually translating his discovery into a new treatment, after he achieved tenure.

As he stated:

Frankly I think it’s the most important thing that I’ll ever do as a scientist because I think it will validly help somebody one day, down the line.

They had more in common with Jeremy, the plant biologist (established) who both assimilated a market logic and reinterpreted it to align with his commitment to environmental sustainability. He did not subordinate his goals to the goals of the forestry industry in order to fund his research and increase their profits; rather he sought to shape forestry industry practices. Similarly, Ralph viewed patenting as a tool that scientists could use to protect their discoveries from exploitation by industry and to ensure that the benefits were reinvested in science.

The way I look at patents is if we discover a target that can be exploited for therapeutics, we just want to make sure the money gets turned back into research dollars, so it goes to the hospital Foundation. And it feeds our enterprise of doing basic science.

He was not opposed to profiting from patented knowledge, but he did not value profit for its own sake or as a means of stimulating economic competitiveness. While conforming to an instrumental logic, as opposed to knowledge for the sake of understanding the natural world, this represents a broader instrumentality that encompasses non-economic outcomes. Therefore, within the constraints of the policy agenda, this symbolic strategy retains continuity with the old symbolic order and keeps the “space of possibles” open to a wider extent than conformance to a strictly economic instrumentality.

Early stage scientists who had internalized aspects of the market-oriented logic and acquired a different set of skills were able to navigate the dual set of criteria without experiencing tension between identity and practice. Because of their exposure to the experiences of supervisors and peers throughout their training to become scientists, early stage assimilators aware of the dangers of becoming too involved with industry
and losing control over their research agenda as well as their ability to profit from their entrepreneurial endeavours. They had a sophisticated sense of how things work, leading them to set clear boundaries around what they were willing and not willing to do. Through their own entrepreneurial experience, or training with senior colleagues who were involved in commercialization projects, they had incorporated a new set of skills and new knowledge for negotiating these complex relationships with industry, patenting offices and technology transfer staff. This gave them a practical know-how for acquiring and converting entrepreneurial capital into scientific capital. Negative industry experiences did not deter them from generating intellectual property or collaborating with industry but did shape their expectations and strategies.

6.4. Conclusion

In this chapter, I have explored the responses of basic scientists trained before and after the rise of market-oriented science policy towards a market-oriented vision of science and new funding structures to implement this vision. Using Todd’s (2005) typology, I have illustrated differences in scientists’ perceptions of market-oriented science and their strategies to defend or redraw the boundaries between science and the market and acquire resources and recognition. As I anticipated at the start of the study, the majority of established scientists experienced dissonance between their perceptions of “good science”, rooted in a traditional scientific habitus, and the evaluation of their research according to funding criteria of economic relevance. Some reacted to this dissonance by defending a traditional vision of science and pursuing a traditional strategy of accumulation of scientific capital, while others resisted market-oriented science symbolically, but acted pragmatically, by pursuing a hybrid strategy of accumulation and conversion of entrepreneurial and scientific capital. A small group had already developed hybrid identities before the rise of market-oriented science policy, which reinforced and legitimated their support for a market vision of science.

I found that scientists trained after the rise of market-oriented science policy were unlikely to experience dissonance between their vision of “good science” and a market vision. However, they did not uniformly construct hybrid identities as we would expect if a hybrid symbolic order had replaced a traditional order. Scientists, occupying varying positions of power, constructed both hybrid and traditional
scientific identities in conformity with their academic training and funding environments. This suggests that early stage scientists perceived it as possible to succeed as scientists by following either a hybrid or traditional strategy of accumulation of capital. Yet, few early stage scientists who constructed hybrid identities engaged in hybrid practices after they began their academic careers. This may be a temporary result of their career stage, in which there is a sequence in moving from knowledge production into commercialization and these participants may move seamlessly from one stage to the next as their careers progress. Alternatively, after achieving tenure, early stage scientists may come to experience the inherent contradictions between discovery-oriented and market-oriented science that many of their senior colleagues experienced.

Interestingly, the findings revealed that scientists’ position-takings towards market-oriented science were not associated with the objective positions they occupied in the scientific field. Scientists, across policy eras, with low, moderate and high volumes of scientific capital, acted with and against market-oriented funding. When hysteresis occurs, individuals who occupy similar positions in social space may react differently to changes in field conditions (Bourdieu 1990b). Hysteresis was a source of change among some established scientists who adjusted their identities and practices to take advantage of new market opportunities and a source of “misadaptation as well as adaptation, revolt as well as resignation” among others who sought to re-establish or longed for the former conditions of the field (Bourdieu 1990a:62). Early stage scientists who occupied similar positions in social space adopted different position-takings in response to the coexistence of two symbolic orders – a traditional and hybrid order – each based on a different logic and a different principle of stratification. Through these position-takings, scientists engaged in symbolic struggle over the dominance of one or the other of these competing symbolic systems. In Chapter 7, I examine how and why scientists constructed traditional and hybrid identities, contributing to the construction of a traditional or hybrid order. I also consider the study’s contribution to the science and technology studies literature on continuity and change in academic knowledge production, reflect on the study’s theoretical and methodological approach, and explore study limitations and future research directions.
Chapter 7
Discussion

7.1. Introduction
This study explored how basic health scientists perceive and respond to Canadian science policy and research funding initiatives designed to stimulate economic growth. The study used a Bourdiesian lens to understand interactions between the day-to-day actions of individuals and the social structures that emerge from, and constrain and enable, social practice. Theoretically, the study questions were based on two key assumptions about how the social world operates. According to the first assumption, participants were engaged in an ongoing competition for the forms of capital that are most valued according to the dominant definition of “good science”. A primary question of this study was, therefore, whether the definition of “good science”, and the forms of capital associated with that definition, are changing to incorporate policy-driven criteria of economic relevance. Second, the study assumed that scientists’ perceptions and strategies were structured by their positions of power in a stratified social field and contributed to the construction of the social structure. The study, therefore, also sought to understand how differences in power shaped the strategies scientists used to achieve resources and recognition, and the implications of these strategies for the distribution of power in the scientific field.

To address these questions, I explored and compared the responses of scientists trained before and after the rise of market-oriented science policy. If the logic of the scientific field has remained stable despite the efforts of science policymakers to impose change, I anticipated that scientists trained in both policy eras would retain a traditional scientific vision and seek scientific recognition according to a traditional scientific logic. In the case of stability, symbolic struggles over a scientific and market vision of “good science” would play themselves out between the scientific and policy fields. Scientists, seeking to retain a traditional vision of science, would resist changes imposed from the policy field. If the logic of the scientific field is changing, I expected to find differences in responses among and between scientists trained during the two policy eras. Among established scientists, the dissonance between habitus and field – hysteresis – would lead them to respond to new opportunities and constraints in unpredictable ways. This gap would, however, be moving towards resolution among early stage scientists who were trained in the market-oriented science policy
era, as they embodied a new kind of scientific habitus. In the case of change, symbolic struggle would occur within the scientific field among scientists seeking to maintain or improve their position by aligning themselves with or against market-oriented quality criteria.

