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Essays On Labor And Corporate Finance

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Abstract
This dissertation consists of two chapters that relate labor issues and corporate finance. In the first chapter, I investigate the impact of restricting labor mobility on two components of growth: entrepreneurship and capital investment. To identify the mechanism, I combine LinkedIn's database of employment histories with staggered changes in the enforceability of non-compete agreements that come mostly from state supreme court rulings. Stronger enforceability leads to a substantial decline in employee departures, especially in knowledge-intensive occupations, and reduces entrepreneurship in corresponding sectors. However, these shocks increase the investment rate at existing knowledge-intensive firms.

I estimate a state of median size gains $50 million in capital investment from publicly-held knowledge-intensive firms, but loses almost 200 small firms entering knowledge-intensive sectors.

In the second chapter, I explore a specific mechanism through which corporate social responsibility (CSR), and in particular pro-employee policies, may benefit companies. I propose that socially responsible behavior generates public goodwill toward the firm, which can be redeemed when the company needs public approval, such as when applying for public contracts. To provide causal support for this mechanism, I use the staggered state passage of Other Constituency (OC) laws, which allow directors to orient policies toward non-shareholder constituents. Following the passage of OC laws, employee safety and health measures systematically improve, indicating more employee-oriented behavior. At the same time, firms incorporated in these states systematically become more likely to obtain public contracts and obtain contracts of higher value.

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ESSAYS ON LABOR AND CORPORATE FINANCE

To my family and Devin for their tireless counsel and boundless support.

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ABSTRACT

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This dissertation consists of two chapters that relate labor issues and corporate finance. In the first chapter, I investigate the impact of restricting labor mobility on two components of growth: entrepreneurship and capital investment. To identify the mechanism, I combine LinkedIn's database of employment histories with staggered changes in the enforceability of non-compete agreements that come mostly from state supreme court rulings. Stronger enforceability leads to a substantial decline in employee departures, especially in knowledge-intensive occupations, and reduces entrepreneurship in corresponding sectors. However, these shocks increase the investment rate at existing knowledge-intensive firms. I estimate a state of median size gains $50 million in capital investment from publicly-held knowledge-intensive firms, but loses almost 200 small firms entering knowledge-intensive sectors.

In the second chapter, I explore a specific mechanism through which corporate social responsibility (CSR), and in particular pro-employee policies, may benefit companies. I propose that socially responsible behavior generates public goodwill toward the firm, which can be redeemed when the company needs public approval, such as when applying for public contracts. To provide causal support for this mechanism, I use the staggered state passage of Other Constituency (OC) laws, which allow directors to orient policies toward non-shareholder constituents. Following the passage of OC laws, employee safety and health measures systematically improve, indicating more employee-oriented behavior. At the same time, firms incorporated in these states systematically become more likely to obtain public contracts and obtain contracts of higher value.

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1.1. Introduction

Recent research and policy proposals have renewed the debate over labor mobility restrictions, defined as provisions that prevent workers from leaving their employers (The White House, 2016). On the one hand, limiting mobility can dampen innovation and regional growth (Saxenian, 1994; Gilson, 1999). On the other hand, limiting mobility may have a positive impact on existing firms. In particular, an ex-ante agreement for the employee not to leave the employer may foster growth by safeguarding the employer's investment, especially where search frictions or learning on the job make it difficult to replace human capital (Acemoglu and Shimer, 1999; Zingales, 2000).

Thus mobility, in theory, trades the benefit of reallocating labor to more productive ventures against the cost of dampening investment at existing firms. The objective of this paper is to empirically document and quantify this bidirectional effect. Specifically, I estimate the impact of restricting labor mobility on two ways of exploiting growth opportunities: entrepreneurship, and capital investment. These outcomes are of particular interest for two reasons. First, new firm entry is a key ingredient of economic growth, yet the startup rate of new businesses has declined in recent decades, including in high-tech sectors (Decker et al., 2014). Second, business investment is an important source of productivity growth and has been uneven since the crisis (Furman, 2015). Studying the impact of labor mobility restrictions is therefore important because it can help shed some light on these trends.

Examining the above trade-off presents two key empirical challenges. First, it requires a strategy to address the potential endogeneity of mobility with respect to economic outcomes. Short of randomly assigning mobility restrictions to employees, this means finding a source of variation in labor mobility that is otherwise uncorrelated with capital investment and entrepreneurship. Second, it calls for observing mobility for a large and diverse set of workers, as well as information on their employment before and after moving.

To address the first challenge, I focus on a particular restriction on labor mobility, covenants not to compete (CNCs). CNCs are contract provisions that preclude employees from moving to, or establishing, a competitor for a period of time after leaving their employer. I rely on state-level variation in the enforceability of these contracts to tackle the endogeneity concern. Specifically, I use a novel identification strategy based on seven state supreme court rulings and one law that modified the enforceability of CNCs between 2008 and 2014. Court rulings provide a particularly useful empirical setting because courts are not subject to lobbying and other pressures in the way that legislators are, alleviating worries of a political explanation.[1] Indeed I find no evidence that the changes are anticipated or otherwise systematically associated with different types of workers, firms or political and economic environments in a way that would bias the results. Moreover, CNCs affect mobility for a large number of workers: Starr et al. (2016a) estimate that 18% of all labor force participants are currently subject to a CNC, with rates as high as 35% among tech workers and engineers. Thus CNC enforcement changes are not only an interesting setting in which to analyze mobility generally, but are important events themselves.

For the second challenge, measuring labor mobility, I also use a novel data source: the detailed de-identified employment histories of LinkedIn members.[2] A key advantage of these data is the presence of standardized position-level information such as occupation and seniority. This allows me to pinpoint workers and firms for which CNCs matter the most, namely those engaged in and relying on knowledge-intensive activities (Starr et al., 2016a). Moreover, I observe company-level information such as industry, year founded and size both before and after an employee move. As a result I am able to isolate moves to competitors and to new and small businesses, thus identifying departures to entrepreneurship. Another important advantage is that the data encompass a wide range of workers in all fifty states

[1] I find similar results when limiting the analysis to court rulings only.

[2] In 2015, LinkedIn awarded access to its database to a small number of researchers selected through a competitive process called the Economic Graph Challenge, of which I was a winner. The data contain no name information and numerical member identifiers in the data were hashed.

and foreign countries. Looking only at active LinkedIn members, I observe employment paths for 52 million workers in the U.S., or roughly one-third of the U.S. workforce.[3,4] I use founded years in company pages to capture the entry of new firms, in particular small new firms in knowledge-intensive industries.

I establish the internal validity of my approach by verifying that CNC enforcement has a significant impact on labor mobility. In my setting, an increase in CNC enforceability leads to a 2.6 percentage point drop in the departure rate. This drop is economically large, representing 24% of the average departure rate of 10.8%. The median firm retains 17 more workers every year, relative to the median size of 649 employees. As expected, declines are particularly pronounced for within-industry departures, and for departures to more senior positions, which proxy for moves that build on previous experience. I further focus on a subsample of knowledge-intensive occupations which I call "knowledge workers." Because of the knowledge involved in their occupations, the mobility of these workers is both more likely to be restricted by CNCs and to be costly to firms (Starr et al., 2016a). Indeed, I find that departure rates in these occupations are particularly affected by increases in CNC enforceability.

I estimate the economic impact of these changes in labor mobility by considering two sets of outcomes. First, I examine the impact of CNC enforceability on capital investment. If human capital is hard to replace and its relationship with physical capital is complementary – for example, expensive computers are worth acquiring if the firm can retain talented programmers – then tighter restrictions on labor mobility will increase the rate of capital investment. Consistent with this hypothesis, I find that in firms that are more highly dependent on human capital, the net capital investment rate rises. Knowledge-intensive firms increase investment by $5-9k for every $100k of capital, or $1.5-2.5 million for the median firm. This represents roughly $100-150k per marginal retained worker. For the median U.S. state, this translates to about a $50 million aggregate increase in capital

[3] According to the BLS, the size of the U.S. labor force was 158 million by the end of 2015.

[4] Active members are defined as members who have logged into LinkedIn in the past month.

investment coming from these firms.

Second, I analyze the consequences of mobility restrictions for entrepreneurship. I approximate experienced departures to entrepreneurship by counting departures to newly founded small businesses. I find that departures from knowledge-intensive firms to small new firms decrease by 0.65 following stronger enforceability of CNCs. This represents a large drop, 32%, relative to the average departures to entrepreneurship captured in my sample. In turn, entry of small knowledge-intensive firms declines by 16%. I use the Census Bureau's Business Dynamics Statistics to gauge the new firm entry coverage rate of my data, which I discuss in more detail in the paper. For the median state, I estimate 195 fewer small knowledge-intensive firms enter the market.

The results stand up to a range of robustness analyses. Throughout, I include industry-year fixed effects to account for industry conditions year to year, and firm fixed effects to allow for different firm-specific baselines whenever applicable. However, we may be concerned that unobserved local conditions drive both court rulings and mobility and investment outcomes. To address this I show that my findings are robust to including state-year fixed effects whenever possible – e.g., when I compare knowledge-intensive firms to the rest of the sample in a triple differences setting.[5] Similarly, I consider the possibility that a different representation of firm types (size, R&D intensity, investment) in different states could drive the results. My findings are unchanged when I interact year fixed effects with each of these observables, to allow for trends along these dimensions. Finally, I show that the responses I document occur *after* the court rulings, and are not anticipated, by breaking out the difference estimate by years from CNC enforcement change.

This paper contributes to three literatures. First, this paper builds on questions raised by Zingales (2000) about corporate finance in the context of firms' increasing dependence on human capital, and picked up in a small but growing literature relating frictions in human

[5] In the difference-in-differences specification, I cannot include state-year fixed effects because they would absorb the treatment indicator. In the triple differences specification, state-year fixed effects absorb the treatment indicator, but not the treatment interacted with the subsample.

capital to firm value (Chen et al., 2011; Eiling, 2013; Donangelo, 2014). More specific to my context, a few studies have begun relating these frictions to capital decisions (Autor et al., 2007; Garmaise, 2011). Garmaise (2011) finds that the capital expenditure to labor ratio decreases after CNCs become more enforceable in three states. In contrast, I find in a more recent context that investment increases with CNC enforcement, and argue that this is due to complementarities between human and physical capital in knowledge-intensive firms. Moreover, I build on this by quantifying the trade-off with entrepreneurship. My findings support the argument that control rights over human capital are a critical component of the theory of the firm, insofar as they influence whether investment opportunities are exploited within existing firms or as new ventures (Zingales, 2000). More broadly, this paper contributes to research on the drivers of capital investment and of entrepreneurship. [6]

Second, other papers studying the relationship between CNCs and employee departures include Marx et al. (2009) Marx (2011), Garmaise (2011), Lavetti et al. (2014), and Starr et al. (2015). [7] However, empirical work has been hampered by endogeneity concerns and a lack of detailed employee data on a large scale (Barnett and Sichelman, 2016). My paper proposes a novel identification strategy using staggered rulings in recent state supreme court cases, and uses a unique data set covering millions of employment histories to show these rulings systematically led to lower entrepreneurship in knowledge sectors. In the process, I also provide evidence that the impact of CNCs applies to a broad range of occupations, albeit with greater impact in knowledge-intensive sectors. Recently, Starr et al. (2016a) and Starr et al. (2016b) have advanced our understanding of CNCs by surveying labor force

[6] Other frictions which may influence investment include financing constraints (Fazzari et al., 1988; Cleary, 1999; Kaplan and Zingales, 1997), agency frictions (Jensen, 1986), short-termism (Graham et al., 2005; Ladika and Sautner, 2016; Edmans et al., 2013), corporate governance (Wurgler, 2000; John et al., 2008), costs and price uncertainty (Zeira, 1990), and access to public markets (Sheen, 2011; Asker et al., 2015; Gilje and Taillard, 2016). Other factors which may drive entrepreneurship are venture capital access (Samila and Sorenson, 2011), entrepreneurial clusters (Chatterji et al., 2013), and government policies (Hombert et al., 2014; Pfeiffer and Reize, 2000; Hellmann, 2007; Landier, 2004). At the level of the parent firm, knowledge transfer (Gompers et al., 2005; Klepper and Sleeper, 2005; Anton and Yao, 1995), financial distress (Babina, 2015), and compensation structure (Carnahan et al., 2012) constitute relevant factors.

