EMPLOYEE MOBILITY AND THE APPROPRIATION OF VALUE FROM KNOWLEDGE: EVIDENCE FROM THREE ESSAYS

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ABSTRACT

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Employee Mobility and the Appropriation of Value from Knowledge:
Evidence from Three Essays

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In this dissertation, I argue that employee mobility is a key consideration of the firm. Firms often rely on human assets to generate and maintain knowledge. When key individuals depart the firm, they take knowledge with them, potentially undermining the firm or helping competitors. Specifically, I theorize as to how the potential for employee departure affects firm value, and empirically examine my hypotheses in strategy contexts such as M&As, R&D, and equity investment.

Across three studies, I find a consistent pattern of evidence to suggest employee mobility has economically significant and strategically important effects for the firm. In essay one, I argue that acquirers incorporate expectations about employee departure into M&A decisions. Using a natural experiment, I find causal evidence that constraints on employee mobility raise the likelihood that affected firms will become acquisition targets. This effect is stronger when firms are more exposed to negative consequences of employee mobility and weaker when firms are protected by intellectual property rights that can mitigate the consequences of employee mobility. In essay two, I argue that secrecy is a valuable, and even primary, mechanism for protecting knowledge. For firms to invest in R&D, it is essential that they are able to capture value from the knowledge that they create, and many firms use employee non-compete agreements to stem the leakage of secrets to competitors. I find causal evidence that constraints on employee mobility initially boost *Tobin's q* by as much as 25%, but that the effect is eventually undone as firms are harmed by the slower circulation of talent and ideas, causing them to become more myopic in their R&D. In essay three, I argue that investors anticipate the potential for employee departure, resulting in a 'mobility discount' for firms exposed to greater costs of employee departure. I find evidence that firms with a high reliance on key scientists are discounted in the market by as much as 12%. The mobility discount increases when scientists are more central to the future growth opportunities of the firm, and decreases when firms enjoy legal protection from employee departure through the enforcement of non-compete agreements.

Keywords: Employee mobility, non-compete agreements, secrecy, knowledge, proprietary knowledge, knowledge protection, firm value, market valuation, innovation, science, scientists

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CHAPTER 1

INTRODUCTION

How does employee mobility affect the appropriation of value from knowledge? In this dissertation, I develop new theory and evidence to argue that employee mobility is a central consideration in how firms create, and then capture, valuable knowledge from human assets. In particular, I examine the strategic implications of employee departure, and how firms and investors anticipate the costs of employee departure in strategy contexts such as mergers and acquisitions (M&As), research and development (R&D), and equity investment. Across three studies, I find evidence that restrictions on mobility, in the form of employee agreements to not compete (i.e., "non-competes"), affect firm value in consistent and important ways. Theory and evidence also suggest that firms and investors are sensitive, *ex ante,* to the "consequences" of undesired employee departure, thereby affecting firm value whether consequential employee departure happens or not. These findings have important implications for knowledge-based views of the firm, as well as for strategy-based recommendations regarding the management of human and knowledge assets.

In this introduction, I position my research agenda into a broader view of rent appropriation. I review the costs of employee departure, and show how agreements by employees to not compete against their employer by moving to a competitor mitigate those costs. By constraining employee mobility, reducing uncertainty about the departure of key individuals at inopportune times (such as after an acquisition or during a high-stakes research and development program), and by preventing the leakage of secrets to competitors, non-competes help firms to capture and protect value. However, theory suggests that non-competes are not a panacea for the firm – the enforcement of non-competes can harm firms by slowing down the circulation of talent and ideas. Whereas the downside of non-competes to social welfare has already been examined at the regional level (e.g., Saxenian 1994; Gilson 1999), I develop evidence that restrictions on employee mobility have long-term consequences even at the level of the firm. Overall, the objective of this dissertation is to demonstrate how employee mobility, and restrictions placed on employee mobility, affect the appropriation of value from knowledge.

Employee Mobility and the Appropriation of Value from Knowledge

For firms to invest in the generation of new knowledge, it is essential that they be able to capture value from the knowledge that they create. Although firms may attempt to commercialize knowledge discoveries through licensing (Teece 1986), the intangible, non-rivalrous nature of information complicates the process of securing rents in the market (Arrow 1962; Romer 1990). Firms may therefore seek to avoid the problems of disclosure and opportunism (Stigler 1961; Williamson 1979) by exploiting knowledge directly within the firm, thereby improving operational efficiency and/or creating differentiated products and services (Gans, Hsu et al. 2002). Ultimately, however, firms earn supranormal returns only to the extent that they can exclude other firms from obtaining and using the knowledge that they generate. Moreover, using knowledge internally does not fully guard it against expropriation: products can be reverse engineered; processes can be leaked; and key employees can leave, taking proprietary information with them. As such, I examine the costs and risks of employee departure, and how these undermine the appropriation of rents from knowledge.

Whereas much research has focused on the challenges of transacting knowledge (Nelson 1959; Stigler 1961; Arrow 1962; Stiglitz 1985), generating knowledge (Nonaka 1994; Conner and Prahalad 1996; Nickerson and Zenger 2004), transferring knowledge (Kogut and Zander 1992; Szulanski 1996; Nahapiet and Ghoshal 1998), acquiring knowledge (Hall 1988; Hitt, Hoskisson et al. 1991; Capron and Mitchell 1998; Ahuja and Katila 2001), or forming alliances with other firms to share knowledge (Kogut 1988; Hamel 1991), there has been surprisingly little research on the importance of *protecting* knowledge. As Liebeskind argues (Liebeskind 1996; Liebeskind 1997), and as surveys of managers confirm (Levin, Klevorick et al. 1987; Cohen, Nelson et al. 2000), the protection of knowledge is a key concern for most firms.

Economic theory and foundational strategy research suggests that firms need *isolating mechanisms* over market positions if they are to prevent *ex post* equilibration of rents (Rumelt 1984). Theory from the resource-based view (Wernerfelt 1984; Barney 1991; Peteraf 1993; Conner and Prahalad 1996) highlights similar concerns on the input side of production; firms need to control resources that are valuable, rare, inimitable and nonsubstitutable if they are to generate supranormal returns. Given that knowledge is a core input and output of the production function of many firms (Winter 1987; Grant 1996), I argue that establishing access to, and excludability over, knowledge is at the very core of strategy research. Even when the exclusion of knowledge from competitors is not the central concern, simply maintaining access to knowledge is nevertheless important. Knowledge is valuable to the firm only when the firm knows that it can access the knowledge at the time when the firm actually needs the knowledge.

The most frequently studied measure for protecting knowledge is patenting; since Griliches (1981), numerous scholars have argued that patent protection contributes to the market value of firms (Cockburn and Griliches 1988; Schankerman 1998; Harhoff, Narin et al. 1999; Hall 2000; Bessen 2008). Surveys, however, suggest that secrecy is often as important, or even more important, to managers than patenting. Empirically, relatively little is known about secrecy as an appropriation mechanism.[1] I argue that employee mobility is an important complication and strategic consideration in the pursuit of secrecy. Whereas firms may be able to establish 'possession rights' over knowledge (Liebeskind 1996), human assets are fundamentally inalienable (Demsetz 1967; Grossman and Hart 1986) and therefore always at risk of departure (Coff 1997). Much knowledge is tacit and embodied in people (Polanyi 1962; Polanyi 1966), and knowledge therefore spreads through the mobility of employees (Almeida and Kogut 1999). All told, employee mobility poses potential costs to the firm that can undermine the appropriation of value from knowledge. In the next section, I examine the consequences of employee departure.

Strategic Consequences of Employee Departure

When employees depart, they impose costs on the firm in many ways. First, employee departure can reduce the stock of knowledge held by the firm. Employees accumulate tacit knowledge about successful search strategies (Nelson and Winter 1982) and provide repositories of past experience (Walsh and Ungson 1991) to support productive activities or from which

[1] See, however, (Rajan and Zingales 2001) for a theoretical examination of dedicated hierarchies, wherein firms may organize internally to prevent expropriation of knowledge by employees.

others may learn. Particular individuals are important to the firm when their accumulated tacit knowledge is not otherwise codified or shared within the organization (Kogut and Zander 1992; Zollo and Winter 2002), or when there are extended time lags between the initial discovery and subsequent use of knowledge, requiring individuals to 'transfer forward' knowledge over time (Garud and Nayyar 1994). Departing employees can also disrupt the functioning of other employees who remain with the firm. Knowledge work is increasingly embedded into the social fabric between individuals (Huber 1991) and organized into teams (Wuchty, Jones et al. 2007). Firms also coordinate the use of knowledge across multiple parts of the firm (Grant 1996), generating knowledge-based activity systems (Spender 1996) that generate additional value from the sum of the parts (Conner and Prahalad 1996). The loss of a team member can therefore reduce the productivity of remaining employees by degrading routines or breaking social ties (Leonard-Barton 1995), degrading the effectiveness of knowing who-knows-what in the organization (Reagans, Argote et al. 2005), lowering the rate of organizational learning (Carley 1992), and removing an important source of spillovers between individuals (Azoulay, Zivin et al. 2010). Firms also derive value from knowledge by combining it with other complementary assets of the firm, such as marketing or production capabilities (Teece 1986). The loss of knowledge through a departing employee can also decrease the value of *other*, complementary assets in the firm.

Departing employees can also have firm-level, competitive effects when they transfer valuable knowledge or routines to a competitor (Wezel, Cattani et al. 2006). Many firms rely on secrecy to protect proprietary knowledge (Levin, Klevorick et al. 1987; Cohen, Nelson et al.

2000), and, absent other means of intellectual property protection, the loss of an employee can transfer that knowledge to a competitor. Departing employees may ‘spin-out’ to form a new firm (Klepper 2001), with strong adverse effects on incumbent performance in knowledge intensive settings (Franco and Filson 2006; Campbell, Ganco et al. 2012).

Employee Non-Compete Agreements

Throughout this dissertation, I focus on the role of non-compete agreements in constraining employee mobility and how these (legal institutional) constraints affect *ex ante* expectations of mobility and therefore strategic decisions of the firm. Employee non-compete agreements are contractual provisions that expressly prohibit employees from joining a competitor, or forming a new firm as a competitor, within particular industries and geographic locations for a certain time period (Gilson 1999). Also known as “covenants not to compete,” non-competes have become a nearly ubiquitous feature of employment contracts for top executives, knowledge workers, and scientists in the United States. Recent empirical studies have confirmed the negative relationship between the enforceability of non-competes and the mobility of employees, ranging from executives (Garmaise 2011), to inventors (Marx, Strumsky et al. 2009), and to knowledge workers more generally (Fallick, Fleischman et al. 2006).

Although non-compete agreements have been shown to reduce employee mobility, almost no research examines the relationship between non-competes and firm-level outcomes. The work in this area that does exist (e.g., Stuart and Sorenson 2003; Samila and Sorenson 2011), examines the effect of non-competes at different levels of analysis (e.g., the MSA level or state level) and does not explicitly connect to the strategy literature. This dissertation is one of the first studies to establish a direct connection between non-compete agreements and firm-level strategy.[2] Although non-competes have important public policy and management implications of their own (Hyde 2010), I undertake an analysis of non-competes as a means to an end; I use the enforceability of non-compete agreements as a proxy for the expectations that managers and investors are likely to hold with respect to employee mobility in different contexts and under different conditions. Ultimately, I aim to show how these expectations relate to firm value.

Evidence from Three Essays

As a preview of the evidence developed in the dissertation, I provide a brief summary of my main results. In the first essay (Chapter Two), I argue that acquirers incorporate expectations about employee mobility into decisions about whether to bid for a firm. Because employee departure after an acquisition affects the standalone value and/or combination value of a target firm, I expect there to be a negative relationship between the expected employee mobility in a target firm post-acquisition and the likelihood of a firm becoming an acquisition target. I exploit a natural experiment in Michigan wherein an inadvertent change in the enforcement of non-compete agreements provides an observable, exogenous source of variation in employee mobility. Using a difference-in-differences approach, I find causal evidence that constraints on employee mobility in Michigan raise the likelihood that a Michigan firm becomes an acquisition target. I also find that the effect is stronger when a firm is more exposed to the negative consequences of employee mobility, such as when a firm employs more knowledge

[2] I am aware of only one working paper on this topic: (Conti, 2011). Conti investigates whether the enforcement of non-compete agreements affects a firm's propensity to undertake path-breaking R&D projects.

workers in its work force and when a firm faces greater in-state competition; by contrast, the effect is weaker when a firm is protected by a stronger intellectual property regime that mitigates the consequences of employee mobility.

In the second essay (Chapter Three), I argue that secrecy is a valuable, and even primary, mechanism for protecting knowledge. For firms to invest in R&D, it is essential that they capture value from the knowledge that they create. Ironically, we have little quantitative evidence regarding how firms protect knowledge aside from the patent system. But much knowledge is tacit, residing primarily in the minds of workers and leading firms to use employee non-compete agreements to prevent their leakage via interorganizational mobility. In a sample of public firms reporting R&D, I estimate the firm-level returns to non-competes using the same natural experiment in Michigan from the first essay, and find causal evidence that constraints on employee mobility can boost *Tobin's q* by as much as 25%. Moreover, the boost in Tobin's q is increasing in R&D spending but decreasing in the number of patents, and the boost is stronger in industries were secrecy is more important as a means of protecting knowledge. These advantages, however, appear to be short-lived. In the long-run, the initial boost in q attenuates over time as the circulation of talent and ideas slows down, and as firms adopt myopic R&D strategies.

In the third essay (Chapter Four), I argue that investors value scientific activity by firms, but that investors also anticipate the potential for scientists to depart the firm, and that this results in a 'mobility discount' in the market valuation of firms exposed to risks of employee departure. High-tech firms that invest into basic scientific research are often dependent on key scientists remaining with the firm, especially when they are young and growing. After controlling for the positive value of science, I expect that a concentrated reliance on key individuals is a systematic risk to firm value. I also expect that employee non-compete agreements moderate that risk by restraining mobility, and that these effects depend on the intensity to which firms pursue basic science. In a sample of newly-public, high-tech, high-growth firms, I find evidence that firms with the greatest reliance on key scientists are discounted in the market by as much as 12%. I also find that the mobility discount increases when scientists are more central to objectives of the firm, and that the mobility discount decreases when firms enjoy legal protection from employee departure through the enforcement of non-competes.

Contributions

Across three studies, six dependent variables, and nine moderating conditions, I find a consistent pattern of evidence to suggest that the potential departure of key employees is a central concern of firms. By demonstrating that employee mobility can have large-scale effects on activities such as M&As, R&D, and the raising of capital – activities that are core to the viability and performance of the firm – I make the case that employee departure really does matter. I also show that the potential for employee mobility matters to firm values and corporate strategy decisions, whether or not it actually happens.

This dissertation contributes to research on employee mobility (Fallick, Fleischman et al. 2006; Marx, Strumsky et al. 2009; Palomeras and Melero 2010) and to research on the role of knowledge in the firm (Arrow 1962; Winter 1987; Foss 1996; Grant 1996; Liebeskind 1996). In doing so, I join a small but emerging body of research at the intersection of these two streams (see, for example, Agarwal, Echambadi et al. 2004; Campbell, Coff et al. 2011; Campbell, Ganco et al. 2012). Whereas extant work in this area has focused on the ‘upside’ of spinouts, and the benefits to new firms from inheriting knowledge and employees from paternal incumbents, my research focuses more on the ‘downside’ of employee departure to source firms. Moreover, I focus on both large, established firms, and young firms.

This dissertation also makes several methodological contributions to strategy research. Whenever possible, I use econometric methods, difference-in-differences designs, and natural experiments to develop causal evidence in support of my theory (Angrist and Pischke 2010). I also advance the use of statistical simulation and graphing techniques for the interpretation of non-linear interactions (King, Tomz et al. 2000; Zelner 2009), techniques that I demonstrate to be of particular value in the interpretation of difference-in-differences models. I also advance the use of matching methods in strategy research, by incorporating the use of coarsened exact matching (Iacus, King et al. 2009) as a more reliable method compared to propensity score matching (Smith and Todd 2005; Iacus, King et al. 2011).

CHAPTER 2

HOW ANTICIPATED EMPLOYEE MOBILITY AFFECTS ACQUISITION LIKELIHOOD: EVIDENCE FROM A NATURAL EXPERIMENT

1. Introduction

Strategic management scholars share the view that acquisitions represent an important strategy for sourcing resources to broaden a firm's knowledge base, foster innovation, and improve organizational performance (Hall 1988; Hitt, Hoskisson et al. 1991; Capron and Mitchell 1998). Academic research and anecdotal evidence suggests that acquisitions often are driven by firms' desire to acquire the human talents of the target companies (Buono and Bowditch 1989; Wysocki 1997; Wysocki 1997; Coff 1999; Coff 2002). As a result, the management of human capital has been an important topic in the growing literature on mergers and acquisitions (see Haspeslagh and Jemison 1991; and Bruner 2004).

Prior research suggests that acquisition of human capital from a target company can present challenges to the acquiring firm. Acquiring firms routinely confront problems of information asymmetry before an acquisition (Akerlof 1970; Ravenscraft and Scherer 1987) and risks of employees departing the target company after an acquisition (Jemison and Sitkin 1986; Buono and Bowditch 1989). These challenges are likely to be heightened in human capital-intensive companies, whose most valuable assets "walk out the door every night" (LaVan 2000). Prior acquisition research has examined how *ex ante* information problems associated with human capital can affect firms' acquisition strategies (e.g. Coff 1999), as well as how acquirers can work to reduce *ex post* employee departure from the acquired company (e.g. Ranft and Lord 2000). To our knowledge, however, no research has investigated how the anticipated departure of employees from a firm *ex post* may affect acquirers' decisions as to whether to bid for the firm *ex ante*.

In this study, we examine how anticipated post-acquisition employee departure from a firm affects the likelihood of the firm becoming an acquisition target. As Barney (1986) suggests, acquirers incorporate expectations about future acquisition outcomes into their acquisition decisions. Human capital, in particular, is important for generating a competitive advantage (Castanias and Helfat 1991) and is therefore important for the future outcome of an acquisition. We therefore argue that the anticipation of employee departure from a potential target firm will shape acquirers' *ex ante* decision regarding whether to bid for that firm. Specifically, we suggest that the potential for employee departure from a target firm introduces uncertainty into acquirers' assessment of the attractiveness of the acquisition: to the extent that a target firm's employees are less likely to depart, the firm will be more attractive to acquirers and we predict that acquirers will be more likely to bid for the firm.

To empirically test our argument, we exploit a natural experiment in Michigan wherein an inadvertent increase in the enforcement level of non-compete agreements (NCAs) provides an observable, exogenous source of variation in employee mobility (Marx, Strumsky et al. 2009). Using a difference-in-differences approach, we find evidence that constraints on employee mobility, due to an increase in NCA enforcement, raise the likelihood that a Michigan firm becomes an acquisition target by 115.3% on average, compared to firms in other non-enforcing states that did not increase NCA enforcement. We further test a set of conditions under which constraints on employee mobility produce a more or less pronounced effect on the likelihood of acquisition. We find that the effect of NCA enforcement is stronger when a firm is faced with a greater exposure to the negative consequences of employee mobility, such as when a firm employs more knowledge workers and when a firm faces greater in-state competition. By contrast, we find that the effect is weaker when a firm is protected by a stronger intellectual property regime that can mitigate some of the negative consequences of employee mobility. Taken as a whole, our results provide a consistent pattern of evidence suggesting that employee mobility is a major consideration affecting acquirers' M&A decisions.

2. Theory and Hypotheses

Acquisitions have become an increasingly important means for firms to source external knowledge (Arora and Gambardella 1990; Cohen and Levinthal 1990; Grant 1996). Practitioners have long observed that firms often undertake acquisitions to obtain new knowledge and fresh talents (e.g., Link 1988; Wysocki 1997; Wysocki 1997; Roberts 2006). Empirical studies have provided ample evidence attesting to many of the benefits that acquisitions can bring to the acquiring firms, including desired knowledge, greater innovation, speedy new product introduction, and enhanced organizational performance (e.g. Capron 1999; Ahuja and Katila 2001; Puranam, Singh et al. 2006).

Despite these potential benefits, significant challenges exist for firms that pursue acquisitions as a knowledge sourcing strategy. A prominent stream of research has focused on the specific challenge related to the management of human capital in a target firm after an acquisition. As one example, researchers have suggested that acquiring firms confront increased difficulty in the *ex ante* assessment of human capital-intensive targets due to information asymmetry (e.g. Akerlof 1970; Ravenscraft and Scherer 1987). Prior research has examined how firms may structure their acquisition deals involving human capital-intensive targets (Coff 1999; Datar, Frankel et al. 2001), and has shown that the likelihood of deal completion is negatively affected by a target's human capital intensity (e.g. Coff 2002).

In addition to such *ex ante* difficulty, acquiring firms also face significant uncertainty about whether the human talents of the acquired company will remain with the company post-acquisition. Both industry reports and academic research point to the challenge of post-acquisition employee retention and the negative consequences of employee turnover to acquisitions. As a *Wall Street Journal* article indicates, "it has never been easier for talented people to walk away, leaving the acquired company an empty shell" (Wysocki 1997). Research has documented the high frequency of executive turnover subsequent to acquisitions and has also shown that executive departures can negatively affect acquisition performance (Walsh 1988; Walsh 1989; Cannella and Hambrick 1993; Hambrick and Cannella 1993). Retaining other employees of the acquired companies is just as important as retaining the top executives, however (Ranft and Lord 2000). Extant research has shown that turnover of employees after an acquisition is detrimental to acquisition performance (Haspeslagh and Jemison 1991), and that successful acquirers such as Cisco Systems often seek to retain and motivate even the average employees in their acquired companies (O'Reilly and Pfeffer 2000; Dyer, Kale et al. 2004).

Given the high rate of employee turnover in target firms after an acquisition, acquiring firms are likely to incorporate the consequences of such mobility into their acquisition decisions. This argument is based on the foundational research in strategic management that emphasizes that firms' expectations about the future outcome of a strategy, such as an acquisition, influence their decisions on whether to implement the strategy (Barney, 1986). In a corporate acquisition, departures of employees from the target firm introduces several uncertainties into the acquirer's assessment of the target and the acquisition deal: First, the acquirer faces uncertainty about the target's estimated "stand alone" value (Coff, 1999), if critical knowledge or other assets are lost as employee leave the target firm after the acquisition, or if employee departures negatively affect the performance of others who remain with the firm (O'Reilly & Pfeffer, 2000). Second, the acquirer faces uncertainty about the transferability of assets and personnel and thus the synergy potential of the deal (Barney 1988; Coff 1999), given that asset combinations are often involved. Third, the acquirer also faces uncertainty about potential sources of competitive advantage of the target firm being eroded, as the departed employees join or form a rival company to compete with the acquirer. As a result, to the extent that acquirers incorporate anticipated employee mobility of targeted firms into their acquisition decisions, a firm that is more likely to experience employee turnover after an acquisition will be less attractive to the acquirers and less likely to be the target of an acquisition, everything else constant. By contrast, a firm that is less likely to experience such turnover will then be more likely to be an acquisition target.

Extant acquisition research on employee mobility has focused on how acquiring firms can retain the acquired companies' employees after an acquisition has taken place (e.g., Schweiger and DeNisi 1991; Ranft and Lord 2000). Little research has investigated how potential employee departures from a company following an acquisition *ex post* may influence acquiring firms' *ex ante* decisions regarding whether to bid for the company, thus affecting the likelihood of the company becoming an acquisition target. We suspect that this important topic has not been studied for at least two reasons: first, acquiring firms' expectations about post-acquisition employee turnover are unobservable to researchers; and second, while acquirers see the risk of losing people after an acquisition, there is a great deal of uncertainty about the magnitude of post-acquisition employee turnover, making it difficult for them to incorporate their expectations about such turnover into their acquisition decisions. However, certain *observable* institutional factors exist that can reduce acquirers' uncertainty about employee turnover, and thus can inform their acquisition decisions, as we explain further in the hypotheses below.

Employee mobility and acquisition likelihood

A prevalent finding in the acquisition literature is that acquisitions are disruptive to the people involved (Jemison and Sitkin 1986; Haspeslagh and Jemison 1991; Larsson and Finkelstein 1999). All else equal, "people-related problems" increase the turnover of individuals in the target company (Jemison and Sitkin 1986: 147). Even when an acquirer may ultimately want to downsize or replace certain employees from the acquisition target, it stands to reason that the acquirer would prefer to be in a position to decide who will stay and who will go in order to minimize the loss of employees whom the acquirer would otherwise prefer to retain.

Turnover can have a negative impact on acquirers in several ways. First, the departure of employees reduces the stock of knowledge assets held by the target firm. Firms often store knowledge in the experience of individuals (Walsh and Ungson 1991), especially when such knowledge is tacit or otherwise hard to articulate (Kogut and Zander 1992). Thus, a portion of the targeted knowledge assets in an acquisition may be lost when employees leave the target firm, especially if they do so quickly before they are able to transfer their knowledge to others (Anand, Manz et al. 1998). Second, the departure of employees can disrupt the social system in which the employees are situated. Departures have been shown to reduce team coordination with respect to knowing who knows what (Reagans, Argote et al. 2005) and the subsequent rate of organizational learning (Carley 1992). Individuals' departures can also have a direct negative impact on the performance of others that are connected to them. For example, research has shown that the sudden and unexpected loss of a superstar scientist leads to a lasting 5% to 8% decline in the collaborators' quality-adjusted publication rates (Azoulay, Zivin et al. 2010). In acquisitions, researchers have found that departures of employees from the target company following an acquisition can damage the morale of those who stay, negatively affecting acquisition success (O'Reilly and Pfeffer 2000). Third, the departure of employees can give away valuable sources of competitive advantage to immediate or future competitors. Firms have routinely sought to import product line strategies (Boeker 1997), product innovations (Rao and Drazin 2002), and key technical knowledge (Rosenkopf and Almeida 2003) by recruiting talent from their rivals. Spin-outs founded by former employees also pose potential competition to the firm (Agarwal, Echambadi et al. 2004). Stuart and Sorenson (2003), for example, have reported high founding rates of new biotech firms by former employees following liquidity events such as acquisitions in the industry. Such spinouts have the potential to become strong rivals in the future.

For these reasons, given that firms incorporate expectations about future outcomes into their acquisition decisions, we expect that acquirers will be less (more) likely to bid for a firm that is more (less) likely to experience employee mobility after an acquisition. This line of argument suggests a negative relationship between the expected employee mobility in a firm and the likelihood that a firm will become an acquisition target.

Our study focuses on the role of non-compete agreements (NCAs) in constraining employee mobility. Employee non-compete agreements are contractual provisions that expressly prohibit employees from joining a competitor, or forming a new firm as a competitor, within particular industries and geographic locations for a certain time period (Gilson 1999). Also known as "covenants not to compete," NCAs have become a nearly ubiquitous feature of employment contracts in the U.S.; surveys show that a large majority of knowledge workers and upper-level management have signed non-compete agreements with their employers (Kaplan and Stromberg 2001; Leonard 2001; Kaplan and Stromberg 2003). Theoretical research has long suggested that varying levels of enforcement of NCAs contribute to the differential employee mobility observed in different states (e.g., Saxenian 1994; Gilson 1999; Franco and Mitchell 2008). Recent empirical studies have confirmed the negative relationship between the enforceability of NCAs and the mobility of employees ranging from executives (Garmaise 2011), to inventors (Marx, Strumsky et al. 2009), and to knowledge workers more generally (Fallick, Fleischman et al. 2006).

We suggest that varying levels of enforcement of NCAs are an observable, exogenous source of variation in employee mobility that acquirers can be expected to incorporate into their acquisition decisions. Specifically, as the enforcement of the NCAs that govern a firm's employee mobility increases, the expected employee departure from the firm post-acquisition decreases; to the extent that acquirers incorporate the expectation into their acquisition decisions, they are more likely to bid for the firm, increasing the likelihood that the firm will become an acquisition target. Therefore, we propose:

> ***Hypothesis 1:*** *An increase in the enforcement of non-compete agreements will increase the likelihood that a firm will become an acquisition target.*

Hypothesis 1 is our baseline hypothesis. The strength of H1, however, should depend upon several conditions of the target firm. Specifically, we suggest that the effect of NCA enforcement will be strengthened when a target firm is exposed to greater chances of employee turnover, and that the effect will be weakened when the firm has other means to mitigate the negative consequences of employee mobility. We examine these moderators below both to develop boundary conditions for our theory and to develop a coherent pattern of predictions to test the consistency of our theory.

Exposure to employee mobility

An acquisition allows the acquirer to obtain certain assets of the target firm. The degree to which the acquirer can control the acquired assets, however, may depend upon the type of assets acquired. Acquiring firms, for example, will have more secured rights over physical assets, but only limited control over human assets due to the inalienability of human capital (Becker 1964). In particular, people can quit, or they can bargain for a higher wage if they remain with the organization (Coff 1997: 372). We examine two conditions under which acquirers will be exposed to greater negative consequences of post-acquisition turnover, and accordingly benefit to a greater extent from the enforcement of NCAs: first, when the target firm employs a greater proportion of knowledge workers in its workforce; and second, when the target firm faces greater in-state competition.

Knowledge workers. Knowledge workers present a higher risk of post-acquisition mobility for several reasons. First, knowledge workers tend to be more professionalized and resistant to managerial control (Raelin 1991). Prior research has argued that knowledge workers are more likely to depart the target company after an acquisition (O'Reilly and Pfeffer 2000), and has shown that such departure creates uncertainty for the acquiring firm regarding the transfer and replacement of personnel and other assets. The uncertainty associated with employee turnover in human capital-intensive targets can cause otherwise attractive deals to break down (Coff 2002). Second, knowledge workers are more likely to have access to confidential information and first-hand knowledge of the key capabilities of their employer. They are, therefore, more likely to take that knowledge with them to a competitor when they depart, or use that knowledge to generate spin-outs (Bhide 2000). Third, legal theory and the justification for non-compete agreements is rooted in the concept that workplace knowledge is a form of employer intellectual property (Fisk 2009; Hyde 2010). Employers apply non-compete agreements specifically to protect workplace knowledge from appropriation by knowledge workers (Bishara 2006).

Overall, these arguments suggest that knowledge workers are particularly likely to create mobility-related problems following an acquisition, such as loss of valuable knowledge, disruption of existing routines, and promotion of competitors. At the same time, knowledge workers are also more likely to be covered by a non-compete agreement, compared to other types of employees (Kaplan and Stromberg 2001; Leonard 2001; Kaplan and Stromberg 2003). Thus, an increase in the enforcement of NCAs should reduce the risk of undesired employee departure and increase the attractiveness of a firm for acquisition, everything else constant. We therefore hypothesize that an increase in NCA enforcement will have an even stronger effect on acquisition likelihood when knowledge workers comprise a larger proportion of a firm's workforce.

> ***Hypothesis 2:*** *An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a greater extent for firms with more knowledge workers.*

In-state competition. Similar to firms employing more knowledge workers, firms facing greater in-state competition also need to contend with greater chances of employee mobility. In-state competition can raise the likelihood and consequences of post-acquisition employee departure for the following reasons. First, competitors are more likely to raid employees from proximate competitors than distant competitors. As professional networks tend to be geographically localized (Sorenson and Stuart 2001; Stuart and Sorenson 2003), a firm's employees are more likely to be raided by nearby competitors within the state. Second, more in-state competition presents greater opportunities for employment outside of the target firm.

Greater in-state competition reduces the direct and indirect costs for employees to change their jobs (Almeida and Kogut 1999). Thus, even if competitors do not actively seek to recruit away a target firm's employees, greater opportunities for employment nevertheless increase the likelihood of employee departure. Finally, with more external opportunities, employees have more bargaining power against their firm. Increased bargaining power can lead to firms paying higher wages and benefits, even if employees do not leave the firm (Coff 1999). By contrast, employees with fewer external opportunities will be less likely to leave and have less leverage against their employers.

An increase in the enforcement of NCAs will, in particular, constrain employees from changing employment to work for an in-state competitor, because NCAs are more easily enforced within the same state (Gilson, 1999; Garmaise, 2009). For firms that face greater in-state competition and thus are likely to anticipate greater employee departure, an increase in NCA enforcement is particularly likely to reduce acquirers' uncertainty about employee turnover, thereby increasing these firms' attractiveness as acquisition targets. We therefore hypothesize that an increase in the enforcement of NCAs will have an even stronger effect on the likelihood of acquisition when a firm faces greater in-state competition:

> ***Hypothesis 3:*** *An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a greater extent for firms with greater in-state competition.*

Mechanisms limiting knowledge loss due to employee mobility

While the departure of employees from an acquired company has negative consequences for acquiring firms in general (Cannella and Hambrick 1993; O'Reilly and Pfeffer 2000; Ranft and

Lord 2000; Coff 2002), such consequences may vary across individual companies based on their ability to protect and control their knowledge. In this study, we focus on the intellectual property (IP) regime as one mechanism for protecting knowledge and limiting the negative consequences of employee mobility. Patents are the strongest form of intellectual property protection in that they unambiguously exclude competitors from using the underlying knowledge (Teece 1998). Patents also protect firms' interest by preventing the firm's own employees from appropriating the knowledge by starting up new ventures or working for rivals. Kim and Marschke (2005), for example, find that the risk of scientist departure leads to a higher propensity for a firm to patent innovations. Research, however, demonstrates that patents vary in their effectiveness across different industries (Levin, Klevorick et al. 1987; Cohen, Nelson et al. 2000). Patents are not particularly effective when competitors can easily invent around them, when the underlying technology is changing so fast that patents become irrelevant, or when the basis for the patents is easily challenged in court (Levin, Klevorick et al. 1987).

The strength of the IP regime therefore affects the extent to which firms can use patents to retain knowledge for their exclusive use. If the IP regime is weak, firms are less able to protect their knowledge in patents, and the departure of employees is more likely to cause a significant loss of knowledge. By contrast, if the IP regime is strong, firms have a stronger claim on their patented knowledge and, even if certain employees leave the firm, the firm is better able to retain and control their knowledge. A stronger IP regime therefore helps a firm limit the risk of knowledge loss due to employee mobility. Consequently, while an increase in NCA enforcement will reduce employee departures and better protect firms' knowledge assets, that effect should be weaker for firms operating in a stronger IP regime, compared to firms operating in a weaker IP regime. As a result, an increase in NCA enforcement will increase the attractiveness of firms protected by a stronger IP regime as acquisition targets to a lesser degree, compared to firms operating in a weaker IP regime:

> ***Hypothesis 4***: *An increase in the enforcement of non-compete agreements will increase the likelihood of acquisition to a lesser extent for firms protected by a stronger IP regime.*

3. Research Design

Empirical challenges exist in developing causal evidence on the link between the enforcement of NCAs and acquisition likelihood. In particular, the level of NCA enforcement within a state rarely changes, and when it does change, it usually changes by only a modest amount (Gilson 1999; Garmaise 2011). While there is considerable variation in the level of NCA enforcement between states, a cross-sectional analysis can be confounded by selection effects and unobserved heterogeneity. To overcome the issue of endogeneity in our study, we exploit a natural experiment related to a policy change in NCA enforcement that occurred in Michigan.

The Michigan Natural Experiment

In 1985, the Michigan legislature passed the Michigan Antitrust Reform Act (MARA) to harmonize Michigan state law with the Uniform State Antitrust Act (Lifland 1984; Bullard 1985). In passing MARA, however, research suggests that legislators also inadvertently repealed Michigan statute 445.761, a statute that previously prohibited the enforcement of non-compete agreements in Michigan (Alterman 1985). As a consequence, Michigan employers suddenly, and unexpectedly, obtained the legal means to prevent employees from leaving their firms to work for a competitor in Michigan or other state that enforced out-of-state NCAs. Marx *et al.* (2009) have demonstrated that MARA produced a negative and significant impact on the mobility of knowledge workers in Michigan. Legal research suggests that MARA, in addition to repealing statute 445.761, also strengthened remedies and the enforcement of anti-trust regulation in Michigan (Folsom 1991). Because stronger enforcement of anti-trust regulations alone is unlikely to cause an *increase* in M&A activity (Brodley 1995), anti-trust aspects of MARA should work against us finding our hypothesized effects. It would therefore appear that the repeal of Michigan statute 445.761 provides a natural experiment for assessing the effect of employee mobility on acquisition likelihood.