The findings from this study support both continuity and change in the meaning of “good science” and the forms and distribution of capital. A segment of scientists trained in both policy eras continued to perceive it as possible to succeed as scientists by pursuing a traditional strategy of accumulation of scientific capital. However, participants did not predominantly resist a market vision of science and market practices, as we would expect if the traditional symbolic order in science remained unaffected by market-oriented policy initiatives. Some acted upon new possibilities for the accumulation and conversion of entrepreneurial capital into scientific capital. What can this study tell us about why scientists adopted the position-takings they did? Why were some able to “seize the opportunity created by the critical break in the ordinary order” (Bourdieu 1990b:175) and use market-oriented practices to achieve scientific recognition, while others persisted in using traditional strategies of accumulation? As we saw in Chapter 6, the volume of capital was not associated with particular position-takings, as scientists used their capital as a resource to act both with and against a traditional and hybrid symbolic order. Position-takings were shaped by structured dispositions, which inclined participants to resist or accept a market vision of science, and by access to symbolic and material resources that supported traditional or hybrid identity construction and practices. Ongoing experience and interactions also influenced scientists’ abilities to take advantage of market-oriented opportunities. Scientists who adopted hybrid identities and practices had to be able to, first, perceive the opportunities presented by market-oriented funding as opportunities that they could realistically actualize. Second, they had to be able to develop a new set of competencies to accumulate and convert entrepreneurial capital into scientific capital and to work across the boundary between science and the market.

In the first part of this chapter, I discuss how and why scientists constructed traditional and hybrid scientific identities and practices and how these contributed to the construction of a scientific or hybrid symbolic order. In the second and third parts,
respectively, I consider the implications of study findings for the knowledge production literature and for science policy and practice. Part four is a reflection on the value of Bourdieu’s theory and relational methodology to science and technology studies and the boundary work literature, more broadly. Finally, I conclude with some considerations about study limitations and recommendations for future research.

7.2. Identity Construction, Scientific Practice and the Symbolic Order

Market-oriented funding alters the “space of possibles” for the construction of identities and for scientific practice. Scientists who participated in this study responded to these new possibilities in a variety of ways. Those who adopted position-takings of reaffirmation and adaptation constructed their identities as traditional basic scientists, but varied in their emphasis on the fundamental and applied dimensions of their research. Those who assimilated constructed a different kind of basic science identity – a hybrid identity – that incorporated both a market and traditional scientific logic. Through symbolic strategies to construct a traditional or hybrid vision of “good science” and practical strategies to accumulate capital, scientists participated in symbolic struggles over the dominant symbolic order. In this section, I will consider why and how traditional and hybrid identities were constructed, their relationship to practice, and whether they supported a traditional, hybrid or dual symbolic order.

Scientists trained in both policy eras and with varying amounts of scientific and entrepreneurial capital constructed both traditional and hybrid identities and pursued both traditional and hybrid strategies of accumulation of capital. This is suggestive of both continuity and change in the dominant symbolic order in the scientific field. Scientists neither uniformly resisted change imposed from the policy field onto the scientific field, nor did scientists trained within a market-oriented funding context uniformly accept a market vision of science. The majority of scientists trained before the rise of market-oriented science policy experienced hysteresis – a gap between their categories of perception of “good science” and the requirements of market-oriented funding programs. Of these, some responded by altering their identities and practices to bring them into alignment with the new social conditions, while others resisted and sought to reinstate a traditional vision of “good science” as the dominant symbolic order. A minority of established scientists who had adopted hybrid identities
and practices before changes in science policy and funding both legitimized, and were legitimimized by, the rise of a new symbolic order centred on a market-oriented vision of “good science”. Scientists trained after the introduction of market-oriented policies and funding constructed both traditional and hybrid identities. However, most continued to pursue traditional strategies of accumulation of scientific capital.

It is not surprising that the majority of established scientists reaffirmed a traditional scientific identity – the dispositions of habitus are durable and have a tendency to persist, even after they have outlived the conditions of their formation (Bourdieu 1990a). Why, then, did some established scientists assimilate and others adapt to a market vision of science? Scientists who constructed hybrid identities before the rise of market-oriented science policy were predisposed to perceive the value of industry collaboration before it had become an accepted practice for basic scientists. In order to adopt practices that did not align with the dominant definition of “good science” without compromising their own position in the field, this group relied on having a moderate to high volume of scientific capital. Those who occupy dominant positions within a field often have more freedom to innovate and are more likely to lead the revolutions that transform the types and value of capital:

The fomenters of scientific revolutions are recruited, not among the least armed among the newcomers, but on the contrary, from among those who are scientifically best endowed. We thus know that inaugural revolutions – which have given birth to new fields by constituting new realms of objectivity – have nearly always been the doing of holders of considerable amounts of specific capital who, owing to their membership in a class or an ethnic or religious group improbable in this universe, found themselves in an ambivalent position likely to foster nonconforming and nonconformist dispositions (Bourdieu 1991:18-19).

Established scientists who assimilated to market-oriented science were non-conformists in their time. They actively supported and engaged in the legitimation of hybrid science, using their scientific capital as a resource to subvert the traditional symbolic order, and helped to bring into being a hybrid symbolic order through their symbolic strategies and through their entrepreneurial or political practices. The rise of market-oriented science policy affirmed their hybrid identity, enhancing the congruence between their identity and the funding environment, in addition to their identity and practice.
Unlike scientists who assimilated, those who reaffirmed and adapted were not predisposed to value or participate in market-oriented science. They experienced dissonance between their traditional identities and the funding environment. How can we understand why some responded to this dissonance by altering their identities and practices, while others resisted change and reasserted the superiority of a traditional vision of science? To some extent, all scientists, particularly in the life sciences, have laid down the conceptual and practical building blocks to foreground either the “purity” or “utility” of basic science research. These are, as Kinchy and Kleinman (2003) say, historically resonant discourses in science, and the emphasis shifts back and forth between them depending on the political context. Since the category of basic science itself contains both of these discourses, it provides scientists with a degree of flexibility in how they frame their research without threatening their identity (Calvert 2004). Thus, it is not surprising that all scientists in this study used strategies to make their research appear applied or commercially relevant when applying for research funding. This strategy reduced, but did not eradicate, the dissonance they experienced between their traditional values and funding criteria.

Some established scientists were unwilling or unable to alter their identities or practices to further reduce this dissonance. Engaging in commercialization practices in order to acquire resources for the advancement of their research was outside of the realm of the possible for this group. Established scientists who reaffirmed a traditional vision of science expressed disenchantment, anger, or bewilderment in response to expectations to justify their research in terms of economic relevance, to patent their research or to engage in industry collaboration. For those who had a high volume of scientific capital, reaffirmation was a form of resistance to the intrusion of economic values and practices within the academic sphere, whereas those who had a low volume of scientific capital appeared resigned to the new field conditions. Scientists who occupied a dominant position in the field were protected by their ability to successfully compete for highly competitive investigator-driven grants. They were able to ignore or opt out of market-oriented funding competitions without perceiving a threat to their objective position of power. Those who were not at the top of their field experienced intense pressure to publish in order to remain competitive in investigator-driven grants competitions. They were more likely to resign themselves to a decline in prestige than to alter their practices.
Established scientists who adapted to market-oriented funding by altering their identities and engaging in a new set of market practices sought to reduce the dissonance they experienced between their traditional identity and practices and the funding context. Identity construction is an ongoing process in which individuals can consciously and intentionally choose to adapt to social or political change by bringing uncalled elements of meaning buried within the habitus to the foreground of their social identity (Todd 2005). These scientists were more inclined to be open to market opportunities, had access to symbolic and material resources that supported market practices, and were exposed, through ongoing interactions and experiences, to specific market opportunities that they were able to benefit from.