[7] Babina (2015), Matray (2014) and Samila and Sorenson (2011) also reference CNCs, but only indirectly.

participants on the frequency and use of CNCs. I use their findings on CNC prevalence to define knowledge-intensive occupations.

Finally, this paper contributes to the discussion on labor mobility and business dynamism (Decker et al., 2014) by investigating a particular factor that impedes the reallocation of human resources in the economy. I show CNC enforcement has a large impact on firm entry, but also present a potential drawback of limiting CNC agreements. Quantifying this trade-off is particularly important in light of recent proposals to limit the use of CNCs at both the state and federal levels. While the intention is to spur dynamism, policy makers appear to ignore the potential impact on existing firms (The White House, 2016). In fact in my setting, mobility restrictions generate substantially higher capital investment at existing knowledge-intensive firms. CNCs are thus a form of private ordering, and any government intervention into this arrangement must justify reallocating resources from one set of agents (e.g. existing firms) to another set of agents (e.g. entrepreneurs). [8]

The remainder of the paper is structured as follows. Section 1.2 explains the CNC policy setting. Section 2.2 describes the data. Section 2.3 outlines the hypotheses and empirical approach. Section 2.4 presents the main results, and section 1.6 covers robustness. Section 2.6 concludes.

1.2. Empirical Setting: Covenants Not to Compete (CNC)

A covenant not to compete (also known as a non-compete agreement, hereafter CNC) is a contract provision between an employer and an employee that precludes the employee from working for a competitor, usually for a limited time and space after separating from the employer.

[8] Justifications could include inefficient externalities generated by the arrangement, or other frictions such as asymmetric information between the agents. I leave these questions open to future research.

1.2.1. Prevalence of CNCs

In a survey of more than 11,000 U.S. labor force participants, Starr et al. (2016a) find that 38% workers have at some time signed a CNC, and that 18% are currently subject to one. CNCs are more common for more knowledge-intensive occupations: They estimate that 39% of college-educated workers and those earning more than $100k have CNCs, and 35% of those in architecture, engineering or computer and mathematical occupations are currently subject to a CNC. Senior employees and in particular executives are also more likely to be subject to CNCs: Garmaise (2011) finds evidence of CNCs for top executives in 70% of the public companies he examines. Nonetheless, CNCs are prevalent across the board: Almost one in 10 employees without a Bachelor's degree and earning less than $40k annually is subject to a CNC (Starr et al., 2016a). Anecdotally, news organizations from the Wall Street Journal to the Washington Post and the Atlantic have reported on the increased prevalence of CNCs over the past 15 years, including in relatively unskilled positions such as sandwich-maker.[9]

1.2.2. CNC Enforceability

CNCs are governed at the state level, and there is wide variation across states in the type of CNCs that are permitted and how they are enforced. At one extreme, California bans the use of CNCs. At the other extreme, several states allow enforcement of CNCs even for employees who are laid off. Bishara (2011) identifies six broad dimensions of enforcement: whether a state statute exists, the employer's protectable interests, the plaintiff's burden of proof, whether CNCs can apply to terminated employees, consideration, and modification. Consideration refers to the requirement in contractual law that both parties must receive something in order for a contract to be valid. In the case of CNCs, many states consider continued employment in and of itself to be sufficient consideration; others require any new CNC or CNC amendment to be paired with a material benefit to the employee, usually a

[9] *The Wall Street Journal*, Feb. 2, 2016, "Noncompete Agreements Hobble Junior Employees"; *The Washington Post*, Feb. 21, 2015, "The Rise of the Non-compete Agreement, From Tech Workers to Sandwich Makers"; *The Atlantic*, Oct. 17, 2014, "How Companies Kill Their Employees' Job Searches".

promotion (Starr, 2016b).

Modification, also called reformation or blue/red pencil doctrine, refers to the way in which courts deal with overly broad restrictions. To illustrate this, Kenneth J. Vanko, an Illinois attorney who maintains a blog on CNC law, provides a useful example. Consider the following fictional CNC:

> "Employee agrees not to work in any sales capacity for any business competitive with the Employer for a period of six months in the following Illinois counties: Cook, DuPage and Kane."

Suppose the court considers this agreement overly broad because the employee only sold to customers in Cook County, and the agreement includes DuPage and Kane counties. If the state does not allow for any form of modification, that means the CNC is simply unenforceable. The court cannot modify it to apply only to Cook County. If the state allows a blue pencil approach, the court may strike out portions of the agreement to make it enforceable, though they may not make other changes. So in this case, they can strike out DuPage and Kane counties to make the agreement enforceable. However, if the employee worked in product development rather than sales, the court cannot use the blue pencil to modify the prohibited activity ("any sales capacity"). They would need a "red pencil", or reformation, to modify the language to match the occupation.[10] A consequence of the reformation approach is that employers have an incentive to draft CNCs as broadly as possible, knowing that an overly broad contract will not disqualify their case but that it may dissuade employees from a broader range of activities (Thomas et al., 2014).

Building on these six dimensions identified by Bishara (2011), Starr (2016b) constructs an index of CNC enforceability for 1991 and 2009. In Table 4, I show that treated and control states were at similar levels of CNC enforceability in 2009.

[10]The example and full discussion are available at http://www.non-competes.com/2009/01/quick-state-by-state-guide-on-blue.html

1.2.3. Changes in Enforceability

CNCs are governed by both statute and precedent, meaning that a case is determined by both the law that the state has in place, and the rules established by the state's highest court. Table 1 outlines the CNC enforcement changes that I use for my empirical setting. I identify seven state supreme court decisions between 2009 and 2013 by combing through practitioner blogs and verify all cases using Westlaw.[11] For each case, I also verify what local attorneys wrote about the expected impact of the decision. If the decision took place in the last three months of the calendar year, I assign the following year as the relevant first year of change. Court decisions are retroactive in the sense that they affect the enforceability of CNCs entered into before the decision was issued, as well as new CNCs.

Two states merit further note. In Illinois, the state supreme court's decision to expand the scope of business interests in Reliable Fire Equipment v. Arredondo et al was followed the next year by an appellate court decision (which the state supreme court declined to revisit) that decreased the enforceability of CNCs. To reflect this, I code Illinois as an increase in 2012 but no longer an increase in 2013.[12] In Montana, the decision concerns the applicability of CNCs to terminated employees and is therefore a more narrow change. I verify that my results are robust to excluding Montana.

Finally, during my sample period, Georgia passed a law allowing modification. Laws may be more likely anticipated or lobbied for than court decisions, creating a setting in which the outcome variables of interest are possibly endogenous to the shock. The statute change is not retroactive: employers must secure new CNCs with their employees in order to benefit from modification. While there is no evidence of pre-trends in my outcome variables for Georgia, I also verify that all of my results are robust to removing the state.

[11]The highest court in a given state is not always called the supreme court, but in the states I discuss state supreme court is equivalent to state high court.

[12]For a discussion of this reversal, see *The National Law Review*, Oct. 22 2013, "Non-Compete Agreements: Lessons from Illinois Courts."

1.2.4. State Supreme Court Composition

Table 2 shows the composition of the state supreme courts for these states. On average, the courts are composed of seven justices serving terms of 10 years. Of these, only the Montana, Texas and Wisconsin justices face contested re-election. They were on average six years from re-election at the time of their decisions, suggesting their rulings were unlikely to be affected by immediate re-election concerns. In Colorado and Illinois, justices face uncontested retention re-elections, and in South Carolina and Virginia, justices are re-appointed by the state's general assembly. In these four states justices were on average seven years from re-appointment at the time of their decisions.

The "treated" states constitute a geographically and economically diverse group of states. Figure 1 shows the geographic location of these states. In Section 2.3, I show these states are similar to the control states in terms of ex-ante political and economic characteristics. In untabulated results I also verify that *trends* in these dimensions are similar in the treated and control group in the years leading up to the decisions.

1.2.5. Impact on Mobility

The main mechanism by which we expect CNC enforcement to dissuade labor mobility is deterrence, but this can manifest itself in several complementary ways. First, higher enforceability can mean that there are more cases brought to court, and more cases where an employee is found to be at fault, which can discourage employees uncertain about leaving. Figure 2 shows that since 2008, courts have decided 900-1,000 CNC cases every year. This number does not include cases that were settled, not to mention disputes that did not reach courts. Second, employees who seek legal advice about a potential move will be counseled differently following a change in enforceability. For example, following Illinois' decision in Reliable Fire, attorneys may counsel employees that the risk of litigation for a CNC breach has increased.[13] Third, employers can and do remind workers of their obligations.[14] Fourth, prospective employers refrain from hiring workers who are likely subject to CNCs.[15] Fifth, in the case of modification, employers have incentives to draft broader CNCs going forward (Thomas et al., 2014). Finally, there could be a peer effect as well: As fewer colleagues move to or establish a new firm, this could reduce the impetus for an employee to follow that path.

I assign firms to the state in which most employees reside, according to LinkedIn. If deterrence is the main mechanism through which enforcement impacts employee mobility, what matters is the environment to which employees are exposed. For prosecution of CNCs, the relevant jurisdiction is typically the place of performance of the economic activity, though where the contract was formed and the parties reside may also enter into consideration (Lester and Ryan, 2010).

1.3. Data

1.3.1. LinkedIn data

In 2015, LinkedIn selected a small number of researchers to be part of the Economic Graph Challenge, an initiative to harness LinkedIn's data to gain new economic insight. As a winner of the challenge, I was granted access to detailed de-identified data from LinkedIn's platform. The data contain no name information and numerical member identifiers in the data were hashed.

LinkedIn is an online professional networking platform, which began in 2003 and has since grown to over 450 million users worldwide. For this paper, I use employment histories for active members in the U.S., i.e. employees who have been on the site in roughly the past

[13] *The National Law Review*, Dec. 9 2011, "Illinois Supreme Court's Decision in Reliable Fire Broadens Enforceability of Restrictive Covenants."

[14] See Kenneth Vanko's blog, www.non-competes.com, Dec. 31 2010. "It is fairly standard now for any departing employee to receive a not-so-friendly reminder from an ex-employer about the terms of a non-compete agreement."

[15] For an example, see *The Wall Street Journal*, Feb. 2, 2016, "Noncompete Agreements Hobble Junior Employees."

month. About 52 million employment histories fall into this category during my sample period, or roughly one third of the US workforce.

Users' profiles are essentially online CVs, listing where they have worked and in what capacity, as well as where they went to school and other interests. These are the data which I use to track employee movements. Note that an individual did not have to be a LinkedIn member in a given year to have employment information for that time. LinkedIn standardizes employer information, so that users are made to select an existing employer if at all possible. Standardized employer information includes industry, year founded, size and employer type (e.g. private company, government entity). The platform also standardizes position-level information such as occupation and seniority level. This information allows me to further focus on within-industry moves, as well as pinpoint employees in knowledge-intensive occupations.