A good natural experiment for research is one in which there is an unexpected, exogenous, and transparent assignment of a 'treatment' status (Meyer 1995). Such assignment can allow researchers to identify exogenous variation in the explanatory variables and rule out the possibility that policy makers adopted the treatment because of conditions in the prior period (Ashenfelter 1978; Heckman and Smith 1999). An unexpected treatment also rules out the possibility that firms could have incorporated expectations of the treatment into their economic decisions. It is, therefore, particularly important for the purposes of this study that the change in the NCA enforcement was accidental and unplanned. Marx and colleagues (2009) have examined relevant legislative reports (e.g. Bullard 1985) and legal reviews (e.g. Alterman 1985), and have concluded that the change in the enforcement of NCAs in Michigan was an unexpected shock and a truly exogenous source of variation in the mobility of knowledge workers.

The Michigan natural experiment lends itself to a difference-in-differences (DID) econometric technique (Meyer 1995). The DID is frequently used to study the effect of policy changes in observational data when the researcher is unable to randomly assign subjects into a treatment group versus a control group. Card and Krueger (1994) provide a classic example of the use of DID in labor economics, and Chatterji and Toffel (2010) provide a recent example of the use of the technique in strategic management. In our analysis, we assigned firms in Michigan to the 'treated group' in that firms in Michigan experienced the MARA policy change. We followed prior research and assigned firms in the states of Alaska, California, Connecticut, Minnesota, Montana, North Dakota, Nevada, Oklahoma, Washington, and West Virginia to the 'comparison group' in that these states did not enforce NCAs before or after MARA (Malsberger, Brock et al. 2002; Stuart and Sorenson 2003; Marx, Strumsky et al. 2009). By assuming that trends in the comparison group represent trends in what would have happened in the treatment group in the absence of treatment, the DID identifies a causal treatment effect as the before-to-after difference in Michigan, netting out trends from the comparison group. To strengthen an 'equal trends' assumption between the groups, we also included a number of covariate controls (described below) and used Coarsened Exact Matching (CEM) (Blackwell, Iacus et al. 2009) to improve the covariate balance of firms between the two groups (described below).

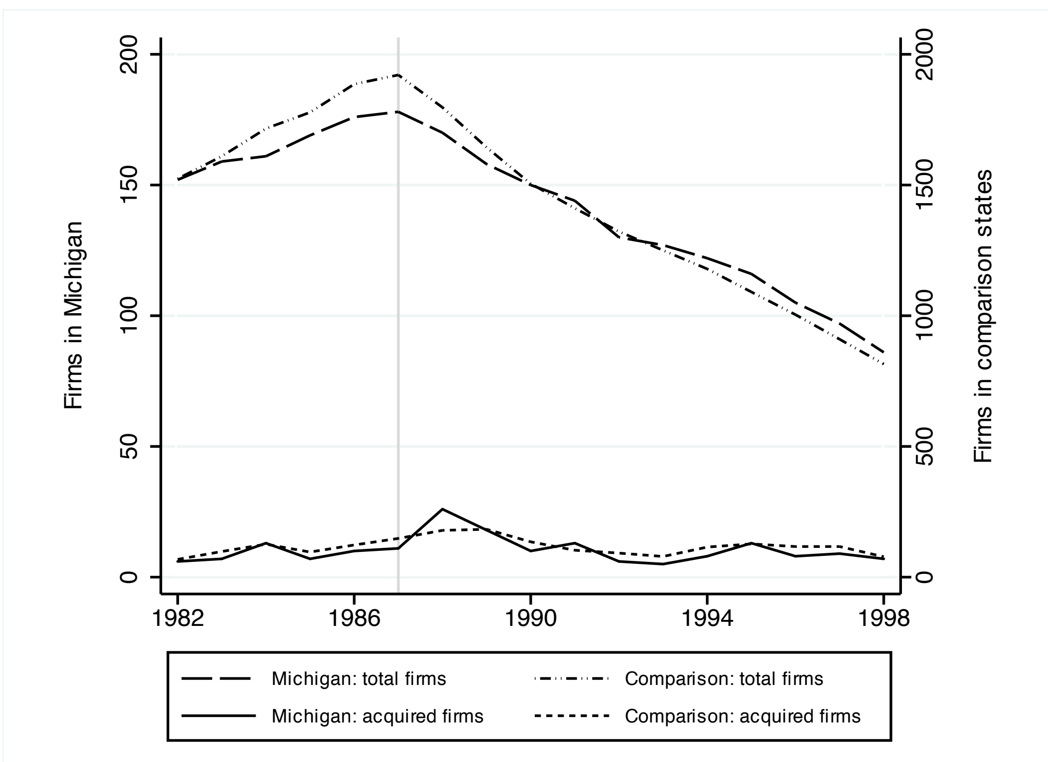
Sample and Data

Our sample construction started with all publicly traded firms in the United States between 1979 and 1998 that could potentially become an acquisition target. We first obtained the base sample from Compustat, and we excluded financial instruments (e.g., ADRs and ETFs) and securities used internally by the firm (i.e., CUSIPs ending in 990-999 or 99A-99Z). Next, we restricted that sample to include only firms located in Michigan or a comparison state defined earlier, and we further limited the sample to only firms that were listed prior to MARA. We excluded new firms listed after MARA from the sample in order to ensure that the composition of the sample itself was not affected by MARA (we included new firms into the sample in a robustness check to be reported below). After these steps, we arrived at a sample of 19,020 firm-year observations, which served as the base population of firms that could potentially become a target for acquisition (Song & Walking, 1993, 2000). We then obtained information on acquisition events from Thomson Financial's SDC Platinum M&A database, and matched the acquisition events to our base population of firms available for potential acquisition based on their CUSIPs. We obtained firms' historical CUSIPs from Compustat's historical files. We followed Song and Walkling (2000) and excluded acquisition bids where the deal value was less than $500,000. We also followed prior research to exclude deals labeled as buybacks, exchange offers, privatizations, spinoffs, carveouts, self-tenders, and recapitalizations.

Figure 1A shows the temporal trends of the base population of firms and the acquisition bids for those firms from 1982 to 1998. The top two lines in the figure represent the number of firms by group (Michigan vs. comparison states), and they reveal that the numbers for both groups grew up to 1987 and then declined as firms were acquired or delisted due to firm failure. The bottom two lines represent the number of acquisition bids by group, and they show a spike of acquisition bids for firms in Michigan following MARA in 1988. Figure 1B presents the rates of acquisition (number of acquisitions as a percentage of the number of firms that could become an acquisition target) for both groups.

Figure 1: Descriptive trends in Michigan and comparison states

Panel A: Base population and number of acquisitions in Michigan and comparison states



***Notes*:** We restricted our base population to firms that were publically listed prior to MARA. This population declined after 1987 in both groups (Michigan and comparison states) as firms were acquired or delisted due to firm failure. The vertical line in the figure denotes 1987; the top two lines represent the total count of firms by group; and the bottom two lines represent the count of acquisition events by group.

Figure 1 Continued

Panel B: Rate of acquisition in Michigan and comparison states

***Notes*:** The rate of acquisition is calculated as the number of acquisitions expressed as a percentage of the number of firms in either Michigan or the group of comparison states.

Measures

Dependent variable. Consistent with prior research on acquisition likelihood (e.g. Song and Walkling 1993; Song and Walkling 2000), our dependent variable, *Acquisition*, is a dichotomous variable that equals one if the focal firm is the target of an acquisition in a given year based on the acquisition announcements reported by SDC, and zero otherwise. According to Song and Walkling (1993: 441), this approach "avoids *ex post* selection bias" and offers the advantage to sample "all firms" that become the targets of acquisition.

Given our research focus, our right-hand-side variables are limited to those that are available for all listed firms that could potentially receive an acquisition bid. Variables that are defined only at the acquirer level, the dyadic level, or the acquisition deal level, therefore, can *not* be included in our models given the research design.

Explanatory variables. The DID 'treatment group' variable, *Michigan*, is an indicator variable that equals one if a firm was located in Michigan based on the historical location of the firm's corporate headquarters, and zero otherwise. Compustat's historical files provide information on firms' historical locations required for this variable. The DID 'after' variable, *After*, is an indicator variable that equals one for all years after 1987, and zero otherwise. We assume that it took a year for knowledge of the MARA policy change to diffuse in the legal and investment communities, and that it took an additional year for potential acquirers to then act upon the knowledge, given the significant amount of time that is usually involved in target search and selection, due diligence, and negotiation before announcing an acquisition. We therefore selected the break between 1987 and 1988 as the dividing point for the before and after periods in the DID analysis, and tested alternative models of knowledge diffusion in a robustness check below. Following prior DID research (Meyer 1995), we then created an interaction variable *Michigan * After* and used this variable to identify the treatment effect of MARA and test H1.

To test the moderating effects proposed in H2, H3, and H4, we first developed three variables: *Knowledge Workers*, *In-state Competition*, and *IP Protection*; then we extended the basic DID model by including the three-way interaction of *Michigan * After* and each of these three variables (Meyer 1995). We mean centered the continuous variables at zero to simplify interpretation of the interaction effects.

For *Knowledge Workers*, we followed prior research (e.g., Farjoun 1994; Coff 1999; Coff 2002) and measured the level of knowledge-workers employed in a focal firm's industry as a proportion of the total workforce employed in that industry. We obtained data on employment levels from the Occupational Employment Statistics (OES) survey from the Bureau of Labor Statistics (BLS). Using the OES occupational codebook, we defined knowledge workers to be those with an occupational code below 50,000. This definition includes occupations such as managers, sales workers, scientists, engineers, editors, computer programmers, IT professionals, and so forth. The OES provides data on the breakdown of the total number of people employed in each SIC code by OES occupational code. From the OES data, we calculated the proportion of the total workforce for each 3-digit SIC code that was knowledge workers, and then assigned that measure to each focal firm in our sample according to the firm's SIC code. *Knowledge Workers* is a continuous measure that varies from 0 to 1.

For *In-state Competition*, we followed prior research by Garmaise (2011) and measured the proportion of sales for a focal firm's industry by other firms located in the same state as the focal firm; the focal firm's own sales was excluded. We determined the distribution of sales by industry and by state from Compustat's segment file. We calculated this measure individually for each firm at the 4-digit NAICS industry level because the segment file reports data by the NAICS instead of the SIC. The variable *In-state Competition* is a continuous measure that ranges from 0 to 1.

For *IP Protection*, we followed Cohen, Nelson, and Walsh (2000) and used their measure of the mean percentage of product innovations for which patents are an effective mechanism for protecting the underlying knowledge and appropriating the returns. We obtained their measure by industry and assigned the measure to the manufacturing firms in our sample. We assigned a value of zero to non-manufacturing firms, as the measure is not relevant to those firms. We rescaled this continuous measure to vary from 0 to 1 for ease of interpretation.

Control variables. We included several variables that have been shown in prior M&A research to affect the likelihood of acquisition. To control for year-by-year variation, we included a full set of annual indicators. To control for potential industry effects, we included a set of seven industry indicator variables (i.e. *Auto*, *Drugs*, *Chemicals*, *Computers & Communication*, *Electrical*, *Wholesale*, and *Retail*) (Marx *et al.*, 2009), and treated "service industries & others" as the base category. Following prior research (e.g. Palepu 1986), we control for *Industry Prior Acquisitions*, measured as the rate of acquisitions within the focal firm's industry averaged for the previous three years. We also included a control for the firm's *Years Public* and followed Garmaise (2009) specifically to measure this variable by considering the firm's year of public listing. To control for the past performance of the firm, we measured the 3-year trailing average return on assets (*ROA*) for each firm that is in excess of the 3-year trailing average return on assets for the focal firm's 3-digit SIC industry. To control for the size of the firm, we measured *Assets* for each firm (log transformed). We also followed prior research (Song and Walkling 1993; Field and Karpoff 2002) to control for the firm's *Liquidity*, defined as the average ratio of net liquid assets (current assets minus current liabilities) to total assets. To control for the labor market supply of knowledge workers, we included a measure for *Beale Urban Index*, defined as the level of urbanization based on the local population size and proximity to a metropolitan area.

The data were provided by the Department of Agriculture, Economic Research Service. Finally, to control for general economic conditions within a state (Garmaise, 2009), we incorporated a measure for *State GDP* (log transformed), using data from the Department of Commerce, Bureau of Economic Analysis.

Tables 1a and 1b present the descriptive statistics and correlations for all variables used in the study for the raw, unmatched sample, and for the CEM-matched sample (see Azoulay *et al.* (2010) for a recent application of CEM).

Table 1a: Summary statistics.

	Matched Sample (n=18,713)	Mean	Std. Dev.	Min	Max
1	Acquired	0.087	0.281	0.000	1.000
2	Ind. Prior Acquisitions	0.067	0.049	0.000	0.500
3	State GDP (log)	12.490	1.109	9.184	13.898
4	Assets (log)	4.504	2.178	0.001	10.713
5	Liquidity	0.012	11.655	-1293.000	16.238
6	ROA	-0.091	1.625	-247.078	86.321
7	Years Public	14.122	13.493	0.000	73.000
8	Beale	0.727	1.654	0.000	9.000
9	Auto Industry	0.022	0.146	0.000	1.000
10	Knowledge Workers	0.000	0.197	-0.317	0.585
11	In-state Competition	0.000	0.152	-0.129	0.871
12	IP Protection	0.000	0.348	-0.342	0.658
13	Michigan	0.101	0.301	0.000	1.000
14	After (post-MARA)	0.599	0.490	0.000	1.000
	Unmatched Sample (n=19,020)	Mean	Std. Dev.	Min	Max
1	Acquired	0.081	0.273	0.000	1.000
2	Ind. Prior Acquisitions	0.067	0.048	0.000	0.500
3	State GDP (log)	12.471	1.115	9.184	13.898
4	Assets (log)	3.932	2.125	0.001	10.713
5	Liquidity	-0.913	112.418	-15415.000	16.238
6	ROA	-0.141	2.262	-247.078	86.321
7	Years Public	12.339	11.732	0.000	73.000
8	Beale	0.774	1.724	0.000	9.000
9	Auto Industry	0.020	0.141	0.000	1.000
10	Knowledge Workers	0.002	0.197	-0.317	0.585
11	In-state Competition	-0.008	0.148	-0.129	0.871
12	IP Protection	0.000	0.353	-0.342	0.658
13	Michigan	0.095	0.293	0.000	1.000
14	After (post-MARA)	0.607	0.488	0.000	1.000

Table 1b: Correlations (matched sample; n=18,713)

		(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	Acquired	1.000*						
2	Ind. Prior Acquisitions	0.071*	1.000*					
3	State GDP (log)	0.043*	0.129*	1.000*				
4	Assets (log)	0.026*	0.084*	0.041*	1.000*			
5	Liquidity	0.005*	-0.007*	0.012*	0.037*	1.000*		
6	ROA	0.004*	-0.004*	-0.016*	0.084*	0.098*	1.000*	
7	Years Public	-0.019*	0.078*	-0.015*	0.541*	0.001*	0.034*	1.000*
8	Beale	-0.004*	-0.024*	-0.429*	-0.066*	-0.004*	-0.003*	-0.024*
9	Auto Industry	0.007*	0.043*	-0.032*	0.026*	0.003*	0.008*	0.051*
10	Knowledge Workers	0.019*	-0.050*	0.158*	-0.071*	0.000*	-0.015*	-0.162*
11	In-state Competition	0.018*	0.045*	0.288*	0.122*	0.009*	-0.001*	0.029*
12	IP Protection	-0.011*	0.063*	0.100*	-0.049*	0.022*	-0.005*	0.014*
13	Michigan	-0.015*	0.026*	-0.110*	0.009*	0.004*	0.009*	0.074*
14	After (post-MARA)	0.042*	0.328*	0.218*	0.105*	-0.015*	-0.005*	0.151*
		(8)	(9)	(10)	(11)	(12)	(13)	(14)
8	Beale	1.000*						
9	Auto Industry	-0.028*	1.000*					
10	Knowledge Workers	-0.067*	-0.151*	1.000*				
11	In-state Competition	-0.153*	0.136*	0.109*	1.000*			
12	IP Protection	-0.119*	0.190*	-0.312*	0.095*	1.000*		
13	Michigan	0.037*	0.284*	-0.118*	-0.035*	0.054*	1.000*	
14	After (post-MARA)	-0.015*	-0.015*	0.022*	0.005*	0.018*	-0.004*	1.000*

Notes: Summary statistics are shown for both the matched and unmatched samples. Coarsened Exact Matching drops 307 out of 19,020 observations and results in a sample of firms that are somewhat larger, older, more profitable, and more liquid. *Knowledge Workers*, *In-state Competition*, and *IP Protection* are mean-centered at zero in the matched sample. Correlations marked with an asterisk are significant at less than 5%.

Given the historical nature of the Michigan experiment (1980s), we faced several limitations in the availability of data. The SEC did not mandate electronic proxy statements before 1993, and we were therefore unable to control for firm-level differences in anti-takeover defenses. A detailed, geographic breakdown of operations by state was unavailable for most firms in our sample, and we therefore followed prior research (e.g., Garmaise 2011) and assigned firms to the treatment group (Michigan) versus the comparison group by referring to a firm's corporate headquarters (not their state of incorporation). We do not believe, however, that these data limitations bias our analysis. The 'difference-in-differences' technique removes fixed differences between treatment and comparison groups (observed or unobserved), provided that those differences remain fixed over time. We also believe that the assignment of firms to states based on the corporate headquarters is conservative: Michigan firms with employees in other states should experience less of the hypothesized effects, while firms headquartered in the comparison states with employees in Michigan should experience at least some of the hypothesized effects. Because a "difference-in-differences" analysis measures the relative effect of the policy change between the treatment and comparison groups, our measure of firm location should work against us finding our hypothesized results.

Coarsened Exact Matching

We implemented "Coarsened Exact Matching" (Blackwell, Iacus et al. 2009) to improve the covariate balance of our sample. CEM is a recent multivariate matching technique that is monotonic imbalance bounding (Iacus, King et al. 2011), and, as such, reduces causal estimation error, model-dependence, bias, and inefficiency (Iacus, King et al. 2009; Iacus, King et al. 2009). Because matching on too many covariates will create a 'curse of dimensionality' (Heckman, Ichimura et al. 1998) and cause the analysis to lose much of its statistical power, we followed prior research and used CEM to match on a subset of firm-level covariates. We matched on $Assets$, *Liquidity*, and ROA, in that differences in these covariates have been shown in prior research (e.g., Field and Karpoff 2002), as well as our own regression results, to affect the likelihood of acquisition. We used CEM's coarsening function to non-parametrically separate the joint distribution of these covariates into coarsened strata, and then matched firms in Michigan to firms in comparison states by strata, weighting each comparison by the number of matched observations. We matched on the pre-MARA average for each measure in order to ensure that MARA itself could not affect the matching process.

The CEM procedure improved the in-sample multivariate imbalance of our data from $L_1 = 0.1612$ to $L_1 = 0.0772$ (for a definition of the L1 statistic, see Iacus, King et al. 2011). A comparison of the unmatched and matched samples (see Table 1) indicates that CEM reduced our sample size from 19,020 to 18,713 firm-year observations, increased the proportion of firms based in Michigan (most observations in Michigan are matched and only similar observations in the comparison states are matched), and increased slightly the average size, years public, ROA, and liquidity of the firms in the sample. As we describe below, we used the CEM matched sample in Model 5 (and all models thereafter) to better estimate our hypothesized effects; nevertheless, we find support for all of our effects in Model 4 without using CEM.

Models

We estimated a set of logit models in a DID analysis to test whether the increase in the enforcement of NCAs in Michigan affects the likelihood of a Michigan firm becoming an acquisition target (H1), and to examine the conditions under which this relationship is strengthened or weakened (H2-H4). Instead of estimating the before-to-after change in outcomes for the treatment group (i.e., Michigan) and then assuming that the difference is the effect of the policy change, the DID approach corrects the analysis for the counterfactual trend of what would have happened to the treatment group in the absence of the treatment. Our DID model does this by estimating the change in the treatment group and the change in the set of ten comparison states, and then taking the difference of these two differences (hence, the 'difference-in-differences'). Our analysis is at the firm-year level, and we clustered the standard errors by the firm to account for the non-independence of repeated observations with a firm.

4. Results

To begin the analysis, we used the raw, unmatched sample to compare the rate of acquisition of firms in Michigan with the rate of acquisition in comparison states. Panel 2A in Table 2 indicates that the rate of acquisition rose by 9.11 percentage points in Michigan, from 6.18% in 1987 before MARA to 15.29% in 1988 after MARA. Not all of this change, however, can be attributed to the effect of MARA, because the rate of acquisition also increased in the comparison states during the period. A difference-in-differences analysis subtracts the difference in the comparison states (2.26 percentage points) from the difference in the treated state Michigan (9.11 percentage points), to determine the effect of the policy change without the confounding influence of other trends that were underway in the economy. This "difference-in-differences" statistic is presented in the bottom right cell of Panel 2A: The treatment effect of MARA was a 6.86 percentage point increase in the acquisition rate from 1987 to 1988. For comparison, we also examined the effect of MARA for two other time windows: 1983-1992 and 1982-1998. The former time window adds four years of data on either side of the base window 1987-1988. The latter time window begins in 1982, because data on acquisitions were thin before the early 1980s and because we needed three prior years' data to calculate the control variable for the industry's prior rate of acquisitions; it ends in 1998 to avoid the acquisition wave associated with the Internet bubble in the late 1990s (Moeller, Schlingemann et al. 2004). As shown in Panels 2B and 2C, the effect of MARA was a 1.63 percentage point increase in the acquisition rate from 1983 to 1992, and a 0.13 percentage point increase from 1982 to 1998, respectively. The pattern of the results in the three panels indicates that the effect of the policy reversal weakened over time and as the time window widened.

Table 2: Rates of acquisitions of firms in Michigan and comparison states.

Panel A: 1987-1988	+/– 1 year window			**Panel B**: 1983-1992 +/– 5 year window			**Panel C**: 1982-1998 All data		
	Before	After	*Diff.*	Before	After	*Diff.*	Before	After	*Diff.*
Michigan	6.18	15.29	9.11	5.69	9.71	4.01	5.43	8.75	3.33
Comparison	7.70	9.96	2.26	6.63	9.01	2.38	6.32	9.51	3.20
Difference	-1.52	5.33	**6.86**	-0.94	0.70	**1.63**	-0.89	-0.76	**0.13**

Notes: All values are in percent and are calculated from the entire population of firms listed in 1987 or earlier. The "difference-in-differences" value appears in the lower-right cell of each panel.

Hypotheses Testing

Table 3 reports multivariate analyses results. The results were obtained based on the widest time window (1982-1998), which can provide a more conservative test of our hypotheses, because the effect of MARA weakened over time as shown in the table above. In the meantime, using this time window produces a larger sample size and therefore can provide a more powerful test. A robustness check section below reports results across different time windows, however. Model 1 includes all control variables, the indicators for Michigan (*Michigan*) and post-MARA (*After*), as well as their interaction term, *Michigan * After*. Models 2-4 successively add the three-way interaction of *Michigan * After* and each of the three explanatory variables: *Knowledge Workers*, *In-state Competition*, and *IP Protection*; Model 4 is the full model including all of the variables at the same time.

Table 3: Logit models of the likelihood of being an acquisition target.

	Model 1	Model 2	Model 3	Model 4	Model 5
Ind. Auto	0.2576	0.2387	0.3400	0.3084	0.4535
	(0.2340)	(0.2356)	(0.2758)	(0.2828)	(0.3125)
Ind. Drugs	0.1423	0.1495	0.1575	0.1593	0.2732
	(0.1947)	(0.1951)	(0.1954)	(0.1936)	(0.2053)
Ind. Chemicals	0.0652	0.0738	0.0824	0.0792	0.1948
	(0.2009)	(0.2001)	(0.2005)	(0.2001)	(0.2348)
Ind. Computers & Communication	0.0456	0.0516	0.0495	0.0560	0.0112
	(0.1240)	(0.1246)	(0.1248)	(0.1253)	(0.1449)
Ind. Electrical	0.0767	0.0824	0.0851	0.0875	0.1046
	(0.1146)	(0.1145)	(0.1146)	(0.1146)	(0.1240)
Ind. Wholesale	0.0912	0.0943	0.0919	0.0879	0.0658
	(0.1911)	(0.1912)	(0.1915)	(0.1916)	(0.1994)
Ind. Retail	0.0421	0.0516	0.0526	0.0510	0.1310
	(0.1512)	(0.1527)	(0.1528)	(0.1527)	(0.1766)
Industry Prior Acquisitions	4.1340***	4.0846***	4.0923***	4.0971***	4.0206***
	(0.5914)	(0.5926)	(0.5945)	(0.5949)	(0.6696)
State GDP (log)	0.0782*	0.0788*	0.0772*	0.0783*	0.0952*
	(0.0364)	(0.0365)	(0.0366)	(0.0366)	(0.0439)
Assets (log)	0.1171***	0.1169***	0.1173***	0.1182***	0.0647**
	(0.0171)	(0.0170)	(0.0170)	(0.0170)	(0.0205)
Liquidity	0.0217	0.0219	0.0216	0.0213	0.0378
	(0.0204)	(0.0206)	(0.0203)	(0.0200)	(0.0297)
ROA	0.0114	0.0115	0.0115	0.0113	0.0062
	(0.0121)	(0.0123)	(0.0123)	(0.0121)	(0.0117)
Years Public	-0.0091**	-0.0092**	-0.0091**	-0.0092**	-0.0114**
	(0.0033)	(0.0033)	(0.0033)	(0.0033)	(0.0040)
Beale Urban Index	0.0295	0.0298	0.0304	0.0309	0.0328
	(0.0219)	(0.0220)	(0.0219)	(0.0219)	(0.0242)
Knowledge Workers (KW)	0.2252	-0.0838	-0.0778	-0.0887	-0.1391
	(0.2082)	(0.2934)	(0.2958)	(0.3023)	(0.3115)
In-state Competition (IC)	0.1220	0.0985	0.1247	0.1227	0.3174
	(0.2674)	(0.2719)	(0.4295)	(0.4322)	(0.4911)
IP Protection (IP)	-0.0323	-0.0365	-0.0374	-0.0524	-0.3775+
	(0.1271)	(0.1276)	(0.1277)	(0.1902)	(0.2200)
Michigan	-0.2186	-0.5832*	-0.7398*	-0.7904*	-0.9171**
	(0.1918)	(0.2787)	(0.2999)	(0.3069)	(0.3071)
After	0.7072**	0.7168**	0.7174**	0.7156**	0.6623*
	(0.2535)	(0.2538)	(0.2538)	(0.2530)	(0.3159)
Michigan * After	**0.1198**	**0.5183+**	**0.6736***	**0.7323***	**0.8542****
	(0.2146)	(0.2938)	(0.3169)	(0.3221)	(0.3232)
Michigan * KW		-2.7901*	-3.8088**	-2.8608+	-3.0826*
		(1.3783)	(1.4282)	(1.5692)	(1.5693)
After * KW		0.4731	0.4693	0.4642	0.4627
		(0.3260)	(0.3326)	(0.3449)	(0.3817)
Michigan * After * KW		**3.0457***	**4.2971****	**3.3003***	**3.2637***
		(1.4620)	(1.5017)	(1.6391)	(1.6484)
Michigan * IC			-2.9061*	-3.5664*	-3.6949*
			(1.4354)	(1.4338)	(1.4622)
After * IC			0.0286	0.0299	-0.3103
			(0.5217)	(0.5279)	(0.5981)
Michigan * After * IC			**3.3163+**	**4.0795***	**4.3622***
			(1.7446)	(1.7479)	(1.7640)
Michigan * IP				1.2825*	1.4119*
				(0.6423)	(0.6543)
After * IP				-0.0067	0.2343
				(0.2002)	(0.2429)
Michigan * After * IP				**-1.3475+**	**-1.5813***
				(0.7473)	(0.7707)
Constant	-5.0000***	-5.0151***	-5.0020***	-5.0189***	-4.7974***
	(0.4891)	(0.4898)	(0.4911)	(0.4907)	(0.5901)
Log likelihood	-5176.20	-5170.48	-5167.44	-5165.42	-5105.81
Observations	19,020	19,020	19,020	19,020	18,713

Notes: Robust S.E. clustered by firm, in parentheses. All models include year indicators; two-tailed tests: *** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.10$.

Model 5 has the same structure as Model 4 and uses the CEM-matched sample. We note that the results obtained from the CEM-matched sample in Model 5 are qualitatively similar to those in Model 4. Because matching can generate a better counterfactual comparison between the treatment and control groups, using a CEM-matched sample can provide a better estimate of the true magnitude of the treatment effect of MARA, and can also strengthen the equal trends assumption that is important for a DID analysis (Blundell and Costa-Dias 2000). We therefore interpret the results from Model 5.

Concerning the control variables, we find that the intensity of an industry's past acquisition activity has a positive and highly significant impact on the firm's likelihood of being a target ($p<0.001$), a finding consistent with prior research (e.g. Palepu 1986). Firms in a state with a higher GDP are more likely to become acquisition targets ($p<0.05$); larger firms are more likely to become acquisition targets ($p<0.01$), while older firms are less likely to become acquisition targets ($p<0.01$). We also find that firms based in Michigan are overall less likely to be targets for acquisition ($p<0.01$), and that the likelihood of acquisition generally increased from the before period to the after period for firms in both Michigan and the comparison states ($p<0.05$).

Next we turn to the hypotheses testing results. In our baseline hypothesis (H1), we posit that an increase in the enforcement of NCAs, such as MARA, will increase the likelihood of a firm becoming an acquisition target. The positive and highly significant coefficient for the interaction of *Michigan * After* provides strong support for this hypothesis ($p<0.01$). This result indicates that firms in Michigan are more likely to be acquisition targets following the passage of

MARA, after adjusting for the concurrent increase in the likelihood of acquisition of firms in the comparison states. Hypotheses 2-4 further identify several conditions under which constraints on employee mobility due to the increase in the enforcement of NCAs will be more or less important in shaping firms' likelihood of being a target. Consistent with the prediction in H2, we find that the three-way interaction of *Michigan * After * Knowledge Workers* is positive and significant ($p<0.05$), suggesting that the effect of MARA on acquisition likelihood is stronger when firms employ more knowledge workers in their workforce that present a greater risk of employee mobility. Similarly, the three-way interaction of *Michigan * After * In-state Competition* is positive and highly significant ($p<0.01$), providing strong support for H3; this result suggests that the effect of MARA is stronger when firms face greater in-state competition, a condition that can increase the risk of employee turnover. Finally, H4 predicts that the effect of MARA will be weaker when firms have other means such as IP protection to protect knowledge from appropriation by competitors. There is evidence supporting this hypothesis: the three-way interaction of *Michigan * After * IP Protection* is negative and significant ($p<0.05$), suggesting that the effect of MARA is weaker when firms are protected by a stronger IP regime.

Predictions

To demonstrate the economic significance of our results, we calculated the predicted probability of a Michigan firm becoming an acquisition target from Model 5 and report results in Table 4. We predicted the outcome at the mean of all other predictor variables; moderator variables are mean-centered at zero. For illustrative purposes, we predict results for *Knowledge Workers* and *In-state Competition* at the +1 standard deviation level and we predict results for

IP Protection at the -1 standard deviation level (i.e. H4 is a negative interaction, so the main DID effect should be stronger at a lower level of *IP Protection*). "Before" refers to the probability in Michigan before MARA; "DID Increase" refers to the before-to-after increase in probabilities in Michigan, after subtracting the concurrent change in comparison states; "% Change" refers to the increase in probability as a percentage of the "Before" period probability (i.e. the division of "DID Increase" by "Before" expressed as a percentage).

Table 4: Summary of predicted probabilities of acquisition by hypothesis.

	H1	H2	H3	H4
	Main Effect	*Knowledge Workers* (+1 SD)	*In-state Competition* (+1 SD)	*IP Protection* (−1 SD)
Before	3.5%	2.1%	2.2%	2.5%
DID Increase	4.0%	5.5%	6.7%	6.6%
% Change	115.3%	258.3%	300.6%	261.2%

Notes: Table 4 summarizes the predicted 'diff-in-diff' probability of becoming an acquisition target in Michigan, calculated from the full Model 5. The *Main Effect* is predicted at the mean of all other predictor variables; moderator variables are mean-centered at zero. For illustrative purposes, *Knowledge Workers* and *In-state Competition* are predicted at the +1 standard deviation level, while *IP Protection* is predicted at the -1 standard deviation level (which also happens to be the approximate level of no IP protection). "Before" refers to the probability in Michigan before MARA. "DID Increase" refers to the before-to-after difference in probabilities in Michigan, after subtracting the concurrent change in probabilities in comparison states. "% Change" refers to the increase in probability as a percentage of the 'Before' period probability of acquisition (i.e. we divide 'DID Increase' by 'Before' expressed as a percentage). All predicted increases are statistically significant at $p<0.05$ or better (two-tailed test).

The results in Table 4 demonstrate sizable effects across all four hypotheses. For an average firm in Michigan, the sudden enforcement of non-compete agreements increased the likelihood of a Michigan firm becoming a target for acquisition by 115.3%, compared to firms in the comparison states. Michigan firms with a one standard deviation higher level of Knowledge Workers experienced a 258.3% increase, firms with a one standard deviation higher level of In-state Competition experienced a 300.6% increase, and firms with a one standard deviation lower level of IP protection (which happens to be approximately equivalent to no IP protection) experienced a 261.2% increase, in the likelihood of acquisition compared to firms in the comparison states. All increases are statistically significant at a two-sided test of $p<0.05$ or better.

Robustness Checks

We performed a series of robustness checks of the model specifications and report these test results in Table 5. All models in the table use the CEM-matched sample and include the full set of control variables discussed earlier. To ease side-by-side comparison, Model 6 reproduces our main results from Model 5 in Table 3. Model 7 drops annual indicators to facilitate our graphical assessment of the baseline effect of MARA, and the *After* indicator now reflects the average change in the rate of acquisitions after MARA. In Model 8, we checked whether our specification of the 'before' and 'after' periods affects our results. Across all other models, we assumed that knowledge of MARA took a year to diffuse in the legal and investment communities, and that it then took an additional year for acquirers to undertake the steps of target selection, due diligence and negotiation that are typically conducted before an acquirer announces an acquisition (see Bruner 2004). It may be unrealistic, however, to assume that acquisitions announced in December of 1987 had no knowledge of MARA. To check the sensitivity of this assumption and account for a gradual diffusion of knowledge about MARA, we followed Davis (2004) and tested Model 8 using a linear spline for interaction terms with *Michigan * After*.

Table 5: Robustness checks of different model specifications and samples.