Two key differences emerged between those who resisted and those who adapted to change in the funding context. First, those who altered their identity and practices had to be able to perceive market opportunities as opportunities that they could realistically actualize. Second, they had to successfully convert their entrepreneurial capital into scientific capital. In some cases, scientists were motivated to file a patent application or collaborate with industry because they recognized a clear potential for the development of an application based on a scientific discovery. However, the ability to recognize market potential may itself require certain market competencies, as what has value in economic terms is very different from what has value in scientific terms (Gittelman and Kogut 2003; Packer and Webster 1996).

According to Packer and Webster (1996), the ability to perceive and act on the market potential of a scientific discovery depends on the acquisition of a new set of “sociotechnical competencies” that are unrelated to scientific knowledge or competency (p. 450). Scientists who engaged in market practices had to learn to: speak a new language in order to translate the scientific value of their research into economic terms; write business cases for grant applications; file patent applications; and communicate with industry representatives, lawyers, and technology transfer staff. As they gained market knowledge, they became more adept at identifying aspects of their research with commercial potential. Through the development of these competencies they were able to accumulate entrepreneurial capital in the form of patents, licenses, and industry funding, and in some cases, by spinning of their own companies.
Yet, acquiring the competencies to accumulate entrepreneurial capital was not sufficient. They also had to be able to convert this economic form of capital into scientific capital, by investing market-oriented resources into laboratory equipment and staff that generated scientific knowledge, leading to peer-reviewed scientific publications. As was clearly illustrated in Robert’s case, a high volume of entrepreneurial capital did not ensure a favourable stance towards a market vision or science, or continued engagement in market practices. Under some conditions, engaging in market practices was a means to maintain or enhance scientific prestige; under others, it became a threat. The scientists in this study who were most satisfied with their ability to accumulate and leverage entrepreneurial capital into scientific capital were those who had access to boundary organizations and used boundary maintenance strategies to manage contradictions between scientific and market goals and practices.

Technology transfer offices, the university interdisciplinary research centre and market-oriented funding organizations functioned as boundary organizations (Guston 1999, 2001) that both pressured and aided scientists to work across the boundary between science and the market. These organizations helped scientists to acquire market competencies and lowered the cost of investing in the accumulation and conversion of entrepreneurial capital into scientific capital. They played an important role in enabling scientists with a low volume of scientific capital to adopt market practices by providing material support to cover the costs of acquiring patents and the diversion of time and labour to non-scientific practices. In this study, hospital-based scientists experienced both the greatest formal institutional pressure and support for research commercialization. Technology transfer offices in hospitals paid the patenting costs and facilitated relationships with industry representatives. Hospitals also evaluated scientists on the number of patents they attained which contributed to the development of a commercialization culture.

University-based scientists, in contrast, received little formal support from the university technology transfer office and little pressure, in terms of formal evaluation, to commercialize their research. For university-based scientists who chose to commercialize their research, access to informal social networks helped to compensate for the shortcomings of the university technology transfer office.
University scientists located in the interdisciplinary research centre had access to an extensive network of scientists and industry representatives from which they gained assistance with writing business plans and accessing industry funding. Most scientists in this centre also had reduced teaching commitments which may also have freed up time needed to invest in the acquisition of market competencies and industry relationships. Funding organizations themselves also facilitated the development of market competencies and helped scientists to develop the capacity to perceive the market potential of their research. Networks of Centres of Excellence (NCEs) were, in particular, attributed with bringing scientists into direct contact with industry representatives over a prolonged period of time.

While adaptation reduced the dissonance established scientists experienced between their habitus and the field, it also created internal dissonance between identity and practice. To manage these contradictions, scientists redrew the symbolic boundaries around the meaning of a traditional vision of “good science” to emphasize the dimension of utility in order to accommodate their market practices (Calvert 2004, 2006; Kinchy and Kleinman 2003). However, they simultaneously resisted the imposition of funding criteria that restricted scientific autonomy or made scientific success contingent on the production of market products. Market practices were seen as a means to an end and were subordinated as much as possible to the dominant goal of advancing scientific knowledge. By acting both with and against a market-oriented vision of “good science”, this group of scientists contributed to the construction of both a traditional and hybrid symbolic order. Conversely, those who reaffirmed and assimilated experienced congruence between their scientific identity and practice, supporting, respectively, the construction of a traditional or hybrid symbolic order.

Like established scientists, early stage scientists, possessing varying amounts of scientific and entrepreneurial capital, constructed both traditional and hybrid identities. However, they constructed their identities for different reasons, and experienced their identities differently, than established scientists. Scientists who assimilated within the context of market-oriented science policy were, like established scientists who assimilated, predisposed to value entrepreneurial capital and market outcomes. However, their scientific dispositions were directly related to their training experiences and seemed to reflect conformity to the funding context. Most early stage
scientists who constructed hybrid identities had trained with supervisors who were involved in commercialization or had filed patent applications before beginning their first academic position. They acquired the lens which enabled them to perceive market-oriented opportunities in their routine training as doctoral students and post-doctoral fellows. Their perceptions of the opportunities available through market-oriented science were, therefore, based on a realistic assessment of their objective chances of realizing those opportunities, as the features of a hybrid habitus were well-adjusted to a hybrid symbolic order.

The assimilation of a market-oriented vision of “good science” by early stage scientists through their routine training suggests that change in the policy field may be in the process of being institutionalized within the scientific field. Although several studies suggest that changes in scientific values and practices may be occurring among pre-tenure scientists (Ambos et al. 2008; Crowe and Goldberger 2009; Lee 2000), support for this view is mixed. Change at the symbolic level, in the taking-for-granted of a new language of economic relevance, is not necessarily accompanied by changes in practice. As found by Ambos and colleagues (2008), pre-tenure scientists may be more comfortable with the dual demands of science and industry, but only a small minority produced both scientific and market products. Similarly, in this study, early stage scientists who assimilated were more likely to redraw the symbolic boundaries around the legitimate vision of “good science” than to pursue a hybrid strategy of accumulation of entrepreneurial and scientific capital. Like established scientists who adapted, early stage assimilators supported the construction of both a traditional and hybrid symbolic order. Rather than reflecting internal tensions between identity and practice, this contradiction may reflect the unique constraints that scientists experienced relating to the early stage of their career.

Early stage scientists faced three obstacles to participating in market practices. First, the formal rewards and recognition in science continued to depend primarily on producing peer-reviewed publications, leading early stage scientists to focus their time and resources on traditional scientific practices. University-based scientists, in particular, had a singular focus on publications until they achieved tenure. Second, they were constrained from participating in market-oriented practices until they had accumulated a stock of scientific knowledge with commercial potential. Third, the
availability of early career grants intended to support them in establishing their scientific reputations also insulated them from pressures to apply for market-oriented funding. As they progress into the mid-stage of their careers, early career scientists with hybrid identities may begin to carry out hybrid practices, bringing their identities and practices into alignment. Alternatively, this contradiction between identity and practice may persist, at least for some, as they move into the mid-career stage, due to ongoing contradictions between scientific and market goals and practices.