I match firms in LinkedIn to publicly-held firms in the Compustat database in order to observe firm investment, and to link to observable characteristics such as size and NAICS industry. Figure 3 shows the coverage of LinkedIn by sector for this matched sample. I calculate the ratio of active members employed in each sector in 2014 to the number of employees reported on Compustat for the same firms. The aggregate coverage rate is 30%. The figure indicates that my sample over-represents knowledge-intensive sectors. However, that is precisely the population in which I am most interested. First, these sectors represent occupations that are most likely to be affected by CNCs (see Figure 5). Second, companies which employ knowledge workers likely depend more on human capital for their production function.

We may be concerned that individuals lie about their past employment history. However, unlike lying on a resume that only a prospective employer will see and cannot easily verify, lying on a LinkedIn profile is publicly visible. That public accountability makes it more difficult for individuals to make false claims about their employment. Alternatively, we may be concerned about individuals who forget to update their profile. For this reason I use data only from active members. Moreover, the data come from a 2016 snapshot of employment histories, which leaves time for individuals to have updated their 2008-2014 employment histories, particularly if they are active on the site.

An individual counts as employed at company A in year t if she lists a job at company A at any time during year t. She is considered as a departure in year t if she is no longer employed at company A in year t+1. The departure rate of a firm is its number of departures scaled by its employee size. I use the Compustat number of employees rather than the LinkedIn count of employees to scale departures, in order to avoid any biases that could come from LinkedIn disproportionately representing employees in transition. Results are similar if I use LinkedIn number of employees as the denominator instead.

Table 3 Panels A and C contains summary statistics for the departure analyses at the firm-year level. In Panel A, all variables are scaled by the same number of employees, so the total departure rate is mechanically higher than all other departure rates. The average departure rate is 10.80% (median 4.37%), and the average knowledge worker departure rate is 4.8% (median 1.63%). In other words, knowledge workers represent about 40% of all departures. Panel C shows unscaled firm-level departure numbers to newly founded firms. The sample is slightly bigger than in Panel A because the Compustat employee variable is not required to be populated. For the average firm-year in my sample, 1.56 LinkedIn members leave to a newly founded firm with 50 or fewer employees (median 0).

I also use LinkedIn company pages to measure the entry of new firms. Using year founded, location, industry and size, I create an industry-state panel of new firms founded. I restrict the data to private companies to avoid capturing spin-offs and mergers. Since these are private firms, I use the industry classification provided within LinkedIn, which contains roughly 130 unique industries.[16] To make numbers comparable across states, I scale the number of newly founded firms by population. In Table 3 Panel D, I show summary statistics

[16] Not all industries are represented in all states. For the typical state there are 94 unique industries represented in the new firm data.

for both scaled and unscaled observations, for easier interpretation. The average number of firms of any size entering per year per industry is 2.53, while the median is 1. Notably, the number of small firms entering is not much lower, confirming that the data are not capturing disproportionately large firms.

1.3.2. Investment data

I use Compustat data to measure the investment response of firms. To avoid bias from mergers or acquisitions, I exclude firm-year observations with more than 100% growth in sales or assets. I also remove financial and regulated industries, and exclude observations with missing stock market data.

I define net investment as capital expenditures less the sale of property (Compustat capxv - sppe), and scale by one year-lagged net capital (Compustat ppent) to obtain the investment rate, I/K. I winsorize the top and bottom 1% and exclude observations with less than 0.5 million in net capital. These steps are to ensure that my sample is focused on the most relevant observations and that my estimates are not driven by outliers, but my results are robust to less stringent cleaning as well. I choose to scale investment by net capital because I think of the firm's decision on investment opportunities as occurring in each period, conditional on the depreciated stock of capital. However, results are qualitatively similar when I scale by gross capital or assets instead.

Table 3 Panel B presents summary statistics for this sample. The average net capital investment rate is 0.30, and the median 0.22.

1.4. Hypotheses and Empirical Approach

1.4.1. Hypothesis Development

Labor Mobility and Firm Investment

Consider a typical firm with two inputs to production: physical capital and human capital. Workers become more valuable to the firm as they gain more tenure, either because they learn firm-specific expertise on the job, or because they gain general skills and search frictions make replacement difficult (or both). In this case, making it easier for employees to leave reduces tenure and, as a result, the accumulation rate of human capital. Moreover, it can be hard to properly gauge the quality of prospective hires. If that is the case, high turnover can also increase uncertainty about the quality of human capital tomorrow. An alternative way to frame this is to think of the depreciation rate of human capital. In the setting I propose, decreasing labor mobility (increasing CNC enforceability) is akin to lowering the average and volatility of the depreciation rate of human capital, because human capital accumulates more quickly and the quality of human capital tomorrow is less uncertain.

We expect the firm's investment into human capital to increase, because returns to this investment go up when workers are leaving less readily. However, the impact on investment into physical capital depends on whether the two inputs are complements or substitutes. If physical and human capital are complementary – say, because expensive equipment requires skilled labor to operate – then physical capital investment will increase with the stability of human capital. If they are substitutes – say because the tasks performed by human capital can be automated – then physical capital investment will decrease with labor mobility.

As this example highlights, it is possible that the nature of the relationship between both inputs depends on the type of human capital. Specifically, occupations that are less knowledge-intensive may be easier to automate, and thus have lower complementarity with physical capital than more knowledge-intensive occupations, such as engineering or design. Not coincidentally, CNCs are more common in occupations that are knowledge-intensive, since their goal is to preserve valuable human capital. Figure 5 shows the incidence of CNCs by occupation from Starr (2016a). I expect firms that employ more workers from high-CNC occupations to have a greater positive response to CNC enforcement increases, both because they are more likely to employ CNCs and because their human capital may be less substitutable.

Labor Mobility and Entrepreneurship

Just as CNCs preclude individuals from moving to a competitor, they preclude individuals from establishing a competitor. Thus in a very direct sense, an increase in the enforceability of CNCs should preclude some individuals from leaving to join or start a new firm. However, the impact on overall firm entry is less direct. For example, it could be the case that investment opportunities no longer pursued by employees subject to CNCs will be seized by individuals not limited by these agreements (e.g. college students). At the same time, most entrepreneurship emanates from ideas encountered in previous employment (Bhide, 1994), and experienced entrepreneurs tend to be more successful (Franco and Mitchell, 2008). This supports the hypothesis that new firm entry will also decline in response to stronger enforceability of CNCs. Finally, I expect more knowledge-intensive firms to have a greater response because they are more likely to be concerned by changes in the enforceability of CNCs.

1.4.2. Methodology

I use a generalized difference-in-differences approach with CNC enforcement changes as my treatment of interest. The specification is as follows, for company i in industry j, state s

and year t:

$$y_{ijst} = \alpha + \beta(treated_s * post_t) + \gamma_i + \theta_{jt} + \epsilon_{ijst}$$

where $treated_i * post_t$ is 1 for an increase in enforceability relative to 2008, 0 for no change, and -1 for a decrease in enforceability.[17] I include company fixed effects γ_{it} and industry-year fixed effects θ_{jt} in all regressions, with industry defined as four-digit NAICS code. I cluster all errors at the state level. I do not include time-varying firm controls such as firm size, because such controls may be affected by the treatment. If that is the case, including these controls would result in inconsistent estimates of the treatment effect. However, in unreported analysis I find similar results when include log market capitalization, assets, and employee size.

The main assumption underlying this approach is that absent the CNC enforcement changes, the average change in the treated and control groups would have been the same (the two groups would have experienced parallel trends). The coefficient estimate on $treated_i * post_t$ captures the additional change in treated states, relative to untreated states, following a change in CNC enforcement.

In Figure 4, I show that there was no anticipation or differential trend between both groups prior to the treatment, in terms of departures. I estimate the following equation:

$$\text{departure rate}_{ijst} = \alpha + \sum_k \beta_k(treated_s * \text{k years to treatment}) + \gamma_i + \theta_{jt} + \epsilon_{ijst}$$

Figure 4 plots a time series of the coefficient estimate on $treated_s * k\ years\ to\ treatment$ against *k years to treatment*. Prior the enforcement change, the estimated difference between the treatment and control groups is virtually zero. However, following the change in enforcement, the departure rate in the treatment group drops significantly relative to the

[17] I make this symmetric assumption for convenience. Most of the changes that I use have a similar weight in the enforceability indexes constructed by Starr (2016b) and Bishara (2011), so this seems to be reasonable. An exception is Montana, where the court ruling affects only terminated employees. However, since relatively few firms are located in Montana, this is not a driver of my results.

departure rate in the control group.

In Table 4, I compare relevant characteristics for eventually treated and control observations, prior to the start of my sample. Having the same ex-ante levels of observables is not a necessary condition for the identifying assumption – trends can be at different levels as long as they are parallel – but similar ex-ante characteristics reinforce the assumption that the average change in both groups would have been the same absent treatment. There are four columns of statistics in Table 4. The first shows the ex-ante mean for states in which CNC enforcement does not change in my sample period (Never Treated). The second shows the ex-ante mean for all states in which CNC enforcement changes during the sample period (Eventually Treated). I then break this group into two groups, based on whether CNC enforcement increases or decreases. The last group contains relatively few observations, as only Montana and South Carolina experience decreases in the enforceability of CNCs. Results are robust to excluding these two states.

Looking at local political and economic conditions, treated and control states are not statistically different, although Montana and South Carolina have lower average GDP per capita and are more conservative than the rest. States in which CNC eventually increases are very similar to control states. Although not shown, trends in GDP growth, GDP per capita and unemployment rate are also similar between both groups. The same pattern emerges when looking at measures of mobility and firm characteristics. Observations for which CNC enforceability eventually increases are very similar to control observations. Observations in Montana and South Carolina have a lower departure rate, though it is not statistically different. They are smaller in terms of market capitalization and larger in terms of employees. I verify that results hold when I exclude South Carolina and Montana observations. Moreover, in Section 1.6 I verify that results are robust to allowing for different trends by market, asset and employee size.

Finally, using Starr et al. (2016a) I separate out occupations that are most likely to be affected by CNCs. Figure 5 reproduces the incidence of CNCs by occupation found in their survey. I call individuals in the highest CNC occupations knowledge workers. They encompass the following occupations within the LinkedIn classification: arts & design, business development, consulting, education, engineering, entrepreneurship, finance, information technology, media & communication, operations, product management, program & project management, and research. Knowledge workers represent close to half of the workers for the average firm in my sample. Knowledge *firms* are firms which employ a greater than median fraction of knowledge workers. I use this classification and high R&D intensity as ways to proxy for firms that are more dependent on human capital.[18]

1.5. Results

1.5.1. Employee Mobility

To verify that CNC enforcement impacts mobility, I first look at departure rates to determine whether CNCs have an impact on employee mobility. Table 5 shows results from a difference-in-differences regression of departure rate on an indicator for increased CNC enforceability. The sample universe is public companies matched between LinkedIn and Compustat. Following an increase in CNC enforceability, the departure rate drops by 2.6 percentage points, which represents 24% of a 10.8% average departure rate. While large, this number is consistent with survey evidence from Starr et al. (2016b).[19] As expected, this effect is pronounced for within-industry moves, which drop by 1.4 percentage points relative to a 3.6% average within-industry departure rate. To better focus on voluntary departures, I look at employee moves where the next position is at a higher seniority level. Again, increased enforceability of CNCs leads departures to more senior jobs to decrease substantially, by 85 basis points relative to an average rate of 2.68%. This is even more pronounced when I focus on within-industry departures to more senior positions, which drop by 31 basis points relative to an average rate of 0.78%.

[18] More than 50% of R&D is wages paid to research activities (Hall and Lerner, 2010), suggesting R&D intensity is a reasonable proxy for a firm's dependence on skilled human capital.