	Model 6	Model 7	Model 8	Model 9	Model 10	Model 11	Model 12	Model 13
	Full Model	*No Year FE*	*Spline 1987*	*New Firms*	*Collapsed*	*OH Placebo*	*CT Placebo*	*TX Antitrust*
Ind. Auto	0.4535	0.4432	0.4489	0.5246*	0.6101	-0.0668	0.0426	-0.2546
	(0.3125)	(0.3075)	(0.3119)	(0.2617)	(0.4106)	(0.3862)	(0.4151)	(0.4321)
Ind. Drugs	0.2732	0.2661	0.2759	0.3552**	0.2205	0.2283	0.0515	0.0709
	(0.2053)	(0.2029)	(0.2054)	(0.1376)	(0.3199)	(0.2123)	(0.2035)	(0.1870)
Ind. Chemicals	0.1948	0.1894	0.1970	0.1242	0.2190	0.0209	0.1127	0.0362
	(0.2348)	(0.2341)	(0.2348)	(0.1927)	(0.2715)	(0.1935)	(0.2073)	(0.2101)
Ind. C & C	0.0112	0.0104	0.0117	0.0806	-0.1122	0.2270	0.0384	-0.0691
	(0.1449)	(0.1440)	(0.1450)	(0.1182)	(0.1918)	(0.1507)	(0.1304)	(0.1238)
Ind. Electrical	0.1046	0.1058	0.1044	0.0358	0.1616	0.0710	0.0666	-0.0205
	(0.1240)	(0.1230)	(0.1240)	(0.1060)	(0.1609)	(0.1260)	(0.1189)	(0.1157)
Ind. Wholesale	0.0658	0.0591	0.0653	0.0625	0.1837	0.1267	0.1224	0.0839
	(0.1994)	(0.1978)	(0.1994)	(0.1656)	(0.2063)	(0.2050)	(0.1942)	(0.1578)
Ind. Retail	0.1310	0.1344	0.1302	0.0733	0.1455	0.0884	0.0232	0.0331
	(0.1766)	(0.1749)	(0.1766)	(0.1297)	(0.2044)	(0.1725)	(0.1639)	(0.1315)
Ind. Prior Acq.	4.0206***	4.0637***	4.0177***	4.1369***	6.5134***	4.1554***	4.1352***	3.8180***
	(0.6696)	(0.6517)	(0.6696)	(0.5630)	(1.2798)	(0.6575)	(0.6258)	(0.5301)
State GDP (ln)	0.0952*	0.1043*	0.0950*	0.0779*	0.0424	0.1069*	0.1002*	0.0563
	(0.0439)	(0.0425)	(0.0439)	(0.0361)	(0.0514)	(0.0469)	(0.0417)	(0.0354)
Assets (ln)	0.0647**	0.0644**	0.0648**	0.0800***	0.1147***	0.0211	0.1276***	0.1189***
	(0.0205)	(0.0200)	(0.0205)	(0.0181)	(0.0292)	(0.0213)	(0.0179)	(0.0147)
Liquidity	0.0378	0.0405	0.0381	0.0227	0.3904	0.0230	0.0178	0.0230
	(0.0297)	(0.0286)	(0.0298)	(0.0193)	(0.2885)	(0.0185)	(0.0164)	(0.0190)
ROA	0.0062	0.0025	0.0061	0.0109	0.3504*	0.0032	0.0097	0.0159
	(0.0117)	(0.0089)	(0.0116)	(0.0106)	(0.1376)	(0.0073)	(0.0111)	(0.0122)
Years Public	-0.0114**	-0.0120**	-0.0114**	-0.0102**	-0.0117*	-0.0023	-0.0114**	-0.0123***
	(0.0040)	(0.0039)	(0.0040)	(0.0035)	(0.0052)	(0.0035)	(0.0036)	(0.0032)
Beale Index	0.0328	0.0336	0.0328	0.0236	0.0330	0.0133	0.0422	0.0011
	(0.0242)	(0.0237)	(0.0241)	(0.0193)	(0.0284)	(0.0279)	(0.0247)	(0.0153)
KW	-0.1391	-0.1056	-0.1299	-0.0914	-0.1090	-0.2524	-0.0260	0.0716
	(0.3115)	(0.3093)	(0.3115)	(0.3094)	(0.3960)	(0.3674)	(0.3183)	(0.4817)
IC	0.3174	0.3179	0.2935	-0.1234	0.2656	-0.3389	-0.2737	0.5360
	(0.4911)	(0.4898)	(0.4925)	(0.4852)	(0.4583)	(0.4773)	(0.4389)	(0.4971)
IP	-0.3775+	-0.3717+	-0.3793+	-0.2964	-0.1446	-0.1025	-0.0132	0.1939
	(0.2200)	(0.2180)	(0.2196)	(0.2114)	(0.2630)	(0.2236)	(0.2060)	(0.2771)
Michigan	-0.9171**	-0.9001**	-1.1599**	-0.9274**	-0.8928*	-0.3464+	0.2012	0.0146
	(0.3071)	(0.3022)	(0.3536)	(0.3149)	(0.3473)	(0.1826)	(0.2007)	(0.1798)
After	0.6623*	0.1269	0.6616*	0.7521**	0.9221***	1.0515***	0.7490**	0.9020***
	(0.3159)	(0.0856)	(0.3163)	(0.2820)	(0.1078)	(0.3142)	(0.2731)	(0.2308)
MI * After	**0.8542****	**0.8459****	**1.0995****	**0.7508***	**0.9865***	**0.0524**	**-0.1818**	**-0.1294**
	(0.3232)	(0.3182)	(0.3725)	(0.3293)	(0.3960)	(0.2120)	(0.2157)	(0.1824)
MI * KW	-3.0826*	-2.9618+	-4.0804*	-3.1081*	-3.3016+	-0.5153	0.1172	-0.4335
	(1.5693)	(1.5410)	(1.7520)	(1.5641)	(1.9959)	(1.1101)	(1.1171)	(0.9023)
After * KW	0.4627	0.4099	0.4476	0.3169	0.7571	0.4446	0.5302	0.1793
	(0.3817)	(0.3790)	(0.3816)	(0.3330)	(0.4682)	(0.4206)	(0.3673)	(0.4914)
MI*After*KW	**3.2637***	**3.1269+**	**4.2572***	**3.2169***	**3.9952+**	**0.6993**	**-1.3323**	**0.3410**
	(1.6484)	(1.6223)	(1.9482)	(1.6255)	(2.1705)	(1.2832)	(1.1795)	(0.9403)
MI * IC	-3.6949*	-3.6506*	-3.8561*	-3.4580*	-4.2149*	1.9941*	2.3008*	-0.7557
	(1.4622)	(1.4419)	(1.6202)	(1.4427)	(1.7770)	(0.9512)	(1.0654)	(0.7796)
After * IC	-0.3103	-0.3118	-0.2679	0.2520	0.0263	0.2285	0.2534	-0.3911
	(0.5981)	(0.5935)	(0.5959)	(0.5374)	(0.6518)	(0.6062)	(0.5513)	(0.5290)
MI*After*IC	**4.3622***	**4.3113***	**4.3399***	**3.6040***	**4.6864***	**-1.7266**	**-1.1206**	**0.7216**
	(1.7640)	(1.7470)	(1.9569)	(1.6976)	(2.2677)	(1.3882)	(1.3397)	(0.8080)
MI * IP	1.4119*	1.4148*	1.6581*	1.3257*	1.4275	0.2823	0.2522	-0.2036
	(0.6543)	(0.6412)	(0.8255)	(0.6430)	(0.8798)	(0.5445)	(0.5657)	(0.5281)
After * IP	0.2343	0.2152	0.2355	0.2178	0.3873	-0.0643	-0.0088	-0.2172
	(0.2429)	(0.2406)	(0.2417)	(0.2157)	(0.2734)	(0.2441)	(0.2149)	(0.2783)
MI*After*IP	**-1.5813***	**-1.5941***	**-1.8072+**	**-1.6726***	**-1.9737+**	**-0.4876**	**-0.2952**	**0.4923**
	(0.7707)	(0.7593)	(0.9337)	(0.7332)	(1.0702)	(0.6323)	(0.6379)	(0.5460)
Constant	-4.7974***	-4.2374***	-4.7938***	-4.6239***	-2.3967***	-5.1009***	-5.3377***	-4.6919***
	(0.5901)	(0.5333)	(0.5901)	(0.5170)	(0.6485)	(0.6290)	(0.5573)	(0.4643)
Log likelihood	-5105.81	-5171.35	-5105.21	-8173.44	-1935.39	-5634.54	-4705.94	-6696.99
Observations	18,713	18,713	18,713	24,865	3,323	19,876	17,216	24,500

Notes: Robust standard errors are clustered by firm and presented in parentheses. All models use the CEM matched sample. All models except Model 7 include annual indicators. Model 8 uses a one-year linear spline in 1987. Model 9 includes firms listed after 1987. Model 10 collapses observations into two periods (before vs. after). Placebo Models 11 and 12 test OH and CT as if they were Michigan. Placebo Model 13 tests TX as if it were Michigan and passage of the FEAA. Placebo models are, as expected, not significant. *KW=Knowledge Workers*; *IC=In-state Competition*; *IP=IP Protection*. Two-tailed tests: *** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.10$.

Specifically, we modeled acquisition events during 1987 by weighting interaction terms with *Michigan * After* by the month in which they we announced (e.g., an acquisition announced in February of 1987 was coded with a value of 0.1667 (i.e., 2/12) for the interaction term). Model 8 shows that we obtain qualitatively similar results by modeling a diffusion of knowledge in this way.

In Model 9, we checked whether limiting our sample selection to firms in existence prior to MARA affects our results. While our selection of a *prior-only* population can rule out the possibility that the sample selection is endogenous to MARA, and this practice follows prior research (e.g. Marx, Strumsky et al. 2009; Marx, Singh et al. 2011), the selection of a *prior-only* population might cause our results to be influenced by the characteristics of firms that survived into the later years of the analysis. We therefore also performed an analysis in Model 9 on a full population sample that also included new firms emerging after MARA. We find that Model 9 produces a slightly weaker result for H1 (from $p<0.01$ to $p<0.05$), while all other results continue to hold.

In Model 10, we checked whether non-independence of repeated observations at the state level affects our results. While we clustered standard errors in all models at the firm-level, recent research suggests that difference-in-differences models can suffer from serial auto-correlation and within-group dependence, because the indicator variable for ‘treatment’ (i.e., Michigan) is highly correlated between periods within state-level clusters (Bertrand, Duflo et al. 2004; Cameron, Gelbach et al. 2008). Block-bootstrapping or cluster bootstrap methods are often used in this situation to correct for non-independence; such methods are preferred by many researchers because they also help to maintain the full sample size and statistical power of the analysis (Cameron and Trivedi 2009). Block-bootstrapping, however, conflicts with the application of CEM in that it does not allow for the application of weighted observations. We therefore turned to an even more conservative approach recommended by Bertrand *et al.* (2004) and other statisticians (e.g., Judd and McClelland 2009). Specifically, we collapsed the panel of repeated observations into just one before period and one after period. We took the mean of all right-hand-side variables for each period and marked acquisition activity as occurring when there was an acquisition announcement any point during each period. This is a more conservative approach for dealing with non-independence, but it also loses considerable statistical power as the number of observations drops from 18,713 to just 3,323, especially given the relatively rare occurrence of acquisition events (King and Zeng 2001). Nevertheless, we still find significant results for all of our hypothesized effects in Model 10.

In Models 11 and 12 we performed a series of 'placebo' tests by replacing Michigan with a placebo state that did not experience an increase in NCA enforcement. We performed this analysis to address the concern that our results may be due to a general pattern of acquisitions and not to the policy change in Michigan in particular. In Model 11, we report the test results for Ohio, an enforcing state that had an economy similar to Michigan (e.g., a large presence of the auto industry). In Model 12, we report the test results for Connecticut, a non-enforcing state (we performed this test by removing Connecticut from the set of comparison states and then substituting it for Michigan). As expected, we did not find significant results for the hypothesized effects in any of these placebo tests. We repeated this test for a range of other states, including Florida, Missouri, New York, Oregon, Texas, and Wisconsin, and (as expected) did not find significant results in any of the tests.

In Model 13 we tested whether our results might be due to concurrent changes in Michigan's antitrust law and regulations, instead of Michigan's inadvertent increase in NCA enforcement. Theoretically, we expect that an increase in antitrust enforcement should *decrease* the likelihood of acquisition (see Brodley 1995), although this is an empirical question to be tested. Folsom reports (1991: 955, footnote 54) that 5 states, in addition to Michigan, enacted major new antitrust legislation in the 1980s. Of these, Texas was the largest state and closest in time to MARA. We therefore tested whether the Texas Free Enterprise and Antitrust Act of 1983 produced effects similar to those we report for Michigan. We performed this test by substituting Texas in for Michigan (as we did for the placebo tests) and changing the *After* variable to 1985. As expected, we did not find significant results in Model 13 for our hypothesized effects.

Finally, the automotive industry accounts for 13% of firms in Michigan in our sample. The importance of the automotive industry raises the concern that difficulties in the industry might explain differences in the likelihood of acquisition, independent of NCA enforcement. While we control for the automotive industry in all of our models with an indicator variable, it is possible that our results are sensitive to time-trends in the automotive industry. As an additional robustness check, therefore, we added control variables for (*Michigan * Auto)*, (*After * Auto)*, and (*Michigan * After * Auto)* to Model 5 to control for the before-to-after trend in autos (results not tabulated due to space constraints, but available on request). The addition of these variables strengthened the effect size and statistical significance of H1, H2, and H3, further supporting those results. This model however weakened the effect size and significance of H4 (p=0.14, two-tailed test).

Table 6 reports the analyses we performed to check the robustness of our results to the use of different time windows. Model 14 examines the immediate +1 year/–1 year time period around MARA, and Model 24 reproduces the results reported in Model 5 of Table 3 above, which were based on the full range of data available in the sample. The effect of MARA should be stronger for shorter time windows around MARA, yet such analyses might suffer from low statistical power due to the reduced sample size and relatively rare occurrence of acquisition events in a logistic regression (King and Zeng 2001). We find that the results for shorter time windows are generally weaker, though the sign and magnitude of the estimated coefficients are consistent across all models. In particular, the results for H2, H3, and H4 remain consistent and become statistically significant as the time window widens. The overall pattern observed in this table gives us confidence that our results are not driven by the use of a specific time window.

Interpretation of the Interaction Effects

Greene (2010: 291) suggests graphical representation to interpret higher-order interaction effects in nonlinear models. We therefore followed Zelner (2009) and used simulation techniques (King, Tomz et al. 2000) to assess the boundary conditions of the 'difference-in-differences' effect at different levels of each moderating variable (see Holburn and Zelner 2010 for a recent application of this approach in strategy research). Our objective is to predict the before-to-after

Table 6a: Robustness checks across different time windows.

	Model 14	Model 15	Model 16	Model 17	Model 18
	1987-1988	1986-1989	1985-1990	1984-1991	1983-1992
Ind. Auto	1.8513***	0.8691*	0.8256**	0.7584*	0.5735+
	(0.5564)	(0.4195)	(0.3177)	(0.3521)	(0.3413)
Ind. Drugs	0.5332	0.4540	0.3961	0.3461	0.2575
	(0.4117)	(0.3165)	(0.2762)	(0.2585)	(0.2363)
Ind. Chemicals	0.6802	0.5577	0.5182	0.4596	0.3360
	(0.4680)	(0.4273)	(0.3431)	(0.3032)	(0.2868)
Ind. C & C	-0.4756	-0.1758	-0.0791	-0.0668	-0.0058
	(0.3407)	(0.2467)	(0.2203)	(0.2025)	(0.1877)
Ind. Electrical	0.4138+	0.2793	0.2930+	0.2437+	0.2200
	(0.2450)	(0.1751)	(0.1569)	(0.1467)	(0.1388)
Ind. Wholesale	0.1663	0.2241	0.1550	0.1327	0.0930
	(0.4427)	(0.3067)	(0.2751)	(0.2773)	(0.2710)
Ind. Retail	0.3727	0.5039*	0.3145	0.3289	0.3126
	(0.3300)	(0.2444)	(0.2284)	(0.2184)	(0.2088)
Industry Prior Acquisitions	4.0031**	2.8616**	2.7981**	2.8244**	2.5321**
	(1.4812)	(1.0956)	(0.8863)	(0.9104)	(0.8698)
State GDP (log)	0.2385**	0.1313*	0.0881	0.0996+	0.1077*
	(0.0865)	(0.0653)	(0.0576)	(0.0539)	(0.0514)
Assets (log)	0.0697+	0.0769*	0.0939**	0.0942***	0.0947***
	(0.0411)	(0.0324)	(0.0287)	(0.0271)	(0.0253)
Liquidity	0.1964	0.0148	0.0681	0.0031	0.0117
	(0.2216)	(0.0583)	(0.0751)	(0.0063)	(0.0206)
ROA	0.4831*	0.4613**	0.1943	0.1116	0.0087
	(0.2197)	(0.1534)	(0.1353)	(0.0971)	(0.0284)
Years Public	-0.0070	-0.0056	-0.0040	-0.0039	-0.0046
	(0.0074)	(0.0058)	(0.0047)	(0.0048)	(0.0047)
Beale Urban Index	0.0680	0.0284	0.0064	0.0127	0.0127
	(0.0468)	(0.0375)	(0.0343)	(0.0326)	(0.0306)
Knowledge Workers	0.2985	-0.4899	-0.2127	-0.0550	-0.0823
	(0.6302)	(0.4681)	(0.3872)	(0.3570)	(0.3386)
In-state Competition	-1.2165	-0.2655	0.1608	0.2337	0.3116
	(0.9564)	(0.7547)	(0.6402)	(0.5768)	(0.5289)
IP Protection	-0.3860	-0.5230+	-0.4775+	-0.4038	-0.3164
	(0.3811)	(0.3057)	(0.2641)	(0.2512)	(0.2364)
Michigan	-1.1099*	-0.7995*	-0.8600*	-0.8573**	-0.9397**
	(0.5502)	(0.3659)	(0.3498)	(0.3265)	(0.3252)
After	0.1238	0.3619*	0.0521	-0.3567+	0.0476
	(0.1396)	(0.1521)	(0.1856)	(0.2123)	(0.2352)
Michigan * After	**1.2999***	**0.7919+**	**0.7411+**	**0.8751***	**0.9605****
	(0.6094)	(0.4202)	(0.3941)	(0.3653)	(0.3591)
Michigan * Knowledge Workers	-2.6332	-1.9349	-2.9180	-3.3131+	-3.4587*
	(3.4151)	(2.2935)	(2.1070)	(1.7869)	(1.7394)
After * Knowledge Workers	-0.6324	0.2625	0.4430	0.3347	0.3533
	(0.7671)	(0.5659)	(0.5006)	(0.4679)	(0.4537)
Michigan * After * Knowledge Workers	**0.8308**	**0.3334**	**1.3635**	**2.4786**	**2.9982**
	(3.5128)	(2.4290)	(2.2735)	(1.9913)	(1.9560)
Michigan * In-state Competition	-5.8682+	-4.1479+	-3.0174+	-3.5666*	-3.6618*
	(3.2549)	(2.2000)	(1.8180)	(1.6089)	(1.5561)
After * In-state Competition	0.2492	-0.6890	-0.7844	-0.6890	-0.7651
	(1.0792)	(0.8572)	(0.7653)	(0.6942)	(0.6469)
Michigan * After * In-state Competition	**2.5232**	**3.8285**	**2.7472**	**3.6314+**	**4.1595***
	(3.6415)	(2.5088)	(2.1189)	(1.9786)	(1.9624)
Michigan * IP Protection	1.1856	1.3353	0.7665	0.9825	1.0215
	(1.1014)	(0.9775)	(0.8968)	(0.7735)	(0.7206)
After * IP Protection	-0.0789	0.4877	0.4544	0.4397	0.3174
	(0.4569)	(0.3349)	(0.2899)	(0.2804)	(0.2704)
Michigan * After * IP Protection	**-0.9384**	**-0.8241**	**-0.4847**	**-0.8763**	**-0.6259**
	(1.4571)	(1.2525)	(1.1149)	(0.9730)	(0.9374)
Constant	-5.8125***	-4.5585***	-4.1869***	-4.2641***	-4.8131***
	(1.1297)	(0.8578)	(0.7454)	(0.6930)	(0.6629)
Log likelihood	-875	-1699	-2385	-2981	-3440
Observations	2,833	5,483	7,966	10,332	12,553

Notes: Robust standard errors are clustered by firm and presented in parentheses. All models use the CEM matched sample and include annual indicators. Two-tailed tests: *** p<0.001, ** p<0.01, * p<0.05, + p<0.1.

Table 6b: Robustness checks across different time windows.

	Model 19 1982-1993	Model 20 1982-1994	Model 21 1982-1995	Model 22 1982-1996	Model 23 1982-1997	Model 24 1982-1998
Ind. Auto	0.6076+	0.5365	0.5640+	0.5396+	0.4987	0.4535
	(0.3384)	(0.3308)	(0.3199)	(0.3122)	(0.3095)	(0.3125)
Ind. Drugs	0.3730	0.3075	0.2991	0.2574	0.2478	0.2732
	(0.2331)	(0.2196)	(0.2063)	(0.2069)	(0.2079)	(0.2053)
Ind. Chemicals	0.2910	0.1908	0.1574	0.2029	0.2163	0.1948
	(0.2793)	(0.2725)	(0.2644)	(0.2432)	(0.2365)	(0.2348)
Ind. C & C	-0.0051	-0.0732	-0.0725	-0.0596	-0.0276	0.0112
	(0.1850)	(0.1772)	(0.1696)	(0.1673)	(0.1658)	(0.1449)
Ind. Electrical	0.2287+	0.1507	0.0761	0.1093	0.1055	0.1046
	(0.1346)	(0.1321)	(0.1314)	(0.1242)	(0.1235)	(0.1240)
Ind. Wholesale	0.0547	0.0754	0.0585	0.0291	0.0676	0.0658
	(0.2564)	(0.2360)	(0.2221)	(0.2186)	(0.2009)	(0.1994)
Ind. Retail	0.2537	0.1391	0.0973	0.0800	0.1121	0.1310
	(0.2093)	(0.2067)	(0.2000)	(0.1930)	(0.1807)	(0.1766)
Industry Prior Acquisitions	2.7553**	3.0091***	3.5568***	3.9413***	4.0236***	4.0206***
	(0.8555)	(0.8119)	(0.7500)	(0.7194)	(0.6983)	(0.6696)
State GDP (log)	0.1181*	0.1161*	0.1188**	0.1134*	0.1001*	0.0952*
	(0.0495)	(0.0476)	(0.0456)	(0.0444)	(0.0439)	(0.0439)
Assets (log)	0.0901***	0.0829***	0.0777***	0.0723**	0.0638**	0.0647**
	(0.0245)	(0.0234)	(0.0227)	(0.0224)	(0.0221)	(0.0205)
Liquidity	0.0154	0.0193	0.0197	0.0245	0.0315	0.0378
	(0.0220)	(0.0228)	(0.0209)	(0.0267)	(0.0289)	(0.0297)
ROA	0.0038	0.0053	0.0053	0.0205	0.0236	0.0062
	(0.0116)	(0.0144)	(0.0146)	(0.0762)	(0.0736)	(0.0117)
Years Public	-0.0049	-0.0064	-0.0095*	-0.0114**	-0.0112**	-0.0114**
	(0.0045)	(0.0044)	(0.0043)	(0.0042)	(0.0041)	(0.0040)
Beale Urban Index	0.0287	0.0355	0.0324	0.0340	0.0408+	0.0328
	(0.0290)	(0.0275)	(0.0267)	(0.0256)	(0.0243)	(0.0242)
Knowledge Workers	-0.1417	-0.0723	-0.0816	-0.0812	-0.1011	-0.1391
	(0.3269)	(0.3212)	(0.3175)	(0.3165)	(0.3159)	(0.3115)
In-state Competition	0.2580	0.2689	0.2785	0.2930	0.3266	0.3174
	(0.5139)	(0.5083)	(0.5038)	(0.4996)	(0.4912)	(0.4911)
IP Protection	-0.4034+	-0.3630	-0.3597	-0.3711+	-0.3626	-0.3775+
	(0.2294)	(0.2256)	(0.2224)	(0.2227)	(0.2231)	(0.2200)
Michigan	-0.9666**	-0.9465**	-0.9406**	-0.9287**	-0.9271**	-0.9171**
	(0.3127)	(0.3108)	(0.3095)	(0.3086)	(0.3078)	(0.3071)
After	0.1410	0.8365**	1.0335***	0.8847**	0.9826***	0.6623*
	(0.2965)	(0.2836)	(0.2779)	(0.2855)	(0.2986)	(0.3159)
Michigan * After	**0.9113****	**0.8972****	**0.9589****	**0.9213****	**0.8552****	**0.8542****
	(0.3476)	(0.3362)	(0.3285)	(0.3265)	(0.3245)	(0.3232)
Michigan * Knowledge Workers	-3.1575*	-3.0992+	-3.1086*	-3.1115*	-3.1064*	-3.0826*
	(1.6046)	(1.5947)	(1.5835)	(1.5806)	(1.5770)	(1.5693)
After * Knowledge Workers	0.4846	0.5555	0.5655	0.6163	0.5674	0.4627
	(0.4287)	(0.4069)	(0.3896)	(0.3862)	(0.3829)	(0.3817)
Michigan * After * Knowledge Workers	**2.4676**	**2.7745**	**3.4114***	**3.2975***	**2.9885+**	**3.2637***
	(1.8053)	(1.7497)	(1.6876)	(1.6669)	(1.6609)	(1.6484)
Michigan * In-state Competition	-4.1157**	-3.9487**	-3.9405**	-3.8702**	-3.7995**	-3.6949*
	(1.5087)	(1.4958)	(1.4892)	(1.4779)	(1.4694)	(1.4622)
After * In-state Competition	-0.8146	-0.8875	-0.6170	-0.4813	-0.2087	-0.3103
	(0.6300)	(0.6168)	(0.6001)	(0.6078)	(0.5973)	(0.5981)
Michigan * After * In-state Competition	**4.7322***	**4.6093***	**4.7257****	**4.4372***	**4.0599***	**4.3622***
	(1.8757)	(1.8584)	(1.8105)	(1.7694)	(1.7404)	(1.7640)
Michigan * IP Protection	1.4425*	1.4210*	1.3911*	1.3979*	1.3939*	1.4119*
	(0.6554)	(0.6536)	(0.6527)	(0.6564)	(0.6576)	(0.6543)
After * IP Protection	0.3715	0.3718	0.3588	0.3020	0.2575	0.2343
	(0.2604)	(0.2526)	(0.2484)	(0.2487)	(0.2473)	(0.2429)
Michigan * After * IP Protection	**-1.0873**	**-1.2472**	**-1.4622+**	**-1.4424+**	**-1.6056***	**-1.5813***
	(0.8276)	(0.7907)	(0.7858)	(0.7747)	(0.7702)	(0.7707)
Constant	-5.2626***	-5.1680***	-5.1348***	-5.0336***	-4.8541***	-4.7974***
	(0.6590)	(0.6383)	(0.6150)	(0.6007)	(0.5945)	(0.5901)
Log likelihood	-3775	-4074	-4381	-4642	-4906	-5106
Observations	14,566	15,539	16,440	17,268	18,026	18,713

Notes: Robust standard errors are clustered by firm and presented in parentheses. All models use the CEM matched sample and include annual indicators. Two-tailed tests: *** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$.

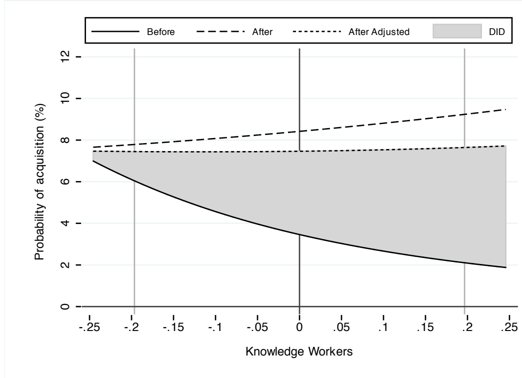
effect of MARA in Michigan, adjusting for changes in the comparison states that represent what would have happened in Michigan in the absence of MARA; and to examine the hypothesized 'difference-in-differences' effect for each of the three moderating variables (*Knowledge Workers*, *In-state Competition*, and *IP Protection*). We used *Clarify*, a simulation tool for Stata developed by Tomz, Whittenberg, and King (2003) to perform the simulations. We followed prior research (e.g. Marx, Strumsky et al. 2009) and focused on a model that drops annual indicators (i.e. Model 7 in Table 5) so that the *After* variable, and *Michigan * After* interactions, represented the full effect of the before-to-after change. We simulated 1,000 predictions, for both the before-MARA period and the after-MARA period, for both Michigan and the comparison states, and for 60 separate levels of each moderating variable, resulting in a total of 240,000 simulations for each effect hypothesized in H2 - H4. We present our results in Figures 2, 3 and 4 below, which correspond to Hypotheses 2, 3, and 4, respectively.

Panel A. Panel A in each figure shows the predicted before-to-after change in the likelihood of being an acquisition target at different values of the moderating variable. The 'naïve' effect of MARA is the difference between the bottom, "Before" line and the top, "After" line; the 'difference-in-differences' effect is the shaded region between the "Before" line and the middle, "After Adjusted" line.

Panel B. Panel B in each figure shows the 'difference-in-differences' line, obtained from subtracting the value on the "Before" line from the corresponding value on the "After Adjusted" line; and it graphs the 95% confidence interval around the 'difference-in-differences' line. This

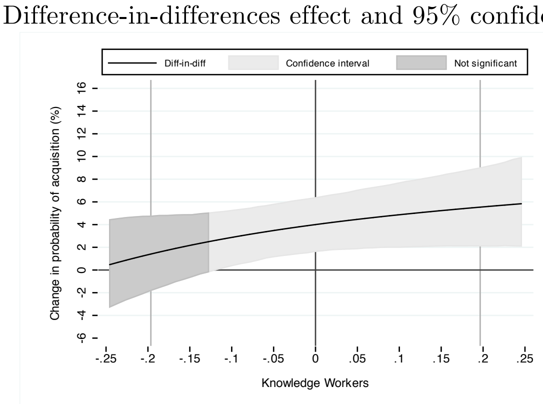
Figure 2: The effect of MARA by level of *Knowledge Workers*

Panel A: Predicted probabilities of being an acquisition target



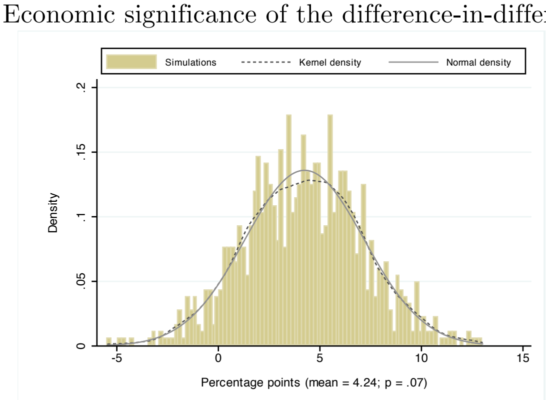
Notes: This graph shows the predicted before-to-after change in the probability of being an acquisition target by level of *Knowledge Workers*. The naïve effect of MARA is the difference between the 'Before' line and the 'After' line. The difference-in-differences effect of MARA is the shaded region between the 'Before' line and the 'After Adjusted' line upon removing the effect of coinciding changes in the comparison states. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *Knowledge Workers* (M=0.000; SD=0.197).

Panel B: Difference-in-differences effect and 95% confidence interval



Notes: This graph shows the predicted difference-in-differences increase in the probability of being a target by level of *Knowledge Workers*. A 95% confidence interval is graphed around the effect, with the non-significant portion in a darker shade. As predicted in H2, this graph indicates that the effect of an increase in the enforcement of NCAs on firms' likelihood of being a target is stronger for firms that employ a greater proportion of knowledge workers. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *Knowledge Workers* (M=0.000; SD=0.197).

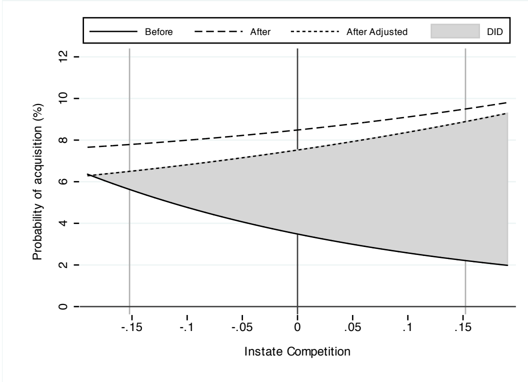
Panel C: Economic significance of the difference-in-differences effect



Notes: This graph shows a histogram of 1,000 simulations of the predicted difference in the difference-in-differences between 1 SD below and 1 SD above the mean of *Knowledge Workers*. The graph indicates that a change from 1 SD below to 1 SD above the mean of *Knowledge Workers* leads to an economically and statistically significant increase in firms' likelihood of being a target by 4.24 percentage points (p=0.07).

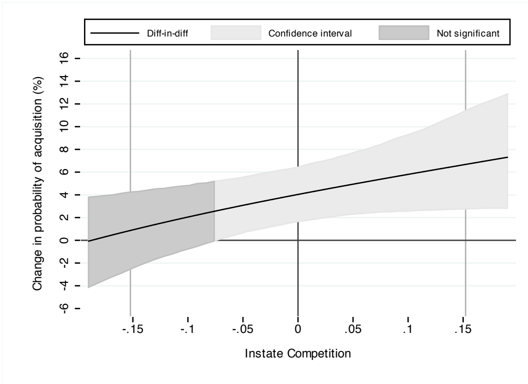
Figure 3: The effect of MARA by level of *Instate Competition*

Panel A: Predicted probabilities of being an acquisition target



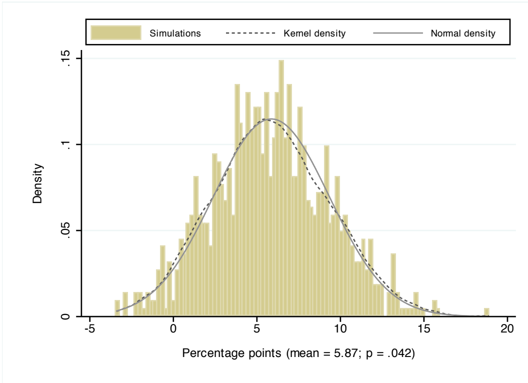
Notes: This graph shows the predicted before-to-after change in the probability of being an acquisition target by level of *In-state Competition*. The naïve effect of MARA is the difference between the 'Before' line and the 'After' line. The difference-in-differences effect of MARA is the shaded region between the 'Before' line and the 'After Adjusted' line upon removing the effect of coinciding changes in the comparison states. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *In-state Competition* (M=0.000; SD=0.152).

Panel B: Difference-in-differences effect and 95% confidence interval



Notes: This graph shows the predicted difference-in-differences increase in the probability of being a target by level of *In-state Competition*. A 95% confidence interval is graphed around the effect, with the non-significant portion in a darker shade. As predicted in H3, this graph indicates that the effect of an increase in the enforcement of NCAs on firms' likelihood of being a target is stronger for firms that face greater in-state competition. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *In-state Competition* (M=0.000; SD=0.152).

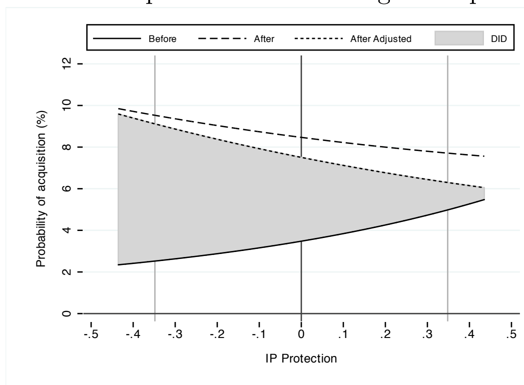
Panel C: Economic significance of the difference-in-differences effect



Notes: This graph shows a histogram of 1,000 simulations of the predicted difference in the difference-in-differences between 1 SD below and 1 SD above the mean of *In-state Competition*. The graph indicates that a change from 1 SD below to 1 SD above the mean of *In-state Competition* leads to an economically and statistically significant increase in firms' likelihood of being a target by 5.87 percentage points (p=0.042).

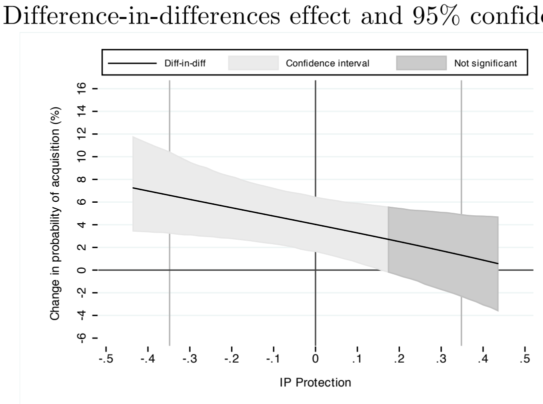
Figure 4: The effect of MARA by level of *IP Protection*

Panel A: Predicted probabilities of being an acquisition target



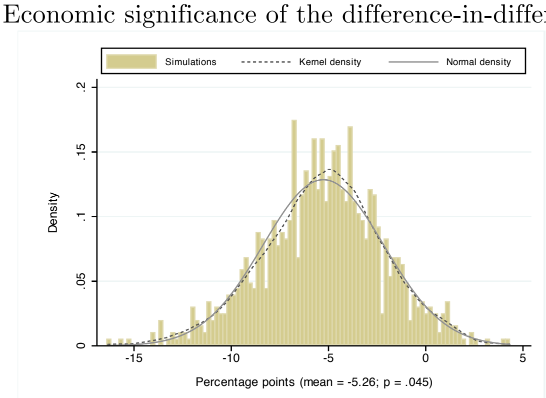
Notes: This graph shows the predicted before-to-after change in the probability of being an acquisition target by level of *IP Protection*. The naïve effect of MARA is the difference between the 'Before' line and the 'After' line. The difference-in-differences effect of MARA is the shaded region between the 'Before' line and the 'After Adjusted' line upon removing the effect of coinciding changes in the comparison states. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *IP Protection* (M=0.000; SD=0.348).