The sole early stage assimilator who was actively involved in research commercialization resembled established assimilators in that he possessed a high volume of scientific and entrepreneurial capital, and adopted a vision of “good science” that was out of step with his peers. While all early stage scientists took it for granted that research funding was contingent on demonstrating relevance, they continued to support scientific quality criteria and peer-review processes as the dominant mechanisms for evaluating scientific worth. The entrepreneurial assimilator, in contrast, claimed that market products (as reflected in entrepreneurial capital) should be recognized as objects of value in their own right, and not just used as a resource to invest in the accumulation of scientific capital. He sought to raise the value of entrepreneurial capital to be the equivalent to scientific capital. Through this stance, he supported the replacement of a traditional symbolic order with a hybrid symbolic order, rather than their continued coexistence.

Unlike early stage assimilators, early stage scientists who had not acquired market competencies during their training constructed traditional identities and pursued traditional practices. Like their established counterparts, they reasserted a traditional symbolic order. However, they took for granted that two symbolic orders coexisted. Most were not concerned that opting out of market practices would compromise their ability to succeed as scientists. They were confident that producing good publications would be sufficient to acquire grants, tenure and recognition. New scientists entering the scientific field continued to construct traditional dispositions and expectations that they could succeed as scientists without engaging in market practices. However, just as the practices of hybrid scientists may change as they move into the mid-career-stage, reaffirmers may experience increasing pressure to alter their identities and
practices in order to acquire resources, particularly if they are not among the top performers who are assured success in investigator-driven grant competitions.

Market-oriented science policy introduced a new set of rules, produced a new form of capital (entrepreneurial capital) and opened up a new “space of possibilities” for the achievement of scientific recognition. In this new space, scientists participated in symbolic struggles over the dominance of a traditional or hybrid symbolic order and system of stratification. Because change in the vision of “good science” was only found at the point of accessing resources, and not at the point of gaining recognition, it remained possible for scientists to conform to traditional scientific practices. However, because a change in the vision of “good science” produced a new form of entrepreneurial capital, scientists could also engage in hybrid practices to maintain or improve their scientific prestige, contributing to the production of a hybrid symbolic order.

Scientists who adopted both traditional and hybrid strategies occupied varying positions of power in the scientific field, suggesting that there were no clear “winners” or “losers” in this ongoing symbolic struggle. However, given the greater prevalence of both pressure and support for scientists to participate in research commercialization in hospital research settings and the university interdisciplinary research centre, the space in which scientists can resist market pressures may be shrinking to traditional academic departments, where tenure continues to protect academic autonomy. Even in traditional settings, freedom from market criteria may only continue to be possible for the highest performers, who are able to successfully compete for investigator-driven grants.

7.3. Continuity and Change in the Evaluation of Academic Quality

Canadian science policy has introduced a range of funding programs that promote the transformation of traditional academic science from a “Mode 1” to a “Mode 2” system of knowledge production (Gibbons et al. 1994; Nowotny et al. 2001). According to the Mode 2 thesis, scientists are being held accountable to novel quality criteria. In the science and technology studies (STS) literature, there has been little research that focuses explicitly on the evaluation of academic quality and the evidence that does exist is contradictory (Hessels and van Lente 2008). One strand of research suggests that, in contradiction to Mode 2 claims, there has been no change in the
evaluation of scientific quality or the distribution of rewards in science. Scientists continue to value, and to be evaluated by, traditional scientific criteria and peer-review processes (Gulbrandsen and Langfeldt 2004; Henkel 2005; Morris and Rip 2006). The second suggests that the conditions under which scientists compete for prestige have changed, lending support to Mode 2 claims. Scientists are held accountable to market-oriented criteria, and scientists may, under some conditions, be able to enhance their scientific prestige and elevate their status relative to non-industry-involved faculty (Van Looy et al. 2004, 2006; Owen-Smith 2003). The dichotomy of the Mode 1/2 framework leads to the polarization of arguments that either support or contradict its claims. Findings from this study suggest that scientific quality criteria remain dominant but are no longer the sole criteria, as the attainment of resources is increasingly linked to market-oriented criteria.

The widespread persistence of traditional scientific values and identities among academic faculty suggests that certain aspects of academic quality criteria persist. A number of studies that have found that academic faculty across research fields and national settings continue to value and defend a traditional vision of science (Albert 2003; Calvert 2004, 2006; Gulbrandsen and Langfeldt 2004; Henkel 2005; Kinchy and Kleinman 2003; Morris and Rip 2006). Further support has been provided for the continued dominance of traditional evaluation criteria by studies of the publications scientists produce (Gulbrandsen and Smeby 2005; Ranga et al. 2003; Van Looy et al. 2004). Similarly, this study found that scientists used boundary strategies of demarcation to defend a traditional vision of science based on serendipity, novelty, methodological rigour and peer review. They continued to value and produce peer-reviewed publications in basic science journals in order to acquire scientific prestige. Through their symbolic strategies and practices they sought to maintain academic control over the evaluation of scientific quality, according to a traditional vision of “good science”.

The continued dominance of traditional scientific quality criteria is, however, only one part of the picture. Market-based criteria may, as this study has shown, coexist alongside traditional values and practices, at the point of accessing resources. Hence, this study also supports the second strand of research in STS which suggests that a change may be occurring in the conditions under which scientists compete for
scientific prestige. It is feasible, as Van Looy and colleagues (2004) found, for scientists to successfully combine their scientific and market practices and leverage their entrepreneurial productivity into greater scientific productivity, resulting in a “compounded Matthew effect”. Like Merton’s (1968) Matthew effect, in which scientific recognition begets further recognition, Van Looy and colleagues found that success according to market criteria (patents, licenses, spin-offs) multiplies and enhances success according to both market and scientific criteria (publications). According to Owen-Smith (2003), traditional and market-oriented reward structures have become integrated in a hybrid system of stratification, in which success according to scientific criteria is increasingly dependent according to success according to market criteria.

While the findings from this study support the emergence of a hybrid system of stratification, they do not support the replacement of the traditional system of stratification with a hybrid system. Scientists trained in both policy eras perceived it as possible to succeed without engaging in market practices. While it is feasible, as Van Looy and colleagues (2004) and Owen-Smith (2003) have illustrated, for scientists to achieve both scientific and market goals, and use success in one realm to further their success in the other, this can, however, be a hazardous process. The ongoing contradictions that scientists experience when they combine market and scientific practices, and the difficulties involved in converting entrepreneurial capital into scientific capital, make it unlikely that it will become common practice for the majority of scientists to adopt hybrid practices. If, however, there is a decline in access to investigator-driven funding or academic institutions adopt market-oriented criteria as a condition for tenure or promotion, traditional strategies may no longer be sufficient.

The findings from this study add support to a growing body of qualitative research which suggests that too much industry-involvement or too much market success can undermine scientific and market goals and practices (Blumenthal et al. 1986; Shinn and Lamy 2006; Tuunainen 2005; Ylijoki 2003). In the first large-scale study of university-industry involvement conducted by Blumenthal and colleagues (1986) over two and a half decades ago, scientists productivity fell when they derived more than two thirds of their support from industry. The threat of market involvement to
academic science may be heightened when clear boundaries are not maintained between scientific and market practices. Contrary to Mode 2 assumptions, the intensification of university-industry relationships does not necessarily result in boundary erosion, but may lead to heightened boundary work among academic scientists.

This may help to explain why Van Looy and colleagues (2004) found such strong support for a compounded Matthew effect. Their study was conducted at a university in Belgium that had adopted a strategic stance towards research in the 1970s. The university developed an infrastructure at that time to support industry collaboration and research commercialization, including a centralized research and development office and decentralized research divisions. The industry-involved scientists who participated in their study belonged to research divisions which carried out contract research and received support for patenting and spinning off companies from the central office. Both the research and development office and decentralized research divisions appeared to function as boundary organizations, facilitating both market and scientific goals. Scientists who lack this kind of structured support may not be as successful in overcoming the inherent contradictions between scientific and market goals and practices.