[19] In their survey of over 11,000 labor force participants, they find that the projected departure rate to a competitor is about 2.5 percentage points, or 20% lower for individuals who are subject to CNCs.

In Table 6, I show that these results are driven by workers in knowledge occupations by estimating difference-in-difference-in-differences (or "triple differences") regressions. These regressions compare the impact of higher CNC enforceability on all occupations to the impact on the subsample of occupations that Starr et al. (2016a) identify as more prone to CNCs. The coefficient estimate on *treated*post*knowledge worker* represents how much more knowledge occupations respond to the CNC enforcement changes. For all categories, this estimate is negative. It is strongly statistically significant for overall, within-industry, and "to more senior job" departure rates, though not for the last, most narrow category, which counts only departures to more senior positions within the same industry.

Taken together, these results indicate an important economic impact of increased CNC enforceability. For an average firm size of 4,044 employees, 2.6 percentage points represent 106 individuals. Since firm size is right-skewed, a perhaps more relevant statistic is that the median firm with 649 employees retains about 17 workers more than it would have otherwise.

1.5.2. Firm Response

Having established that CNC enforceability represents a shock to human capital mobility, I turn to the firm's investment response. In Table 7, I show that tighter restrictions on labor mobility consistently lead to a higher investment rate. Following the discussion in Section 1.4.1 and the results in Section 1.5.1, I expect that the response will be more pronounced in firms that are more dependent on specialized human capital. To test this, I run two triple differences using different proxies for knowledge-intensive firms: firms with a higher than median fraction of knowledge workers (knowledge firms), and firms with a higher than median R&D intensity.

The results show that I/K increases substantially with CNC enforceability, and that this is driven by firms that are more dependent on specialized human capital. Specifically, the results indicate that overall, firms increase investment by \$6k for every \$100k of net capital.

For knowledge firms, the estimate is \$9k for every \$100k of net capital, and for firms with high R&D intensity, \$5k (additional to other firms). At first, the effect appears extremely large: Comparing to the average investment rate, the coefficient estimates imply an increase of 21%. In knowledge firms, the estimated additional increase is 26% relative to an average I/K of 0.35, and in high R&D intensity firms it is 17% relative to an average I/K of 0.32. This suggests a direct elasticity with departure rates, which experience an approximately commensurate relative decrease (24%). However, the estimates are best understood in an economic context. Multiplied by the average net capital stock, the estimates indicate \$17 million increase in investment in knowledge firms and a \$12 million increase in high R&D intensity firms. Put differently, the median knowledge firm increases investment by \$2.6 million, and the median R&D intensive firm by \$1.6 million.[20]

Relating back to the results in 1.5.1, this is equivalent to roughly \$93-164k per marginal retained worker, whether we use averages or medians. These estimates seem to be within a reasonable magnitude when we take into account the replacement cost of skilled employees. At an aggregate level, my sample contains 83 knowledge firms and 104 high R&D intensity firms in states which experience an increase in CNC enforcement. Multiplied by the average estimated investment increase per knowledge-intensive firm, this implies an aggregate increase of \$1.3-1.4 billion in investment from publicly-held knowledge-intensive firms in those states. For the median U.S. state, it represents a \$50 million increase.

1.5.3. Entrepreneurship

If knowledge firms gain in investment opportunities, what happens to growth opportunities outside of the firm? One of the main costs to CNCs cited in the literature is that fewer experienced employees leave to start their own businesses. This is especially important if firms started by experienced, skilled employees tend to be more successful than other new firms (Franco and Mitchell, 2008).

[20] The median net capital for knowledge firms is \$28.49 million, and for high R&D firms is \$29.88 million. The average net capital is \$192 million for the former and \$229 million for the latter.

I proxy for entrepreneurship by looking at departures to newly founded, small businesses. Specifically, a business is new if it is founded within a year in either direction of the employee's departure. Founded year comes from LinkedIn company pages, as does firm size. Firm size does not depend on the number of employees present on LinkedIn, but is a category on the company's page. A limitation of this approach is that I observe only the latest size: any company that has grown significantly since it was founded will not be captured as small.

In Table 8, I look at the total number of departures to newly founded businesses in three categories: 1-10 employees, 1-50 employees, and all sizes. I look at total departures rather than departure rates in this analysis because I am interested in the overall impact on entrepreneurship rather than the firm's loss. I focus on departures from knowledge firms as in the previous section and following the discussion in Section 2.3. Results are similar in a difference-in-differences where the left hand side is total departures of knowledge workers.

The results suggest that an increase in CNC enforceability is particularly discouraging for workers from knowledge firms leaving to start or join small new firms. Indeed, the coefficient estimates suggest an incremental 40% fewer departures to new firms with 10 or fewer employees and 32% fewer departures to new firms with 50 or fewer employees, relative to averages of 1.05 and 2.03 departures in those categories for knowledge firms. In Section 1.5.1, I estimated that the median firm retained an incremental 17 workers (106 on average). The estimates here indicate that about up to 4% of these marginal retained workers also did not become entrepreneurs.

In Table 9, I turn to the number of new businesses founded per industry-year. Again, I find a more pronounced effect in knowledge sectors, defined here as firms in technology, professional, scientific and technical services, or education.[21] I scale by state population to make the numbers comparable, but this makes estimates difficult to interpret directly. In relative terms, the estimates suggest that the knowledge sector experiences an 11% decrease

[21] I use Starr et al. (2016a) as a reference for industries in which CNCs are more frequently used.

in new firms entering with 10 or fewer employees and a 16% decresase in new firms entering with 50 or fewer employees, following higher CNC enforceability. To better understand the economic interpretation of the results, I break them out by sector in Table 10, for new firms with 50 or fewer employees. New small firm entry declines 6% in the technology sector, 12% in professional, scientific and technical services, and 27% in education. Multiplied by the number of industries in each sector, the estimates imply 5, 13, and 2 fewer new firms in my sample (for each sector in turn) per 10 million people. However, this very likely underestimates the total impact on new firm entry, since my sample only captures private firms for which I observe year founded on LinkedIn.

As a point of comparison, I download the number of opening establishments for new firms from the Business Dynamics Statistics (BDS) database of the Census. The comparison is meant as a back-of-the-envelope calculation. It is imperfect because I am comparing establishments to firms (the database does not provide the count of new firms, only establishments belonging to new firms), and because other aspects of the datasets do not line up exactly. Nonetheless, if we assume most new firms have a limited number of establishments in their first year and are willing to make other assumptions, this serves as a useful reference. BDS statistics are aggregated in broad sectors, including an "education and health services" sector and a "professional and business services" sector. None of the other sectors line up with the technology sector, so I infer a coverage rate from the professional and business services sector comparison. According to the BDS, about 11,000-15,000 establishments belonging to new firms in the education and health services sector opened each year between 2008 and 2014, in the treated states. For professional and business services, this number was 15,000-19,000. In contrast, 200-300 new education and health services firms entered my LinkedIn sample each year during the same period, and 700-1,100 new professional, scientific and technical services firms did the same.

Overall, I estimate a coverage rate of about 5% for the latter group (and by extension, for the technology sector), and a coverage rate of about 3% for the former. In turn, this suggests that for every 10 million people the total impact of increased CNC enforceability would be 103 fewer new technology firms, 251 fewer new professional services firms, and 82 fewer new education firms. Aggregated over states in my sample which experienced an increase in CNC enforceability, this represents 2,900 foregone entrants. For the median state with 4.5 million individuals, this represents 195 fewer small firms entering knowledge sectors. Relative to the estimated $50 million investment increase for the median state, this would place the trade-off at $250,000 in investment per non-entrant.

1.6. Robustness

1.6.1. State-Year Fixed Effects and Size Trends

One potential concern is that unobservable local economic or political trends differed in states that experienced CNC enforcement changes, relative to states that did not. To address this concern I repeat my investment and departures to entrepreneurship analysis with state-year fixed effects. This is possible because I am comparing results for knowledge-intensive firms to results in the overall sample in a difference-in-difference-in-differences setting. By including state-year fixed effects, I am identifying the response of knowledge-intensive firms in a given state relative to non-knowledge-intensive firms in the same state. State-year fixed effects absorb the $treated * post$ indicator.

Table 11 reports the result of these regressions for firms' investment response. Whether knowledge-intensive firms are identified as knowledge firms or high R&D intensity firms, the estimate on increased CNC enforceability is positive and statistically significant at a 90% confidence level. The estimated magnitudes are lower than in Table 7, but still relatively high: for the average firm, the estimates indicate an $11 million increase in investment, and for the median firm, a $1.5 million increase. The implied investment per marginal retained worker remains around $100k.

Table 12 reports the results for departures to entrepreneurship. The coefficient estimates for departures to new small firms remain negative and statistically significant. The point estimates are slightly lower than without state-year fixed effects, but remain high, representing about a 35% decrease relative to average for departures to 50 or fewer and 10 or fewer new firms. In turn, Table 13 shows that estimates are largely unchanged when I add state-year fixed effects to the triple differences regression with new firm entry.

In Table 14, I also show that investment results are not driven by differential trends for firms with high market capitalization, asset size, employee size, or investment rate. The classification for each firm is, of course, static for the sample period. The point estimates remain very similar to the results in Table 7, indicating that differential trends are not a driver of these results.

1.6.2. Years to CNC Change

To verify that the results are driven by the CNC enforcement changes, I estimate the coefficient on $treated*post$ separately for each year from the CNC enforcement change, including one year prior to the change. Tables 15-18 present the estimates of these regressions for my main results. The omitted years are two or more years prior to the CNC enforcement change, to allow for enough observations to be in the omitted category as a comparison point. I pool observations more than three years post change, since only Wisconsin observations can fall into that category.

The results show no evidence that differences between the treated and control groups preceded the CNC enforcement changes in any of the situations. For departure and investment rates, the point estimates change sign in the year of the change but become statistically significant one year following an enforcement change. The estimates for departures to entrepreneurship also become statistically significant in t+1, yet the impact on new firm entry appears to be statistically significant from t+0. This could be explained by the fact that entrepreneurs sometimes wait until their venture is off the ground before quitting their "day job."

1.7. Conclusion

Recent research and policy proposals have renewed the debate over labor mobility restrictions. In particular, one issue that has received a lot of attention is the enforcement of covenants not to compete (CNCs), mostly for its potentially negative effects on knowledge spillovers and entrepreneurship (Samila and Sorenson, 2011; Matray, 2014). However, CNC enforcement may be an important tool for firms to safeguard capital investments. Establishing and quantifying this trade-off is especially important in light of recently proposed regulation (The White House, 2016) that appears to miss the latter effect.

In this paper, I consider the impact of CNC enforceability on two outcomes: entrepreneurship and capital investment. To address concerns about unobservable differences biasing cross-sectional results, I identify a series of recent state supreme court rulings that changed the enforceability of CNCs in various states. I combine these with detailed data on employee movements from LinkedIn's wide-reaching database of employment histories. This allows me to pinpoint workers in knowledge-intensive occupations, as well as departures to newly founded small businesses to capture entrepreneurship.

I find that changes in the enforceability of CNCs lead to substantial effects on both entrepreneurship and investment. The effects are particularly pronounced in knowledge-intensive occupations, where the average departure rate drops by a quarter. The median knowledge-intensive firm increases its investment rate by an estimated $2-2.5 million annually. At the same time, the rate of entry of new, small firms in knowledge sectors declines by 16% relative to average.

These results point to an important trade-off of labor mobility, between encouraging the entrance of new firms on the one hand and investment at existing firms on the other hand. While the magnitudes are difficult to quantify, my estimates place the trade-off at around $50 million in capital investment against 200 foregone small entrants for a state of median size. A limitation of this study is that I cannot quantify the value of the marginal new firms or of the marginal investment, and I cannot capture all welfare-relevant outcomes. Nonetheless, this paper contributes to the existing literature by painting a more complete picture of labor mobility and policies that regulate it, and opens the door for future inquiries to refine quantitative estimates of the trade-offs involved.