Panel B: Difference-in-differences effect and 95% confidence interval



Notes: This graph shows the predicted difference-in-differences increase in the probability of being a target by level of *IP Protection*. A 95% confidence interval is graphed around the effect, with the non-significant portion in a darker shade. As predicted in H4, this graph indicates that the effect of an increase in the enforcement of NCAs on firms' likelihood of being a target is weaker for firms that are protected by a stronger IP regime. Vertical lines appear at zero, as well as at 1 SD above and 1 SD below the mean of *IP Protection* (M=0.000; SD=0.348).

Panel C: Economic significance of the difference-in-differences effect



Notes: This graph shows a histogram of 1,000 simulations of the predicted difference in the difference-in-differences between 1 SD below and 1 SD above the mean of *IP Protection*. The graph indicates that a change from 1 SD below to 1 SD above the mean of *IP Protection* leads to an economically and statistically significant decrease in firms' likelihood of being a target by 5.26 percentage points (p=0.045).

panel shows the strength of the effect of MARA at different values of the moderating variable and the range over which the effect is statistically significant.

Panel C. Panel C in each figure shows a histogram of the simulated predicted difference in outcomes between 1 standard deviation below and 1 standard deviation above the mean value of each moderating variable. By examining the percent of simulated values that fall below (or above) zero, we can also determine the probability that the predicted interaction effect between the two levels is statistically different from zero. Figure 2C shows that a change from one standard deviation below to one standard deviation above the mean of *Knowledge Workers* leads to a 4.24 percentage point increase in the likelihood of acquisition (p=0.070); Figure 3C shows that a change from one standard deviation below to one standard deviation above the mean of *In-state Competition* leads to a 5.87 percentage point increase in the likelihood of acquisition (p=0.042); and Figure 4C shows that a change from one standard deviation below to one standard deviation above the mean of *IP Regime* leads to a 5.26 percentage point decrease in the likelihood of acquisition (p=0.045).

5. Discussion

Drawing on a difference-in-differences analysis of a natural experiment in Michigan, we have shown that an increase in the enforcement of non-compete agreements causes an increase in the likelihood of a firm becoming an acquisition target. Because NCAs have also been shown to constrain employee mobility (Marx, Strumsky et al. 2009), we have argued that our results demonstrate the importance of retaining employees in a strategy to source knowledge and human talents through acquisitions. Moreover, we find that the effect of NCA enforcement is amplified for firms that are more likely to experience the negative consequences of employee turnover, such as for firms that employ higher levels of knowledge workers and firms that face greater in-state competition. We also find that the effect is attenuated for firms that are protected by a stronger intellectual property regime, where firms can prevent the appropriation of intellectual property by competitors and the consequences of employee mobility are therefore less severe.

Taken as a whole, across multiple specifications and robustness tests, our results support our theoretical argument that acquirers incorporate expectations about employee mobility in their acquisition decisions, and that acquisition likelihood is contingent on the consequences of employee mobility. Our paper makes several contributions to theory and research. First, our study contributes to an important stream of M&A research that focuses on the human capital aspect of acquisitions. Prior research has pointed to the significant challenges acquirers face in retaining the employees of the acquired company post-acquisition (Haspeslagh and Jemison 1991; Hambrick and Cannella 1993; O'Reilly and Pfeffer 2000; Ranft and Lord 2000; Coff 2002). Our study complements this research by studying how the anticipated risk of employee departure from a firm following an acquisition *ex post* may affect acquirers' decision regarding whether to bid for the firm *ex ante*. Prior research has emphasized the importance of human capital for a firm to gain a competitive advantage (Lado and Wilson 1994; Coff 1997; Barney and Wright 1998). We show that acquirers are sensitive to employee mobility in using acquisitions to source human capital, and we offer new evidence on the challenges firms face in dealing with human capital in the context of corporate acquisitions (Coff 1999; O'Reilly and

Pfeffer 2000; Coff 2002; Bruner 2004). While acquirers may not be able to control the regime of non-compete agreements, our research suggests that acquirers may refer to *ex-ante* mechanisms to mitigate the anticipated costs of post-acquisition mobility. Future research could examine other means by which acquirers can retain employees after an acquisition.

Second, our study builds on a central assumption in strategic management research that firms formulate a strategy based on expectations about future returns from that strategy (Barney 1986). This assumption, while straight forward, has not been the focus of much empirical research, perhaps because firms' expectations are largely unobserved. We model acquisition decisions as strategic choices based on variations in *ex ante* expectations about the outcome of potential acquisitions. Our approach departs from the majority of acquisitions research, which focuses on *realized* acquisition deals, in that we focus on the entire population of listed firms that could become a target for an acquisition bid (see Palepu 1986; Song and Walkling 1993; Song and Walkling 2000). While our approach avoids problems of sample selection bias (Heckman 1979) and allows us to examine target-side factors that shape acquirer-side expectations, it prevents us from examining characteristics at the acquirer level, the deal level, or the dyadic level. Extensions to our study could focus on a sample of realized acquisition deals before and after MARA to examine other important questions such as whether the passage of MARA might be less important to acquiring firms with a capability to retain employees post-acquisition (O'Reilly and Pfeffer 2000), how the policy reversal may affect acquiring firms' bidding strategies (Coff 1999), and whether these changes may lead to a structural shift in the types of acquisitions occurring in Michigan.

Third, our study extends existing research on NCAs and employee mobility. Prior research on NCAs has examined the relationship between NCAs and employee mobility (Fallick, Fleischman et al. 2006; Marx, Strumsky et al. 2009; Garmaise 2011) and has studied how this relationship may affect firm innovation, new venture founding, and regional development (Saxenian 1994; Gilson 1999; Stuart and Sorenson 2003; Franco and Mitchell 2008; Samila and Sorenson 2011). Our study brings together research on NCAs and research on acquisitions by examining how constraints on interorganizational employee mobility may affect firms' strategic choices regarding their interorganizational relationships. Our findings are consistent with recent research on the link between individual-level employee mobility and firm-level strategies and outcomes (Stuart and Sorenson 2003; Agarwal, Echambadi et al. 2004), and we contribute to this research by explicitly considering how individual mobility and considerations about human capital may shape firms' boundary decisions through acquisitions.

Fourth, this study advances the use of several new methodologies in strategic management research. We exploit a natural experiment and a "difference-in-differences" analysis to control for endogeneity, a frequent concern in strategy research; in doing so, we aim to establish causal evidence on the antecedents of strategic choices. We introduce the use of coarsened exact matching (Iacus, King et al. 2009) as a new procedure in strategy research to ensure better covariate balance and better counterfactual comparisons in observational studies. In the future, strategy researchers may be able to use CEM for other research questions, such as matching firms that choose to diversify with firms that prefer to stay focused in order to better understand the origins of the "diversification discount" (Villalonga 2004). Finally, we extend techniques of graphing interaction effects in nonlinear models (King, Tomz et al. 2000; Zelner 2009) for "difference-in-differences" analyses with higher-order interaction terms. Our graphs aid the interpretation of nonlinear interactions by showing the specific domain in which the interaction effect is statistically significant.

Conclusion

Our study demonstrates that factors affecting employee mobility in a firm have a significant and economically important impact on the likelihood of a firm becoming an acquisition target. Our results suggest that acquirers incorporate expectations about employee mobility into their acquisition decisions and that these corporate decisions are shaped by target companies' characteristics. As human capital grows in prominence in today's economy and firms make more acquisitions to source knowledge and talents, it becomes increasingly important to understand the relationship between employee mobility and corporate acquisitions.

CHAPTER 3

THE MARKET VALUE OF KNOWLEDGE PROTECTION:

EVIDENCE FROM A NATURAL EXPERIMENT

1. Introduction

For firms to capitalize on R&D activities, they must both create valuable knowledge and then capture its value in the marketplace. Given the intangible, non-rivalrous nature of information (Arrow 1962; Romer 1990), protecting knowledge can be a challenge: products can be reverse engineered; processes can be leaked; and key employees can be hired away, taking proprietary information with them (Liebeskind 1996). Firms earn supranormal returns only to the extent that they can exclude competitors from expropriating the knowledge they create.

One of the most frequently studied measures for establishing appropriability over knowledge is patenting. Since Griliches (1981), numerous researchers have argued that patent protection contributes to the market value of firms (Cockburn and Griliches 1988; Schankerman 1998; Harhoff, Narin et al. 1999; Hall 2000; Bessen 2008). But patenting is not without its drawbacks. To obtain a patent, applicants must publicly disclose proprietary information which may in fact afford competitors a technological map.[3] Rivals can also work around patents by reverse engineering, and patent litigation can be costly – especially for small firms with limited

[3] The value of reading granted patents by competitors may be diminished somewhat by the delay in processing applications, which can take years. Since 2001, however, the U.S. Patent and Trademark Office has enabled electronic download of patent applications beginning 18 months after their submission.

resources to defend themselves (Lerner 1995). Further, non-codifiable knowledge does not lend itself to patent protection; software patents, for example, have questionable value (Hall and MacGarvie 2010) and may offer minimal protection in industries where the rate of innovation outstrips the speed of patent examination (Hall and Ziedonis 2001).

Given the limitations and costs of protecting knowledge via patenting, firms may seek to guard technical knowledge as well as other information including customer lists, strategic plans, and tacit knowledge embodied in individuals (Polanyi 1962), through other "counterdiffusion" measures. One such measure is a non-disclosure agreement, in which employees covenant to keep secret any proprietary knowledge they have obtained during their employment. While such contracts lay claim to all information that an employee either generates or becomes aware of while working at the firm and banning its disclosure at any time, whether while under the firm's employ or thereafter, non-disclosure agreements are difficult to enforce due to the legal challenges of documenting disclosure. One way to limit the potential damage from unauthorized disclosure is to disaggregate tasks within the firm such that any one individual has only partial access to proprietary knowledge (Rajan and Zingales 2001). Disaggregation, however, may compromise the operational efficiency of the firm and does not prevent employees from leaving the firm with the knowledge to which they did have access. A more thorough counterdiffusion policy is to prevent ex-employees working for competitors where said disclosure could be particularly harmful. Indeed, courts have ruled that an ex-employee working for a competitor is at substantial risk of "inevitably disclosing" information from their prior job, even if attempting to honor a non-disclosure agreement (Whaley 1998). Accordingly, U.S. states have granted firms the right to restrict the post-employment opportunities of a firm's current workforce by means of employee non-compete agreements (hereafter, "non-competes").

Non-competes are employment contracts that place restraints on the behavior of workers for a set period of time after they leave the firm, usually 1-2 years. A non-compete either lists specific firms at which the ex-employee may not work or describes an industry within which the employee is not allowed to compete either by joining or starting another company. As such, non-competes restrict ex-employees from leaving to join competitors (Fallick, Fleischman et al. 2006).[4] Non-competes are generally easier to enforce than non-disclosure agreements because it is easier to verify that an ex-employee is working for a competitor than to establish that said worker has leaked proprietary information. Non-competes are widely used by firms in a variety of technical industries (Marx 2011) and appear in nearly 90% of venture capital contracts (Kaplan and Stromberg 2001).

In contrast to the well-developed literature on the market value of patent protection, we know very little regarding how counterdiffusion measures impact firm-level outcomes. This gap is puzzling given that several surveys of appropriability mechanisms suggest that the ability to keep proprietary information secret is as important to R&D managers as patenting, if not more so (Levin, Klevorick et al. 1987; Cohen, Nelson et al. 2000; Arundel 2001). In the only known study of non-competes and firm value, Garmaise (2011) finds that increased non-compete enforcement drives down R&D spending per capita but does not influence a firm's return on

[4] Non-disclosure agreements typically allow an employee to list "prior inventions" which are excluded from the employer's claim. In this way, employees are able to separate intellectual property that they created while not under the jurisdiction of the firm from that supported by the employer's R&D activities. By comparison, non-compete agreements typically do not allow the employee to identify skills or expertise they acquired previously as not being governed by the contract.

equity or market-to-book ratio. The only tangible benefit to firms in Garmaise's study is the ability to pay lower wages, a benefit that appears to be offset by other inefficiencies. If this is correct, then it is puzzling that most U.S. states sanction the use of non-compete agreements in light of the aforementioned costs to individuals as well as negative implications at the regional level.[5]

The impact of non-competes on firm value may be non-obvious given a key distinction in how non-competes protect proprietary knowledge. Whereas other forms of intellectual property render the *output* of the inventive process excludable, non-compete agreements exclude access to the *inputs* of the inventive process. For example, a patent protects an invention, whereas a copyright governs a particular work. Although the name of the inventors or authors may be listed on the artifact, excludability is in no way specific to the author. A patent does not block its inventor from moving to another firm to work on something similar, as long as the patent is not infringed upon. By comparison, a non-compete provides excludability over the *inputs* of invention. Non-competes do not address the artifact itself. Instead, they exclude other firms from accessing (that is, employing) the expertise needed to generate similar outputs.[6]

[5] Stuart and Sorenson (2003) find that the spawning rate of new biotech firms following liquidity events was attenuated in states that more strictly enforce non-compete agreements. Further evidence for the deleterious effect of non-competes on the entrepreneurial environment was found by Samila and Sorenson (2011), who found that venture capital yielded fewer patents, jobs, and new establishments where non-competes were enforced. Beleznon and Schankerman (2012) show that non-competes act as a brake on the diffusion of university inventions. Marx, Singh, and Fleming provide evidence that non-compete enforcement leads to an exodus of skilled workers, who seek more favorable employment terms in regions that restrict the use of non-competes.

[6] The deadweight loss associated with non-competes differs from that of more output-based forms of IP protection. Patents, trademarks, and trade secret protection enable inventors to set a monopoly price for an invention that would otherwise be available at near-zero cost. The resulting deadweight loss is often rationalized in that the invention might never have been realized if not for the promise of a temporary monopoly. In the case of non-competes, however, the deadweight loss incurred is not of the output but the input into the inventive process, as the expertise of workers cannot be appropriated outside the firm where the agreement was signed.

The ability to exclude inputs may have ambiguous implications for firm value, given two potentially opposing factors. On the one hand, the ability to exclude employees as inputs to invention enhances a firm's ability to capture value from R&D. By banning (ex-)employees from moving to competitors, firms stem the outward flow of trade secrets and proprietary knowledge. In a sense, this slows down the depreciation process for technical knowledge. Accordingly, the value of the present stock of knowledge within the firm should rise given enforceable non-competes. On the other hand, the state sanction of non-competes may compromise longer-term value creation (even as non-competes enhance value capture) by affecting the ability and willingness of employees to explore higher-return R&D projects. In regions where non-competes are enforceable, the circulation of both workers (Fallick, Fleischman et al. 2006; Marx, Strumsky et al. 2009) and ideas (Belenzon and Schankerman 2012) is curtailed, limiting the diffusion of knowledge and the ability of firms to explore new research areas. Moreover, realizing that extraorganizational career options are tightly circumscribed when non-competes are enforceable, inventors and managers may focus efforts on activities that are more central to the firm's current commercialization paths. For both of these reasons, enforceable non-competes may result in firm's R&D activities becoming myopic (if unwittingly).

In this study, we attempt to tease apart the effects of non-competes on firm performance by leveraging an apparently-unexpected reversal of non-compete policy in Michigan during the 1980s. We find that Michigan firms reporting R&D, which were able to use non-competes following the policy reversal, enjoyed a initial boost in Tobin's q relative to firms in other states, consistent with the notion that non-competes enable firms to capture more value from proprietary knowledge, and that the current stock-on-hand of proprietary knowledge would rise in value. This initial boost is robust to a variety of alternative specifications including changes in business-combination laws, and we fail to recreate the effect in a series of placebo regressions. Moreover, the boost in Tobin's q is increasing in R&D spending but decreasing in the number of patents, as well as stronger in industries were secrecy is more important as a means of protecting knowledge. In the long run, and consistent with our value-creation story, the initial boost in q attenuates over time as the circulation of talent is attenuated and firms consequentially become more myopic in their R&D. Our results suggest that immediate actions by firms to capitalize on non-competes as a means to protect proprietary information may eventually be offset by detrimental effects related to the external labor market.

2. Empirical Strategy

Establishing the impact of non-competes on firm value is nontrivial for several reasons. First, although considerable state-level variation exists, unobserved heterogeneity renders cross-sectional estimates of dubious value. Second, while several states have altered their non-compete enforcement statutes, many of these involve slight modifications that may not substantially affect the behavior of either firms or employees. For example, in 2008 Idaho granted judges greater latitude in establishing whether an employer had a "legitimate business interest" in enforcing the non-compete. While such a change in the law may affect the behavior of judges, it is unclear to what extent its reverberations would be felt outside the courtroom. In a field study of non-competes, Marx (2011) found that none of the ex-employees in his sample who took "career detours" in order to avoid infringing upon the non-compete were actually taken to court;

rather, in believing that a non-compete was enforceable and acting accordingly, they made worst-case assumptions about how a potential lawsuit would play out. Accordingly, for purposes of our identification strategy we want to make use of a policy change that involved not just a procedural modification of the law but a substantial reversal. We believe that Michigan offers such an opportunity.

Non-compete enforcement in Michigan had long been governed by Public Act No. 329 of 1905, Section 1: "*All agreements and contracts by which any person, copartnership, or corporation agrees not to engage in any avocation, employment, pursuit, trade, profession or business, whether reasonable or unreasonable, partial or general, limited or unlimited, are hereby declared to be against public policy and illegal and void.*"[7] This Act prohibited the use of non-competes until 1985, when the Michigan Antitrust Reform Act (MARA) was passed. The stated purpose of MARA was to centralize and standardize existing doctrine regarding antitrust policy, including collusion and price-fixing (Bullard 1985). Doing so involved both introducing a new body of law regarding antitrust (i.e., MARA itself) and repealing existing laws and acts that touched upon such issues. Among the statutes repealed was Public Act No. 329, which largely addressed antitrust issues including "maintain[ing] a monopoly of any trade" (Sections 2-4) and "combinations in restraint of trade" (Sections 5 and 6).

The available evidence suggests that Public Act No. 329 was repealed as part of MARA due to its antitrust implications, and not specifically because of a desire to change the law regarding employee non-compete agreements. More than twenty pages of legislative analysis by

[7] The language of the Act resembles that of California's Business and Profession Code Section 16600: "Except as provided in this chapter, every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void."

both House and Senate subcommittees in Michigan (Bullard 1985) discuss antitrust extensively as the motivation behind MARA but fail to mention "non-competes", "covenants not to compete", "restrictive covenants" in the deliberations leading up to the passage of the new statute—and the accompanying repeal of obsolete statutes. Thus it appears that the legislature did not realized it had repealed the ban on non-competes.

Further evidence for the inadvertent nature of Michigan's non-compete reversal is found during the period after MARA was enacted. Although we could not locate any discussion of Michigan's non-compete policy in law journals just prior to 1985, multiple articles appeared in the months following the reversal (Alterman 1985; Levine 1985; Sikkel and Rabaut 1985), apparently as a result of practicing lawyers having reviewed the repealed statutes. These articles highlighted the new enforceability of non-competes in the state, news which may have spread among law firms, which would have then informed their clients in hopes of generating contractual or prosecutorial work. These developments suggest that the legal community was not aware of the potential for the law to be reversed but learned of the change quickly thereafter.

Moreover, less than two years after the passage of MARA, the Michigan legislature amended MARA section 4(a), effective retroactively to the enactment of MARA. Importantly, the reasonableness doctrine did not reinstate the pre-MARA ban on non-compete agreements but merely provided some limitations regarding the scope and duration of non-competes, as is common in most states that permit their enforcement. This post-hoc, retroactive amendment suggests that the legislature later realized it had repealed the non-compete ban without fully considering its implications. Indeed, both House and Senate legislative analyses of the section

4(a) amendment to MARA state that a key motivation for the amendment was "to fill the statutory void" (Trim 1987).

Interviews with Michigan labor lawyers active at the time of MARA are supportive of an interpretation of the repeal as inadvertent. Robert Sikkel (2006) reported, "There wasn't an effort to repeal [the ban on] non-competes. We backed our way into it. The original prohibition was contained in an old statute that was revised for other issues. We were not even thinking about non-compete language. All of a sudden the lawyers saw no proscription of non-competes. We got active and the legislature had to go back and clarify the law." His account was independently corroborated in a separate interview with Louis Rabaut (2006): "There was no buildup, discussion, or debate of which I was aware—it was really out of the blue. As I talked to others, this appeared to be a rather uniform reaction. I have never been able to identify any awareness—and I examined this at the time—that this was a conscious or intentional act. It was part of the antitrust reform and it may have been overlooked. I am unaware of anyone that lobbied for the change."

Alternative Explanations

Taken together, the evidence above suggests that the MARA policy reversal was an unanticipated and exogenous event, one that provides an opportunity to analyze a natural experiment with respect to the enforcement of non-competes. Yet certain aspects of the reform may suggest alternative explanations for the results we find. We note five principal alternative explanations here and address each with separate analyses in the robustness section.

Anti-trust Reforms. First, our identification strategy assumes that aspects of MARA other than the reversal in the enforcement of non-compete agreements do not increase the valuation of firms in Michigan. We also note that MARA, although it unintentionally repealed the ban on non-competes, purposefully set in force antitrust reforms designed to reduce price-fixing and collusion. Although the literature on antitrust enforcement is divided regarding whether governmental actions discourage price-fixing (Feinberg 1980; Choi and Philippatos 1983; Sproul 1993; McCutcheon 1997), event studies from both U.S. and Europe indicate that markets respond negatively to antitrust actions against firms (Bosch and Eckard Jr 1991; Gunster and Van Dijk 2011). Thus we expect that the state antitrust provisions of MARA would have depressed firm values in Michigan. To obtain contemporary evidence as to the probable antitrust effects of MARA on Tobin's q, we perform several placebo analyses of anti-trust reforms in states other than Michigan that were similar to MARA and that occurred around the same time as MARA.

Automotive Industry. Second, given the concentration of automobile production in Michigan (see Singleton 1992 for an overview), one might wonder whether an effect in Michigan is primarily representative of the automotive industry. While we include industry fixed effects in our specification in order to estimate only within-industry variation, we also take the additional step of performing several robustness tests and subsample analyses to rule out the possibility that the effect is attributable solely to the automobile industry.

Midwest Effect. Third, we investigate whether the effect we identify in Michigan represents a larger "Midwest effect" that spuriously relates to MARA. To rule out this possibility, we perform several placebo analyses of other Midwest states by inserting firms from those other states into the analysis in place of Michigan firms.

California Effect. Fourth, given the potential sensitivity of difference-in-differences analyses to the composition of the comparison group – and in particular the disproportionate influence of California, a large state with a unique history tied to Silicon Valley – we investigate whether the effect we identify in Michigan represents a spurious effect driven that is driven by California and/or the comparison group. To rule out this possibility, we tested several different sets of comparison states, including a set that excluded the state of California.

Business Combination Laws. Fifth, prior research highlights that many states passed business combination (BC) laws in the late 1980s (Bertrand and Mullainathan 2003), not long after MARA. By reducing the threat of hostile takeover, BC laws may weaken corporate governance and increase the opportunity for managerial slack, leading to a decrease in firm valuation (Giroud and Mueller 2010). To the extent that BC laws might depress the market valuations of firms in comparison states during the post-MARA period, a difference-in-differences analysis would over-estimate the effect of MARA in Michigan. To control for this possibility, we include an indicator variable for the passage of a business combination law in each firm's state of incorporation.

3. Data and Methods

The sample includes firm-level data from Compustat for 1977 through 1996, the 10 year window before and 10 year window after MARA. We selected all publically listed firms in Michigan and a set of control states, including Alaska, California, Connecticut, Minnesota, Montana, North

Dakota, Nevada, Oklahoma, Washington, and West Virginia, which did not enforce non-competes before or after MARA (Stuart and Sorenson 2003). We limited the sample to firms that reported R&D expenses, were headquartered in either Michigan or a control state, were publically listed prior to MARA, and were not in the agriculture, forestry, fishing, or financial industries, resulting in a preliminary sample of 6,164 records. State affiliation was based on the location of corporate headquarters (not the state of incorporation) and historical moves between states were corrected based on the "comphist" table of corporate moves in Compustat. Given that ratios take on extreme values when the scaling variable becomes too small or too large, we dropped the top and bottom 0.1% of observations for *Tobin's q* and the top 1.0% of observations for *R&D Intensity*, dropping 94 observations. In a robustness test, we retained all outliers and obtained similar results to those reported below, including statistical significance of at least 10% or better.

Next, we stratified and matched the sample through Coarsened Exact (CEM) (Iacus, King et al. 2011). Coarsened Exact Matching is a non-parametric algorithm used to improve the common support between treated and control observations and to reduce model dependence (see Azoulay, Zivin et al. 2010 for a recent application). Coarsened Exact Matching segments the joint distribution of covariates into a limited number of strata, and then weights observations from each strata to match observations between the treatment and control groups. We matched on the basis of *Assets*, *Beta*, and *Debt-to-Equity*, measured on a pre-MARA basis in order to ensure that the matching criteria were not influenced by the policy reversal. Rather than assign arbitrary cut-points, we relied on the automatic CEM implementation in Stata to determine the number and boundaries of each strata based on an objective function and Sturge's rule (Iacus, King et al. 2011). The matched (but original and unchanged) observations were then retained and analyzed in the analysis described below. Our implementation of CEM dropped 2,613 observations from the sample, producing a sample size of 3,457 observations for the basic analysis.[8]

In our analysis, we examined an un-matched sample without CEM and found results that were somewhat larger and more significant than those from the matched sample, suggesting that the use of CEM produced more conservative findings. However, because CEM matches most observations in Michigan and only matches similar observations in comparison states, CEM can also produce a sample that is more representative of the types of firms in the treated state (Michigan). It is possible, therefore, for CEM to limit the generalizability of estimates made from this sample, although the matching procedure itself should increase the internal validity and causal interpretation of the estimates. To assess the generalizability of our sample, we calculated the relative distribution of public firms across SIC2 industry groupings and identified industries with a disproportionate representation in Michigan. As might be expected, the automobile and transportation equipment industries stand out in the Michigan economy, although this is also true, to a lesser extent, for industries associated with the production of metal products, steel, glass, cement, rubber, plastics, wood and furniture. We undertook

[8] While matching methods can improve common support and causal inference, our use of CEM also dropped 43% of the sample. Reductions of this magnitude can weight results toward the type of firms observed in the treated group (i.e., firms of a similar size, risk and capital structure as firms in Michigan). We therefore conducted sensitivity checks using less stringent matching; when we manually stratified observations into only 5 bins per matched variable, we dropped 10% of the sample and obtained results that were qualitatively similar to our key findings.

additional robustness checks (available on request) to ensure that our results are not disproportionately related to those industries.

Next, we merged firm-level observations for *Patents* (and patent citations) into the existing sample based on data from the NBER Patent Citations Data File (Hall, Jaffe et al. 2001; Hall, Jaffe et al. 2005). *Patents* was assumed to be zero when a firm did not have a patenting record, and a dummy variable (*Patenting Indicator*) was created to control for firms with missing patent records. In models with covariate controls, an additional 429 observations were dropped due to missing data, producing a final sample size of 3,028 observations.

Dependent Variable

Consistent with prior research on the valuation of intangible assets (e.g., Hirschey 1982; Villalonga 2004; Hall, Jaffe et al. 2005), the dependent variable in our valuation models is *Tobin's q*. Recent research suggests that computationally costly approaches to the calculation of Tobin's *q* (e.g., Lindenberg and Ross 1981; Lewellen and Badrinath 1997) may induce sample-selection bias due to data unavailability (DaDalt, Donaldson et al. 2003). We therefore calculated a simple approximation of *Tobin's q*, defined as the market value of common stock + book value of total assets – book value of common equity, all divided by the book value total assets.[9] As an unreported robustness check, we also tested the Chung & Pruitt (1994) approximation of Tobin's *q* and found similar results. Prior research also suggests that intangible assets may have a multiplicative rather than additive effect on value due to fixed costs in developing intangible assets, and that therefore a semi-log functional form is strongly

[9] Based on variable names in Compustat, we calculated *Tobin's q* as: `((PRCC_F * CSHO) + AT - CEQ) / AT`

preferred over a linear functional form for our regression equation. We therefore take the natural logarithm of *Tobin's q* as our dependent variable.[10]

Explanatory Variables

The DD treatment group variable, *Michigan*, is an indicator for firms located in Michigan based on the historical location of the firm's corporate headquarters. Compustat's historical files provide information on firms' historical locations required for this variable. The DD 'after' variable, *After*, is an indicator that equals one for all years following 1986, and zero otherwise. Following prior DD research, we then created an interaction variable *Michigan * After* and used this variable to identify the treatment effect of MARA.

Although MARA was enacted in 1985, which could suggest 1986 and later as the "treated" period for our analysis; we instead used 1987 and later as the treatment period. It was not until late 1985 that legal scholars first recognized the change in the enforcement of non-competes (as opposed to the general anti-trust provisions) in the legislation and published their findings in the Michigan Bar Journal (Levine 1985; Sikkel and Rabaut 1985). We assume that it would have taken some time, perhaps several months, for information about the policy change to diffuse from the legal community to firms and R&D managers in particular. Moreover, unlike individual mobility decisions, which can be acted upon quickly by workers, organizational changes to take advantage of the newfound ability to protect trade secrets may have taken additional time to implement. For these reasons, we believe that 1987 is the first year in which

[10] Logging the dependent variable also helps to control for heteroskedasticity in the residuals.

changes made by firms in response to MARA would be fully reflected in Tobin's q and thus use 1987 and later as the treatment period for the analysis.[11]

In addition to the main effect of MARA on Tobin's q, we explore three moderating factors: *R&D Intensity, Patents,* and *Secrecy.* For our measures of *R&D Intensity* and *Patents*, we use the pre-MARA value for each and hold it constant throughout the post-MARA period.[12] First, given that non-competes in theory serve to block the diffusion of trade secrets, we anticipate that the impact of enforceable non-competes on q should be increasing in the level of R&D. We measured *R&D Intensity* as the level of R&D expense divided by total sales (e.g., Cohen, Levin et al. 1987; Cohen and Levinthal 1989), interacting it with the *Michigan * After* interaction and mean-centering it at zero to simplify interpretation of the interaction effects. Second, we examined the extent to which the effectiveness of non-competes is moderated by other means of protecting intellectual property. Our variable here is the number of eventually-granted patent applications for a given year, though we have weaker priors as to the effect of the variable. Theoretically, prior research suggests that non-compete agreements may most benefit firms that have not taken other steps to protect their intellectual property by patenting (as suggested by Samila and Sorenson 2011). Moreover, the act of codifying knowledge in a patent may help to reduce the firm's dependency on a particular employee; firms that do not – or cannot, due to the tacit nature of that firm's knowledge – undertake codification may be at greater risk of harm if

[11] We also note that these complications in determining a sharp break-point in the diffusion of understanding about the policy change prevent us from undertaking other methods of analysis, such as calculating a cumulative abnormal return around the time of the policy change. Instead, we perform a pre-to-post analysis of the average effect of MARA (in a difference-in-differences specification) covering the 10 years before and after MARA, and then examine the year-by-year effects of MARA in each year over the time period.
[12] We checked the sensitivity of that approach by also testing the 5 year pre-MARA average of each moderator and found similar results. Results are available from the authors.

an employee departs and thus benefit more from non-competes. A strong substitution effect between non-compete and patent protection is yet to be established, however,[13] so the extent to which patenting moderates the effect of enforceable non-competes on firm value may not be large. Finally, if non-competes truly serve as tools for protecting trade secrets, we should expect to see the effect of non-competes to be more pronounced in sectors where secrecy is more important. For this analysis, we used an index of the importance of secrecy by industry, as assessed by managers in a survey and reported by Cohen, Nelson, and Walsh (2000). We averaged the Cohen *et al.* scores for product and process secrecy to calculate the new variable *Secrecy* used in this analysis. Although *Secrecy* does not enter into our regressions as a variable,[14] we do perform subsample analyses based on a median-split of this variable.

Control Variables

Finally, we added covariate controls into most of our models to adjust for potential differences in trends between the treated state (Michigan) and the group of comparison states. While trends over time in the group of comparison states serve as the basic control in a differences-in-differences specification, additional covariates help to control for specific regional differences. At the state level, we control for personal income and the change in state employment. At the firm level, we controlled for each firm's financial condition and other characteristics that might affect a firm's market valuation. We also added *Business Combination Law* as an indicator variable to control for the passage of a business combination law in each firm's state of incorporation. Table 1 presents summary statistics for the mean,

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[13] Marx *et al.* (2009) found that patenting rates in Michigan were largely unchanged after the MARA policy reversal.
[14] *Secrecy* was measured by industry and therefore conflicts with our use of industry fixed effects.

standard deviation, and range of all variables for the final matched sample (n=3,028). All of the control variables are also summarized in Table 1.

Model Specification

The Michigan natural experiment lends itself to a difference-in-differences (DD) model specification. For our basic DD analysis (Table 3), we used ordinary least squares (OLS) and robust standard errors clustered by firm to estimate the following regression equation without covariate controls:

$$\ln(q_i) = \beta_0 + \beta_1 Michigan_i + \beta_2 After_i + \beta_3 (Michigan_i * After_i) + \boldsymbol{T}_t + \boldsymbol{I}_j + \varepsilon_i \qquad (1)$$

Ln(q_i) is the natural logarithm of *Tobin's q* for each i firm-year observation, $\boldsymbol{I}_j$ is a vector of industry dummy variables at the SIC-4 level, $\boldsymbol{T}_t$ is a vector of year dummy variables, and ε_i is the error term. We included as series of annual indicators to control for aggregate changes in *Tobin's q* over time due to business cycles, market swings, and so forth; we included industry indicators to control for between-industry differences.

For the moderated DD analyses (Tables 4 & 5), we added interactions for *R&D Intensity (RDI)* and *Patents*, as well as a set of covariate control variables ($\boldsymbol{X}_i$), and estimated the following regression equation:

$$\begin{aligned}\ln(q_i) = {} & \beta_0 + \beta_1 Michigan_i + \beta_2 After_i + \beta_3 (Michigan_i * After_i) + \beta_4 RDI_i + \beta_5 (Michigan_i * RDI_i) + \\ & \beta_6 (After_i * RDI_i) + \beta_7 (Michigan_i * After_i * RDI_i) + \beta_8 Patents_i + \beta_9 (Michigan_i * Patents_i) + \\ & \beta_{10} (After_i * Patents_i) + \beta_{11} (Michigan_i * After_i * Patents_i) + \delta \boldsymbol{X}_i + \boldsymbol{T}_t + \boldsymbol{I}_j + \varepsilon_i \qquad (2)\end{aligned}$$

Table 1: Summary statistics.

Panel A: Descriptive Statistics (n=3,028).