The apparent contradictions between the academic world and the world of the market which necessitate ongoing boundary work may not rest simply on the pursuit of different, yet equal, values. As Kleinman and Vallas (2001) suggest, they may be based on asymmetrical power relationships between science and industry. They suggest that universities and industries are undergoing a process of asymmetrical convergence, in which the norms and practices of public and private science converge in both academic and industry settings. However, in this process of convergence, industry retains the upper hand. Because of the weight in favour of market forces, scientists who cross traditional boundaries to practice hybrid science must carry out boundary work to protect and retain control over their scientific agendas. This process of asymmetrical convergence has produced a contradictory knowledge regime in which the cultural norms and practices of science and the market coexist in uneasy tension (Vallas and Kleinman 2008).
What are the implications of the emergence of a change in the system of stratification in the scientific field for the nature and quality of academic knowledge? The science and technology studies literature has explored three primary threats of market-involvement to traditional science: the skewing of research agendas from basic to applied topics ("skewing effect"), the substitution of market practices for scientific practices ("substitution effect") and reduced openness and sharing of scientific knowledge ("secrecy effect"). If the culture and reward system of science are changing, these threats may, in fact, be more subtle and more widespread, among industry-involved and non-industry involved alike, than the current research suggests.

While patent-publication output studies have found little support for a "skewing effect" (Gulbrandsen and Smeby 2005; Ranga et al. 2003; Van Looy et al. 2004), these studies do not capture skewing that may occur before the publication stage. Participants in this study reported that they altered their research problems and methods, quite substantially in some cases, in order to access market-oriented resources. The shift in emphasis from basic to applied dimensions of research was done at the stage of framing research problems and acquiring funding, rather than publishing in applied science journals or producing reports for a non-scientific audience. To explore whether a market logic is shifting scientists’ research agendas, further research should focus on how scientists select and frame research topics rather than where they publish their findings.

Because basic research, according to its traditional definition, contains both applied and fundamental dimensions (Calvert 2004, 2006), it may be difficult to assess the extent to which the nature of research is being steered according to a market logic. Academic scientists may alter the appearance of their research in order to meet funding criteria without altering their actual research questions or methodologies. Yet, if market norms become more prevalent within academic settings, scientists may be unaware of the extent to which they frame their research questions to conform to cultural expectations. As long as recognition continues to be awarded based on a traditional definition of "good science", scientists may unconsciously defend their research as fundamental, even if they increasingly carry out their research in areas with greater potential, or the appearance of greater potential, for application. In this case, basic science research will increasingly be carried out in “Pasteur’s Quadrant”
(Stokes 1997), the category of use-inspired basic research which lies between ‘pure’ basic research (Bohr’s Quadrant) and ‘pure’ applied research (Edison’s Quadrant) in Stokes’ typology of modes of knowledge production.

While secrecy, data withholding and publication delays have been identified as a potential threat to the advancement of science (Blumenthal et al. 1986, 1996; Slaughter et al. 2004; Zinner et al. 2009), few instances of secrecy emerged in this study. Vallas and Kleinman suggest that secrecy is a product of the growing competition in science, leading traditional scientists to withhold data or material from one another until after they have published. The pressure scientists experience to frame their research as applied in order to get funding may also lead traditional scientists to withhold the true nature of their research from funders and carry out fundamental research in secret.

The findings from this study help to illustrate some of the conditions under which a “substitution effect” may occur. While scientific productivity may, under some conditions, be compatible with and even enhanced by market practices, it may, under other conditions, be undermined. There may be a cost of market-involvement in terms of time and resources spent on meeting market-oriented funding criteria, managing multiple grants, negotiating new relationships, learning new competencies and monitoring boundaries between scientific and market practices. Even the most entrepreneurial participants in this study experienced ongoing tradeoffs between their market and scientific practices. Scientists who do not have adequate resources or who fail to convert their entrepreneurial capital into scientific capital may pay a high price for their market engagement. A different kind of “substitution effect” may exist for non-market-involved scientists who invest significant amounts of time and energy tailoring their research to make it appear more relevant. While competitive processes for funding and recognition are mechanisms to ensure academic quality, these processes may fail when the competition becomes so steep that scientists become focused on meeting these requirements rather than on carrying out scientific research.

7.4. Implications for Canadian Science Policy and Practice
Canadian science policy has an explicit goal to steer basic research in applied, economically relevant directions. Some participants in this study reported altering their research topics, methodologies and working relationships in order to produce
both market and scientific products. However, participants also adopted strategies to minimize the steering effects of funding criteria. Market-oriented science policy is based on several weak or faulty assumptions adopted from the Mode 2 thesis that may undermine its effectiveness according to both scientific and market criteria. First, it assumes that traditional and market-oriented science practices are complementary. Under some conditions, for some scientists, they may be complementary, but this is not always the case. At what point do these tradeoffs result in a decline in scientific quality or productivity?

Second, market-oriented science policy assumes that scientists are equally able to participate in market-oriented science. Yet, as this study has illustrated, scientists may be unequally positioned to engage in and benefit from market practices. If scientists who are unable to effectively engage in or benefit from market practices are pressured into carrying out these activities, they may adopt strategies to make their research appear to meet funding criteria that they have little intention of actually meeting. This could result in an inefficient use of resources, both by policymakers and scientists.

Third, market-oriented science policy is based on the assumption that a Mode 2 system of knowledge production will lead to an acceleration in the speed of production of more useful knowledge. There is little, if any, research evidence to support this claim (Hessels and van Lente 2008; Gittelman and Kogut 2003). Commonly used indicators, such as the volume of industry funding raised, number of patents and number of spin-off companies, are proxies for success as they do not actually measure the development of new technologies, services or companies that drive economic growth. As this study suggests, matched funding may be “window dressing”, patents may not lead to successful product development and revenue generation. Spin-off companies may fail to advance scientific knowledge or generate products and may even go bankrupt. In the absence of clear evidence supporting a change in the mode of knowledge production, an adequate funding base for traditional, discovery-oriented science needs to be provided. This would ensure that scientists who lack the competencies and resources to successfully engage in market-oriented science can continue to compete for scientific recognition according to a traditional scientific logic. Furthermore, the opposition that many scientists expressed towards market-oriented funding may be reduced if policymakers ensure that the
monetary value of grants and success rates for investigator-driven and market-oriented grants are comparable.

I would also offer a recommendation to scientists themselves. Science policy emphasizes economic growth as the core benefit of applied research. This may steer research towards health interventions (e.g. drug-based) that have the potential to generate profits and away from research on disease prevention, the social determinants of health, and diseases prominent in the developing world. As scientists participated in symbolic struggle over what constitutes legitimate science, they overwhelmingly accepted the economic vision of the public good embedded within science policy. Surprisingly little voice was given to what constitutes the legitimate vision of the public good. Only two participants identified the economic focus of science policy as a potential threat to the public good. By defending science as an elite practice that is beyond the grasp of ordinary citizens, instead of aligning themselves with non-scientists who oppose an economic vision of the public good, scientists may themselves be losing out on an opportunity to influence the direction of science policy. If voters demand that their governments fund the kind of research that serves the non-economic interests of public safety and social welfare, politicians may be more likely to invest in traditional research.