Figure 1: Map of CNC Enforcement Changes

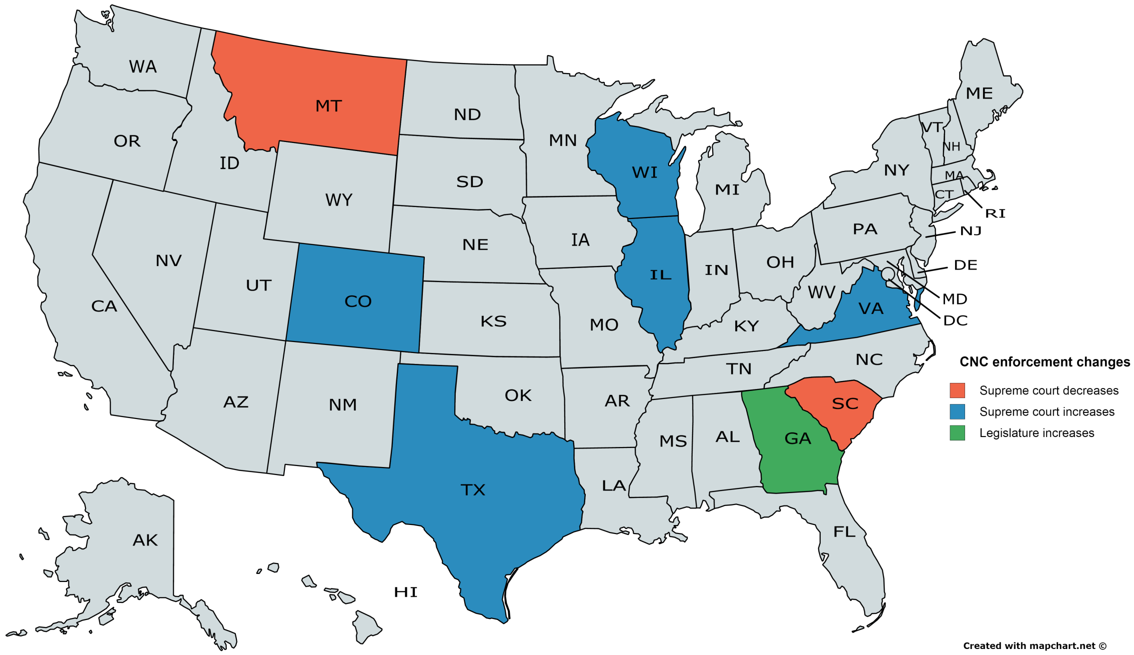
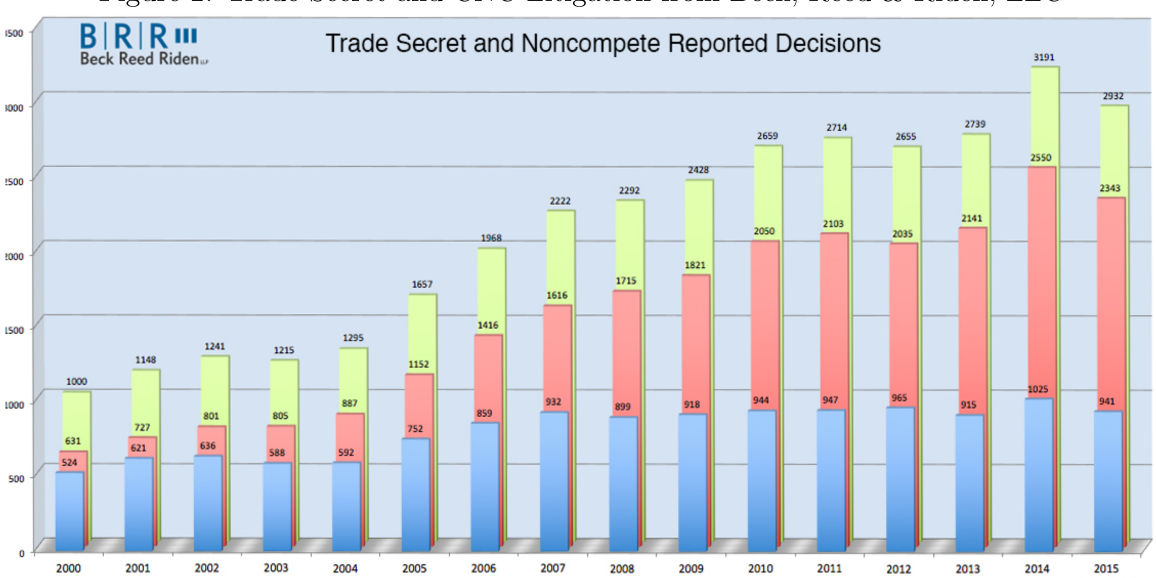


Figure 2: Trade Secret and CNC Litigation from Beck, Reed & Riden, LLC



Source: Beck, Reed & Riden, LLC, Jan. 11 2016

Figure 3: LinkedIn Coverage by Sector

This figure presents an estimated coverage rate for firms in my LinkedIn-Compustat merged sample. I divide my sample firms by Global Industrial Classification (GIC) sector, and take the total count of individuals employed in each sector according to LinkedIn in 2014, divided by the count of employees from Compustat in 2014.

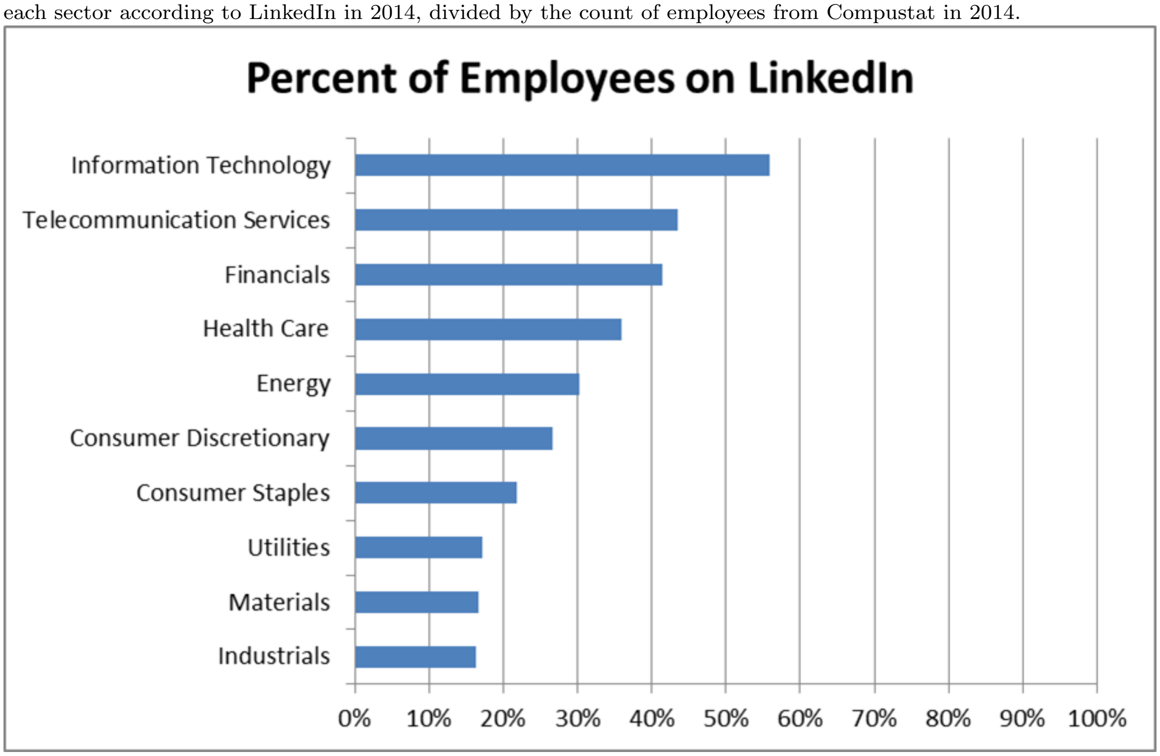


Figure 4: Difference in Departure Rate by Years to Treatment.
This figure presents the coefficient estimate β_k from the equation below, against years to treatment k. The estimate represents the difference in departure rate between the treated and control observations, before and after the change in enforcement of CNCs.

$$100 * \frac{\#\text{ departures}}{\#\text{ employees}}_{it} = \alpha + \sum_k \beta_k \{\text{treated}_i * \text{k years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

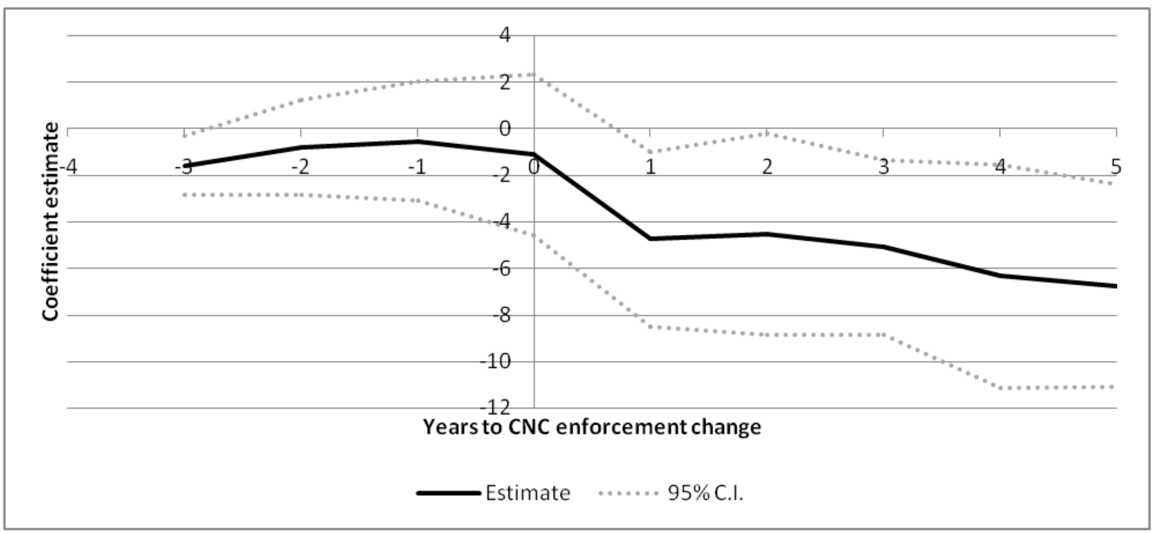
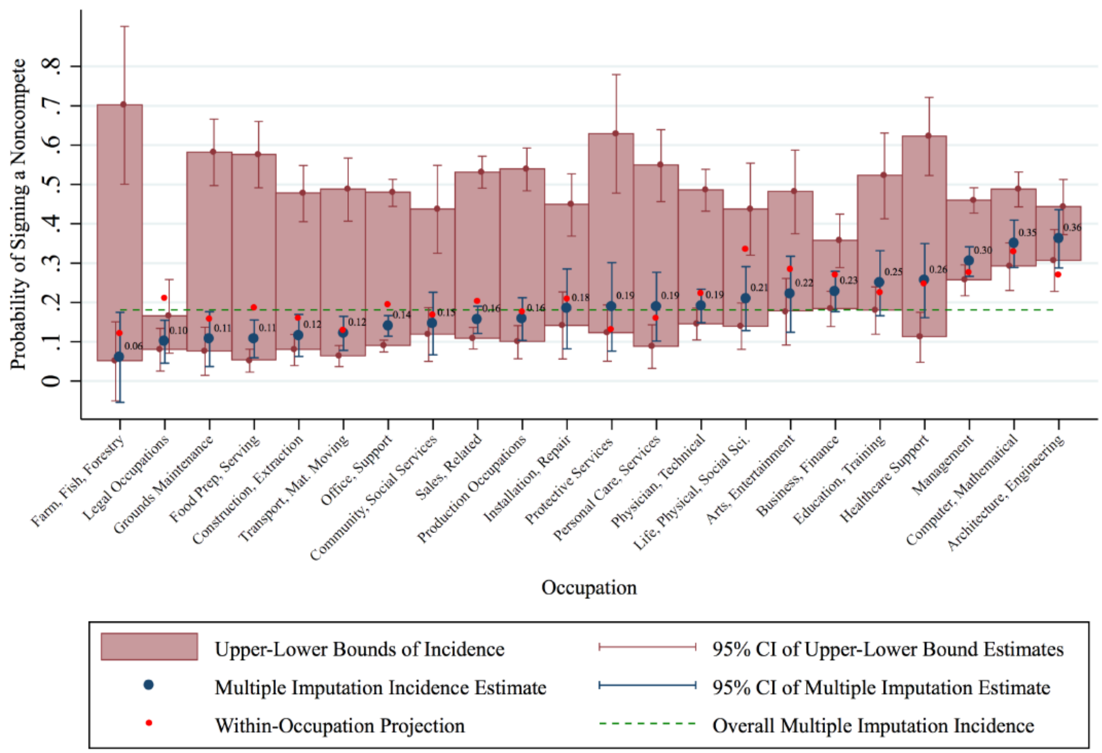