Variable		Mean	SD	Min	Max	Description	
1	Tobin's q	1.50	0.89	0.48	9.33	Tobin's q defined as ((PRCC_F*CSHO)+AT–CEQ)/AT	*Compustat*
2	Tobin's q (logged)	0.30	0.43	-0.73	2.23	Natural log of Tobin's q	*Compustat*
3	State Personal Income (log)	18.87	1.04	15.56	20.52	Natural log of state-level personal income	*BLS*
4	State Employment Change	0.02	0.02	-0.05	0.13	Yearly change in state-level employment	*BLS*
5	Assets (log)	4.77	1.39	2.18	9.15	Natural log of total assets	*Compustat*
6	Employees	4.57	8.75	0.05	80.60	Count of employees	*Compustat*
7	SG&A Expense	100.26	198.92	1.31	2566.70	Selling, general and administrative expense	*Compustat*
8	Age	14.61	10.77	0.00	59.00	Proxy for age based on earliest year listed	*Compustat*
9	Net Income	18.54	57.25	-580.00	705.40	Net income	*Compustat*
10	Return on Assets	0.04	0.11	-1.21	1.06	Return on assets	*Compustat*
11	Revenue Growth	0.14	0.29	-1.00	4.59	Percent change in revenue from prior year	*Compustat*
12	Capital Exp. Intensity	0.07	0.05	0.00	0.55	Ratio of capital expenditures to total assets	*Compustat*
13	Leverage	0.45	0.21	0.03	2.90	Ratio of book value of debt to market value of equity	*Compustat*
14	Liquidity	0.34	0.22	-2.37	0.94	(Current assets - current liabilities) / by total assets	*Compustat*
15	Beta	1.19	1.23	-8.31	9.42	12-month correlation of stock returns to market returns	*CRSP*
16	Dividend Policy	0.12	4.47	-2.71	213.37	Ratio of dividend payments to the book value of equity	*Compustat*
17	Diversified Operations	0.99	0.09	0.00	1.00	Indicator variable for operation in 2+ business segments	*Compustat*
18	Business Combination Law	0.32	0.47	0.00	1.00	Indicator variable for passage of a business combination law	*Other*
19	Patenting Indicator	0.00	0.50	-0.56	0.44	Indicator variable for firms with (1) a patenting record	*NBER*
20	R&D Intensity	0.00	0.08	-0.05	4.51	Ratio of R&D expense to total sales	*Compustat*
21	Patents	0.00	19.62	-7.71	232.29	Number of new patents	*NBER*
22	Michigan	0.17	0.37	0.00	1.00	Indicator variable for the state of Michigan	*Compustat*
23	After	0.40	0.49	0.00	1.00	Indicator variable for "after" period (1987 through 1996)	*Compustat*

Panel B: Correlations.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)
1	Tobin's q	1.00																						
2	Tobin's q (logged)	0.94	1.00																					
3	State Pers. Inc. (log)	0.16	0.19	1.00																				
4	State Employ. Chg.	-0.09	-0.06	-0.03	1.00																			
5	Assets (log)	0.03	0.04	0.02	-0.10	1.00																		
6	Employees	-0.10	-0.10	0.00	0.01	0.70	1.00																	
7	SG&A Expense	0.12	0.14	0.10	-0.07	0.70	0.63	1.00																
8	Age	-0.14	-0.15	-0.04	-0.14	0.58	0.57	0.43	1.00															
9	Net Income	0.22	0.22	0.04	-0.01	0.52	0.46	0.74	0.33	1.00														
10	Return on Assets	0.21	0.22	-0.11	0.05	0.13	0.07	0.09	0.01	0.32	1.00													
11	Revenue Growth	0.33	0.35	0.03	0.13	-0.05	-0.05	-0.04	-0.18	0.04	0.27	1.00												
12	Capital Exp. Int.	0.13	0.16	-0.03	0.04	0.01	0.03	0.01	-0.13	0.04	0.04	0.23	1.00											
13	Leverage	-0.19	-0.17	0.02	0.00	0.07	0.11	0.05	0.13	-0.11	-0.55	-0.12	-0.03	1.00										
14	Liquidity	0.11	0.07	-0.10	0.04	-0.26	-0.25	-0.25	-0.24	-0.10	0.45	0.16	-0.17	-0.70	1.00									
15	Beta	0.05	0.07	0.04	0.07	0.02	-0.01	-0.02	-0.07	-0.02	0.04	0.11	0.08	-0.03	0.06	1.00								
16	Dividend Policy	0.00	0.00	0.03	0.00	-0.03	-0.01	-0.01	-0.01	0.00	0.00	-0.01	0.00	0.04	-0.03	0.00	1.00							
17	Diversified Ops.	0.03	0.03	0.06	-0.07	0.07	0.04	0.04	0.08	0.02	-0.04	-0.02	-0.01	0.02	-0.03	0.04	0.00	1.00						
18	Biz Combo Law	0.19	0.20	0.28	-0.22	0.24	-0.02	0.22	0.22	0.12	-0.02	-0.05	-0.12	0.02	-0.10	0.01	0.03	0.06	1.00					
19	Patenting Indicator	0.03	0.03	-0.09	-0.03	0.41	0.28	0.27	0.22	0.20	0.12	-0.01	0.07	-0.02	-0.02	0.05	0.02	-0.01	0.08	1.00				
20	R&D Intensity	0.25	0.27	0.24	0.00	-0.10	-0.09	-0.04	-0.19	-0.02	-0.11	0.12	0.04	-0.14	0.14	0.07	0.00	0.02	0.12	0.00	1.00			
21	Patents	0.00	0.02	0.04	-0.01	0.47	0.58	0.45	0.31	0.26	0.05	-0.02	0.02	0.02	-0.13	0.02	0.00	0.02	0.05	0.33	0.06	1.00		
22	Michigan	-0.03	-0.05	-0.09	-0.16	0.02	0.06	0.06	0.17	0.11	0.07	-0.04	0.04	-0.06	-0.05	-0.05	-0.01	-0.02	0.00	-0.01	-0.15	0.00	1.00	
23	After	0.18	0.19	0.36	-0.17	0.21	-0.05	0.19	0.19	0.10	-0.05	-0.06	-0.14	0.00	-0.08	0.01	0.03	0.07	0.84	0.06	0.14	0.03	0.00	1.00

Notes: The sample includes firm-level data from Compustat for 1977 through 1996, the 10 year window before and 10 year window after the Michigan Antitrust Reform Act (MARA). Compustat produced 6,164 records for firms that reported R&D expenses, were headquartered in either Michigan or a comparison state (AK CA CT MN MT NV ND OK WA WV), were publically listed prior to MARA, and were not in the agriculture, forestry, fishing, or financial industries. Comparison states included those states that did not enforce non-competes before MARA, and that continued to not enforce non-competes after MARA. State affiliation was based on the location of corporate headquarters (not the state of incorporation) and historical moves between states were corrected in the sample based on the "comphist" table of corporate moves in Compustat. The top and bottom 0.1% of observations for *Tobin's q*, and the top 1.0% of observations for *R&D Intensity*, were dropped as outliers, dropping 94 observations. The sample was stratified by coarsened exact matching (CEM) on the basis of *Assets*, *Beta*, and *Debt-to-Equity* to improve common support in the sample on the basis of firm size, risk, and capital structure; 2,613 observations were dropped for lack of common support. An additional 429 observations were dropped due to missing data, leaving a final sample size of 3,028 observations. Firm-level data on *Patents* (and patent citations) was merged into the sample based on data from the NBER Patent Citations Data File. *Patents* was assumed to be zero when a firm did not have a patenting record, and a dummy variable (*Patenting Indicator*) was created to control for firms with missing patent records. The variables *R&D Intensity, Patenting Indicator*, and *Patents* are mean-centered to zero.

4. Results

We begin with a simple visualization of the temporal trend in *Tobin's q* from 1975 through 2000. Panel A of Figure 1 plots the yearly average of *Tobin's q* for firms in Michigan and for firms in comparison states. We calculate the within-industry average of *Tobin's q* (weighting within-industry averages by firm asset size) and then averaged results across industries, plotting the final result by year and by group (Michigan vs. comparison states). The chart suggests that average within-industry trends in *Tobin's q* were roughly similar between Michigan and comparison states before MARA.

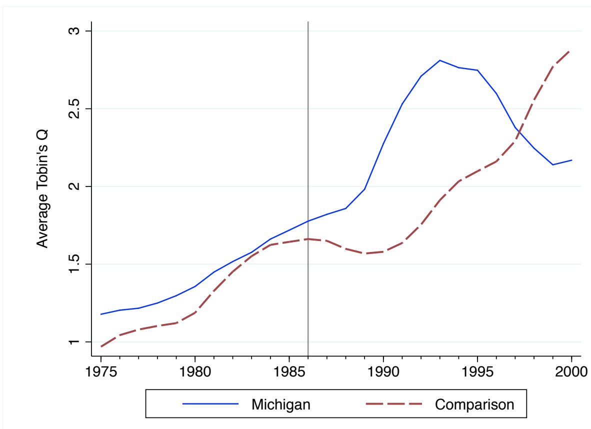
Following MARA, however, the trends for Michigan and the control states diverge. In the first half of the 15-year window following MARA, *Tobin's q* rises sharply for Michigan firms, both on an absolute basis and with respect to the control group. This upward trend is generally consistent with our anticipation that Michigan firms quickly seized on the opportunity to capture value afforded by the sudden enforceability of non-compete agreements. In the second half of the post-MARA window, however, these trends reverse as *Tobin's q* in Michigan levels off (in an absolute sense) in the mid-1990s and then drops in the late 1990s. Meanwhile, *Tobin's q* for firms in the control states grows steadily over the entire post-MARA window – spiking somewhat in the late 1990s, though not until after Michigan's absolute decline has set in. The downward trend in the second half of the post-MARA period is likewise reminiscent of our expectation that non-competes eventually erode firms' value creation efforts.[15]

[15] Although not strictly required for our empirical strategy, it is nevertheless reassuring to observe that the difference-in-differences trend is driven not simply by changes in the control group but absolute movement in the treatment group (Michigan).

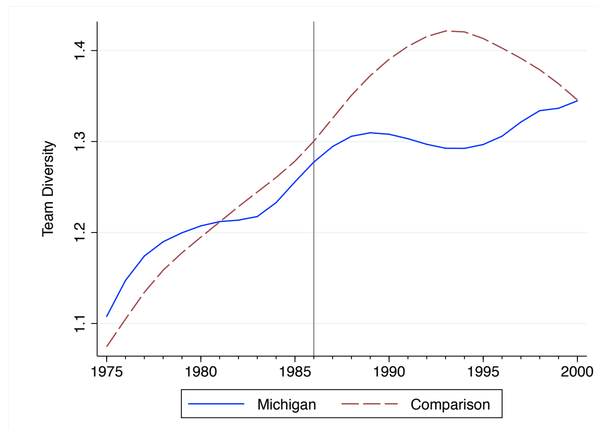
Figure 1: Descriptive trends for Michigan and comparison states, 1975 – 2000.

Figure 1 plots descriptive trends for firms that report R&D in Michigan (the treated state) in comparison to firms in a set of control states (AK CA CT MN MT NV ND OK WA WV). Panel A plots the within-industry average of *Tobin's q* in each year, weighted by firm asset size. Panel B plots the average number of unique USPTO technical classes in which each firm patents (counting only the primary classification for each patent). Panel C plots the average age of prior art, calculated as the average number of years for all backward citations made by a firm in a given year. Panel D plots the unique number of primary USPTO technical classifications previously pursued by inventors on each patent.

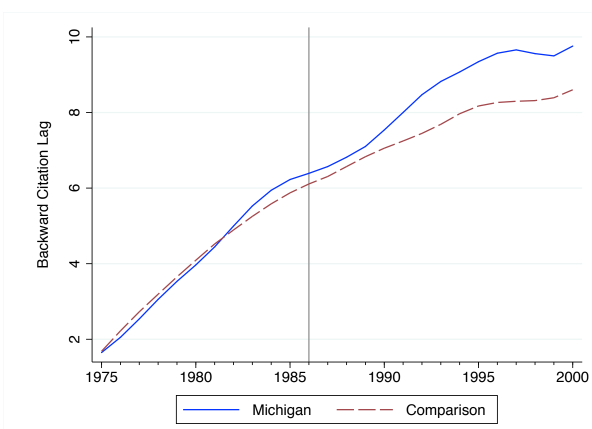
Panel A:
Average *Tobin's q*



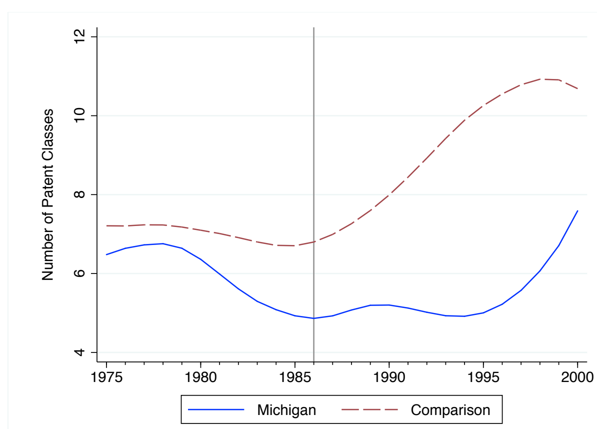
Panel B:
Average *Team Diversity*



Panel C:
Average *Backward Citation Lag*



Panel D:
Average number of *Patent Classes*



We now delve further into the value-capture and value-creation trends in the post-MARA period. We begin by establishing in a multivariate framework the rise in *Tobin's q* among Michigan firms in the short-to-medium run (10 years) following the policy reversal, subjecting the analysis to a battery of robustness and placebo tests. We then examine the longer-run mechanisms of myopia to explain how non-competes eventually work against value creation and reverse the initial rise in *Tobin's q*.

Increased Value Capture and the Rise in *Tobin's q*

The unanticipated change of non-compete policy in Michigan created an environment in which firms were able to enforce non-compete agreements against ex-employees. Consequently, firms were at less of a risk of trade secrets and other technical knowledge leaking to rivals, effectively lengthening the window over which information would remain proprietary. The present value of R&D should correspondingly rise in the short-to-medium term following MARA, as the effective depreciation rate for such assets fell. As is visible in Panel A of Figure 1, Table 2 demonstrates growth in *Tobin's q* for Michigan firms in the 10 years following MARA, both as an absolute gain and also in comparison to the control group (which does not shift substantially in the post-MARA period). *Tobin's q* rose by 0.55 points in Michigan, from a value of 1.25 before MARA 1.80 in after MARA while the trend in the control states was more modest, growing only from 1.61 to 1.66. We report a DD univariate statistic of 0.50, representing an 40.0% increase in *Tobin's q* over the pre-MARA Michigan average.

Table 2: Univariate Difference-in-Differences Analysis of *Tobin's q*

	Before	After	***Difference***
Michigan	1.25	1.80	0.55
Comparison	1.61	1.66	0.05
Difference	-0.36	0.14	**0.50**

Notes: Table 2 reports the average difference in *Tobin's q*, both before and after MARA, and between Michigan and comparison states. The descriptive "difference-in-differences" (DD) effect appears in the lower-right cell and is equal to the difference of the other two differences. The DD effect of 0.50 corresponds to the coefficient estimate of 0.4979 reported in Column 1 of Table 3 (the basic DD model with no year fixed effects, industry fixed effects, or matching). The sample used here is broader than the sample reported in Table 1 in that observations are not restricted by missing values or coarsened exact matching.

Table 3: Basic Effect of MARA on *Tobin's q*

	(1)	(2)	(3)	(4)	(5)
Treated State	-0.3550***	-0.0041	0.0271	0.0705	0.1499
	(0.0885)	(0.0814)	(0.0962)	(0.0442)	(0.1100)
After	0.0523	0.3135***	0.2751***	0.2120***	-0.0791*
	(0.0469)	(0.0354)	(0.0539)	(0.0387)	(0.0423)
Treated State * After	0.4979***	0.2494***	0.1653**	-0.0415	0.0735
	(0.1842)	(0.0626)	(0.0686)	(0.0490)	(0.2441)
Constant	1.6063***	0.1713***	0.1505***	0.1251***	0.3077***
	(0.0360)	(0.0192)	(0.0278)	(0.0194)	(0.0245)
Observations	6,070	6,070	3,457	5,600	4,429
R-squared	0.009	0.319	0.411	0.335	0.404
Treated State	Michigan	Michigan	Michigan	Texas	Delaware
Year of Treatment	1985	1985	1985	1983	1980
DV Logged *Tobin's q*	--	Yes	Yes	Yes	Yes
SIC-4 Indicators	--	Yes	Yes	Yes	Yes
Year Indicators	--	Yes	Yes	Yes	Yes
CEM Matching	--	--	Yes	Yes	--

Notes: Table 3 analyzes of the basic effect of policy reversals on *Tobin's q* for a +/− 10 year window before and after the policy change. The sample is generally the same as the sample reported in Table 1, except that it is not restricted by missing values and is therefore larger, and Columns 4 and 5 change the sample and treated state to Texas and Delaware. Column 1 uses the unlogged value of *Tobin's q* as the dependent variable to reconcile the 10 year difference-in-differences effect reported in Table 2 (0.50) with the estimate for *Michigan * After* (0.4979) reported in Column 1. Column 2, and all models thereafter, use the natural logarithm of *Tobin's q* as the dependent variable to control for heteroskedasticity and to better estimate the non-linear effects of intangible assets on market value. Column 2, and all models thereafter, also include industry fixed effects at the SIC-4 level and annual indicators for year fixed effects. Column 3, and all models thereafter, add coarsened exact matching (CEM) to stratify and match observations on the basis of *Assets*, *Beta*, and *Debt-to-Equity* to improve common support in the sample, on the basis of firm size, risk, and capital structure, between firms in the treated state and firms in comparison states. Columns 4 and 5 perform a placebo test on Texas and Delaware, which passed antitrust reform (but with no change to non-compete enforcement) in roughly the same era as Michigan. An insufficient number of firms headquartered in Delaware precluded creating a matched sample for Delaware. All models are estimated by OLS and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Moving to a regression framework, we first reconcile the univariate difference-in-differences results reported in Table 2 to a series of simple models that exclude any possibly endogenous factors. Limited to indicators for Michigan, the post-MARA period, and their interaction, these models are reported in Table 3. The DD effect of 0.50 in Table 2 is equivalent to the coefficient estimate of 0.4979 reported in Column 1 of Table 3, the basic model with no year fixed effects, no industry fixed effects, no matching, and an un-logged value of *Tobin's q* as the dependent variable. Having thus reconciled the start of the regression analysis, we then switch to the natural logarithm of *Tobin's q* in Column 2 and thereafter to better control for the non-linearity of intangible assets (Hirsch and Seaks 1993) and heteroskedasticity. Given a semi-log specification, we can now interpret coefficients in Column 2 (and subsequent models) as a percentage change in *Tobin's q*. Column 2 also includes year and industry fixed effects (at the SIC-4 level). In Column 3 we restrict the sample to those observations matched by CEM; it suggests that enforceable non-competes increased the market value of firms in Michigan during the 10-year window following MARA by 16.53%, with statistical significance at the 5% level.

Before proceeding to analysis with the full set of covariates, we wish to address a key concern with our methodology: that the anti-trust nature of the MARA reform may be conflated with the imposition of enforceable non-competes in producing our estimates. As noted earlier, MARA intentionally set in force antitrust reforms designed to reduce price-fixing and collusion, in addition to unintentionally repealing the ban on non-competes. While it might seem unlikely that tougher antitrust enforcement could raise the value of the publicly-traded firms that are the subject of this study, we nonetheless perform two placebo tests of anti-trust reforms in states other than Michigan. Folsom reports (1991: page 955, footnote 54) that Texas and Delaware also enacted major new antitrust legislation of a similar nature around the same time as MARA. To confirm that *Tobin's q* did not rise in Texas or Delaware after their antitrust reforms, we performed the following two placebo tests. Column 4 of Table 3 tests the DD effect on *Tobin's q* for Texas vs. comparison states after the Texas Free Enterprise and Antitrust Act of 1983, and we find an insignificant coefficient on the *Texas * After* variable in this model. A similar story is found in Delaware (Column 5 of Table 3), where we test the Delaware Antitrust Act of 1980.[16] These two placebo tests suggest that the antitrust aspects of MARA alone are unlikely to explain a boost in *Tobin's q* for Michigan firms in the early post-MARA period.

We now move, in Table 4, to an analysis using the full set of covariates described above in Table 1. While the comparison group serves as the basic control mechanism for any number of unspecified covariates in a diff-in-diff specification, the inclusion of specific covariates may help to control for a departure from the equal trends assumption (Blundell and Costa-Dias 2000). Across Table 4, we find that *Age* and *Dividend Policy* payouts have a negative and significant association with *Tobin's q*, while *Revenue Growth*, *Net Income*, and *Return on Assets* all have a positive and significant association with *Tobin's q*. All other control variables are insignificant.[17] After adding covariate controls, we find that the basic *Michigan * After* effect of MARA

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[16] We assign firms to states based on their location of corporate headquarters, and not their state of incorporation. While more than 50% of all public firms incorporate in the state of Delaware , only 0.23% of firms in our sample are actually headquartered in Delaware. The lack of Delaware firms prevented us from matching observations in the analysis tested in Column 5 of Table 3, and so this model does not use CEM.

[17] *Ex ante*, we included a broad range of covariates to control for potential differences in trends between Michigan and the comparison states that could also affect *Tobin's q*. Given that many of the control variables turned out to be insignificant, we conducted a sensitivity analysis by removing all insignificant control variables and re-estimating the model on the same sample; we found that removal of insignificant controls generally strengthened, or left unchanged, the magnitude and statistical significance of our predicted effects. We left all insignificant controls in the full model to avoid *ex post* fine tuning of the results.

increased the market value of firms in Michigan by 11.76%, with statistical significance at the 10% level. The statistical significance of the *Michigan * After* effect strengthens when we consider moderating factors in Column 2, where we add three-way interactions for *Michigan * After * R&D Intensity* and *Michigan * After * Patents* in what we label our "Full Model." In the Full Model, the basic *Michigan * After* effect of MARA is equivalent to a 25.88% increase in *Tobin's q* and statistically significant at the 5% level.

Columns 2-4 in Table 4 moreover enable us to explore the mechanisms behind the initial rise in *Tobin's q* following MARA. Our claim that non-competes increased firms' ability to capture the value from R&D is supported by three findings. First, the effect of MARA on *Tobin's q* is increasing in R&D spending, as confirmed by the large, positive, and significant coefficient on *Michigan * After * R&D Intensity* in Column 2. Second, and by contrast, we also find in Column 2 that the effect of non-competes on *Tobin's q* is attenuated slightly for firms that protected their intellectual property more aggressively using patents. Third, Columns 3 & 4 show that the benefit of non-competes is largely realized among firms in industries where R&D managers report that secrecy is of greater importance (Cohen et al. 2000). This step of the analysis is accomplished with a split-sample analysis of firms in industries reporting above- vs. below-the-median importance of secrecy.[18] No effect is obtained for firms in industries reporting below-median importance for secrecy (Column 3), but the effect for the above-median group (Column 4) is similar in size and significance to the result in Column 2.

[18] Secrecy and patenting clearly interact. While we could interact Michigan, post-MARA, patent counts, and secrecy, interpreting four-way interactions is complex. Instead, we graphically represent these differences in the different subpanels of Figure 2.

Table 4: Moderated Effect of MARA on *Tobin's q*

	(1) No Moderators	(2) Full Model	(3) Low Secrecy	(4) High Secrecy
State Personal Income (log)	0.0186	0.0174	0.0138	0.0065
	(0.0197)	(0.0190)	(0.0292)	(0.0308)
State Employment Change	-0.2933	-0.0197	3.0500**	-1.8210*
	(0.8460)	(0.8335)	(1.1725)	(0.9648)
Assets (log)	0.0061	0.0022	0.0100	0.0163
	(0.0224)	(0.0233)	(0.0290)	(0.0338)
Employees	-0.0029	-0.0022	0.0007	-0.0050
	(0.0029)	(0.0029)	(0.0036)	(0.0046)
SG&A Expense	-0.0000	-0.0000	-0.0004*	-0.0000
	(0.0001)	(0.0001)	(0.0002)	(0.0001)
Age	-0.0051**	-0.0050***	-0.0074***	-0.0032
	(0.0020)	(0.0019)	(0.0023)	(0.0027)
Net Income	0.0013***	0.0013***	0.0006**	0.0014***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Return on Assets	0.6126***	0.7060***	0.7878***	0.7316**
	(0.1862)	(0.1859)	(0.2220)	(0.2916)
Revenue Growth	0.3258***	0.3069***	0.3824***	0.2701***
	(0.0522)	(0.0564)	(0.0829)	(0.0659)
Capital Expenditure Intensity	0.4207	0.3614	-0.2532	0.8537**
	(0.3148)	(0.3128)	(0.3614)	(0.3730)
Leverage	0.0864	0.1009	0.3370**	-0.0998
	(0.1053)	(0.1041)	(0.1323)	(0.1572)
Liquidity	-0.1096	-0.1318	-0.2461*	0.0361
	(0.1530)	(0.1516)	(0.1283)	(0.1662)
Beta	0.0064	0.0081	0.0194***	0.0099
	(0.0069)	(0.0071)	(0.0074)	(0.0095)
Dividend Policy	-0.0004*	-0.0004*	0.4712	-0.0007**
	(0.0002)	(0.0002)	(0.9351)	(0.0003)
Diversified Operations	0.0301	0.0396	-0.0290	0.0588
	(0.0636)	(0.0616)	(0.0679)	(0.0833)
Business Combination Law	0.1266	0.1152	0.1487	0.1901*
	(0.0861)	(0.0844)	(0.0920)	(0.1050)
Patenting Indicator	0.0223	0.0206	0.0603	-0.0045
	(0.0237)	(0.0238)	(0.0378)	(0.0313)
R&D Intensity (RDI)	0.4210	1.6080***	2.4916***	0.8959*
	(0.2726)	(0.4389)	(0.6950)	(0.4764)
Patents	0.0008	0.0006	0.0014	0.0006
	(0.0009)	(0.0010)	(0.0014)	(0.0013)
Michigan	0.0729	0.0565	0.5797***	0.0986
	(0.0773)	(0.0860)	(0.0975)	(0.0701)
After	0.1886*	0.2006**	0.3277**	0.1443
	(0.1032)	(0.1010)	(0.1493)	(0.1319)
MI * After	0.1176*	0.2588**	-0.1749	0.2528**
	(0.0645)	(0.1262)	(0.2750)	(0.1231)
Michigan * RDI		-0.3703	10.1617***	-0.1708
		(1.4584)	(2.1147)	(0.8551)
After * RDI		-1.3318***	-1.6981**	-0.6631
		(0.4329)	(0.7917)	(0.4624)
Michigan * After * RDI		7.2116**	-1.2097	8.1355***
		(3.5340)	(1.9524)	(3.0398)
Michigan * Patents		-0.0006	0.0106	-0.0003
		(0.0011)	(0.0107)	(0.0016)
After * Patents		0.0023*	0.0047	0.0005
		(0.0012)	(0.0029)	(0.0014)
Michigan * After * Patents		-0.0055***	-0.0326	-0.0046*
		(0.0019)	(0.0381)	(0.0024)
Constant	-0.3267	-0.2882	-0.3768	-0.1494
	(0.3810)	(0.3664)	(0.5408)	(0.5676)
Observations	3,028	3,028	1,196	1,504
R-squared	0.515	0.528	0.512	0.632

Notes: Table 4 analyzes the moderated effect of MARA on *Tobin's q* for $-/+$ 10 years before/after MARA. The sample and measures are reported in Table 1, the dependent variable is the natural log of *Tobin's q*, and all models hold *R&D Intensity* and *Patents* constant after MARA. Column 1 adds covariate controls to the basic DD specification. Column 2 adds interaction terms for *R&D Intensity* and *Patents* and is our Full Model. Column 3 estimates the same specification as the Full Model based on a bottom subsample split at the median of *Secrecy*; Column 4 estimates the top subsample split at the median of *Secrecy*. All models are estimated by OLS, include year and industry fixed effects, use a CEM matched sample, and cluster robust standard errors by firm (S.E. in parentheses). ***$p<0.01$, **$p<0.05$, *$p<0.10$.

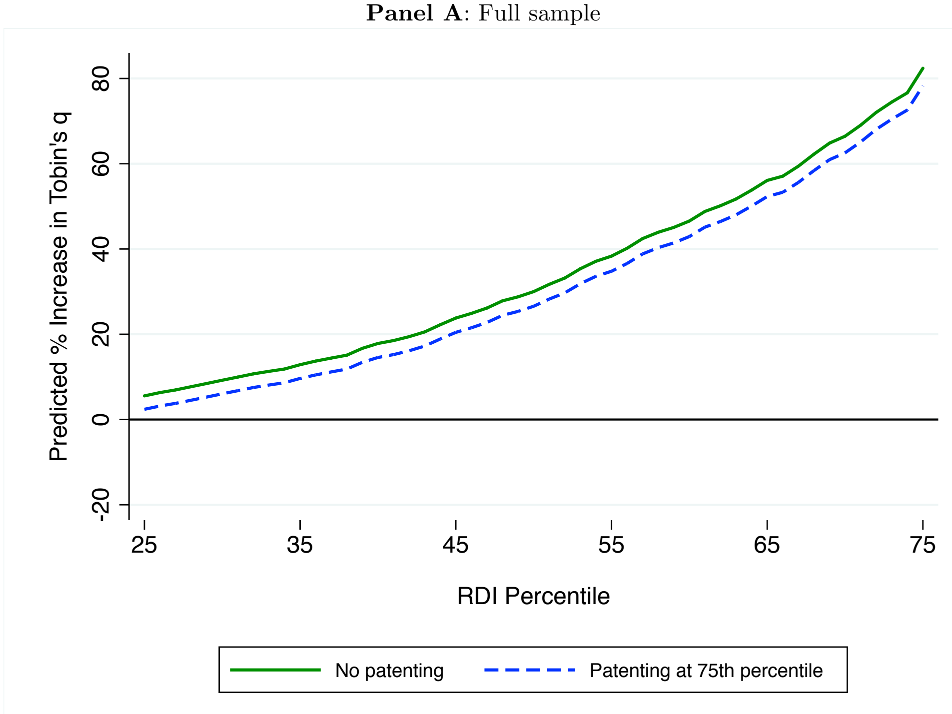
To illustrate these findings, Figure 2 plots the predicted difference-in-differences percentage increase in *Tobin's q* (y-axis) across a range of *R&D Intensity* percentiles (x-axis). Predictions are based on the Full Model (Column 2 of Table 4), except that we estimate year fixed-effects with a complete set of orthogonal polynomial contrast codes, allowing us to predict an average effect across the year fixed effects (Davis 2010). The solid (green) line predicts the percentage increase in *Tobin's q* for firms that do not patent. The dashed (blue) line predicts the percentage increase in *Tobin's q* for firms that patent at the 75th percentile of all firms. Panel A illustrates that, although patenting has a statistically significant offsetting effect to the basic effect of MARA, patenting may be of relatively minor importance to firms as an anti-diffusion mechanism for the protection of knowledge. For those firms that invest into high levels of R&D, patenting provides very little protection in relation to the benefit received from restricting the mobility of employees in the first place. By extension, the evidence suggests that other anti-diffusion protections provided by MARA, namely better secrecy brought about by the restriction of knowledge flows embodied in departing employees, may be of greater importance than patenting to many firms. This especially appears to be true for firms with large, dedicated R&D programs where innovation represents a major source of growth options and future cash flows.

The remaining panels of Figure 2 explore the degree to which the rise in *Tobin's q* following Michigan's adoption of enforceable non-competes depends on the importance of secrecy. We repeat the split-sample analysis of Columns 3 & 4 in Table 4 and show the predicted difference-in-difference effect for firms in industries where R&D managers report secrecy to be

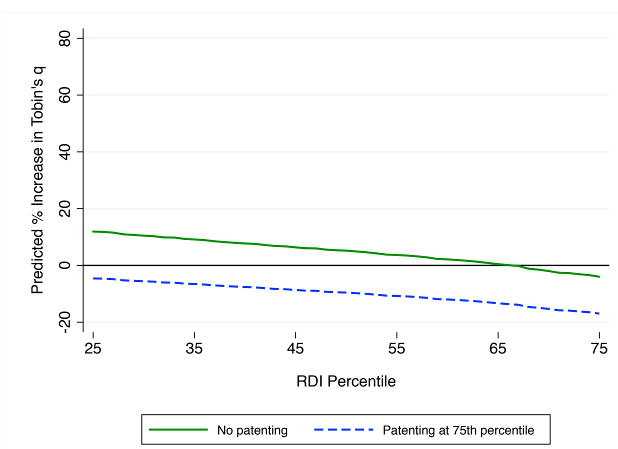
Figure 2: Predicted difference-in-differences percentage increase in *Tobin's q*.

Figure 2 graphs the predicted difference-in-differences increase (as a percentage change) in the level of *Tobin's q* by percentile of *R&D Intensity* (along the X axis). The solid (green) line predicts the percentage increase in *Tobin's q* for firms that do not patent. The dashed (blue) line predicts the percentage increase in *Tobin's q* for firms that patent at the 75th percentile of all patenting firms. Panel A is predicted based on estimated coefficients from the Full Model and the full sample (Column 2 of Table 4). Panel B is predicted from a subsample of firms in the bottom half of a median-split on *Secrecy* (Column 3 of Table 4). Panel C is predicted from a subsample of firms in the top half of a median-split on *Secrecy* (Column 4 of Table 4).

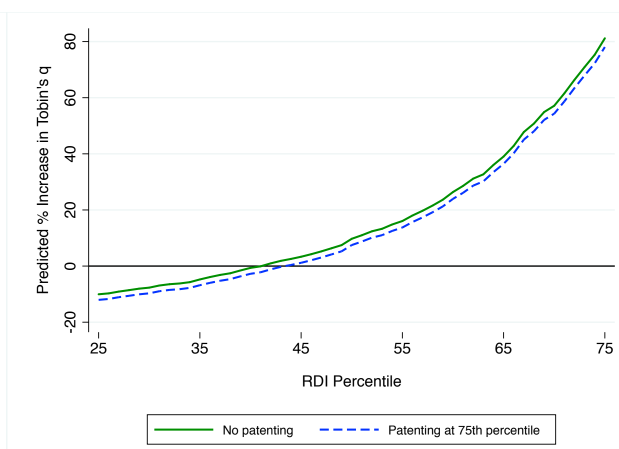
Panel A: Full sample



Panel B: Subsample of firms where secrecy is of **low** importance



Panel C: Subsample of firms where secrecy is of **high** importance



less important in Panel B, and the converse in Panel C. Consistent with our regression analysis, the resemblance between Panels A and C indicates that enforceable non-competes boost *Tobin's q* in industries where secrecy is deemed by managers to be more important. Of particular interest however are the results for industries where secrecy is less prized. We first note that in Panel B (when secrecy is less important), the gap between firms that patent and those that do not is wider than in the other two panels. Moreover, the impact of non-competes on *Tobin's q* does not increase as R&D intensity increases; instead, it may be weakly decreasing. This suggests that where secrecy is less important, firms derive less advantage from non-competes and that the negative, off-setting effects of non-competes may be increasing with R&D intensity. Overall, the evidence in Figure 2 suggests that, for a given level of R&D intensity, patenting reduces the benefits that firms receive from non-competes, while simultaneously exposing firms to more of the downside that firms face when non-competes are not needed, such as in industries where secrecy is of low importance. We explore these implications later in the paper.

Robustness. We test the robustness of our results in Table 5. First, we check the sensitivity of the difference-in-differences comparison by selecting other states for the comparison group. After first excluding Michigan, Column 1 uses states rated by Garmaise (2011) to have a low enforcement level for non-competes,[19] Column 2 uses all US. states, and Column 3 uses all U.S. states except for California, for the control group. We find that these

[19] Garmaise (2011) develops an alternative index of the enforcement level of non-competes that ranges from 0 to 9. Column 1 tests our DD comparison by selecting firms from the bottom third of states ranked by this index for the control (i.e., states ranked with a value of 0 to 3: AK AZ CA CO CT HI MT NH NM NY ND OK RI VA WV WI).

alternative DD comparisons produce slightly smaller magnitudes for the main effect of MARA (20.88%, 21.60%, and 18.14%, respectively, as compared to 25.88% in our preferred specification) and that they retain their statistical significance. Moreover, the magnitude and significance of the moderating effects remain essentially the same. The exclusion of California from the control group in Column 3 confirms that our results are not driven by the size or unique circumstances of California.