7.5. Reflections on Theory and Methodology
Using Bourdieu’s theory of practice and a relational methodology, I have constructed an account of the symbolic struggle over the dominant vision of “good science”, one that suggests tendencies towards continuity and change in the structure of the scientific field. As we can recall from Chapter 4, a relational methodology demands that the researcher focus on both the space of position-takings and the space of positions (Emirbayer and Johnson 2008).

7.5.1. Value of a Relational Methodology
The strength of this approach has been its ability to suggest that symbolic responses to market-oriented science are rooted in power structures and relations. Using a relational approach enabled me to not only describe the strategies scientists adopted to construct a traditional or hybrid symbolic order, but to identify tendencies towards continuity and change, based on the objective positions of power associated with these strategies. It is easy to understand why the science and technology studies
literature is divided on the extent to which change is occurring in the mode of knowledge production. Different studies have concentrated on different parts of the scientific field, highlighting particular position-takings of particular groups of scientists. Blumenthal and colleagues (1986) identified differences in position-takings between industry-involved and non-involved in his first study of scientists’ perceptions of university-industry collaboration. From a Bourdieusian perspective, it seems obvious that these differences reflect different position-takings in a symbolic struggle over the legitimate vision of science. Non-market involved scientists experienced market-oriented science as a threat to their position in the scientific field, which they framed as a threat to the production of “good science”. Yet, without examining the relationship between these position-takings and their holders’ objective positions, it is difficult to interpret their impact they have on the dominant vision of “good science”.

For science and technology studies scholars, Bourdieu’s relational methodology offers an alternative to boundary work as a framework for exploring the changing relationship between academic science and industry. Boundary studies have the tendency to describe the symbolic strategies used to construct symbolic boundaries without explaining the relationship between symbolic boundaries and objective social structures (Lamont and Molnar 2002). Boundary studies are frequently critiqued for their inability to explain variations in the kinds of boundary work that are carried out and variations in their success (Albert et al. 2009; Kinchy and Kleinman 2003; Lamont and Molnar 2002; Pachucki et al. 2007). The likelihood of individuals adopting particular strategies of resistance and accommodation to changes in symbolic categories, and the effectiveness of these strategies, are inextricably linked to the objective positions of those who take up these positions. Due to the relative autonomy of the symbolic order from the social structure, change in one realm cannot be assumed to produce change in the other without empirical investigation (Bourdieu 1990c). Boundary studies will continue to be stymied in their ability to explain variations in boundary strategies, and their outcomes, if they continue to detach the study of meaning from relations of power. Bourdieu’s theory and methodology provides a way to move boundary studies forward.
7.5.2. Value of Todd’s Framework

Bourdieu’s relational methodology guided the analysis of the space between positions and position-takings. Yet it was Todd’s (2005) framework that enabled me to look more closely at change in identity and practice in response to the gap between habitus and field. Todd has reconstructed Bourdieu’s concept of habitus to focus on its generative aspects, as well as its structured aspects, and to allow for the possibility of intentional strategic action, as individuals adjust their identities and practices to broad socio-political change. Given that the position-takings of participants in this study did not closely align with their positions in social space, I needed to find alternate explanations for variations in scientific practice. Because her framework allowed for the possibility of both unconscious, routine action and conscious, strategic action, I was able to generate an account of how and why some scientists were able to take advantage of the new opportunities made available by a change in the symbolic order, while others were not. These strategies appeared to be shaped, in part, by structured dispositions which created tendencies towards or against acceptance of market practices. However they were also shaped by participants’ interactions with other scientists, with funding agencies, with industry, with technology transfer staff, and with hospital and university administrators.

Todd also broadened Bourdieu’s concept of the field. Social fields can, she claims, be constructed around several competing symbolic orders which coexist within a single field. The distinct cultural substance of each symbolic system may be retained, leading members of the field to internalize dominant and recessive aspects of the social order. As we saw in this study, symbolic struggle was not carried out simply with or against a traditional or market vision of science. Each of these symbolic systems provided different opportunities for scientists to achieve scientific recognition, leading some to act both with and against a traditional and hybrid vision. The coexistence of dual symbolic systems and forms of capital may have opened up new possibilities for the strategic action, which were unequally distributed among study participants, to accumulate scientific prestige.

I found that Todd’s typology was more applicable to established scientists, who reconstructed their scientific identities in response to policy change, than to early stage scientists, who were in the process of constructing their identities within the
context of market-oriented science policy. This is not surprising as the typology was developed to explain identity change. The category of adaptation, by definition, did not apply to early stage scientists, as it is a response of individuals who acquire their social identity under one set of social conditions to a change in those conditions. Early stage scientists constructed their identities under market-oriented science policy and had not experienced a change in social conditions that would lead to adjustments in their scientific habitus. However, Todd’s typology was still useful for understanding and comparing how scientists constructed their identities across career stages. While early stage scientists adopted position-takings of reaffirmation and assimilation, they experienced these position-takings differently than established scientists.

Todd’s typology identifies six logically possible responses at the level of identity to social change, yet this study only identified three responses – reaffirmation, adaptation and assimilation. Due to the nature of the scientific field and the study design, three remaining categories – conversion, privatization and ritual appropriation – did not apply. Both categories of conversion and privatization involve the complete loss of identity resulting from withdrawal from the field into the private sphere or into another field. This study did not allow for the inclusion of scientists who had left the scientific field. Ritual appropriation occurs at the organizational or institutional level, when traditional symbols are retained even as the practical significance of that content is replaced. This study did not focus on the organizational or institutional level.

7.6. Limitations and Future Research
The use of a relational methodology is analytically demanding, as it requires the construction of the objective spaces of positions and subjective space of position-takings and the analysis of the relationship between these two spaces. As few Bourdieusian scholars actually carry out a relational methodology (Emirbayer and Johnson 2008), there was little to draw upon in the scholarly literature, outside of Bourdieu’s own empirical research, for guidance.

Any study design, theoretical approach and methodology contains both strengths and limitations. This study took the system of relations of a particular group of scientists as its object of study. I focused on the symbolic struggle occurring among basic scientists in the life sciences who were engaged in health-related research at a research-intensive Canadian university. The study findings therefore reflect the
symbolic struggles that are occurring within this context. Symbolic struggles in the physical and applied sciences and the social sciences and humanities may play themselves out differently, depending on the kinds of symbolic and material resources faculty in these fields bring to bear. While a market logic was objectified in entrepreneurial capital in the biotechnology field, it may take other forms in academic fields such as history or philosophy that have a limited probability of generating patents or founding companies. Different manifestations of symbolic struggle will also exist across different kinds of academic research institutions and national contexts. Nevertheless, I believe that symbolic struggles between a traditional and market vision of academic quality are likely to be found across disciplines and universities, at least within Canada. The dominance of one or the other of these symbolic orders may vary across different contexts. Future research should explore the responses of academic faculty in other disciplines and faculties to see whether they are also being polarized according to particular forms of cultural and economic capital. At the organizational level, it may be possible to track changes in the relative status of different disciplines and fields depending on how they respond to a market vision of science.