Figure 5: Incidence of CNCs by Occupation from Starr et al. (2016)

This figure comes from Starr et al. (2016a). In this paper, I consider knowledge workers to be those in the right-most occupations, starting from Life, Physical, Social Sciences.



The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.
The projections refer to within-occupation average of the projected proportion of noncompete signers.

Table 1: CNC Enforcement Changes

State	Case	Enforcement Direction	Nature of Change
Wisconsin	Star Direct, Inc. v. Dal Pra. (2009)	↑	Supreme Court allows modification
South Carolina	Invs, Inc. v. Century Builders of Piedmont, Inc. (2010)	↓	Supreme Court rejects modification
Colorado	Lucht's Concrete Pumping, Inc. v. Horner (2011)	↑	Supreme Court allows continued consideration
Texas	Marsh v. Cook (2011)	↑	Supreme Court changes requirements on business interests
Montana	Wrigg v. Junkermier (2011)	↓	Supreme Court rejects application to terminated employees
Illinois	Fire Equipment v. Arredondo et al (2011)	↑	Supreme Court expands the scope of interests
Illinois	Fifield v. Premier Dealer Services (2013)	↓	Supreme Court restricts standards
Virginia	Assurance Data Inc. v. Malyevac (2013)	↑	Supreme Court reduces automatic dismissals
Georgia	2011	↑	Legislature allows modification

Table 2: Composition of State Supreme Courts

State	Composition	Appointment	Mean Years from Term End
Colorado	7 justices who serve 10 year terms	Uncontested retention elections after initial appointment	5.25
Illinois	7 justices who serve 10 year terms	Uncontested retention elections after initial contested partisan election	5.57
Montana	7 justices who serve 8 year terms	Nonpartisan election	4.17
South Carolina	5 justices who serve 10 year terms	Elected and re-appointed by SC General Assembly	8.00
Texas	9 justices who serve 10 year terms	Partisan election	6.00
Virginia	7 justices who serve 12 year terms	Elected and re-appointed by VA General Assembly	9.40
Wisconsin	7 justices who serve 10 year terms	Nonpartisan election	6.57

Table 3: Summary Statistics

This table reports summary statistics for key variables. In Panel A, I include all observations for the sample of LinkedIn firms merged with Compustat, including for firms in financial, regulated and construction industries, and observations with missing stock market or asset data. The only requirement is that the Compustat employee variable is not missing, to define the departure rate. In Panel B, I exclude firms in financial, regulated and construction industries, as well as observations with missing stock market or asset data, because that portion of the analysis focuses on net investment rate. In Panel C, the sample is the same as for Panel A, without the requirement that the employee variable be populated. In Panel D, the observations are at the industry-state level, the number of newly founded firms in LinkedIn. The sample period for all is 2008-2014.

Variable	Obs	Mean	SD	25%	50%	75%
Panel A: Departure Analysis						
Departure rate	9,479	10.80	138.98	1.56	4.37	9.69
Within-industry departure rate	9,479	3.59	56.20	0.24	1.00	2.74
Knowledge worker dep. rate	9,479	4.84	63.96	0.41	1.63	4.12
Dep. rate to more senior job	9,479	2.68	41.52	0.20	0.86	2.20
Dep. rate to more senior job within ind.	9,479	0.78	15.09	0.00	0.08	0.45
Employees	9,539	4,044	13,374	157	649	3,087
Panel B: Investment Analysis						
Market cap. ($M)	5,053	1,925	4,860	77	371	1,384
Assets ($M)	5,053	1,544	4,008	80	303	1,219
Employees	4,956	3,613	7,671	250	933	3,500
I/K	5,053	0.30	0.29	0.11	0.22	0.39
Knowledge firms	5,030	0.47	0.50	0.00	0.00	1.00
High R&D firms	5,029	0.62	0.48	0.00	1.00	1.00
Panel C: Departures to Entrepreneurship Analysis						
Departures to new 1-10 emp.	9,785	0.84	4.91	0	0	1
Departures to new 1-50 emp.	9,785	1.56	8.68	0	0	1
Departures to new all sizes	9,785	3.25	29.78	0	0	2
Panel D: New Firm Entry Analysis						
New firms 1-10 emp.	31,850	1.94	6.38	0.00	0.00	2.00
New firms 1-50 emp.	31,850	2.50	8.84	0.00	1.00	2.00
New firms all sizes	31,850	2.53	8.95	0.00	1.00	2.00
New firms 1-10 emp., p. million	31,850	0.29	0.70	0.00	0.00	0.31
New firms 1-50 emp., p. million	31,850	0.36	0.87	0.00	0.09	0.36
New firms all sizes, p. million	31,850	0.37	0.88	0.00	0.10	0.37

Table 4: Ex Ante Sample Characteristics

This table reports means of observables for observations in states where CNC enforcement does not change, and for observations in states where CNC enforcement changes at some time during the sample period. Below the means are p-values from a t-test of differences relative to the "never treated" observations. I break out the latter into observations for which CNC enforcement eventually increases (Colorado, Georgia, Illinois, Texas, Virginia, Wisconsin) and those for which it eventually decreases (Montana, South Carolina). Macroeconomic measures and firm characteristics cover the two years before my sample starts, 2006-2007. For firm level characteristics, I cluster standard errors by state and drop the top and bottom 1% outliers. I take the Partisan Voting Index from the 2010 Cook Political Report, and the CNC enforceability score from the 2009 index constructed in (Starr, 2016b). Mobility measures are taken from 2008 only due to data limitations.

		Eventually Treated		
	Never Treated	All	Increase	Decrease
Political & Economic Measures				
GDP growth, in pct	5.35	5.64	5.24	6.87
	$p =$	0.698	0.893	0.302
GDP per capita, in thsds USD	48.24	44.53	47.50	35.63
	$p =$	0.453	0.896	0.201
Unemployment rate, in pct	4.44	4.45	4.33	4.80
	$p =$	0.969	0.724	0.495
Partisan Voter Index	R+2.4	R+3	R+1.5	R+7.5
	$p =$	0.864	0.821	0.451
CNC enforceability score	0.12	-0.03	0.12	-0.46
	$p =$	0.717	0.995	0.480
Mobility				
Departure Rate	6.72	6.77	6.90	2.10
	$p =$	0.961	0.856	0.299
Departures to New 1-50 Emp.	1.11	0.85	0.86	0.33
	$p =$	0.440	0.465	0.727
New Firms 1-50 Emp., p. million	0.27	0.28	0.29	0.22
	$p =$	0.742	0.402	0.334
Firm Characteristics				
Market Cap. ($M)	1,428	1,592	1,614	1,028
	$p =$	0.591	0.555	0.023
Assets ($M)	864	1,183	1,196	833
	$p =$	0.155	0.151	0.736
Employees	2,802	3,509	3,469	4,469
	$p =$	0.257	0.298	0.000
I/K	0.35	0.32	0.32	0.32
	$p =$	0.268	0.270	0.308
Knowledge firm indicator	0.47	0.38	0.38	0.44
	$p =$	0.220	0.212	0.696
High R&D intensity indicator	0.67	0.53	0.52	0.67
	$p =$	0.099	0.094	0.936

Table 5: Employee Departure Rate

The dependent variable is the firm's departure rate in year t in percentage points (1 to 100). In Column (1), the numerator is all departures. In Column (2), the numerator includes only departures where the origin and destination industries are the same. In Column (3), the numerator includes only departures to a more senior position. In Column (4), it includes only departures to a more senior position, in the same industry. The denominator is the same throughout, so each outcome variable has a different baseline average – mechanically, the number is highest in Column (1). Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{it} \quad = \quad \alpha + \beta\{\text{treated}_i * \text{post}_t\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) Departure Rate	(2) Within- Industry	(3) To More Senior Job	(4) Within-Ind. More Senior Job
Treated*Post	-2.612***	-1.409***	-0.849***	-0.309***
	(0.845)	(0.460)	(0.248)	(0.0948)
Industry-Year FE	Y	Y	Y	Y
Company FE	Y	Y	Y	Y
Observations	9,479	9,479	9,479	9,479
R-squared	0.964	0.963	0.94	0.975

*** p<0.01, ** p<0.05, * p<0.1

Table 6: Employee Departure Rate by Occupation

The observation level is occupation-firm-year. The dependent variable is the firm's departure rate in year t in percentage points (1 to 100). In Column (1), the numerator is all departures. In Column (2), the numerator includes only departures where the origin and destination industries are the same. In Column (3), the numerator includes only departures to a more senior position. In Column (4), it includes only departures to a more senior position, in the same industry. The denominator is the same throughout, so each outcome variable has a different baseline average – mechanically, the number is highest in Column (1). Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$\begin{aligned} 100 * \frac{\text{\# departures}}{\text{\# employees}}_{imt} = \alpha \quad &+ \quad \beta_1\{\text{treated}_i * \text{post}_t * \text{knowledge workers}_m\} \\ &+ \quad \beta_2\{\text{treated}_i * \text{post}_t\} + \gamma_i + \lambda_{sm} + \theta_{mt} + \epsilon_{imt} \end{aligned}$$

	(1) Departure Rate	(2) Within- Industry	(3) To More Senior Job	(4) Within-Ind. More Senior
Treated*Post*Knowledge Worker	-2.536***	-2.048***	-1.430***	-0.423
	(0.540)	(0.515)	(0.362)	(0.284)
Treated*Post	0.988	0.532	-0.513	-0.946
	(1.243)	(1.227)	(1.196)	(1.230)
Company FE	Y	Y	Y	Y
State-Occupation FE	Y	Y	Y	Y
Occupation-Year FE	Y	Y	Y	Y
Observations	311,202	311,202	311,202	311,202
R-squared	0.422	0.404	0.405	0.481