Column 4 of Table 5 defines *R&D Intensity* as equal to R&D expenses divided by total assets (instead of sales), resulting in a main effect of MARA that is smaller but similar in statistical significance, while the moderating effect of R&D weakens somewhat and the moderating effect of patenting remains similar. Column 5 replaces the *Patents* variable with a measure of forward citations and finds similar effects. (Replacing the count of patents with a five-year accumulated stock depreciated 15% per year also yields similar results.) Column 6 defines a new indicator variable (*Auto*) for the automobile industry and then adds *Michigan * Auto*, *After * Auto*, and *Michigan * After * Auto* to the Full Model (the main effect of *Auto* is omitted given industry fixed effects). Results for the additional interactions for *Auto* were statistically insignificant, whereas results for all of the other effects were similar to results from our Full Model. In Column 7 we drop all firms in the automobile industry. Doing so drops 16% of the sample and weakens the magnitude of the moderating effect of *R&D Intensity*, dropping the effect below statistical significance; the statistical significance of the main *Michigan * After* effect drops to the 10% level, and other results are generally consistent with our findings from the Full Model.

Table 5: Robustness Checks

	(1) *Controls = Low States*	(2) *Controls = All States*	(3) *Controls = No CA*	(4) *R&D/ Assets*	(5) *Patent Citations*	(6) *Auto Industry*	(7) *Drop Auto Industry*	(8) *Full Population*
R&D Intensity	2.1567***	2.2612***	2.4505***	1.9259***	1.6441***	1.6051***	0.3347**	0.3953***
	(0.4353)	(0.2553)	(0.3431)	(0.4529)	(0.4399)	(0.4389)	(0.1318)	(0.1274)
Patents	0.0008	-0.0009*	-0.0011**	0.0007	0.0000	0.0007	-0.0003	-0.0002
	(0.0010)	(0.0005)	(0.0006)	(0.0010)	(0.0001)	(0.0010)	(0.0012)	(0.0004)
Michigan (MI)	0.0657	0.0898	0.0947	0.0593	0.0604	0.0654	0.1871*	0.1157*
	(0.0614)	(0.0713)	(0.0632)	(0.0840)	(0.0871)	(0.0917)	(0.1062)	(0.0628)
After	0.3179***	0.3169***	0.3184***	0.2052**	0.2033**	0.1982**	0.3699***	0.3468***
	(0.0659)	(0.0385)	(0.0428)	(0.1022)	(0.1008)	(0.1002)	(0.1050)	(0.0557)
MI * After	0.2088**	0.2160**	0.1814**	0.1643**	0.2513**	0.2646**	0.2952*	0.2356*
	(0.1023)	(0.1022)	(0.0851)	(0.0832)	(0.1256)	(0.1277)	(0.1671)	(0.1306)
MI * RDI	0.0923	2.5638	2.8834	-0.8396	-0.3731	-0.3194	1.0980	2.4289**
	(1.4842)	(2.2992)	(2.3341)	(1.7527)	(1.4781)	(1.4914)	(1.4908)	(1.0828)
After * RDI	-1.7798***	-1.6213***	-1.8536***	-1.4524***	-1.4095***	-1.3261***	-0.1156	-0.2194
	(0.4815)	(0.3188)	(0.4456)	(0.5456)	(0.4274)	(0.4346)	(0.1348)	(0.1464)
MI*After*RDI	8.5437**	7.3544**	7.3925**	4.5791*	7.3873**	8.6370**	5.2450	3.8595
	(3.8177)	(3.6085)	(3.5352)	(2.3475)	(3.5519)	(4.1393)	(4.2839)	(2.7259)
MI*Patents	0.0019**	0.0014**	0.0015**	-0.0002	-0.0000	-0.0004	-0.0023	0.0002
	(0.0009)	(0.0006)	(0.0006)	(0.0012)	(0.0001)	(0.0011)	(0.0045)	(0.0004)
After*Patents	0.0025**	0.0015***	0.0016***	0.0024**	0.0002**	0.0022*	0.0053**	-0.0006*
	(0.0012)	(0.0005)	(0.0005)	(0.0012)	(0.0001)	(0.0012)	(0.0025)	(0.0003)
MI*After*Pat	-0.0053***	-0.0062***	-0.0066***	-0.0061***	-0.0005***	-0.0054***	-0.0068*	-0.0035***
	(0.0020)	(0.0015)	(0.0016)	(0.0019)	(0.0002)	(0.0020)	(0.0035)	(0.0013)
MI * Auto						-0.0635		
						(0.1492)		
After * Auto						0.0512		
						(0.0787)		
MI*After*Auto						0.1273		
						(0.1476)		
Constant	0.3496	0.0692	0.3366	-0.2848	-0.2728	-0.2920	-0.7726*	-0.6441***
	(0.3300)	(0.2024)	(0.2557)	(0.3736)	(0.3663)	(0.3633)	(0.4061)	(0.2195)
Observations	4,668	13,775	11,714	3,028	3,028	3,028	2,554	9,208
R-squared	0.488	0.396	0.420	0.526	0.528	0.529	0.522	0.429

Notes: Table 5 checks the robustness of the Full Model (Column 2 of Table 4). The dependent variable is logged *Tobin's q*, all models include the full set of covariate controls included in Table 4, and *R&D Intensity* and *Patents* are held constant after MARA. Column 1 compares Michigan to 16 states with a generally low level of non-compete enforcement (i.e., AK AZ CA CO CT HI MT NH NM NY ND OK RI VA WV WI). Column 2 compares Michigan to all other U.S. states. Column 3 compares Michigan to all other U.S. states with the exclusion of California. Column 4 scales *R&D* expenses by total assets instead of sales. Column 5 replaces *Patents* with forward patent citations. Column 6 defines a new indicator variable (*Auto*) for the automobile industry and interacts it with *Michigan* and *After* to separately estimate the DD effect of MARA for the auto industry (the main effect for *Auto* is omitted due to industry fixed effects). Column 7 drops all firms in the auto industry. Column 8 expands the sample to include firms that do not report R&D expense by assuming a value of zero for missing data on R&D expenses. Models are estimated by OLS, have year and industry fixed effects, use a CEM matched sample, and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

To test the sensitivity of our analysis to R&D firms, in Column 8 of Table 5 we expand the sample to include firms that do not report R&D expenses in Compustat by assuming zero R&D expenses for missing data rather than dropping observations from the sample (the sample size increases from n=3,028 to n=9,208). This approach, while imputing a zero value for R&D expenses for most observations, allows us to test our predicted effects across a broader population. We find that the main effect of MARA is somewhat less well identified, suggesting that non-competes have greater impact on firms conducting R&D. The moderating effect of *Patents* are similar to the better identified R&D population, although the moderating effect of *R&D Intensity* is no longer statistically significant (perhaps due to the added imprecision in the measurement of R&D).

Finally, we check for a spurious "Midwest effect" by running placebo regressions where we pretend that the MARA reform took place in states near to Michigan (Ohio, Indiana, Illinois, Wisconsin, and Pennsylvania). In Columns 1 – 5 of Table 6, none of the coefficients on *Placebo State * After* or the triple-diff interactions were statistically significant, consistent with the view that our results relate to the passage of MARA in Michigan and not to regional developments shared by neighboring states. In conclusion, we see little evidence that our findings depend upon the specific comparison made in the difference-in-differences analysis or that effects in Michigan were part of a broader Midwest pattern. Results for the moderators were robust to alternative measures, and we found no indication that the generalizability of our findings is limited to dominant industries in Michigan.

Table 6: Placebo Tests

	(1) *Ohio*	(2) *Indiana*	(3) *Illinois*	(4) *Wisconsin*	(5) *Pennsylvania*
R&D Intensity (RDI)	2.0227***	1.7213***	2.7369***	1.8709***	1.9477***
	(0.3433)	(0.5932)	(0.3913)	(0.4788)	(0.3394)
Patents	-0.0011	-0.0009	0.0002	0.0016	-0.0001
	(0.0007)	(0.0007)	(0.0003)	(0.0010)	(0.0004)
Placebo State	0.0081	0.1291*	-0.0150	0.0690	-0.0270
	(0.0469)	(0.0665)	(0.0429)	(0.0492)	(0.0373)
After	0.3786***	0.1442	0.2931***	0.3450***	0.3571***
	(0.0837)	(0.1472)	(0.0784)	(0.1076)	(0.0730)
Placebo State * After	0.0514	0.0244	0.0079	0.0831	0.0092
	(0.0590)	(0.0950)	(0.0427)	(0.1134)	(0.0461)
Placebo State * RDI	-0.1322	0.4756	0.7559	0.1639	0.8427
	(1.3238)	(1.6265)	(1.5286)	(1.1391)	(0.8858)
After * RDI	-1.9340***	-1.4549**	-2.4302***	-1.5626***	-1.7955***
	(0.3404)	(0.5800)	(0.3874)	(0.5303)	(0.3274)
Placebo State * After * RDI	-1.2024	-1.0187	0.5100	0.3879	-0.6117
	(1.8840)	(3.3851)	(1.5364)	(2.5451)	(0.8248)
Placebo State * Patents	0.0011	-0.0074*	-0.0003	-0.0086*	0.0008
	(0.0009)	(0.0043)	(0.0004)	(0.0047)	(0.0006)
After * Patents	0.0000	0.0057*	0.0002	0.0002	0.0003
	(0.0008)	(0.0031)	(0.0002)	(0.0007)	(0.0003)
Placebo State * After * Patents	0.0006	0.0123	0.0001	0.0181	0.0004
	(0.0011)	(0.0097)	(0.0003)	(0.0150)	(0.0006)
Constant	-0.3440	-1.0655**	-0.4260	0.5051	-0.4324
	(0.3627)	(0.5239)	(0.3033)	(0.3728)	(0.3419)
Observations	4,237	1,753	5,220	2,867	4,468
R-squared	0.554	0.637	0.505	0.619	0.492

Notes: The following table tests neighboring Midwest states at the time of MARA that did not change their non-compete enforcement level. Each sample follows the same criteria as reported in Table 1, except that the indicated placebo state is exchanged with Michigan as the "treated state." Given that these Midwest states did not receive treatment, results are expected to be not significant for all of our predicted effects (*MI * After*, *MI * After * RDI*, and *MI * After * Patents*). All models include the full set of covariate controls included in Table 4, the dependent variable is the natural log of *Tobin's q*, and *R&D* and *Patents* are held constant after MARA. All models are estimated by OLS, include year and industry fixed effects (except Column 4), use a CEM matched sample, and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Decreased Value Creation and the Reversal in *Tobin's q*

Having established an initial rise in *Tobin's q* among Michigan firms, we now turn our attention to what appears to be a reversal of this trend in the later post-MARA period. Again, Panel A of Figure 1 shows that most of the gains in *Tobin's q* among Michigan firms were obtained in the first several years following MARA. By the early 1990s, Michigan firms' *Tobin's*

q had reached a plateau and then dropped in the second half of the 1990s while firms in the control states continued to experience steady growth. Our general argument to explain this reversal is that, *ceteris paribus*, Michigan firms become more myopic in their R&D activities after gaining the ability to enforce non-compete agreements. Prior research has linked myopia to poor long-term financial performance (e.g., Stein 1989; Chemmanur and Ravid 1999; Edmans 2009), hence, we expect myopia to lower firm value, especially for a forward-looking measure of value such as *Tobin's q*.

We propose that enforceable non-competes affect the focus of R&D through two mechanisms related to R&D employees. (Importantly, our argument does not presume deliberate shifts in the firm R&D strategies.[20]) First, non-competes restrict the ability of firms to acquire new employees needed to explore new areas of research. Perhaps the most robust finding in the literature on non-compete agreements is that non-competes act as a brake on the mobility of technical professionals (Fallick, Fleischman et al. 2006; Marx, Strumsky et al. 2009; Garmaise 2011). At the same time that firms enjoy the ability to retain key employees, the use of non-competes by competitors makes it more difficult to attract new talent with skill sets unavailable at the focal firm. Coincident with the limited circulation of talent is the reduced diffusion of ideas. Beleznon and Schankerman (2012) exploit the same Michigan experiment used in this paper to show that non-compete agreements indeed act as a brake on the diffusion of knowledge.

[20] Although it is possible that publicly-traded firms (i.e., the type of firms in our sample) might have reacted to earnings pressure associated with the early rise in *Tobin's q* (documented earlier) and directed R&D efforts toward the exploitation of short-term opportunities at the core of the firm's R&D program in order to generate current earnings, it nevertheless would be irrational for owners (or well-incentivized managers) to intentionally sacrifice long-term interests in order to boost current profits. We therefore expect myopia in R&D to arise from indirect changes in regional labor markets and indirect changes in employee incentives, instead of from deliberate actions of the firm.

Hence, even if the firm wishes to engage in exploratory R&D, its ability to do so may be limited by the inaccessibility of extraorganizational resources due to the enforceability of non-compete agreements.

Second, non-competes restrict the willingness of existing employees to engage in higher-risk projects. The barrier to interorganizational mobility engendered by non-competes introduces strong incentives for workers to remain employed by their current firm. Changing jobs may mean changing industries as well to avoid a potential lawsuit from their former employer (Marx 2011). Riskier R&D projects that do not succeed can result in a research team being disbanded and research employees being let go from the firm;[21] workers may therefore choose to increase their job security by pursuing research projects that are closer to their firm's core business and/or projects that have a higher likelihood of a near-term payoff. Along these lines, Hirshleifer and Thakor (1992) show that incentives to build or protect reputations can distort firm's investment decisions in favor of relatively safe, myopic projects.

Taken together, these two implications of non-competes suggest that firms' efforts to engage in long-term, exploratory R&D may be frustrated both by the inability to attract new employees (and their novel ideas) as well as the unwillingness of existing employees to engage in risky projects. To evaluate the effects of non-competes on R&D activities, we limited the sample to firms that file patents (approximately half of firms that report R&D) where we are able to develop direct measures of myopia. We also extended the sample to cover a longer timeframe, from 1975 to 2000, and split the post-MARA time period into two sub-periods (*After1* and

[21] Non-competes typically apply regardless of the reason for separation, whether voluntary or involuntary.

After2), each of seven years (1987–1993 and 1994–2000), in order to examine both short-term and longer-term effects. We begin the analysis by revisiting our earlier results on *Tobin's q*, and then examine three indicators of R&D myopia: team diversity, the time-lag of backward citations, and the number of patent classes pursued by a firm in a given year. We plot descriptive trends in Figure 1 and report multivariate results in Table 7.

Column 1 of Table 7 repeats our earlier analysis of *Tobin's q* and finds that the early effect of MARA (i.e., the DD effect of *Michigan * After1*) increased the market value of firms in Michigan by 15.98% from before MARA to the period 1987–1993, with statistical significance at the 5% level. For the latter post-MARA period (1994-2000), however, we find a near-zero and insignificant result for *Michigan * After2* relative to the omitted pre-MARA period. These coefficients indicate that while *Tobin's q* enjoyed a temporary boost in the early post-MARA period, later in the period there was no discernible difference from the pre-MARA period. The reversal in *Tobin's q* between the two *After* periods confirms what we observe in Panel A of Figure 1, where *Tobin's q* rises sharply in the late 1980s and early 1990s, but then levels off in the mid-1990s and drops precipitously in the late 1990s. In the remainder of Table 7, we explore how the reversal in *Tobin's q* may be related to myopia in R&D activities.

First, we investigate whether non-competes affected the technical diversity of the research teams used in R&D, under the assumption that breakthroughs are generally achieved via recombination of multiple technologies (March 1991; Fleming 2001). Non-competes may have resulted in more homogeneous inventor teams both because existing employees were reluctant to

Table 7: Evidence of Myopia in Innovation Post-MARA, 1975 – 2000

	(1) *Tobin's q*	(2) *Team Diversity*	(3) *Backward Citation Lag*	(4) *Number of Patent Classes*
State Personal Income (log)	0.0067	0.0028	0.1270***	-0.0399
	(0.0207)	(0.0174)	(0.0226)	(0.0671)
State Employment Change	0.7016	0.6414	-1.4040***	-1.7203
	(0.6525)	(0.8790)	(0.5032)	(1.1654)
Assets (log)	0.0520**	0.0360**	0.0475***	0.5386***
	(0.0226)	(0.0156)	(0.0177)	(0.0489)
Employees	-0.0015	-0.0003	-0.0083***	0.0144***
	(0.0022)	(0.0016)	(0.0018)	(0.0028)
SG&A Expense	0.0000	0.0000	0.0001*	-0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Age	-0.0049*	-0.0006	0.0086***	-0.0015
	(0.0027)	(0.0016)	(0.0030)	(0.0048)
Net Income	0.0003*	-0.0001*	-0.0003	-0.0007***
	(0.0002)	(0.0001)	(0.0002)	(0.0001)
Return on Assets	1.6076***	0.2074*	0.1296	0.4773*
	(0.2700)	(0.1128)	(0.1246)	(0.2676)
Revenue Growth	0.1841***	-0.0378	-0.0347	-0.0414
	(0.0400)	(0.0272)	(0.0379)	(0.0660)
Capital Expenditure Intensity	0.5080*	-0.0263	0.2979	-0.7578
	(0.2995)	(0.2389)	(0.2503)	(1.0635)
Leverage	0.0278	0.0400	-0.2936***	-0.3614
	(0.1077)	(0.0918)	(0.0942)	(0.2893)
Liquidity	0.0535	0.0577	-0.1848	-0.0875
	(0.1421)	(0.0927)	(0.1256)	(0.1927)
Beta	0.0203**	0.0129	-0.0217**	-0.0368**
	(0.0087)	(0.0084)	(0.0091)	(0.0154)
Dividend Policy	-0.0004*	-0.0015***	-0.0011***	-0.0009
	(0.0002)	(0.0003)	(0.0002)	(0.0007)
Diversified Operations	-0.0031	-0.0622	0.7486***	-0.0135
	(0.0587)	(0.0528)	(0.0662)	(0.1613)
Business Combination Law	0.1577*	0.1538*	0.0371	0.1644
	(0.0935)	(0.0832)	(0.0309)	(0.1205)
R&D	0.7872**	-0.0002	0.0001	-0.0006
	(0.3371)	(0.0002)	(0.0002)	(0.0006)
Patents	-0.0005	0.0007**	-0.0007**	0.0035***
	(0.0006)	(0.0003)	(0.0003)	(0.0010)
Michigan	0.1349*	-0.0062	-0.0731	0.1544
	(0.0812)	(0.0512)	(0.0651)	(0.1998)
After1 (1987-1993)	0.0497	0.1816	0.3004***	0.0000
	(0.1293)	(0.1161)	(0.0322)	(0.0000)
After2 (1994-2000)	0.2850**	0.1071	0.3702***	-0.7043**
	(0.1376)	(0.1096)	(0.0508)	(0.3194)
MI * After1 (1987-1993)	0.1598**	-0.1666**	0.1305**	-0.0545
	(0.0786)	(0.0690)	(0.0595)	(0.1550)
MI * After2 (1994-2000)	0.0040	-0.1098**	0.1375**	-0.5097***
	(0.0826)	(0.0514)	(0.0643)	(0.1886)
Constant	-0.4769	0.8307***	-1.7267***	-1.1297
	(0.3839)	(0.3115)	(0.4475)	(1.2549)
Observations	2,434	2,447	2,338	2,469
R-squared or [Log-likelihood]	0.623	0.256	0.676	[-5,256.71]
Model	OLS	OLS	OLS	Poisson
DV Transform	Logged	None	Logged	None

Notes: Table 7 tests for evidence of myopia in patenting activity. The sample is restricted to firms that filed patents (approximately half of the original population) and extended to include the years 1975 through 2000 (the first year for which we could obtain patent data, through the last year before the market crash of 2001). Column 1 re-estimates our finding from Column 1 of Table 4 using the new sample and two post-MARA periods, using the same OLS specification as before. Column 2 examines team diversity, calculated as the average number of different primary patenting areas of all team members, using an OLS specification. Column 3 regresses an OLS specification of the average time lag of works cited (in years) by the focal patent. Finally, Column 4 estimates, by quasi-maximum-likelihood (QMLE), a Poisson specification for the number of patent classes in which the firm patented. In Column 1, R&D represents R&D Intensity; in Columns 2-4, R&D represents R&D Expense. All models include year and industry fixed effects, use a CEM matched sample, and cluster robust standard errors by firm (S.E. in parentheses). *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

form such project teams and because firms may have struggled to procure complementary skills from the external labor market. To measure the technical diversity of patenting teams, we trace the prior patent history of all inventors on a focal patent and note the primary USPTO technical classification in which each inventor worked most frequently until the year before that patent was filed. We then take as our measure of diversity the unique number of primary USPTO technical classifications identified among that inventor team. (Patents with a single inventor have a value of one, by definition.) Panel D of Figure 1 indicates that R&D teams in Michigan after MARA became considerably less diverse than in the control states. The same is evidenced in the multivariate analysis in Column 2 of Table 7, where we find a large drop in team diversity for both of the post-MARA time periods; the DD effects for *Michigan * After1* and *Michigan * After2* are both significant at the 5% level. We obtain similar results when substituting a Herfindahl index for the measure of primary USPTO technical classifications described earlier.

Next, we investigate the age of prior art cited, expecting that myopic firms will rely on older sources of knowledge, whereas more exploration-oriented firms will reference more recent discoveries. This result could obtain because the more homogeneous inventor teams described in Column 2 may continue to rely on known technologies; moreover, risk-averse employees may be less eager to learn about new and unproven approaches. Panel C of Figure 1 indeed indicates that while Michigan and the comparison states were on a relatively similar trend with regard to the age of prior art in the pre-MARA period, over time the trends diverged. Although the onset is not immediately visible in the descriptive graph, by the early 1990s it becomes clear that

Michigan firms are citing older prior art. Multivariate results in Column 3 of Table 7 suggest that, after accounting for controls, firms increased the backward citation time lag relative to before MARA by 13.05% even in the first post-MARA period, and increased the backward citation time lag by 13.75% in the second post-MARA period, both significant at the 5% level.

Finally, we assess the breadth of the firm's explorative R&D efforts by counting the number of unique USPTO technical classes (the primary classification for each patent) in which the firm patents in a given year. A narrowing of technical focus may result from the lack of diversity in inventor teams, from the use of older prior art, and from risk aversion on the part of employees. In Panel B of Figure 1, we observe a growing gap between the control states and Michigan in terms of the number of classes in which firms patent. This trend is also evident in Column 4 of Table 7. While the effect of (*Michigan * After1*) is small and insignificant during the first post-MARA period, we find evidence in the second post-MARA period (*Michigan * After2)* that Michigan firms eventually become more focused on a smaller set of R&D investments, as compared with firms in the control states. This effect is significant at the 1% level. Note that we do not argue that myopia itself necessarily took several years to set in, but that the cumulative effect of relying on less diverse teams and older prior art eventually constrained the focus of exploration and resulted in the degradation of firms' value creation efforts, and that this may have then eventually depressed *Tobin's q*.

In sum, the evidence regarding patenting behavior among public firms suggests that non-competes may engender myopia in R&D activities. Again, we do not assume this is intentional on the part of firms or firm owners, which would imply irrationality, but rather suggest that researchers' preference for less exploratory projects, coupled with the limited circulation of talent and ideas among firms, channel the firm toward short-term exploitation.

5. Conclusion

This article examines how non-patent methods of protecting proprietary information affect firm value. Specifically, we test the impact of enforceable employee non-compete agreements on *Tobin's q* for public firms that report their level of R&D investment using a difference-in-differences approach derived from an inadvertent reversal of non-compete policy in Michigan. Michigan firms enjoyed a 25% boost in *Tobin's q* shortly following the imposition of non-compete enforceability in Michigan, as compared with a matched set of firms in states that continued to restrict the use of non-competes, an effect that is increasing in R&D investment, (weakly) decreasing in patenting, and stronger in sectors where secrecy is more important for protecting knowledge and appropriating rents. The effects remain significant under alternative specifications and are not obtained in a battery of placebo regressions. However, the effect does not appear to be cumulative but rather has a strong initial onset following the reform and then attenuates over time. When examining the trend in a longer window from 1975-2000, we observe that the upward trend in *Tobin's q* reverses in the latter portion of the window.

These results are consistent with the view that non-competes have a dual impact on firm value. The ability to prevent the leakage of trade secrets increases the value of current stocks of knowledge, boosting firm value. As in our analysis, firm value rose quickly following MARA because firms can quickly take advantage of the ability to enforce non-competes. We note that this result is consistent with firms' advocacy for the ability to use non-compete agreements, as exhibited most recently in Georgia's constitutional amendment where trade associations organized to support the bill (Ga. HR 178). We speculate that firms consider non-competes to be in their interest because managers can immediately observe the value-capture benefits enjoyed by constraining an employee from joining or founding a competitor, whereas the deleterious effects of myopia (which play out over the longer run) may be more difficult for managers to detect. As Stein observes, "it is precisely those investments that are most easily and accurately summarized on an accounting statement... which are least likely to be sacrificed" (Stein 1989: 664) as firms become myopic.

When examining patenting behavior, we find that firms in Michigan become more myopic as research teams become less diverse, inventors rely on older prior art, and firms concentrate on fewer technical areas; *Tobin's q* also drops correspondingly. We attribute this behavior to negative externalities accompanying the captivity of human capital. The ability to hold on to its employees may seem to benefit a firm, but if other firms can also do the same, the mobility of both talent and ideas is compromised with long-term effects for all firms in the region. We view this outcome as akin to the "tragedy of the anticommons" (Heller and Eisenberg 1998) in which the ability of firms to exclude access to a scarce resource inhibits subsequent innovation.

In sum, non-competes may appear attractive for firms and in fact may be so in the short-to-medium term. Struggling firms may find non-competes particularly attractive for this reason. But in the long-run, limiting the value-creation capability of *all* firms in a region where non-competes are enforceable may have negative implications for welfare. And while we stop short of a welfare analysis, we see this as a natural next step, examining total factor productivity or other key productivity measures. Until such work is complete, the policy debate regarding non-competes will likely continue to be a war of (supposed) interests.

CHAPTER 4

THE VALUE OF SCIENCE AND THE COSTS OF MOBILITY: EVIDENCE FROM NEWLY PUBLIC FIRMS

1. Introduction

Economists have long maintained that science advances technology (Nelson 1959; Mansfield 1972; Jaffe 1989) and management scholars have often argued that "private" scientific research (i.e., science conducted within the firm) can improve firm performance (Cohen and Levinthal 1990; Henderson and Cockburn 1994; Gambardella 1995; Zucker and Darby 1996; Zucker, Darby et al. 2002). In the pursuit of science, however, firms rely heavily on individual scientists (Di Gregorio and Shane 2003). Although human capital can be an important source of value for the firm (Castanias and Helfat 1991), individual scientists are mobile (Fallick, Fleischman et al. 2006) and research suggests that high-value, human assets are problematic to manage (Coff 1997). As Peteraf notes, "a brilliant, Nobel prize winning scientist may be a unique resource, but unless he has firm-specific ties, his perfect mobility makes him an unlikely source of sustainable advantage" (Peteraf 1993: 187).

To date, there has been little empirical research examining how the mobility of scientists affects the value that firms derive from science. On the one hand, scientists provide the intellectual capital, motivation, and creativity that make scientific research possible – there can be no science without scientists. On the other hand, scientists are not owned by the firm and are generally free to depart at any time (Becker 1962). When a scientist departs the firm, it can be costly to the firm in many ways: departure can reduce the stock of knowledge held by the firm (Walsh and Ungson 1991), degrade routines (Ranft and Lord 2000), reduce the effectiveness of knowing who-knows-what (Reagans, Argote et al. 2005), and slow the rate of organizational learning (Carley 1992). A departing scientist may also 'spin-out' to form a new company (Klepper 2001) or transfer proprietary knowledge to a competitor (Rosenkopf and Almeida 2003). Firms also face time-compression diseconomies (Dierickx and Cool 1989) when they have to replace a scientist in a hurry in fast-moving environments.

In this study, we examine how markets value scientific research conducted by firms, as well as how markets value the potential of losing key scientists. First, we develop a new, firm-level measure of "science" to capture the degree to which high-tech firms rely on scientific research conducted within the firm. We survey experts to construct a dictionary of words associated with the scientific method, perform a content analysis of financial documents to measure the importance of those words to the firm, and then estimate the value of the measure in the marketplace. Having thus estimated a main effect for science, we then estimate the degree to which markets discount firm value to account for the possibility of key scientist departure – an effect we call a "mobility discount." Finally, we test the consistency of our theory by examining how these factors interact under different conditions that should increase or decrease the mobility discount. Because state-by-state variation in the enforceability of employee non-compete agreements has been shown to affect employee mobility (Marx, Strumsky et al. 2009; Garmaise 2011), we predict that the mobility discount will be greater when non-competes are not enforceable. We also predict that the mobility discount will be greater when science (and therefore scientists) are more important to the firm.

Based on a sample of 458 newly-public, high-tech, venture-capital backed companies in the United States just after initial public offering (IPO) between 1996 and 2007, we find evidence that equity markets positively value intangible assets and growth opportunities associated with science, but that markets also negatively value the naming of specific, key scientists. We find that the mobility discount increases as science is more important to the firm, and that the discount is most extreme in states that prohibit the enforcement of non-compete agreements. Our results suggest that science can be an important contributor to firm value, but that markets discount firm value by up to 12% when firms face substantial risks of losing key, individual scientists.

This study addresses calls in the literature for more research on the micro-foundations of strategy (Felin and Hesterly 2007; Barney, Ketchen et al. 2011; Coff and Kryscynski 2011; Foss 2011). By linking employee mobility (at the individual-level) to value capture from science (at the firm-level), and by demonstrating the economic significance of the mobility discount, we shed new light on the complications of obtaining competitive advantage from human-based resources. As such, our theory has important implications for capabilities-based, resource-based, and knowledge-based theories of the firm (Winter 1987; Barney 1991; Grant 1996). This study also contributes to the economics of science (Rosenberg 1990; Stephan 1996; Gittelman and Kogut 2003; Toole and Czarnitzki 2009) and the strategic implications of employee mobility (Palomeras and Melero 2010; Campbell, Coff et al. 2011; Campbell, Ganco et al. 2012).

2. Theory and Hypotheses

Despite prior research on “science-based” (Darby, Liu et al. 2004; Ambos and Birkinshaw 2010), “science-oriented” (Stern 2004), “science-driven” (Cockburn, Henderson et al. 2000), and “open-science” (Ding 2011) firms, there is little consensus as to the best way to evaluate the financial impact of science conducted *within* the firm. Some research suggests that internal science can help firm performance (e.g., Henderson and Cockburn 1994; Gambardella 1995; Zucker and Darby 1996; Zucker, Darby et al. 2002), while other research suggests that it involves many tradeoffs and costs (e.g., Gittelman and Kogut 2003; Stern 2004; Toole and Czarnitzki 2009). Although the causal connection between private science and private firm value is highly complex and subject to differences in initial conditions, time-varying internal and external conditions, and idiosyncratic pay-offs (Cockburn, Henderson et al. 2000), research suggests that science can provide value at a high-level in the following ways: First, and perhaps foremost, science can produce new knowledge that the firm can then access and use in other R&D activities. Scientitific knowledge can be particularly valuable to the firm when it is tacit and derived from personal experience (Zucker, Darby et al. 2002); such knowledge is easier to exclude from others and more difficult to acquire through open-marker transactions (Arrow 1962). Science can also provide a template of superior methods to follow and therefore a systematic approach to research and discovery (Brooks 1994), as well as foster the development of routines that then lead to better information gathering, problem solving, and project selection for the firm (Zucker, Darby et al. 2002). Science may also help firms to develop the absorptive capacity needed to understand the importance of discoveries made in other firms and therefore capitalize on the spillovers of others (Cohen and Levinthal 1990). Finally, science can provide a map for search (Fleming and Sorenson 2004) – as Nelson (1982) suggests, it is often knowledge about the ways to *search* a technological space that provide the most value.

Although it may be difficult to predict the particular mechanism through which science generates value for a given firm (Teece 1986; Cockburn, Henderson et al. 2000; Winter 2006: 1104), if one assumes that investments in science rarely *hurt* the firm, then one can conclude that prior-period investments in science should provide at least *some* value to the firm.[22] For the purposes of this study, the objective then becomes one of empirically differentiating firms based on the existence of valuable, science-based intangible assets and/or growth opportunities.[23] However, just as it is difficult to theoretically predict the importance of science to a given firm, it is also difficult to empirically measure the importance of science to a given firm. On the input side of scientific production, it is difficult to determine the proportion of R&D (or even the number of employees) that a firm dedicates to basic science as opposed to applied science, product development, or other activities (Stephan 1996). On the output side of scientific production, artifacts such as publications vary in their relative importance and value between researchers, firms and industries. For example, some researchers may value (and thus produce) a fewer number of ground-breaking publications, whereas other researchers may value (and thus produce) a higher number of incremental publications. Similarly, some firms may depend on the success of a single line of research, whereas other firms may have large, diversified portfolios of

[22] We note that this is true even if firms obtain no economic rents from science, or even if prior-period investments are largely wasteful (i.e., current value is less than investments made). Even if investing in science does not make economic sense for a given firm, the products of that investment should nevertheless have some positive value.

[23] Following research in finance (Miller and Modigliani 1961; Abel and Eberly 2005), we assume that firm value can be decomposed into the value of assets in place and the value of growth opportunities.

research projects. It is also difficult to measure the valuation differences that exist between exploration-driven and exploitation-driven science.[24]

Finally, we note that objective measures of the inputs and outputs of science ignore the valuation pathway that is applicable to a given firm. For example, some firms may support scientific research in order to build the reputation of the firm, whereas other firms may invest into science in order to convert that research into new technologies and products – these two types of firms, however, might rate similarly in terms of publications. As another example, some firms may invest into science and promote open publication as a way to signal (Akerlof 1970; Spence 1973) the higher quality of the firm and thereby acquire better resources (at a lower cost) from labor or capital markets; other firms, however, may be more secretive about their scientific research and therefore have fewer publications – again, these two types of firms would rate differently on an objective measure of scientific publications (the signaling company would rate high, and the secretive company would rate low), even though the scientific activity of each firm could have reversed implications as to the degree that firms rely on science for future earnings.

For all of these reasons, we depart from prior research and develop a novel approach to analyzing the value of science conducted within the firm. We switch from an objective analysis of the inputs and outputs of science, and instead focus on a subjective representation made by each firm regarding the importance of science *to that particular firm*. While we operationalize

[24] For example, we would expect exploration-driven science to have a lower measure of scientific output (such as publications) but a greater impact on long-term cash flows, and we would expect exploitation-driven science to have a higher measure of scientific output but a lesser impact on long-term cash flows (March 1991; Levinthal & March 1993). We know of no way, however, to objectively assess these kinds of valuation differences between exploration and exploitation activities when measuring scientific publications.

and validate our measure of *Science* later (see the Data and Methods section), we note here that we expect firms to express the importance of science to the firm through the frequency of scientific words that they use in financial reports. When firms spend more time in financial reports describing the scientific theories, methods, and discoveries undertaken by the firm, then we presume that those scientific activities are in fact more valuable to the firm. We therefore hypothesize:

> ***Hypothesis 1:*** *High-tech firms that emphasize the use of the scientific method (i.e., "science") will receive higher market valuations.*

Key Scientists

Scientists play an essential role in the production and use of scientific research. As Gittelman and Kogut point out, "the dilemma for firms seeking to profit from scientific knowledge... is that science is not available as ready-made inputs, but is produced by scientists" (2003: 367). Much scientific knowledge is derived from long hours of bench-level research and personal experience (Zucker, Darby et al. 2002; Darby, Liu et al. 2004). As such, investments into science result in valuable stores of tacit knowledge (Polanyi 1962; Polanyi 1966) and not just the codified, nonrivalrous knowledge (Romer 1990) that is often associated with science. Tacit knowledge, of course, is stored in individuals who may leave the firm at any time (Coff 1997). Moreover, scientists are connected to a professional community outside of the firm through academic appointments, publications, and conferences, and those connections generally weaken the ties that hold scientists to firms (Brown and Duguid 2001).