In constructing the space of positions, I faced three challenges in identifying and gathering data on the legitimate forms of capital. The first challenge was unavoidable. There are always likely to be weaknesses in the identification of indicators of the forms of power that are dominant in a given field at a given time, as these forms are the constant object of symbolic struggle (Bourdieu 1990b). Even if these provided a crude measure of the type and volume of scientific (cultural) and market-oriented (economic) power scientists held relative to one another, this still provides an advance over existing studies which have rarely explored power relations as either a cause or consequence of change in the mode of knowledge production (with the notable exceptions of Owen-Smith 2003 and Van Looy et al. 2004, 2006).

The second challenge arose from the small study sample, which meant that I was only able to map the objective positions scientists occupied in relation to each other, rather than in relation to the scientific field. I would speculate that the scientists in this study who were ranked as having a low and high volume of capital would likely retain this ranking, but some of those ranked as having a moderate volume may have been found
to occupy lower or higher positions within the field. The study sample included participants with a wide range of entrepreneurial and scientific capital, which suggests that it was not skewed towards a particularly ‘successful’ or ‘unsuccessful’ group of scientists. I am therefore satisfied that the construction of the space of positions provides an adequate basis for the analysis of the relationship between position within the scientific field and position-takings towards market-oriented science.

The third challenge in constructing the space of positions resulted from the difficulty I encountered in gathering data related to participant’s primary habitus. A question about the participants’ parent’s level of education and occupation was included on a Background Information Form that I asked participants to complete at the time of the interview. Participants were reluctant to complete the section, often questioning me about why it was there. When this section of the form was completed, the information was often unusable as an indicator of class origin (for example, “retired” as a response to father’s occupation). When I found that the positions that scientists occupied in the scientific field were not, for the most part, associated with particular position-takings towards market-oriented science, I was unable to explore whether or not this could be understood in relation to participants’ class habitus.

It is plausible that academics born in higher class strata may embody the economic and cultural forms of capital that allowed them to recognize and take advantage of the opportunities that conform to an economic logic and to join the social networks consisting of lawyers, venture capitalists, managers and industry representatives that are necessary to market success. Kleinman and Vallas (2001) suggest that the “technical intelligentsia” –a narrow portion of the educated classes who have an affinity for instrumental, market-based values are rising in ascendance in the workplace while other workers experience growing marginalization (p. 464). Scientists who reshuffle their identities to accommodate market-oriented criteria may preserve their status in the class structure, even if even if this results in a change in the distribution of resources and recognition within the scientific field. New entrants to basic science may come to resemble entrants to professional fields, such as law and medicine, which have been structured around both economic and cultural (scientific) capital and tend to be composed of members from higher social classes (Bourdieu 1990b). This, however, remains an open question for future empirical study.
The study’s small sample size was also a limitation in constructing the space of position-takings. Although Bourdieu (1990a) says, “a structurally determining position can be represented by a very small number of people and sometimes…by a single person” (p. 76), a stronger account of participants’ position-takings would have resulted if I had conducted interviews until reaching a point of saturation of data (Bowen 2008) for each of the structural positions (e.g., high scientific capital:low entrepreneurial capital) represented within each career stage. Most structural positions were represented by only one or two participants. As an exploratory study using a unique approach to study continuity and change in the system of academic knowledge production, it raises some very interesting questions and points in new directions for future research. Future research should be undertaken with a larger sample size to confirm this study’s findings.

I have argued that a dual symbolic order may persist indefinitely within science if recognition of “good science” continues to be evaluated according to traditional scientific criteria and basic research continues to be funded through investigator-driven grants. It is also possible that the coexistence of a traditional and hybrid order is simply a stage in an advancing process of institutionalization of a hybrid order. To explore this possibility, future research might examine whether the identities and accumulation strategies of scientists trained in the market-oriented science policy era change as they move into mid-career stages. Another strategy would be to examine changes in the volume and composition of capital that are associated with particular position-takings over time.

This study has focused on scientists’ perceptions and responses to an economic vision of scientific worth that is embedded in Canadian science policy. However, economic relevance is only one dimension of relevance that has come to dominate policy agendas. Targeted funding, for example, is driven by a political or lay vision of relevance, but this vision is not always explicitly economic. According to Hessels, Lente and Smits (2009), a new social contract between science and society is being written. While the societal relevance of knowledge has always been a part of this contract, its meaning is ambiguous and shifting. While participants in this study did not distinguish between funding criteria intended to promote social relevance from
criteria promoting economic relevance, further research should explore these different meanings and the strategies scientists use to demonstrate different kinds of relevance.

Methodologically, I would recommend science and technology studies scholars adopt a relational mode of thinking. By focusing on interactions between subjective meanings and objective positions of power, STS researchers may be able to go beyond representing the position-takings of particular groups of actors as scientific accounts and instead produce explanatory accounts of the interactions and struggles that occur between individuals and groups who have different types and amounts of power.

7.7. Conclusion
In this dissertation I set out to explore the responses of basic scientists’ to market-oriented science funding in Canada and the implications of these responses for reproduction and change in the scientific field. Change is a permanent feature of social life even as the distribution of power is largely reproduced. The particular shape that social fields take and symbolic categories of social value are the outcome of the day to day practices of individuals interacting with sweeping social and political forces that may seem, at times, to be beyond the power of individuals. While the Mode 2 thesis makes broad claims to sweeping, homogenous change in academic knowledge production, this study suggests that change is actively negotiated by individuals in daily practice. It advances through the accumulation of new taken for granted meanings and practices, and retreats as these are met with acts of resistance and reinterpretation. This process is structured by the outcome of past struggles and, in turn, produces the structures that shape future struggles (Bourdieu 1990a). There is no “endpoint”. Therefore, it remains a matter of empirical investigation to discover whether a market logic remains institutionalized in a subordinate position to a scientific logic, whether it becomes further entrenched in new economic quality criteria, or whether the scientific field gains autonomy from external pressures to demonstrate economic value.

By adopting a relational epistemology that takes the system of relationships as the object of study, I have tried to not simply produce an account that is a position within the struggle between a traditional and a market-oriented vision of science, but to illustrate the contours of this struggle at a particular point in time and in a particular
context. However, now that the study has concluded, in reflecting on this state of struggle in which I am personally implicated as an aspiring social scientist, I cannot help but see the polarization of academic science according to an economic logic as a threat to the production of non-market-driven knowledge. It does not seem reasonable or logical to expect that the production of research determined by market needs will provide the kind of knowledge needed to create a safe, just and healthy society. I will conclude with the words Jeremy, a scientist who embraced market-oriented science but also felt that it in some ways compromised his integrity.

I sometimes feel complicit in a sham. What I’d rather say is, “We’re studying something that is very interesting and it may turn out to be very important.” It may turn out not to be very important, but can we afford not to take the risk?
References


Prepared for the NCE Directorate by Dennis Rank, Project Manager of KPMG Consulting LP. Project: 34725.


Appendices

Appendix A. Study Information Sheet

UNIVERSITY OF TORONTO
Department of Public Health Sciences

Science Policy and Academic Practice Study: Information Sheet

You are invited to take part in a research study being conducted by Wendy McGuire, PhD candidate in the Department of Public Health Sciences at the University of Toronto. The research is being carried out under a doctoral fellowship granted by the Canadian Institutes of Health Research.

What is the study about?