*** p<0.01, ** p<0.05, * p<0.1

Table 7: Net Investment Scaled by Capital

The dependent variable is net investment scaled by one year-lagged net capital. In Column (1), the regression is a difference-in-differences with all observations pooled. In Columns (2)-(4), the regression is a difference-in-difference-in-differences, or triple differences. In Column (2), the subsample is the set of firms which employ an above-median fraction of knowledge workers. In Column (3), it is the set of firms with an above-median R&D intensity. In Column (4), it is the set of firms in knowledge industries. Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$\frac{I}{K}_{it} = \alpha + \beta_1\{\text{treated}_i * \text{post}_t\} + \beta_2\{\text{treated}_i * \text{post}_t * \text{subsample}_i\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) I/K	(2) I/K	(3) I/K
Treated*Post	0.0633**	0.0147	0.0256
	(0.0283)	(0.0223)	(0.0265)
Treated*Post*Knowledge Firm		0.0900**	
		(0.0347)	
Treated*Post*High R&D Intensity			0.0530*
			(0.0281)
Industry-Year FE	Y	N	N
Subsample-Industry-Year FE	N	Y	Y
Company FE	Y	Y	Y
Observations	5,053	5,030	5,029
R-squared	0.603	0.641	0.620

*** p<0.01, ** p<0.05, * p<0.1

Table 8: Departures to Entrepreneurship

The dependent variable is the number of departures from a given firm to newly-founded companies. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Knowledge Firm is an indicator for firms with a higher than median fraction of knowledge workers. Standard errors are clustered at the state level. Industry is 4-digit NAICS.

$$\text{departures}_{it} = \alpha + \beta_1\{\text{treated}_i * \text{post}_t * \text{knowledge firm}_i\} + \beta_2\{\text{treated}_i * \text{post}_t\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) New $\leq$ 10 Employees	(2) New $\leq$ 50 Employees	(3) New All Sizes
Treated*Post*Knowledge Firm	-0.421**	-0.652**	-14.38
	(0.176)	(0.265)	(13.23)
Treated*Post	0.0737	0.157	13.63
	(0.0813)	(0.243)	(13.59)
Knowledge Firm-Ind.-Year FE	Y	Y	Y
Company FE	Y	Y	Y
Observations	9,785	9,785	9,785
R-squared	0.917	0.927	0.400

*** p<0.01, ** p<0.05, * p<0.1

Table 9: New Firm Entry

The dependent variable is the number of companies founded within an industry-state-year, scaled by the state's population in millions. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Knowledge sector is an indicator for firms in the following three sectors: professional, scientific and technical services, technology, and education. Standard errors are clustered at the state level. Industry is LinkedIn-defined industry.

$$\frac{\text{firms founded}}{\text{million people}}_{sjt} = \alpha + \beta_1\{\text{treated}_s * \text{post}_t * \text{knowledge sector}_j\} + \beta_2\{\text{treated}_s * \text{post}_t\} + \gamma_{sj} + \theta_{jt} + \epsilon_{sjt}$$

	(1) New ≤ 10 Employees	(2) New ≤ 50 Employees	(3) New All Sizes
Treated*Post*Knowledge Sectors	-0.0325*	-0.0590**	-0.0626**
	(0.0189)	(0.0232)	(0.0245)
Treated*Post	-0.00263	-0.000370	0.000343
	(0.00590)	(0.00610)	(0.00560)
Industry-Year FE	Y	Y	Y
Industry-State FE	Y	Y	Y
Observations	31,850	31,850	31,850
R-squared	0.714	0.767	0.768

*** p<0.01, ** p<0.05, * p<0.1

Table 10: New Firm Entry in Knowledge Sectors

The dependent variable is the number of companies 1-50 employees founded within an industry-state-year, scaled by the state's population in millions. In Column (1), I focus on the subsample of industries in the technology sector, in Column (2) on industries in professional, scientific and technical services, and in Column (3) on industries in the education sector. Column (4) includes all other industries. Standard errors are clustered at the state level. Industry is LinkedIn-defined industry.

$$\frac{\text{firms founded 1-50 emp.}}{\text{million people}}_{sjt} = \alpha + \beta\{\text{treated}_s * \text{post}_t\} + \gamma_{sj} + \theta_{jt} + \epsilon_{sjt}$$

	(1) Technology	(2) Prof, Sci. & Tech.	(3) Education	(4) All Other
Treated*Post	-0.0388**	-0.0737*	-0.0769*	-0.000370
	(0.0176)	(0.0414)	(0.0412)	(0.00610)
Industry-Year FE	Y	Y	Y	Y
Industry-State FE	Y	Y	Y	Y
Observations	4,753	6,090	924	20,083
R-squared	0.814	0.811	0.511	0.540

*** p<0.01, ** p<0.05, * p<0.1

Table 11: I/K Analysis With State-Year FE

The dependent variable is net investment scaled by one year-lagged net capital. In Column (1), the subsample of interest is the set of firms which employ an above-median fraction of knowledge workers. In Column (2), it is the set of firms with an above-median R&D intensity. In Column (3), it is the set of firms in knowledge industries. State-year fixed effects absorb the treated*post indicator. Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$\frac{I}{K}_{it} = \alpha + \beta\{\text{treated}_i * \text{post}_t * \text{subsample}_i\} + \gamma_i + \theta_{jt} + \lambda_{st} + \epsilon_{it}$$

	(1) I/K	(2) I/K
Treated*Post*Knowledge Firm	0.0560*	
	(0.0297)	
Treated*Post*High R&D Intensity		0.0460*
		(0.0245)
State-Year FE	Y	Y
Industry-Year FE	Y	Y
Company FE	Y	Y
Observations	5,030	5,029
R-squared	0.626	0.624

*** p<0.01, ** p<0.05, * p<0.1

Table 12: Departures to Entrepreneurship With State-Year FE

The dependent variable is the number of departures from a given firm to newly-founded companies. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Knowledge Firm is an indicator for firms with a higher than median fraction of knowledge workers. State-year fixed effects absorb the treated*post indicator. Standard errors are clustered at the state level. Industry is 4-digit NAICS.

$$\text{departures}_{it} = \alpha + \beta\{\text{treated}_i * \text{post}_t * \text{knowledge firm}_i\} + \gamma_i + \theta_{jt} + \lambda_{st} + \epsilon_{it}$$

	(1) New ≤ 10 Employees	(2) New ≤ 50 Employees	(3) New All Sizes
Treated*Post*Knowledge Firm	-0.293**	-0.577*	-16.53
	(0.109)	(0.307)	(16.65)
State-Year FE	Y	Y	Y
Industry-Year FE	Y	Y	Y
Company FE	Y	Y	Y
Observations	9,785	9,785	9,785
R-squared	0.917	0.926	0.405

*** p<0.01, ** p<0.05, * p<0.1

Table 13: New Firm Entry With State-Year FE

The dependent variable is the number of companies founded within an industry-state-year, scaled by the state's population in millions. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Knowledge sector is an indicator for firms in the following three sectors: professional, scientific and technical services, technology, and education. State-year fixed effects absorb the treated*post indicator. Standard errors are clustered at the state level. Industry is LinkedIn-defined industry.

$$\frac{\text{firms founded}}{\text{million people}}_{sjt} = \alpha + \beta_1\{\text{treated}_s * \text{post}_t * \text{knowledge sector}_j\} + \gamma_{sj} + \theta_{jt} + \epsilon_{sjt}$$

	(1) New $\leq$ 10 Employees	(2) New $\leq$ 50 Employees	(3) New All Sizes
Treated*Post*Knowledge Sectors	-0.0323	-0.0589**	-0.0626**
	(0.0203)	(0.0251)	(0.0265)
State-Year FE	Y	Y	Y
Industry-Year FE	Y	Y	Y
Industry-State FE	Y	Y	Y
Observations	31,850	31,850	31,850
R-squared	0.720	0.772	0.773

*** p<0.01, ** p<0.05, * p<0.1

Table 14: Net Investment With Size Trends

The dependent variable is net investment scaled by one year-lagged net capital. In each column, the regression is a difference-in-difference-in-differences, or triple differences. In Panel A, the interaction subsample is knowledge firms. In Panel B, it is high R&D intensity firms. I allow for differential trends by market capitalization in Column (1); by asset size in Column (2); by employee size in Column (3); and by I/K level in Column (4). Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$\frac{I}{K}_{it} = \alpha + \beta_1\{\text{treated}_i * \text{post}_t\} + \beta_2\{\text{treated}_i * \text{post}_t * \text{subsample}_i\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) I/K	(2) I/K	(3) I/K	(4) I/K
Panel A: Knowledge Firms				
Treated*Post*Knowledge Firm	0.0910**	0.0871**	0.0836**	0.0871**
	(0.0348)	(0.0363)	(0.0338)	(0.0336)
Treated*Post	0.0141	0.0161	0.0168	0.0168
	(0.0221)	(0.0226)	(0.0234)	(0.0236)
Observable-Year FE	Market Cap.	Assets	Employees	I/K
Industry-Year FE	Y	Y	Y	Y
Company FE	Y	Y	Y	Y
Observations	5,030	5,030	5,030	5,030
R-squared	0.642	0.642	0.643	0.644
Panel B: High R&D Intensity Firms				
Treated*Post*High R&D Firm	0.0538*	0.0528*	0.0501*	0.0538*
	(0.0283)	(0.0278)	(0.0279)	(0.0292)
Treated*Post	0.0250	0.0258	0.0272	0.0260
	(0.0268)	(0.0268)	(0.0263)	(0.0273)
Observable-Year FE	Market Cap.	Assets	Employees	I/K
Industry-Year FE	Y	Y	Y	Y
Company FE	Y	Y	Y	Y
Observations	5,029	5,029	5,029	5,029
R-squared	0.621	0.621	0.621	0.623

*** p<0.01, ** p<0.05, * p<0.1

Table 15: Departure Rate by Years to CNC Change

The dependent variable is the firm's departure rate in year t in percentage points (1 to 100). In Column (1), the numerator is all departures. In Column (2), the numerator includes only departures where the origin and destination industries are the same. In Column (3), the numerator includes only departures from knowledge occupations. In Column (4), the numerator includes only departures to a more senior position. The denominator is the same throughout, so each outcome variable has a different baseline average – mechanically, the number is highest in Column (1). Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$100 * \frac{\#\text{ departures}}{\#\text{ employees}}_{it} = \alpha + \sum_k \beta_k \{\text{treated}_i * \text{k years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) Departure Rate	(2) Knowledge Workers	(3) Within-Industry	(4) To More Senior Job	(5) Within-Ind. More Senior
Treated*Post*t-1	0.551	0.363	0.513	-0.106	0.0626
	(0.936)	(0.595)	(0.452)	(0.333)	(0.117)
Treated*Post*t+0	-0.312	-0.409	-0.390	-0.252	-0.0190
	(1.244)	(0.530)	(0.352)	(0.389)	(0.128)
Treated*Post*t+1	-3.680**	-1.779**	-1.085**	-1.217***	-0.386***
	(1.410)	(0.676)	(0.417)	(0.428)	(0.137)
Treated*Post*t+2	-3.593**	-1.553*	-0.992	-1.359**	-0.409**
	(1.720)	(0.850)	(0.637)	(0.546)	(0.153)
Treated*Post*t3+	-4.225***	-1.972***	-1.171**	-1.395***	-0.526***
	(1.508)	(0.621)	(0.528)	(0.358)	(0.110)
Industry-Year FE	Y	Y	Y	Y	Y
Company FE	Y	Y	Y	Y	Y
Observations	9,479	9,479	9,479	9,479	9,479
R-squared	0.964	0.963	0.974	0.940	0.975