When scientists leave a firm they impose costs on the firm in many ways. First, the loss of a scientist reduces the stock of knowledge held by the firm. Especially when there are time lags between a scientific discovery and the development of new technologies or products based on that discovery, firms may require specific individuals to remain with the firm in order to store and 'transfer forward' their knowledge over time (Garud and Nayyar 1994). Second, scientific research is increasingly organized into socially-complex routines (Nelson and Winter 1982) and teams (Wuchty, Jones et al. 2007). The loss of a key scientist can reduce the productivity of a team by breaking the social ties that hold the team together (Leonard-Barton 1995), degrade the effectiveness of knowing who-knows-what within the organization (Reagans, Argote et al. 2005), and lower the rate of organizational learning (Carley 1992). Third, the loss of a key scientist can have firm-level, competitive effects when the scientist transfers valuable knowledge or routines to a new firm (Klepper 2001; Agarwal, Echambadi et al. 2004; Klepper and Sleeper 2005; Campbell, Ganco et al. 2012) or an established competitor (Wezel, Cattani et al. 2006).

Following the departure of a scientist, firms face time-compression diseconomies to replace the lost scientist (Dierickx and Cool 1989). Often, 'just any scientist' will not do to fill a particular gap in a research program or to replace a lost tie to a specific university or alliance partner. To salvage the remaining value of a line of scientific research, or to complement other scientists who remain at the firm, firms must carefully vet and select replacement scientists, a potentially slow and costly process. The vetting and selection process may also require specialized expertise that the firm may have just lost by way of the departing scientist, further complicating the search process. When the firm does find a suitable replacement, many scientists have other academic commitments and professional obligations that delay the date before which the scientist may begin employment with the company. In fast-moving environments, or in situations where firms are attempting to secure first-mover advantages (Lieberman and Montgomery 1988), these types of human resource delays and time-compression diseconomies can have detrimental implications to the competitiveness of the firm.

The above discussion refers to the costs of scientist departure as if the risks of incurring those costs were evenly and randomly distributed amongst firms. We argue, however, that firms vary systematically with respect to the degree that they concentrate scientific activity around key individuals, and therefore the degree to which they are exposed to the costs of scientist departure. For instance, some firms place more importance on a relatively few individuals by hiring (or developing) "star" scientists within the firm (Zucker and Darby 1996; Azoulay, Zivin et al. 2010), whereas other firms may distribute scientific expertise more uniformly across the workforce. Firms that concentrate expertise onto key individuals should be at greater risk of incurring disproportionately greater costs if one of those key individuals were to leave the firm.

Recent research suggests that both firms and markets systematically recognize the risk of employee departure. Kim & Marschke (2005), for example, argue that firms patent more and conduct less R&D when there is a greater risk of employee departure. Chapter 2 in this dissertation finds that firms become more valuable targets to acquirers when there are stronger restrictions on post-acquisition employee mobility (in the form of enforceable non-compete agreements). Chapter 3 in this dissertation finds that the market valuation of R&D-intensive firms in Michigan increased when there was stronger enforceability of non-compete agreements, implying that firms in Michigan were worth more when there was less potential for employee departure. We expect, therefore, that investors should anticipate the systematic risks and costs of employee departure and appropriately discount the valuation of affected firms. Or, in a more general sense, the potential for key scientists to depart a firm can be thought of as introducing uncertainty into the valuation of the firm, and factors that increase valuation uncertainty have been shown to reduce firm value (Riley 1989). We term this reduction in firm value a "mobility discount." In sum, we predict that markets will discount for the marginal effect of specifically naming key scientists, leading to the following hypothesis:

> ***Hypothesis 2:*** *Controlling for science, high-tech firms that emphasize key scientists will receive lower market valuations.*

Moderating Conditions

As we describe later in the paper, we employ a cross-sectional research design for this study. Because we lack an experimental identification strategy, an important aspect of our theory development is to predict and test the consistency of our theory under different moderating conditions. To quote Sutton and Staw (1995: 376), "One indication that a strong theory has been proposed is that it is possible to discern conditions in which the major proposition or hypothesis is most and least likely to hold." Therefore, to further examine how science and scientists relate to market valuation, we develop three additional hypotheses where we expect our main effects (H1 and H2) to strengthen or weaken.

First, we examine the role of non-compete agreements ("non-competes") in constraining employee mobility. Employee non-compete agreements are contractual provisions that expressly prohibit employees from joining a competitor, or forming a new firm as a competitor, within particular industries and geographic locations for a certain time period (Gilson 1999). Also known as "covenants not to compete," non-competes have become a nearly ubiquitous feature of employment contracts for scientists in the United States. Surveys show that a large majority of knowledge workers and upper-level management have signed non-compete agreements with their employers (Kaplan and Stromberg 2001; Leonard 2001; Kaplan and Stromberg 2003). Theoretical research has long suggested that varying levels of enforcement of non-competes contribute to the differential employee mobility observed in different states (e.g., Saxenian 1994; Gilson 1999; Franco and Mitchell 2008). Recent empirical studies have confirmed the negative relationship between the enforceability of non-competes and the mobility of employees ranging from executives (Garmaise 2011), to inventors (Marx, Strumsky et al. 2009), and to knowledge workers more generally (Fallick, Fleischman et al. 2006).

We suggest that state-level variation in the enforcement of non-competes is an observable proxy variation in employee mobility that investors can be expected to incorporate into investment decisions. Specifically, as the enforceability of non-compete agreements increases, concern about the ability and consequences of key scientists departing from the should also decrease; to the extent that investors systematically incorporate such expectations into their investment decisions, investors should bid up the value of the firm when non-competes are more enforceable. In other words, the enforcement of non-competes should weaken the negative valuation effect of exposure to key scientists. We therefore predict:

> ***Hypothesis 3:*** *The negative market valuation effect of emphasizing key scientists will be weaker for firms in states that enforce employee non-compete agreements.*

The consequences of scientist departure should depend on the degree to which scientists are actually important to the firm. In the context of our study, we argue that scientists are more important to the firm when science itself is more important to the firm. We therefore hypothesize that investors factor in the importance of science to the firm as they anticipate the likelihood and probable costs of key scientist departure. When science is more important to the firm, then we expect investors will increase the mobility discount that they impose on the firm for a given level of exposure to key scientists. We therefore predict:

> ***Hypothesis 4:*** *The negative market valuation effect of emphasizing key scientists will be stronger for firms that emphasize science.*

Finally, we hypothesize that the moderating effects of non-compete agreements and scientific intensity mutually interact with the negative valuation effect of emphasizing key scientists. There is, in other words, a triple interaction between exposure to key scientists, the enforceability of non-compete agreements, and the importance of science to the firm. Following our earlier logic, that the negative effects of key scientist departure should depend upon the importance of science to the firm, the enforcement of non-compete agreements should also have a greater effect on the value of the firm as the importance of science to the firm increases. Or, in other words, we expect that the enforcement of non-compete agreements should reduce the costs of scientist departure only to the extent that a firm is actually engaged in value-generating scientific research that puts the firm at risk of incurring costs from scientist departure. When the scientific research is not very important to the firm, then non-compete agreements are less likely to moderate the consequences of key scientist departure. We therefore predict a triple interaction between scientists, non-competes, and scientific intensity, as follows:

> ***Hypothesis 5:*** *The attenuating (positive) effect that enforcing non-compete agreements has on the negative market valuation effect of emphasizing key scientists will be stronger for firms that emphasize science.*

In review, we propose that investments in science have at least some value to the firm, and that systematic exposure to costs related to the potential departure of key scientists from the firm should result in a 'mobility discount' detracting from the positive value that firms otherwise derive from science. We therefore predict that, after controlling for all other sources of

Table 1: Summary of hypotheses.

Hyp	Variable	Description
H1 (–)	*Science*	High-tech firms that emphasize the use of the scientific method (i.e., "science") will receive higher market valuations.
H2 (–)	*PhD*	Controlling for science, high-tech firms that emphasize key scientists will receive lower market valuations.
H3 (+)	*PhD * Non-Compete*	The negative market valuation effect of emphasizing key scientists will be weaker for firms in states that enforce employee non-compete agreements.
H4 (–)	*PhD * Science*	The negative market valuation effect of emphasizing key scientists will be stronger for firms that emphasize science.
H5 (+)	*PhD * Non-Compete * Science*	The attenuating (positive) effect that enforcing non-compete agreements has on the negative market valuation effect of emphasizing key scientists will be stronger for firms that emphasize science.

firm value, high-tech firms that emphasize science to investors will receive higher valuations. We also predict that high-tech firms that emphasize key scientists will receive a lower market valuation than otherwise, again controlling for the value of science and other known determinants of firm value. We also predict that the mobility discount is attenuated when firms are protected by non-compete agreements and when the firm is not actually doing much science. A summary of our hypotheses appears in Table 1.

3. Data and Methods

Empirically, our objective is to differentiate firms based on the importance of private scientific research and key scientists to the firm, and to then determine how markets value those factors. We selected newly-public, high-tech firms as an appropriate context to test our theory. We focus on the time just after initial public offering because an IPO is a prominent event in the life-cycle of every public company, and a comparable point in time at which to estimate market valuation.

Our sample construction started with initial public offerings in the United States occurring between January 1, 1996 and December 31, 2007. We started the sample in 1996 because the SEC started in 1996 to require firms conducting an IPO to file an electronic copy of their prospectus under SEC rule 424b, allowing us to perform a computerized analysis of the textual content of prospectuses, as described below. We ended the sample at the end of 2007 to avoid effects associated with the financial crisis of 2008. We restricted the sample to high-tech firms because we anticipated scientific research and high-skilled scientific workers to be more important to growth opportunities for high-tech firms. We selected high-tech firms based on the

Fama-French 10-industry specification (Fama and French 1997), with the exception that we also included the "pharmaceutical" and "medical equipment" industries from the Fama-French 49-industry specification. We restricted the sample to U.S.-based firms that were listed on a U.S. stock exchange in order to exclude valuation uncertainty tied to international financial conditions.

We obtained our sample from Thomson Financial's SDC VentureXpert database in order to select VC-backed firms, and in order to obtain additional control variables with respect to the number and characteristics of pre-IPO investors, rounds of investment, and underwriter endorsements.[25] Our original sample from the VentureXpert database consisted of 1,841 observations. We dropped 1,002 observations that were not high-tech, not VC-backed, or not based in the U.S. We then matched records from VentureXpert to the Compustat Fundamentals database on CUSIPs, and matched firms to prospectuses based on ticker symbols, obtaining a final sample size of 458 firms. Finally, we supplemented the sample with patenting data, demographic data, and other data as described below.

Dependent Variable

Market Valuation. We adopted total market valuation, at the end of the first day of trading after an initial public offering, as the dependent variable for this study. In keeping with the theme of this dissertation, our objective is to explain market valuation, and not IPO characteristics *per se*. We also note that IPOs are usually underpriced between the offer price at

[25] A larger sample drawn from the SDC New Issues database produced qualitatively similar results. Using the New Issues database, however, would prevent us from controlling for VC backing, pre-IPO investments, and underwriter endorsements. We therefore used the smaller sample obtained from the VentureXpert database for our main analysis. Sampling from VentureXpert resulted in a sample that was younger, smaller, and more growth-oriented.

initial public offering and the market price at end of the first day of trading (Ritter 1998), making IPO valuation before market trading a less accurate measure of firm value. Many IPOs also have an ‘overallotment option’ wherein underwriters are given the right to sell additional shares (often up to 15% of the number of initially offered shares) during the first 30 days after the IPO. Aggarwal (2000) finds that firm value associated with the overallotment option is ***not*** reflected in the initial IPO valuation, but that the overallotment option is positively associated with first-day returns, implying that valuation at the end of the first day of trading is a preferred measure of study for the purposes of this paper.

We therefore adopt initial market valuation, defined as end-of-first-day price multiplied by the post-IPO shares outstanding, as the dependent variable for this study. Because our measure of market valuation is afflicted by non-normality (*Skewness* = 7.96, *Kurtosis* = 88.05), we used the natural logarithm of initial market valuation to improve normality (transforming it to *Skewness* = 0.79, *Kurtosis* = 3.98) and to better control for heteroskedasticity in the regression analyses. Even though valuation studies often adopt a linear specification, research also suggests that a log-linear model of market valuation provides a better fit for firms with substantial intangible assets (Hirsch and Seaks 1993; Hand 2003).

Explanatory Variables

Science. As discussed earlier, objective measures of the value of science to the firm are problematic and/or unavailable for our sample. We therefore developed a subjective measure of the importance of science to the firm in the following manner. Our measure, *Science*, is based on the premise that firms spend more time explaining scientific activities in financial reports when scientific activities are in fact more important to the firm. For example, firms spend more time talking about "experiments" and "hypotheses" when firms derive more value from those activities. This approach follows calls in the literature (Lyon, Lumpkin et al. 2000; Short, Broberg et al. 2010) to analyze word-frequencies to measure difficult to operationalize concepts such as entrepreneurial orientation (also see Uotila, Maula et al. 2009 for a similar approach in measuring the relative importance of exploration versus exploitation).

We selected the IPO prospectus as our document for analysis because the prospectus provides a comprehensive snapshot of every public firm at a comparable point in time. The SEC requires firms conducting an IPO to file a prospectus, and, as Draho points out, "there is rarely any publically disseminated information about the issuer prior to the IPO, except for the offering prospectus" (Draho 2004: 28). Furthermore, companies use the prospectus for the specific purpose of convincing investors to invest in the company. Language used in the prospectus should therefore reflect the assessment of the firm as to the intangible assets and growth opportunities that it sees as valuable, as well as how those factors relate to future earnings after the IPO. In selecting the IPO prospectus for analysis, we follow prior research that has analyzed annual reports, CEO shareholder letters, mission statements and regulatory filings (cf. Duriau, Reger et al. 2007). We obtained an electronic copy of the IPO prospectus as SEC Form 424b from the SEC EDGAR database.

To obtain a measurement of *Science* for each firm, we performed a Computer-Aided Text Analysis (CATA) to count the number of times a "scientific" word appeared in each IPO prospectus for each firm in our sample. We used CATA techniques to count words because they have been found to be cheaper, faster, and more reliable than human coding of large texts (Neuendorf 2002). King and Lowe (2003) have also shown that, given a sufficiently large vocabulary, automated coding of concepts based on word-frequencies can be equally accurate to human coding of texts. Before we could perform a CATA analysis, however, we first had to develop a list of words that we could show would reliably indicate scientific activity. Prior research suggests that generic word list from sources such as the Harvard Dictionary are often misleading (Short, Broberg et al. 2010; Loughran and McDonald 2011) and review articles (e.g., Duriau, Reger et al. 2007) suggest that many studies fail to report the complete origin or justification for the vocabulary that they use in the content analysis. To address these concerns, we developed and validated a new dictionary for content analysis as follows:

First, we focused on the scientific method as a common language used across all branches of science. It is important that the list of words used in the analysis also be general enough to apply to a range of different firms, industries and contexts, and to do so in a consistent way. We therefore focused on the scientific method as a common denominator. The basic concepts of the scientific method are also taught throughout the educational system and therefore known to scientists, investors, and managers.

Next, we surveyed a population of "experts" to develop our dictionary. Although we suspected that the language of the scientific method would be well-known and frequently-used across most contexts, a survey-based methodology for developing the dictionary should only increase the content validity of our analysis. We asked participants to spend five minutes to brainstorm words that they associated with the "practice of science" and/or the "scientific method" (see Table 2 for a copy of the survey), and asked participants to skip obvious derivations and synonyms of words in order to save time (e.g., if a respondent listed hypothesize then they could skip listing hypothesized, hypothesizing, hypothesis, hypotheses, etc.). Upon receiving a response, we added all missing verb tenses and added up to three synonyms from the WordNet lexical database (Miller 1995; Fellbaum 1998) – steps that we believe should expand the coverage and reliability of each word list. We also performed key word in context (KWIC) analysis (Krippendorff 2004) to remove words (e.g., "risk") that we deemed to be misleading ($< 3\%$ of words). Ultimately, we obtained 25 independent wordlist with an average of 194 words (*min.* 96; *max.* 402). Out of 30 survey requests, we received 25 responses for a balanced sample of five respondents in five categories (see Table 3 for a list if experts and categories surveyed).[26]

Table 2: Email Survey

Please brainstorm a list of science-based words - just click reply, type for 5 minutes and send!
* Focus on words that you associate with the **practice of science** and/or the **scientific method**.
* Focus on words that apply widely across different technologies and industries. (e.g., "analyze" or "hypothesis" rather than "chromatography")
* Don't worry about listing variants of a word (e.g., hypothesis, hypothesize, etc.) -- we'll do that.
* Please spend only **5 minutes**! We are looking for prominent words.
* Please **star the top 20 words** that you feel are most important.
* Your reply is confidential -- no personally identifying information will be used.
Thank you!

[26] While we believe that our survey sample is representative of our context (high-tech, newly-public ventures), we did not select individuals at random; our 83.33% response rate reflects the fact that we targeted individuals with whom we had prior connections and a greater likelihood of obtaining a response.

After developing 25 independent wordlists (as described above), we then counted the number of times each word appeared in each IPO prospectus for each firm in our sample, and scaled our results by the length of each prospectus to obtain a cardinal measure of word-use *intensity* that could be compared in magnitude between firms. We then examined the inter-rater reliability for each wordlist (Cronbach 1951; Cronbach 1971) and found that, as a set, the 25 independent wordlists had a *Cronbach Alpha* = 0.98, far in excess of generally accepted standards for reliability (Kerlinger and Lee 2000). Furthermore, all wordlists were highly correlated internally, indicating consistency in the meaning and interpretation of the scientific method between survey respondents (of the 25 respondents, twelve had inter-item correlations greater than 0.90, nine had correlations greater than 0.80, three had correlations greater than 0.75, and only one had a marginal inter-item correlation equal to 0.59). We also performed a principal components factor analysis (Tabachnick and Fidell 2007) to confirm that the 25 wordlists identified just one factor. We found one common factor with an *Eigenvalue* = 19.39, whereas all other factors had an *Eigenvalue* of less than 1.30. The factor analysis also revealed a consistently high loading across all wordlists. Based on the high inter-item correlations and single, high-*Eigenvalue* factor, we concluded that our survey produced an internally-reliable vocabulary of words to measure the scientific method, and that the 25 wordlists could therefore be consolidated (without weighting) into one Master Dictionary of 1,385 unique words. Finally, as the last step in the measurement of our variable *Science*, we used the Master Dictionary to conduct final round of CATA analysis to obtain a final measurement for each firm in our sample. We mean centered *Science* to facilitate the interpretation of interactions.

Table 3: Experts Surveyed.

Respondent	Degree	Title	Industry
Investors			
1	Ph.D.	Financial Analyst	Mutual Fund
2	M.S.	Venture Capital Partner	Venture Capital
3	B.A.	Investment Banker	Investment Banking
4	Ph.D.	Investment Banker	Investment Banking
5	B.A.	Mutual Fund Manager	Mutual Fund
Executives			
6	Ph.D.	Chief Technology Officer	Software
7	M.B.A	Founder & CEO	Internet
8	Ph.D.	Founder & CEO	Biotech
9	M.S.	VP Engineering	Medical Instruments
10	Ph.D.	President	Chemicals
Scientists			
11	Ph.D.	Research Scientist	Biotech
12	Ph.D.	Research Scientist	Defense
13	Ph.D.	Research Scientist	Robotics
14	Ph.D.	Research Scientist	Petroleum
15	Ph.D.	Research Scientist	Ecology
Tech Transfer			
16	Ph.D.	Senior VP	Silicon Valley
17	Ph.D.	Manager	Silicon Valley
18	Ph.D.	Manager	Silicon Valley
19	Ph.D.	Manager	Mountain West
20	Ph.D.	Director	East Coast
Academics			
21	Ph.D.	Lab Manager	Engineering
22	Ph.D.	President	Hazardous Waste & Biotech
23	Ph.D.	Senior Consultant	Hydrology
24	Ph.D.	Statistician	Chemicals
25	Ph.D.	VP Application Development	Software

Having thus obtained a measurement of *Science* for each firm in our sample, we tested the predictive validity of our variable to insure that it could predict other dependent variables that are theoretically, but tangentially, related to the pursuit of science. First, we estimated a logistic regression model and found *Science* to be a highly significant predictor of the likelihood that a firm will patent ($p < 0.001$). We also estimated a negative binomial regression model and found *Science* to be a highly significant predictor of the number of patents obtained by the firm ($p < 0.001$). We note, however, that the relationship between *Science* and patenting also makes it important for us to control for patenting in our main regression analyses.

Ph.D. To measure exposure to the potential departure of key scientists, we developed the variable *PhD* by counting the number of mentions of the term "Ph.D." in each IPO prospectus for each firm in our sample. In conducting the count, we included common variants of "Ph.D." such as "PhD.", "PHD", etc. Whereas prior research has counted the number of ***unique*** individuals in the top management team (Junkunc and Eckhardt 2009; Chok and Kennedy 2012) or in the founding team (Ding 2011) with a Ph.D. degree, in our approach we counted the number of ***mentions*** of an individual with a Ph.D. appearing in the prospectus, on the premise that firms mention the same individual repeatedly in the prospectus when they have a greater reliance on those individuals. For example, some firms may have many scientific employees but not mention those individuals in the prospectus at all because those individuals, as *individuals*, are not critical to the success of the firm. Alternatively, other firms may have key scientists that work in the upper echelons, sit on the scientific advisory board, or serve in another important capacity such that they warrant a specific mention in the prospectus. To the extent that the same individual (along with their degree) is mentioned multiple times in the prospectus, we assume that each mention indicates an increased importance of the individual, and that each mention is therefore also a proxy for the increased risk exposure the firm faces with respect to the potential departure of that individual from the firm. Because we will also control for the knowledge intensity of the firm (i.e., by including variables for *Science*, *R&D*, *Patent Stock*, *Knowledge Workers*, and *Secrecy* in our analysis), we view the variable *PhD* as a proxy for the marginal exposure firms face with respect to the potential departure of key scientists. We mean centered *PhD* to facilitate the interpretation of interactions.

Non-Compete. To measure the enforceability level of non-compete agreements, we defined the variable *Non-Compete* as the index variable developed by Garmaise (2011). Garmaise used results from surveys conducted by the American Bar Association (Malsberger 2004) to assess and assign one point to the *Non-Compete* index for each of 12 questions on the survey that surpassed a given threshold. With a potential range of 0 to 12, the index (as measured) ranges from 0 to 9. Covenants not to compete are enforced by state law based on the location of employment, and the index therefore varies by state. Whereas the level of non-compete enforcement typically remains stable over time, legislative changes in state law or court precedent have changed the level of enforceability in Michigan, Texas, Florida and Louisiana. Those policy changes have allowed other research to exploit those changes as natural experiments, including research presented here in this dissertation in Chapters 3 and 4. The timeframe of the current study, however, precluded us from using a natural experiment. Instead, we exploited cross-sectional variation between states and then tested for consistency in the pattern of interactions between *Non-Compete* and our other theoretical variables of interest.

Control Variables

Timing and Location. Research suggests that IPO markets are characterized by changing conditions in time and space (Ritter 1984; Ritter 1998). To control for year-by-year variation, we included a full set of annual indicators. In addition to the annual indicators, we also included an indicator variable *Boom Period* and coded it 1 for IPOs conducted between January 1, 1997 and April 1, 2000, thereby specifically controlling for the dot.com boom (Aggarwal, Bhagat et al. 2009). To control for spatial conditions that may affect the valuation of new ventures, we included *Location*, calculated as the natural logarithm of 1 + the number of VC funding events in the year prior and in the same Metropolitan Statistical Area (MSA) as a focal IPO.

Industry. Although our small sample size prevented us from implementing a complete set of low-level-industry fixed effects, we included the following indicators to control for industry effects at a high level. Hand (2003) and Bartov *et al.* (2002) find that IPO valuations are significantly higher for Internet companies; we therefore included the indicator variable *Internet* and coded it 1 for Internet firms. A large body of research has examined the biotech industry as special case in the commercialization of science (e.g., Zucker, Darby et al. 2002; Gulati and Higgins 2003; Guo, Lev et al. 2005), where academic research scientists may have played a particular role in the genesis and transformation of the industry (Zucker, Darby et al. 1998); we therefore included the indicator variable *Biotech* and coded it 1 for firms in the biotechnology industry. To control for IPO activity by industry, we calculated *Hot Markets* as the number of IPOs in the prior year prior and same SIC4 industry as a focal IPO. To control for the knowledge intensity of an industry, we followed prior research (e.g., Farjoun 1994; Coff 1999; Coff 2002) to calculate the variable *Knowledge Workers* as the ratio of knowledge workers to total workers in a given industry. We obtained data on employment levels from the Occupational Employment Statistics (OES) survey from the Bureau of Labor Statistics and coded knowledge workers as those with an occupational code below 50,000 (e.g., managers, sales workers, scientists, engineers, editors, computer programmers, IT professionals, and so forth). We then calculated the proportion of knowledge-workers to total-workers for each 3-digit SIC code and assigned this measure *Knowledge Workers* to each focal firm in our sample by SIC code. To control for the relative importance of secrecy (and therefore thus the relative importance of restrictions on employee mobility), we included the variable *Secrecy* based on a survey measure reported by Cohen, Nelson and Walsh (2000); *Secrecy* represents the mean percentage of product innovations for which survey respondents believe secrecy to be an effective mechanism for protecting knowledge and appropriating rents.

Endorsements. Institutional theory and prior research suggest that outside endorsements based on underwriter reputation (Carter and Manaster 1990), venture capitalist backing (Barry, Muscarella et al. 1990; Megginson and Weiss 1991), and prior rounds of funding (Gulati and Higgins 2003) can affect the value of firms at IPO (Rao 1994; Stuart, Hoang et al. 1999). Because higher levels of *Science* might relate to higher endorsements (irrespective of any direct effect of *Science* on market valuation), we included controls for *Underwriter Prestige*, *VC*

Prominence, and *Prior Financing*. We measured the prestige of the lead underwriter for each firm in our sample as *Underwriter Prestige*, based on the nine-point scale developed by (Carter and Manaster 1990), updated by (Loughran and Ritter 2004), and maintained by Ritter (2012). Following the work of (Gulati and Higgins 2003), we measured *VC Prominence* as follows. We calculated the total amount of investment by each VC firm in SDC VentureXpert by year. We then ranked VC firms in each year and coded VC firms as 1 (i.e., "prominent") if they were in the top 25th percentile of VC investors that year and zero otherwise. We then averaged the rating of every VC firm invested into a focal firm from our sample based on the prior year coding of the VC, such that the variable *VC Prominence* varies from 0 to 1 for all of the firms in our sample. We measured *Prior Financing* by calculating the total amount of prior financing received by a focal company based on pre-IPO investments reported in SDC VentureXpert.

Financial Fundamentals. Many aspects of market valuation are closely related to financial fundamentals. One of the strongest predictors of market valuation post-IPO is the size of the IPO itself, for cash on hand obviously has market value – as does the proven ability to raise it. We therefore controlled for the natural logarithm of *Offer Size* as equal to the total proceeds acquired in IPO. Following prior research (Chemmanur and Paeglis 2005), we included three proxies of firm size, representing three functional forms of firm size (log, linear, and squared), to control potential non-linearity between firm size and other IPO characteristics: we included measures for *Book Value*, *Book Value Squared*, and the natural logarithm of total *Assets*. To control for large firms with few assets, we also controlled for *Revenue*. To control for the profitability and financial condition of the firm, we controlled for *Net Income*, *Return on Assets* and *Leverage*.

Other Firm Characteristics. Following prior research (Ritter 1984; Michaely and Shaw 1994; Loughran and Ritter 2004; Chemmanur and Paeglis 2005; Cohen and Dean 2005), we controlled for firm quality by including the variable *Firm Age*, measured as the number of years from founding date to IPO based on data from Ritter (2012), and the variable *Executives*, measured as a count of *Executives* in the top management team and/or the board of directors to control for the size of the upper echelons. We also controlled for *Outside Exposure* as the number of days between the date the company received its first round of outside financing and the date of IPO, on the premise that more time between outside funding and IPO provides markets with more information and a longer track-record for evaluation. Prior research has shown R&D to be a significant driver of IPO valuation (Guo, Lev et al. 2005), but many firms do not report R&D financials at the time of IPO. We therefore included *R&D* as an indicator variable to control for firms that report R&D. Prior research also finds that investors strongly value patent stocks (Guo, Lev et al. 2005). We therefore included *Patent Stock* as the cumulated "stock" of past patenting activity (using a declining balance formula and a 15% depreciation rate). Table 4 summarizes definitions, Table 5 presents descriptive statistics, and Table 6 reports correlations for each variable in the sample.

Table 4: Definition of variables.

	Variable	Description	Source
1	*Market Value (log)*	Market Value at end of first day of trading (logged)	*SDC*
2	*Boom Period* [a]	Indicator for dot-com boom years (1/1/1997 thru 3/31/2000)	*SDC*
3	*Location*	Log of 1+number of VC funding events in same MSA, prior year	*SDC*
4	*Internet*	Indicator for internet-related industry	*SDC*
5	*Biotech*	Indicator for biotechnology industry	*SDC*
6	*Hot Markets*	Count of IPOs in same SIC4 industry in prior year	*SDC*
7	*Knowledge Workers*	Ratio of knowledge-workers to total-workers in focal industry	*OES Survey*
8	*Secrecy*	Degree secrecy is considered important in focal industry	*Cohen et al, 2000*
9	*Underwriter Prestige*	Tombstone ranking of lead underwriter	*Ritter*
10	*VC Prominence*	Proportion of a firm's VCs in top 25th percentile ranking of VCs	*SDC*
11	*Prior Financing*	Total private financing received by firm prior to IPO	*SDC*
12	*Offer Size (log)*	IPO offer size (logged)	*SDC*
13	*Assets (log)*	Total Assets (logged)	*Compustat*
14	*Book Value*	Book value	*Compustat*
15	*Book Value (squared)*	Book value squared	*Compustat*
16	*Total Revenue*	Total revenue	*Compustat*
17	*Net Income*	Net income	*Compustat*
18	*Return on Assets*	Return on assets	*Compustat*
19	*Leverage*	(Long term debt + current liabilities) divided by assets	*Compustat*
20	*Firm Age*	Years from firm founding to IPO	*Ritter*
21	*Executives*	Count of upper echelon executives and outside board members	*SDC*
22	*Outside Exposure*	Days from first round of funding	*SDC*
23	*R&D Indicator*	IPO Company reports R&D expenses (indicator variable)	*Compustat*
24	*Patent Stock*	Count of successful patent applications during prior 5 years	*Hall/NBER*
25	*Non-Compete*	Non-compete enforcement index [0,9] by state	*Garmaise, 2011*
26	*Science*	Count of scientific-method words, divided by document length	*Custom*
27	*PhD*	Count of mentions of PhD, divided by document length	*Custom*

Notes: The sample covers the years 1996 through 2007. Subsections are marked for: dependent variable; timing and location; endorsements; financial fundamentals; other firm characteristics; and explanatory variable main effects.
a) The *Boom* indicator is included in model specifications in addition to year-by-year indicators for year fixed-effects.

Table 5: Summary statistics (n=460).

	Full Sample		Low Scientific Intensity		High Scientific Intensity	
Variable	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Market Value	782.71	1,944.822	734.30	2,135.45	831.13	1,736.78
Market Value (log)	5.86	1.09	5.88	1.18	5.83	0.99
Boom Period	0.45	0.50	0.66	0.47	0.24	0.43
Location	4.36	1.69	4.37	1.58	4.34	1.79
Internet	0.38	0.49	0.41	0.49	0.34	0.48
Biotech	0.14	0.35	0.14	0.34	0.14	0.35
Hot Markets	15.95	24.95	20.74	28.37	11.16	19.91
Knowledge Workers	0.55	0.21	0.58	0.20	0.51	0.21
Secrecy	51.66	3.59	51.40	3.26	51.93	3.88
Underwriter Prestige	8.16	1.19	8.19	1.06	8.12	1.30
VC Prominence	0.42	0.28	0.45	0.28	0.39	0.28
Prior Financing	65,270.01	108,846.10	44,971.60	53,462.90	85,568.41	141,636.70
Offer Size (log)	17.84	0.62	17.70	0.60	17.98	0.62
Assets (log)	4.51	0.80	4.38	0.77	4.65	0.81
Book Value	97.05	176.11	81.12	78.19	112.98	235.68
Book Value (sq.)	40,366.20	431,821.70	12,667.13	31,267.12	68,065.27	609,293.80
Total Revenue	56.61	191.12	33.56	38.25	79.66	265.86
Net Income	-19.01	42.49	-20.03	38.67	-17.99	46.06
Return on Assets	-0.19	0.26	-0.19	0.25	-0.19	0.27
Leverage	0.22	0.19	0.21	0.18	0.23	0.21
Firm Age	11.91	17.86	11.03	17.77	12.79	17.95
Executives	12.74	5.51	12.14	5.24	13.35	5.71
Outside Exposure	1,682.62	1,196.38	1,467.88	1,172.52	1,897.37	1,183.84
R&D Indicator	0.92	0.28	0.93	0.26	0.90	0.30
Patent Stock	3.43	9.60	4.36	10.61	2.51	8.41
Non-Compete	0.00	2.71	-0.26	2.47	0.26	2.91
Science	0.00	22.80	17.60	9.20	-17.60	18.30
PhD	0.00	12.85	-0.62	12.36	0.62	13.31
PhD (not centered)	7.45	12.85	8.07	13.31	6.83	12.36

Notes: Summary statistics for U.S., high-tech, venture capital-backed IPOs between the years 1996 and 2007. Non-Compete, Scientific Intensity, and PhD were mean-deviated and centered to zero. The original count variable of PhDs ranged from 0 to 75.