Academic scientists are increasingly engaging in industry collaboration and commercial activities, such as patenting, licensing, starting companies, and developing commercial products and services. In recent years, Canadian science policy has focused on promoting these activities in order to stimulate economic growth, particularly in areas like biotechnology. Government funding for academic research frequently requires scientists to engage in technology transfer and industrial collaboration. Very little is known about how these pressures to engage in commercial behaviour affect scientific practice and the processes of knowledge production. The aim of this study is to understand how scientists negotiate the current research funding context and how these requirements for technology transfer and commercialization affect academic research. I will be speaking with 20 to 30 non-clinical life scientists at the University of Toronto who are engaged in biotechnology-related research.

What does it involve?

Should you agree to take part in this project, you will be asked to provide your curriculum vitae and to participate in an interview lasting 45 minutes to an hour. You will be asked to discuss your academic and commercial activities, research funding conditions for technology transfer or industrial collaboration you have encountered, how you have responded to these conditions, and how your research has been affected. With your permission, the interview will be audiotaped. In addition, you will be asked to complete a brief form about your personal background and academic and commercial activities.

What are the risks and benefits of my participation?

I know of no risks from participating in the study. Your participation will contribute to knowledge about the changing relationship among universities, government and industry. It will have implications for policymakers on the impact of current research funding strategies. You may receive a summary of the results if you wish.

Will the information I share be kept confidential?
The interviews will be completely confidential and what you say will not be discussed with anyone. No name or personal identifying details will be attached to any of the data. All documents, audiotapes and other research materials will be stored in a secure, locked cabinet and all data will be processed on a secure, password-protected computer. The consent forms will be kept locked separately from other study data. The taped interviews will be destroyed 5 years after the completion of the study. In any presentations or publications resulting from this research, the data will be presented in summary and will not reveal any identifying information, including your name, department, discipline, lab, name of research studies, specific details about patents or licenses held, or the name of any companies you are involved in.

If you would like any further information about any aspect of the study please contact:

Researcher: Wendy McGuire
Tel: 416-407-2086
Email: wendy.mcguire@utoronto.ca

Supervisor: Dr. Peggy McDonough
Tel: 416-946-7936
Email: peggy.mcdonough@utoronto.ca

Thank you for considering taking part in this study.
Appendix B. Consent Form

UNIVERSITY OF TORONTO
Department of Public Health Sciences

Science Policy and Academic Practice Study: Consent to Participate

I have read the Information Sheet about this study. I understand that I will be participating in an interview lasting 45 minutes to one hour. I will be asked questions about research funding conditions for technology transfer and commercialization, how I have responded to these conditions, and how my research has been affected. I will also be asked to provide my curriculum vitae and to complete a brief Background Information Form.

My participation in this study is voluntary and I may withdraw from the study at any time. I don’t have to answer any questions I don’t want to and may ask at a later time for my interview to be removed from the study.

I understand that what I say will be kept confidential. No personal identifying information will be discussed with anyone. My name, department, discipline, lab, name of research studies, specific details about patents or licenses held, or the name of any companies I am involved in will not be reported in any presentations or publications resulting from this study.

With my permission, the interview will be audiotaped. Personal identifying information will be removed from tapes, transcripts and notes about this interview. If I do not wish to be taped, the interview may still be carried out.

I may be contacted at a later date for follow-up or clarification on minor points, but may choose not to give permission for further contact.

If I have any questions, I can ask the student researcher, Wendy McGuire (416-407-2086; wendy.mcguire@utoronto.ca) or her academic supervisor, Dr. Peggy McDonough (416-946-7936; peggy.mcdonough@utoronto.ca). If I have questions about my rights as a research participant, I may contact Jill Parsons, Health Sciences Ethics Review Officer, Ethics Review Office, University of Toronto, at 416-946-5806 or jc.parsons@utoronto.ca.

Participant: ______________________  Researcher: ______________________________

Signature: ______________________  Signature: ______________________________

Date: __________________________  Date: ___________________________________

I have received a copy of this signed consent form.  Yes ______  No ______

I am willing to have this interview audiotaped.  Yes ______  No ______

I would like a copy of the report when the study is completed.  Yes ______  No ______
Appendix C. Interview Guide

In this interview, there are 3 areas I’d like to explore: 1) your academic field and how scientific excellence is defined and achieved, 2) your experience with the current research funding context and 3) the impact of research funding, and the current trend towards commercialization and technology transfer, on how you “do science”, and science overall.

**Academic Excellence**

1. Could you tell me how you recognize “good science” or a “good scientist” in your field? How does someone make it to the “top” of your field and how do you recognize that they are at the “top”?

2. How is scientific excellence defined and evaluated in your department / research institute?
   a. What is recognized as excellence in hiring, tenure and promotion processes?

3. How is scientific excellence evaluated by government research granting agencies?
   a. What separates a successful from an unsuccessful application?
   b. Are there differences between how your scientific peers and funding agencies evaluate “good science”?

4. Has the way “good science” is defined or recognized changed since you began your academic career?

**Research Funding and Commercialization Practices**

5. Have you participated in any commercial activities related to your research, such as patenting, founding a company, or developing a commercial product or service?

6. Can you describe the conditions for technology transfer or industrial collaboration that you have encountered from government funding organizations?

**Effects of Funding Context on Academic Practice**

7. Have funding conditions for targeted research, matched funding and research commercialization affected the way you “do science”?
   a. Research agenda, quality, productivity
   b. Time allocation, daily practice

8. Are you doing what you imagined you would be doing when you began your academic career? What do you like and not like about your career as an academic scientist?
9. Have funding conditions for targeted research, matched funding and research commercialization affected the way new scientists are being trained?

10. Have they affected the way science or scientific excellence is evaluated?

Established Scientists Only:

11. How are the conditions different for new scientists just beginning their career today from when you began your career?
   a. Are new scientists “doing science” differently from when you began your career?
   b. Being evaluated differently?

12. What do you think the impact of these policies and funding strategies on science will be in the long term? Will they achieve their goals of improving human health more rapidly and / or contributing to economic growth?

13. If you had the ear of research funders, what would you say to them?

14. In conclusion, is there anything we haven’t talked about that you would like to add?
Appendix D. Background Information Form

Science Policy and Academic Practice: Background Information Form

ID: __________

Academic Background
Year you began your doctoral training: __________________________
Year of graduation: __________________________
Year you began your tenure-track position: __________________________
Year you attained / anticipate attaining tenure: __________________________
Number of doctoral students you currently supervise: __________________________
Number of post-doctoral fellows you currently supervise: __________________________
Do you have a lab? Yes__________ No__________
If yes, how many people work in your lab?
Staff: __________________________
Doctoral Students: __________________________
Post-Doctoral Fellows: __________________________

What are the most prestigious scientific awards in your field?
__________________________________________________________________________

What are the top three journals in your field?
1. __________________________
2. __________________________
3. __________________________

What are the most prestigious professional organizations in your field?
__________________________________________________________________________

Commercial Background
Have you received industry funding for your research? Yes__________ No__________
Have you used industry funding to fund doctoral students or post-doctoral fellows? Yes__________ No__________
Do you hold any patents related to your research? Yes__________ No__________
If yes, how many? __________________________
Do you hold any licenses related to your research? Yes__________ No__________
If yes, how many? __________________________
Do you own equity in a company related to your research? Yes__________ No__________
Have you been involved in the developing a commercial product or service based on your research?
Yes__________ No__________

Personal Background
Date of Birth: __________________________
Father’s highest level of education: __________________________
Father’s occupation: __________________________
Mother’s highest level of education: __________________________
Mother’s occupation: __________________________
Appendix E. CV and Background Information Form

Summary

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