Table 6: Correlations (n=460)

	Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
1	Market Value (log)	1.000													
2	Boom Period [a]	0.202	1.000												
3	Location	0.091	-0.127	1.000											
4	Internet	0.325	0.288	0.125	1.000										
5	Biotech	-0.181	-0.202	-0.002	-0.288	1.000									
6	Hot Markets	-0.001	0.152	0.038	0.208	-0.152	1.000								
7	Knowledge Workers	0.193	0.363	0.075	0.581	-0.303	0.417	1.000							
8	Secrecy	-0.183	-0.125	-0.025	-0.153	0.115	0.051	-0.207	1.000						
9	Underwriter Prestige	0.371	-0.084	0.146	0.109	0.023	-0.002	0.013	-0.042	1.000					
10	VC Prominence	0.156	0.061	-0.030	0.122	-0.048	0.140	0.121	-0.092	0.210	1.000				
11	Prior Financing	0.164	-0.183	0.055	-0.056	0.067	-0.122	-0.103	0.033	0.128	-0.012	1.000			
12	Offer Size (log)	0.708	-0.168	0.100	0.096	-0.080	-0.123	-0.007	-0.097	0.332	0.062	0.250	1.000		
13	Assets (log)	0.689	-0.119	0.073	0.088	-0.039	-0.101	0.000	-0.091	0.418	0.136	0.308	0.792	1.000	
14	Book Value	0.467	-0.062	0.050	0.069	-0.051	-0.057	0.021	-0.046	0.177	0.076	0.246	0.565	0.636	1.000
15	Book Value (squared)	0.262	-0.054	0.036	0.049	-0.030	-0.041	0.031	-0.011	0.045	0.033	0.148	0.353	0.332	0.904
16	Total Revenue	0.316	-0.064	-0.003	0.045	-0.092	-0.058	0.015	-0.063	0.056	0.069	0.219	0.412	0.421	0.876
17	Net Income	-0.159	-0.061	-0.024	-0.006	-0.066	-0.042	-0.008	0.024	-0.117	0.043	-0.151	-0.100	-0.195	0.260
18	Return on Assets	0.076	-0.015	-0.054	-0.048	-0.139	-0.045	0.015	-0.011	0.070	0.108	-0.083	0.098	0.181	0.136
19	Leverage	-0.047	0.028	-0.068	-0.016	-0.132	-0.020	0.051	-0.117	-0.107	-0.013	0.120	-0.054	-0.039	-0.086
20	Firm Age	-0.165	-0.066	-0.020	-0.126	-0.026	-0.047	-0.074	0.053	-0.071	-0.053	-0.001	-0.034	-0.069	-0.013
21	Executives	0.134	-0.112	0.054	0.028	0.136	0.038	-0.047	0.059	0.147	0.065	0.060	0.187	0.178	0.143
22	Outside Exposure	-0.175	-0.244	-0.025	-0.200	0.073	-0.122	-0.280	0.084	-0.092	-0.071	0.123	0.019	-0.038	0.029
23	R&D Indicator	-0.105	-0.156	0.068	-0.043	0.098	0.127	-0.104	0.008	0.069	0.087	-0.117	-0.159	-0.219	-0.136
24	Patent Stock	0.045	-0.050	0.010	-0.123	0.127	-0.098	-0.224	0.098	0.121	0.047	0.045	0.059	0.089	0.081
25	Non-Competes	-0.128	-0.001	-0.315	-0.055	0.076	0.029	-0.029	0.066	-0.163	-0.017	0.021	-0.088	-0.075	-0.074
26	Science	-0.023	0.524	-0.076	0.075	0.026	0.244	0.203	-0.074	-0.059	0.099	-0.237	-0.282	-0.191	-0.106
27	PhD	-0.218	-0.303	0.063	-0.351	0.658	-0.186	-0.397	0.158	0.009	-0.043	0.083	-0.061	-0.040	-0.031

	Variable	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)	(25)	(26)	(27)
15	Book Value (squared)	1.000												
16	Total Revenue	0.913	1.000											
17	Net Income	0.448	0.481	1.000										
18	Return on Assets	0.071	0.170	0.521	1.000									
19	Leverage	0.002	0.080	-0.135	-0.188	1.000								
20	Firm Age	-0.012	-0.023	0.025	0.042	-0.054	1.000							
21	Executives	0.076	0.088	-0.012	-0.054	-0.038	-0.022	1.000						
22	Outside Exposure	0.045	0.101	0.199	0.209	0.002	0.112	0.156	1.000					
23	R&D Indicator	-0.057	-0.092	0.237	0.086	-0.260	0.049	0.115	0.140	1.000				
24	Patent Stock	0.035	0.038	0.044	0.054	-0.106	0.047	0.105	0.207	0.108	1.000			
25	Non-Competes	-0.045	-0.018	0.040	0.019	0.110	-0.006	-0.061	0.064	-0.068	-0.082	1.000		
26	Science	-0.093	-0.141	-0.099	-0.090	-0.018	0.006	-0.155	-0.227	-0.030	0.066	-0.013	1.000	
27	PhD	-0.016	-0.063	-0.030	-0.146	-0.152	0.012	0.075	0.138	0.130	0.172	0.041	-0.030	1.000

Model

We estimate an ordinary least squares (OLS) equation of the following form:

$$\ln(MarketValue_i) = \beta_0 + \beta_1\, Science_i + \beta_2\, PhD_i + \beta_3\, PhD_i * NC_i + \beta_4\, PhD_i * Science_i$$
$$+ \beta_5\, PhD_i * Science_i * NC_i + \beta_6\, NC_i + \boldsymbol{C_i} + \boldsymbol{T_t} + \varepsilon_i\,,$$

NC refers to *Non-Compete*, βetas 1-5 estimate Hypotheses 1-5, βeta 6 is the main effect of Non-Competes, $\boldsymbol{C_i}$ is a vector of covariate controls, $\boldsymbol{T_t}$ is a vector of year indicators, and ε_i is the normal error term.

4. Results

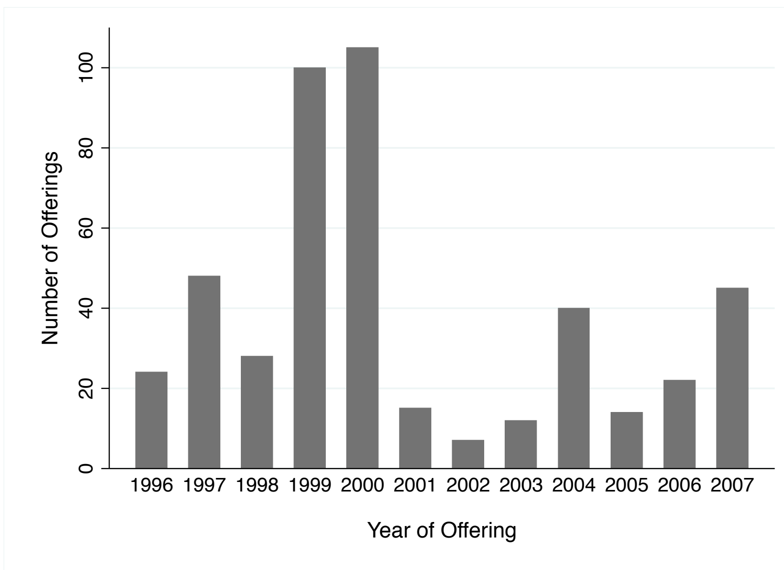
We begin our analysis with a simple graph of IPO activity by year between 1996 and 2007 (Figure 1). There was a surge in IPO activity in 1999 and 2000, followed by a sharp reduction in 2001 through 2003. Our sample consists mostly of Internet (38%), biotech (14%), computing (11%), and semi-conductor (9%) companies that are relatively young (*median* 11.9 years), small (*median* 30.4 million pre-IPO assets, *median* 84.4 million post-IPO assets), and losing money (75% of firms in the sample). The importance of research and development is evident in that 92% of firms reported R&D expense.[27]

As expected, firms in science-driven industries had higher mentions of "Ph.D." in the prospectus (e.g., an average of 28.4 mentions for biotech, as compared to 4.0 for non-biotech), although there was a surprising degree of independence between the two measures (Pearson correlation between *Science* and *PhDs* is close to zero). In other words, firms appear to have

[27] In unreported robustness tests, we found that controlling for R&D Expense, and dropping firms that did not report R&D expense, produced very similar results at the same levels of statistical significance.

considerable discretion over the degree to which they choose to emphasize, or not emphasize, key scientists and/or scientific methods in the prospectus. In some cases, firms may emphasize science but not call out and emphasize the role of individual scientists; in other cases, firms may call out and emphasize the role of key scientists, but not actually detail much in the way of scientific activity. It is also possible that firms actively trade-off these two factors when promoting themselves to outside investors.

Figure 1: Number of IPOs by year.



Hypotheses Testing

Table 7 reports our main results: covariate controls are reported in Table 7a, and the main theoretical effects and interactions are continued in Table 7b. All models include the full set of covariate controls reported in Table 7a, annual indicator variables to control for year-by-year variation, and robust standard errors are clustered by state. Column 1 includes all controls, the main effect of *Non-Compete*, and the main effect of *Science*. Column 2 adds the main effect of

Table 7a: Main results – control variables (n=458) *(results continue in Table 7b)*

	(1)	(2)	(3)	(4)	(5)	(6)
Intercept	-10.469***	-10.432***	-10.677***	-10.686***	-10.911***	-10.868***
	(2.4317)	(2.3738)	(2.4063)	(2.3661)	(2.3736)	(2.4509)
Boom Period	0.7636**	0.7556**	0.7568**	0.7344**	0.7360**	0.7421**
	(0.2447)	(0.2459)	(0.2451)	(0.2428)	(0.2426)	(0.2426)
Location	0.0056	0.0076	0.0036	0.0062	0.0024	0.0030
	(0.0164)	(0.0159)	(0.0168)	(0.0152)	(0.0157)	(0.0176)
Internet	0.2935***	0.2828***	0.2725***	0.2782***	0.2686***	0.2645***
	(0.0485)	(0.0456)	(0.0444)	(0.0412)	(0.0399)	(0.0413)
Biotech	-0.1579**	-0.0781	-0.0870	-0.0671	-0.0758	-0.0844
	(0.0572)	(0.0742)	(0.0696)	(0.0692)	(0.0653)	(0.0636)
Hot Markets	0.0016	0.0015	0.0013	0.0008	0.0007	0.0007
	(0.0020)	(0.0020)	(0.0020)	(0.0019)	(0.0019)	(0.0020)
Knowledge Workers	-0.2190	-0.2372	-0.2112	-0.1737	-0.1506	-0.1418
	(0.1540)	(0.1480)	(0.1467)	(0.1317)	(0.1302)	(0.1369)
Secrecy	-0.0148	-0.0144	-0.0147	-0.0139	-0.0142	-0.0153
	(0.0102)	(0.0104)	(0.0105)	(0.0103)	(0.0104)	(0.0110)
Underwriter Prestige	0.0448+	0.0446+	0.0458+	0.0442+	0.0454+	0.0449+
	(0.0238)	(0.0241)	(0.0236)	(0.0238)	(0.0234)	(0.0239)
VC Prominence	0.0717	0.0812	0.0887	0.0699	0.0774	0.0679
	(0.0729)	(0.0736)	(0.0733)	(0.0790)	(0.0789)	(0.0789)
Prior Financing	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Offer Size (log)	0.8363***	0.8314***	0.8477***	0.8460***	0.8610***	0.8595***
	(0.1417)	(0.1384)	(0.1416)	(0.1390)	(0.1407)	(0.1440)
Assets (log)	0.3091**	0.3093**	0.2981**	0.2935**	0.2833**	0.2939**
	(0.0973)	(0.0960)	(0.1006)	(0.0973)	(0.1010)	(0.1005)
Book Value	0.0002	0.0003	0.0003	0.0004	0.0004	0.0003
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Book Value (sq.)	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Total Revenue	0.0006	0.0006	0.0006	0.0007	0.0006	0.0006
	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Net Income	-0.0027	-0.0027	-0.0025	-0.0026	-0.0025	-0.0025
	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0019)	(0.0020)
Return on Assets	0.1237	0.1055	0.0906	0.1302	0.1155	0.1193
	(0.2046)	(0.1979)	(0.2001)	(0.2054)	(0.2072)	(0.2162)
Leverage	0.0090	-0.0074	0.0069	0.0081	0.0213	0.0136
	(0.1163)	(0.1090)	(0.1095)	(0.1052)	(0.1070)	(0.1009)
Firm Age	-0.0048***	-0.0048***	-0.0049***	-0.0047***	-0.0048***	-0.0049***
	(0.0010)	(0.0010)	(0.0011)	(0.0012)	(0.0012)	(0.0012)
Executives	-0.0006	-0.0010	-0.0007	-0.0001	0.0002	0.0005
	(0.0053)	(0.0052)	(0.0051)	(0.0055)	(0.0055)	(0.0056)
Outside Exposure	-0.0001+	-0.0001+	-0.0001+	-0.0001+	-0.0001+	-0.0001+
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
R&D Indicator	0.4592***	0.4619***	0.4598***	0.4634***	0.4614***	0.4607***
	(0.1000)	(0.1007)	(0.0993)	(0.1001)	(0.0985)	(0.1018)
Patent Stock	0.0027	0.0028	0.0024	0.0040+	0.0036	0.0031
	(0.0026)	(0.0025)	(0.0025)	(0.0023)	(0.0023)	(0.0025)

Table 7b: Main results – main effects and interactions *(see Table 7a for controls)*

	(1)	(2)	(3)	(4)	(5)	(6)
Non-Compete	-0.0063	-0.0056	-0.0058	-0.0056	-0.0058	-0.0052
	(0.0093)	(0.0096)	(0.0091)	(0.0101)	(0.0096)	(0.0090)
Science (SCI)	0.0052*	0.0060**	0.0061**	0.0076**	0.0077**	0.0075**
	(0.0021)	(0.0020)	(0.0020)	(0.0024)	(0.0024)	(0.0024)
PhD		-0.0045*	-0.0049*	-0.0051*	-0.0055*	-0.0051*
		(0.0021)	(0.0022)	(0.0023)	(0.0024)	(0.0022)
PhD * NC			0.0013***		0.0012**	0.0014***
			(0.0003)		(0.0004)	(0.0004)
PhD * SCI				-0.0002**	-0.0002**	-0.0002*
				(0.0001)	(0.0001)	(0.0001)
NC * SCI						-0.0004
						(0.0005)
PhD * NC * SCI						0.0001*
						(0.0000)
Observations	458	458	458	458	458	458
R-squared	0.739	0.740	0.742	0.743	0.745	0.746

Notes: "NC" refers to *Non-Compete* and "SCI" refers to *Science*. All models are estimated by OLS, include year fixed effects, and cluster robust standard errors by state (S.E. in parentheses). Two-tailed tests: ***$p<0.001$, **$p<0.01$, *$p<0.05$, $+p<0.10$.

PhD, conditional on *Science, Non-Compete*, and the other controls. Columns 3-6 successively add the interactions for *Non-Compete* and *Science* onto the main effect of *PhD*; Column 6 is the Full Model, including all variables at the same time, and we therefore interpret the results from Column 6 for our hypotheses testing.

Concerning the control variables, we find that firms during the *Boom Period* had significantly higher market valuations ($p<0.01$) than other periods, a finding consistent with prior research (Aggarwal, Bhagat et al. 2009). Firms in the Internet sector had valuations 26% to 29% higher than other firms ($p<0.001$), also consistent with prior research (Ljungqvist and Wilhelm Jr 2003). Interestingly, firms in the *Biotech* sector had a 16% lower valuation as estimated in Column 1 ($p<0.01$) where the effects of *PhD* are not controlled, whereas the effect of *Biotech* fell to a statistically insignificant 7% discount in Columns 2-6 where the effects of *PhD* are separately controlled. *Underwriter Prestige* is associated with a marginally significant ($p<0.10$) increase of 4.5% in market valuation, consistent with predictions that inter-organizational endorsements reduce uncertainty and raise market value (Stuart, Hoang et al. 1999; Gulati and Higgins 2003). The strongest and most statistically significant predictor is simply the offer size of the IPO ($p<0.001$), with an elasticity of around 0.86. Asset size is also highly significant ($p<0.01$) with an elasticity of around 0.30. *Firm Age* is associated with a reduction in firm value ($p<0.001$), consistent with the view that younger high-tech companies are more dynamic and have greater growth opportunities. Interestingly, firms with a longer period between their first round of funding and IPO have a very slightly lower (–0.01%) valuation, although this is only marginally significant ($p<0.10$) and may reflect a selection effect in the timing of when firms elect to hold their IPO. Finally, firms that report R&D expenses have significantly higher market valuations of nearly 46% ($p<0.001$).[28]

Next we turn to the hypotheses testing of our main results. In our baseline hypothesis about science (H1), we posit that markets will positively value information indicating that firms are engaged in scientific activities. Indeed, the positive and highly significant coefficient for *Science* indicates that reports of scientific activity in IPO prospectus are positively valued by the markets; firms with a *Science* level one standard deviation higher than average have a 17.10% higher market valuation than firms at the mean ($p<0.01$). In our baseline hypothesis

[28] Due to the size and significance of this effect, we conducted a robustness check using R&D Intensity (R&D Expense / Sales). We find very similar results between the two models; results are reported in the robustness checks.

about the potential mobility of scientists (H2), we posit that markets will negatively value information that indicates firms rely on key scientists (i.e., markets will increase the 'mobility discount'). The negative and highly significant coefficient for *PhD* provides evidence in support of this hypothesis ($p<0.05$). This result indicates that each additional mention of an individual with a Ph.D. degree is associated with an additional 0.5% decrease in market valuation. Firms with a *PhD* level one standard deviation higher than average have a 6.57% lower market valuation than firms at the mean.

Now we turn to the hypothesis testing of our moderating conditions. Hypotheses 3-5 identify conditions under which an emphasis on key scientists should have an even bigger (or smaller) effect on firm valuation. Consistent with H3, we find that the interaction of *PhD * Non-Compete* is positive and significant ($p<0.001$), suggesting that the effect of *PhD* is weaker when firms can protect themselves from the negative consequences of employee departure through the enforcement of employee non-compete agreements. Hypothesis 4 predicts that the effect of *PhD* will be stronger and even more negative when firms also emphasize the importance of science. There is evidence consistent with this hypothesis: the interaction of *PhD * Science* is negative and statistically significant ($p<0.05$), suggesting that the negative effect of *PhD* is even stronger when investors can expect key scientists to hold an even greater importance to the future financial success of the firm. Finally, Hypothesis 5 predicts that an increase in *Science* should strengthen the (H3) attenuation effect, whereby the enforcement of non-compete agreements shields firms from some of the negative valuation effects of relying on key scientists. Alternatively, being a triple-interaction, H5 also predicts that an increase in the enforceability of non-compete agreements should attenuate the negative (H4) effect, reducing the negative consequences that increased *Science* has on the negative effect of key scientists. Consistent with either interpretation of H5, we find that the three-way interaction coefficient for *PhD * Non-Compete * Science* is positive and statistically significant ($p<0.05$), providing evidence in support of this hypothesis.

Predictions

To demonstrate the economic significance of our results, we calculated the predicted mobility discount from the Full Model (Column 6 in Table 7) and report results in Table 8. We predicted the percentage change in market valuation based on levels, indicated in Table 8, for each of the variables in the H5 triple-interaction of *PhD * Non-Compete * Science*. We included all lower-level interactions and main effects in the prediction; predictions are made as a percentage change, relative to a prediction where no mention of individuals with Ph.D. degrees are made in the prospectus. Predictions for the first column(s) in Table 8 are therefore zero. For illustrative purposes, we divide effects across the three different dimensions of *PhD, Non-Compete,* and *Science* in the following manner: In Panels A and B, we make predictions at the 25^{th} and 75^{th} percentile levels of *Science*. Along the vertical axis of each grid, we make predictions for no enforceability of non-compete agreements, the 50^{th} percentile of non-compete enforceability (i.e., Garmaise (2011) index equals 2.5), and the 75^{th} percentile of non-compete enforceability (i.e., Garmaise index equals 5.0). Along the horizontal axis of each grid, we make predictions for no mention of individuals with a Ph.D., one mention of an individual with a Ph.D. (approx. the 50^{th} percentile of *PhD*), and nine mentions of one or more individuals with a

Table 8: Predicted mobility discounts for *PhD, Non-Compete,* and *Science.*

Panel A: 25^{th} Percentile of *Science*

	None (PhD Count=0)	50^{th} Percentile (PhD Count=1)	75^{th} Percentile (PhD Count=9)
None (NC Index=0)	0%	0.20%	1.76%
50^{th} Percentile (NC Index=2.5)	0%	0.12%	1.04%
75^{th} Percentile (NC Index=5)	0%	0.06%	0.55%

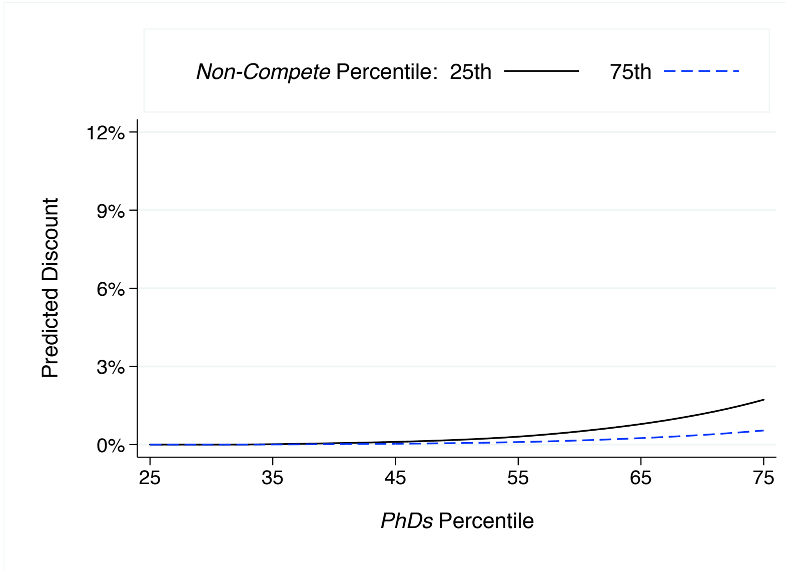
Panel B: 75^{th} Percentile of *Science*

	None (PhD Count=0)	50^{th} Percentile (PhD Count=1)	75^{th} Percentile (PhD Count=9)
None (NC Index=0)	0%	1.36%	11.63%
50^{th} Percentile (NC Index=2.5)	0%	0.70%	6.13%
75^{th} Percentile (NC Index=5)	0%	0.26%	2.28%

Notes: Table 8 reports predictions across the triple-interaction of H5 (*PhD * Non-Compete * Science*), including all effects from lower-level (double) interactions. Panel A makes predictions at the 25^{th} percentile level of *Scientific Intensity* and Panel B makes predictions at the 75^{th} percentile level of *Scientific Intensity*. Predictions are made as the percentage change in market valuation as compared to the market valuation that is predicted when no *PhDs* are mentioned in the IPO prospectus (negative percentage changes are inverted and expressed as a positive "discount"). Labels for "PhD Count" and "NC Index" refer to original, undeviated values (not the mean-centered valuations used in regression models). "PhD Count" ranges from 0 to 75, and 48% of observations are at the no PhDs level. "NC Index" (as measured by Garmaise, 2011), ranges from 0 to 9, with *Median*=2.5, *Mean*=2.6, *S.D.*=2.7, and 47% of observations are at the no enforcement level. All predictions are made from the full model (Table 5, Column 7) at the mean level of all covariates. Year fixed effects are controlled through orthogonal polynomial contrast codes to absorb between-year variation while enabling an average prediction across all years.

Figure 2: Predicted mobility discounts for *PhD, Non-Compete,* and *Science.*

Panel A: 25th percentile of *Science*



Panel B: 75th percentile of *Science*

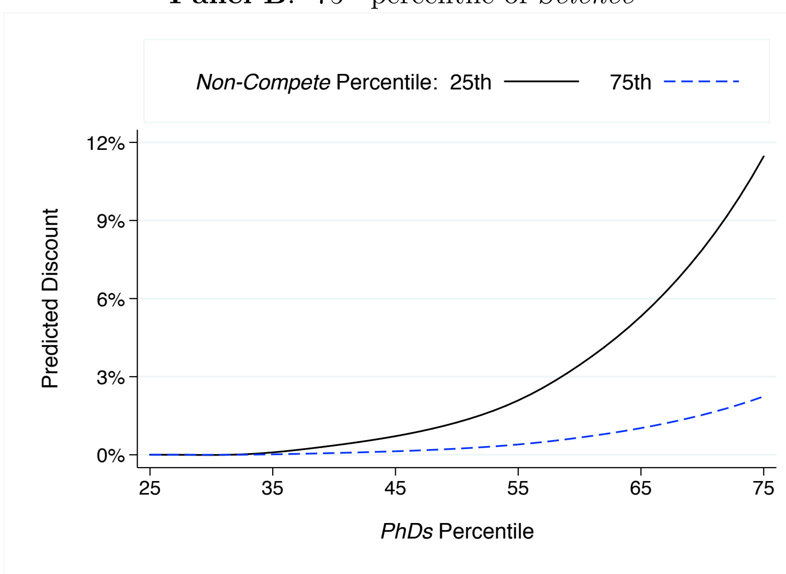


Figure 2 plots the predicted mobility discount across the three variables interacted in H4: *PhD, Non-Compete*, and *Science*. Panel A makes predictions at the 25th percentile level of *Science* and Panel B makes predictions at the 75th percentile level of *Science*. *PhD* is plotted by percentile along the horizontal axis. Two lines are plotted on each graph, one representing the prediction at the 25th percentile of *Non-Compete* enforcement (equivalent to a Garmaise (2011) non-compete enforcement index of 0) and another line representing the prediction at the 75th percentile (equivalent to a Garmaise (2011) non-compete enforcement index of 5). Finally, the prediction in *Market Valuation* is plotted along the vertical axis as a percentage change in market valuation relative to the market valuation predicted when no "Ph.D." are mentioned in the IPO prospectus. Negative percentage changes are reversed and expressed as a positive "discount". All predictions are based on the Full Model (Table 5 – Column 7) and include all lower-order effects. Year fixed effects are controlled through orthogonal polynomial contrast codes to absorb between-year variation while enabling an average prediction across all years.

Ph.D. (approx. the 75th percentile of *PhD*). In this manner we vary levels of all three variables in the H5 interaction, while simultaneously accounting for the lower level and main effects of each variable in each prediction. The results in Table 8 demonstrate sizable differences moving along all three dimensions. When non-competes are highly enforceable, the mobility discount is relatively low. But as the number of mentions of Ph.D. rises, the mobility discount rises, and the effect sharply accelerates as *Science* increases. In a 'worst case' scenario, firms at the 75th percentile of *PhD*, the 75th percentile of *Science*, and no protection from the enforcement of non-compete agreements, can expect to receive an 11.63% market valuation discount that is associated in this model with the potential for key scientists departing the company.

In Figure 2, we graph predicted outcomes across the varying levels of *PhD, Non-Compete,* and *Science* as an alternative means of visualizing the effects reported in Table 8. Panel A in Figure 2 suggests that there is very little association between key individuals with a Ph.D. and market value, under either an enforcement or non-enforcement regime for non-competes, when firms are not actively engaged in science. However, as Panel B in Figure 2 demonstrates, when firms actively engage in science, *and* when firms have little or no protection through the enforcement of non-compete agreements, firms then face a steep mobility discount in the marketplace to compensate investors for the added risks of employee departure and potential adverse effects on firm value.

Robustness Checks

We performed a series of robustness checks of the model specifications and report these test results in Table 9. All models include the full set of control variables discussed earlier (and listed in Table 7a), a complete set of annual indicators, and robust standard errors clustered by state, except as noted below. To ease side-by-side comparison, Column 1 repeats our main results from the Full Model in Column 6 of Table 7. Column 2 uses backward elimination (Lindsey and Sheather 2010) to drop potentially unnecessary controls that reduce an optimal

Table 9: Robustness checks.

	(1) Full Model	(2) Primary Controls	(3) R&D Intensity	(4) Industry Dummies	(5) State Dummies	(6) Clustered by MSA	(7) Applied Science
Non-Compete	-0.0052	-0.0108	-0.0019	-0.0048	-0.0022	-0.0007	-0.0035
	(0.0090)	(0.0095)	(0.0093)	(0.0095)	(0.0250)	(0.0166)	(0.0090)
Science (SCI)	0.0075**	0.0076***	0.0063*	0.0075**	0.0069**	0.0065*	0.1319**
	(0.0024)	(0.0021)	(0.0026)	(0.0025)	(0.0023)	(0.0024)	(0.0461)
PhD	-0.0051*	-0.0064***	-0.0060**	-0.0043+	-0.0057*	-0.0047+	-0.0043*
	(0.0022)	(0.0017)	(0.0020)	(0.0024)	(0.0025)	(0.0026)	(0.0017)
PhD * NC	0.0014***	0.0015***	0.0014***	0.0016**	0.0012**	0.0016+	0.0015***
	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)	(0.0008)	(0.0004)
PhD * SCI	-0.0002*	-0.0002**	-0.0001+	-0.0002*	-0.0002*	-0.0002*	-0.0031*
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0013)
NC * SCI	-0.0004	-0.0002	-0.0006	-0.0004	-0.0006	-0.0003	-0.0092
	(0.0005)	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0004)	(0.0101)
PhD * NC * SCI	0.0001*	0.0001**	0.0001*	0.0001*	0.0001*	0.0001**	0.0016**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0005)
Observations	458	458	420	458	458	438	458
R-squared	0.746	0.737	0.760	0.750	0.763	0.747	0.746

Notes: "NC" refers to *Non-Compete* and "SCI" refers to *Science*. With the exception of Column 2, all models include the full set of control variables listed in Table 5a. Column 1 repeats the Full Model from Column 6 in Table 5 to facilitate comparison across models. Column 2 uses backward elimination (Lindsey and Sheather 2010) to remove potentially unnecessary controls that are detrimental to an optimal Bayesian Information Criterion, resulting in the following controls being retained in the model: *Boom Period*, *Internet*, *Offer Size (log)*, *Assets (log)*, *Firm Age*, *Outside Exposure*, and *R&D Indicator*). Column 3 replaces the *R&D Indicator* with *R&D Intensity* (R&D Expense divided by Sales), dropping 38 observations from the analysis due to missing data. Column 4 adds additional industry indicators for computers, software, and semiconductors. Column 5 adds indicators based on the state where the IPO company maintains its headquarters to control for state fixed effects. Column 6 clusters standard errors by metropolitan statistical area (MSA) instead of state (and drops 20 observations due to missing data for MSA). Column 7 tests an alternative measure of *Science* based on a dictionary of applied science and engineering words, as explained in Appendix A. All models are estimated by OLS, include year fixed effects, and cluster robust standard errors by state (except as noted). Robust standard errors are reported in parentheses. Two-tailed tests: ***$p<0.001$, **$p<0.01$, *$p<0.05$, $+p<0.10$.

Bayesian Information Criterion (BIC).[29] Results from Column 2 confirm that our main results are not sensitive to the inclusion of irrelevant controls. Column 3 replaces R*&D Indicator* with *R&D Intensity* (defined as R&D expense divided by sales), dropping 38 observations from the analysis due to missing data. Results from Column 3 weaken the results for *Science* from 0.0075 to 0.0063 with a drop in statistical significance to ($p<0.05$), but strengthen the results for *PhD* from negative 5.1% ($p<0.05$) to negative 6.0% ($p<0.01$). Column 4 adds industry indicators for computers, software, and semiconductors (in addition to indicators for *Internet* and *Biotech*) to further fix effects by industry. Although we do not have sufficient observations within each industry to implement industry fixed effects at a finer level, these results suggest that our findings are not primarily driven by between industry differences. In Column 5 we add indicators for state fixed effects (we already cluster robust standard errors in all models other than Column 6 by state) and obtain nearly identical results. Column 6 clusters standard errors at the level of the metropolitan statistical area (MSA) instead of state (dropping 20 observations due to missing data for MSA) and obtain similar results, although the interaction of *PhD * Non-Compete* weakens slightly in statistical significance. Column 7 tests an alternative measure of *Science* based on the RCPCE corpus of applied science and engineering words (RCPCE 2012) and finds similar results after accounting for the different scaling of the alternative measure of *Science* (M=0.00; SD=1.00).

[29] We used the `[vselect]` program created by Lindsey & Sheather (2010) in Stata Version 11.2. Backward elimination retained the following controls in addition to the explanatory variables: *Boom Period*, *Internet*, *Offer Size (log)*, *Assets (log)*, *Firm Age*, *Outside Exposure*, and *R&D Dummy*.

5. Conclusion

Drawing on a cross-sectional study of newly-public, high-tech, venture-capital backed firms, we have shown that markets positively value information (reported by firms) that firms are engaged in scientific activities. We also show, however, that markets negatively value information that firms rely on key scientific individuals. We call this phenomena, wherein markets reduce the valuation of scientific activities to account for potential employee mobility, a 'mobility discount.' Our results demonstrate that investors reduce firm valuation by up to 12% in our sample to insure themselves against the possibility that key scientists could leave the firm and destroy value. We also find that the mobility discount is greatest when firms rely on science *and* rely on key scientists for future growth. Non-compete agreements, however, when enforceable, help firms to mitigate the extent of the mobility discount. Taken as a whole, across multiple predictions and moderating conditions, our results support our theory that investors incorporate expectations about employee mobility into investment decisions, and that the market valuation is contingent on the consequences of employee mobility, at least in the context of newly-public, high-tech, high-growth firms.

Our paper makes several contributions. First, our study contributes to an important stream of research on employee mobility (Fallick, Fleischman et al. 2006; Marx, Strumsky et al. 2009; Palomeras and Melero 2010; Marx, Singh et al. 2011; Campbell, Ganco et al. 2012). By demonstrating the economic significance of employee mobility to firm value, we make the case that employee mobility can really matter to the firm. Second, we show that even the *potential* for employee mobility can be costly to the firm – before that mobility happens, or even if it never happens. Firms in our sample pay a higher cost of capital to insure investors against the risk that key scientists could depart the firm. Third, we contribute to research on the market valuation of IPOs and newly public firms (Koop and Li 2001; Sanders and Boivie 2004; Chemmanur and Paeglis 2005; Guo, Lev et al. 2005; Aggarwal, Bhagat et al. 2009). Our results also suggest that subjective measures of factors that affect firm value warrant additional research. By developing and validating a new measure of science, and by releasing our content analysis dictionary for other scholars to use, we contribute an important tool for future research into the commercialization of science.

Opportunities for Future Research

This is the first study we know of to develop a measure for scientific intensity that is broadly applicable across different industries, different firms, and different circumstances. Future research, however, may be required to further establish the validity of this approach. Scholars may also find it beneficial to examine the overlap between qualitative and objective measures as they continue to unpack how firms derive private benefits from science.

Implications for Managers

Our research indicates that firms should attend to the negative effects of organizing future growth opportunities of the firm around a relatively few individuals. In this study, communicating this condition to investors in a financial prospectus was predicted to cost the firm up to 12% of market value. Whereas the enforceability of non-compete agreements substantially helped to mitigate these costs, firms might investigate other contractual arrangements to insure such employees are retained by the firm and not viewed by investors as being at risk of departure. This research helps to focus attention on the management problems associated with the hiring and managing of key employees.

CHAPTER 5

CONCLUSION

In this dissertation, I set out to assess how employee mobility affects the appropriation of value from knowledge. Along the way I have attempted to develop new theory and evidence to demonstrate that employee mobility is a central consideration in how firms create, and then capture, valuable knowledge from human assets. In particular, I have examined the strategic implications of employee departure, and how firms and investors anticipate the costs of employee departure in strategy contexts. Across three studies, this dissertation has developed theory and evidence that restrictions on mobility, in the form of employee agreements to not compete, affect firm value in consistent and important ways. Theory and evidence have also suggested that firms and investors are sensitive, *ex ante,* to the "consequences" of undesired employee departure, thereby affecting firm value whether consequential employee departure happens or not.

In the first essay (Chapter Two), I argued that acquirers incorporate expectations about employee mobility into decisions about whether to bid for a firm. Because employee departure after an acquisition affects the standalone value and/or combination value of a target firm, I predicted a negative relationship between the expected employee mobility in a target firm post-acquisition and the likelihood of a firm becoming an acquisition target. To test this hypothesis, I exploited a natural experiment in Michigan wherein an inadvertent change in the enforcement of non-compete agreements provided an observable, exogenous source of variation in employee mobility. Using a difference-in-differences approach, I found causal evidence that constraints on employee mobility in Michigan increased the likelihood that a Michigan firm would become an acquisition target. I also found that the effect was stronger when a firm is more exposed to the negative consequences of employee mobility, such as when a firm employs more knowledge workers in its work force and when a firm faces greater in-state competition; by contrast, I found that the effect was weaker when a firm was protected by a stronger intellectual property regime that mitigates the consequences of employee mobility.

In the second essay (Chapter Three), I argued that secrecy is a valuable, and even primary, mechanism for protecting knowledge. For firms to invest in R&D, it is essential that they capture value from the knowledge that they create. This dissertation provides some of the first quantitative evidence, aside from the patent system, as to the value of other forms of knowledge protection. In a sample of public firms reporting R&D, I used the same natural experiment in Michigan from the first essay to find causal evidence that constraints on employee mobility can boost *Tobin's* q by as much as 25%. Moreover, evidence suggests that the boost in Tobin's q was increasing in R&D spending but decreasing in the number of patents, and the boost was stronger in industries were secrecy is more important as a means of protecting knowledge. Those advantages, however, appear to be short-lived. In the long-run, the initial boost in q attenuated over time as the circulation of talent and ideas slowed down, and as firms adopted myopic R&D strategies.

In the third essay (Chapter Four), I argued that investors value scientific activity by firms, but that investors also anticipate the potential for scientists to depart the firm. Consequently, markets impose a 'mobility discount' on the market valuation of firms exposed to risks of employee departure. In a sample of newly-public, high-tech, high-growth firms, I found evidence that firms with the greatest reliance on key scientists can be discounted in the market by as much as 12%. I also found that the mobility discount increased when scientists were more central to the objectives of the firm, and that the mobility discount decreased when firms enjoy legal protection from employee departure through the enforcement of non-competes.

The findings presented in this dissertation have important implications for knowledge-based views of the firm, as well as for strategy-based recommendations regarding the management of human and knowledge assets. Across three studies, six dependent variables, and nine moderating conditions, I found a consistent pattern of evidence to suggest that the potential departure of key employees is a central concern of firms. By demonstrating that employee mobility can have large-scale effects on activities such as M&As, R&D, and the raising of capital – activities that are core to the viability and performance of the firm – I make the case that employee departure really does matter.

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