*** p<0.01, ** p<0.05, * p<0.1

Table 16: I/K by Years to CNC Change

The dependent variable is net investment scaled by one year-lagged net capital. The regression is a triple difference regression interacting the treated*subsample indicator with years to treatment. In Column (1), the subsample is the set of firms which employ an above-median fraction of knowledge workers. In Column (2), it is the set of firms with an above-median R&D intensity. In Column (3), it is the set of firms in knowledge industries. Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

$$\frac{I}{K}_{it} = \alpha + \beta_1\{\text{treated}_i * \text{post}_t\} + \sum_k \beta_k\{\text{treated}_i * \text{subsample}_i * \text{k years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) I/K	(2) I/K	(3) I/K
Treated*Post*Subsample*t-1	-0.0382	-0.0671	-0.0335
	(0.0243)	(0.0424)	(0.0302)
Treated*Post*Subsample*t+0	0.0372	0.0718	0.0358
	(0.0353)	(0.0628)	(0.0395)
Treated*Post*Subsample*t+1	0.0604*	0.100*	0.0546**
	(0.0336)	(0.0553)	(0.0253)
Treated*Post*Subsample*t+2	0.0779**	0.0475	0.0665*
	(0.0296)	(0.0289)	(0.0358)
Treated*Post*Subsample*t3+	0.0419*	0.0838	0.0538
	(0.0238)	(0.0537)	(0.0466)
Treated*Post	N	Y	Y
Industry-Year FE	Y	N	N
Subsample-Industry-Year FE	N	Y	Y
Company FE	Y	Y	Y
Observations	5,053	5,030	5,029
R-squared	0.604	0.642	0.621

*** p<0.01, ** p<0.05, * p<0.1

Table 17: Departures to Entrepreneurship by Years to CNC Change

The dependent variable is the number of departures from a given firm to newly-founded companies. The regression is a triple difference regression interacting the treated*knowledge firm indicator with years to treatment. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Standard errors are clustered at the state level. Industry is 4-digit NAICS.

$$\text{departures}_{it} = \alpha + \sum_k \beta_k \{\text{treated}_i * \text{knowledge firm}_i * \text{k years to treatment}\} + \beta_2 \{\text{treated}_i * \text{post}_t\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$

	(1) New ≤ 10 Employees	(2) New ≤ 50 Employees	(3) New All Sizes
Treated*Post*Knowledge Firm*t-1	-0.0230	-0.182	0.320
	(0.210)	(0.191)	(0.607)
Treated*Post*Knowledge Firm*t+0	-0.131	-0.352	-9.494
	(0.244)	(0.280)	(8.514)
Treated*Post*Knowledge Firm*t+1	-0.183	-0.515**	-9.813*
	(0.258)	(0.214)	(5.435)
Treated*Post*Knowledge Firm*t+2	-0.101	-0.391*	-7.718*
	(0.215)	(0.216)	(4.352)
Treated*Post*Knowledge Firm*t3+	-0.328*	-0.735***	-9.663
	(0.183)	(0.233)	(7.238)
Treated*Post	Y	Y	Y
Industry-Year FE	Y	Y	Y
Company FE	Y	Y	Y
Observations	9,785	9,785	9,785
R-squared	0.915	0.924	0.390

*** p<0.01, ** p<0.05, * p<0.1

Table 18: New Firm Entry by Years to CNC Change

The dependent variable is the number of companies founded within an industry-state-year, scaled by the state's population in millions. The regression is a triple difference regression interacting the treated*knowledge sector indicator with years to treatment. In Column (1), this includes only companies with 10 or fewer employees. In Column (2), this expands to companies with 50 or fewer employees, and in Column (3) this includes newly-founded companies of any size. Knowledge sector is an indicator for firms in the following three sectors: professional, scientific and technical services, technology, and education. Standard errors are clustered at the state level. Industry is LinkedIn-defined industry.

$$\frac{\text{firms founded}}{\text{million people}}_{sjt} = \alpha + \sum_k \beta_k \{\text{treated}_i * \text{knowledge sector}_i * \text{k years to treatment}\} + \beta_2\{\text{treated}_s * \text{post}_t\} + \gamma_{sj} + \theta_{jt} + \epsilon_{sjt}$$

	(1) New ≤ 10 Employees	(2) New ≤ 50 Employees	(3) New All Sizes
Treated*Knowledge Sector*t-1	0.0121	-0.000938	-0.00107
	(0.0316)	(0.0327)	(0.0334)
Treated*Knowledge Sector*t+0	-0.0301	-0.0529**	-0.0570**
	(0.0213)	(0.0247)	(0.0261)
Treated*Knowledge Sector*t+1	-0.0134	-0.0473	-0.0480
	(0.0226)	(0.0297)	(0.0301)
Treated*Knowledge Sector*t+2	-0.0371	-0.0827**	-0.0880**
	(0.0273)	(0.0357)	(0.0359)
Treated*Knowledge Sector*t3+	-0.0484	-0.0872	-0.0894
	(0.0506)	(0.0597)	(0.0605)
Treated*Post	Y	Y	Y
Industry-State FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	31,850	31,850	31,850
R-squared	0.690	0.743	0.745

*** p<0.01, ** p<0.05, * p<0.1

CHAPTER 2 : GOODWILL HUNTING: CORPORATE SOCIAL RESPONSIBILITY AS AN INVESTMENT

2.1. Introduction

At least ninety-five percent of the 250 largest companies in the world report engaging in some form of Corporate Social Responsibility (CSR).[1] Many of these firms spend large amounts of money on CSR: Disney spent $248.5 million on CSR in 2012 and Microsoft $904 million, according to the Reputation Institute, a reputation management consultancy.[2] Despite this significant investment on behalf of firms, there is little consensus on the link between CSR activity and financial outcomes.

Two schools of thought dominate the debate on the relationship between CSR and firms' financial interests. The first and arguably most popular is that firms can "do well by doing good." This is touted by businesses that conduct CSR and beneficiaries of CSR (e.g., consumers, activists, communities), but also argued by many in the management literature (see Margolis et al. (2007) for an overview). The second school of thought, famously championed by Milton Friedman, is that CSR is at best a distraction from a firm's main purpose and at worst a way for managers to use other people's money toward their personal projects.[3] This position tends to be supported, though not exclusively, in the finance literature (see for example the recent study by Cheng et al. (2013)).

Critics of the "doing well by doing good" argument point to the lack of solid empirical evidence to support a causal relationship. A related problem is that the rationale underlying the argument is vague. Theories include consumers' willingness to pay more for ethical products; the lower cost of sustainable practices in the long run; and the importance of company ethos in attracting and retaining talent. Few empirical studies attempt to clarify

[1] KPMG, 2011. "International Survey of Corporate Responsibility Reporting."

[2] PR News, January 30, 2011. "How Much the Most Reputable Companies Spend on CSR." Figures are also available from Microsoft and Disney's own Citizenship Reports.

[3] The New York Times Magazine, September 13 1970, "The Social Responsibility of Business is to Increase its Profits"

the mechanism through which the relationship operates. While it is certainly possible to have multiple mechanisms at play, this ambiguity makes it more difficult to feel confident about the causality of the relationship.

On the other hand, proponents of a neutral or negative impact of CSR have a clear mechanism to explain its popularity: agency theory. According to this theory, managers engage in CSR to satisfy some private benefit, not internalizing the costs that befall shareholders. Indeed, there is anecdotal evidence that manager interest is an important factor for companies engaging in CSR.[4] Nonetheless, this explanation is not entirely satisfactory in that managers do not appear to hide their CSR engagement from shareholders, which we might expect if CSR activities constituted an expropriation of shareholders for the private benefit of management. On the contrary, managers tend to be very public about their CSR efforts.

In this paper, I investigate a specific mechanism through which CSR may benefit firms, which can also explain why managers are so public about it. Specifically, I argue that a firm's socially responsible behavior helps it to accumulate goodwill in its community. If the firm finds itself in a situation where it requires public approval, e.g. when applying for public contracts, it may draw down on this goodwill for preferential treatment. Interviews with managers suggest they view this goodwill benefit of CSR as an important motivation for engaging in social responsibility.[5]

To address concerns about omitted variables and reverse causality, I propose a new empirical approach using the staggered passage of state laws as a natural experiment that lowered constraints on directors' ability to engage in non-shareholder-oriented policies. These laws, called Other Constituency (OC) laws, allow directors to consider the interests of stakeholders other than shareholders. In other words, they allow directors to be responsible to society beyond shareholders. The passage of these laws provides a good setting for studying CSR for two reasons. First, the laws are explicitly designed to protect directors who consider

[4]Forbes, October 17, 2012. "The Six Reasons Why Companies Actually Wind Up Embracing CSR."
[5]Financial Times, June 22 2011, "License to Operate: Goodwill May Be Key to Gaining Green Light."

other stakeholder interests, so they should encourage more of this behavior. Second, there is no evidence that these laws were anticipated or otherwise enacted in states that were following a different economic trend than non-OC states. This allows us to plausibly attribute differences in firm outcomes after the laws' passage (relative to the same differences pre-passage) to the change in law.

I first document that OC laws led to an increase in socially responsible behavior by looking at employee safety and health. The motivation for this definition of social responsibility is twofold: employees are an important stakeholder for all firms, and the data are available during the time most OC laws pass, in the late 1980s and early 1990s. I obtain records on inspections and violations from the Occupational Safety and Health Administration (OSHA), and show that following the passage of OC laws, firms had fewer violations, complaints, and accidents in the workplace. To be clear, I do not argue that absent the laws, companies are deliberately harming employees. Rather, I argue that expanded discretion to pursue pro-employee policies result in even lower violations than expected in a standard place of business.

Second, I show that the rise in non-shareholder-oriented policies coincides with an increase in public contract awards. I collect historical federal contracts via the National Archives and find that following the passage of OC laws, firms incorporated in those states are disproportionately likely to obtain public contracts and contracts of greater value.

This paper contributes to the existing literature on CSR by documenting a specific mechanism through which CSR benefits firms, and by proposing a new empirical approach to address well-documented omitted variable (McGuire et al., 1988; McWilliams and Siegel, 2000; Margolis et al., 2007) and reverse causality (McGuire et al., 1988; Hong et al., 2012) concerns in understanding the relationship between CSR and firm outcomes.

By doing so it complements the recent work of Hong and Liskovich (2014) and Albuquerque et al. (2012), who each consider alternative settings in which CSR may benefit firms, and of

Cheng et al. (2013) who argue that managers engage in pro-stakeholder policy for private enjoyment. While others (e.g. Hong and Liskovich (2014)) provide suggestive evidence that good corporate citizens benefit from lower penalties, there has been less empirical research to show that they are also more likely to obtain awards. This work also connects with a long-standing literature documenting the relationship between social performance and economic performance (Margolis et al., 2007).

The remainder of the paper is structured as follows. Section 2.2 presents the data used in the analysis and descriptive statistics, section 2.3 outlines the empirical approach, section 2.4 presents the main results and section 2.5 discusses the robustness of these results.

2.2. Data

The purpose of this paper is to examine whether changes in socially responsible behavior are associated with variation in public goodwill. As such, there are three components to measure empirically: shocks to socially responsible behavior, socially responsible behavior itself, and public goodwill. For shocks to socially responsible behavior, I use the staggered passage of state laws that expand directors' prerogative to pursue stakeholder-friendly policies. To measure socially responsible behavior, I look at workplace characteristics from the Occupational Safety and Health Administration (OSHA) and later an index from the Kinder, Lydenburg, Domini & Co (KLD) database covering additional CSR categories. Finally to measure public goodwill, I use data on government contract awards. The data sources and sample are described below.

2.2.1. Other Constituency laws and state of incorporation

For data on OC laws, I rely largely on a previous paper (Geczy et al., 2015) that reviews state legislative session documents to verify the correct passage dates of the laws. Table 19 contains a list of all relevant laws and passage years. States marked with an asterisk have laws that apply only in takeover contexts. Results reported in section 2.4 are robust to excluding the states which pass these laws. For an in-depth review of the laws, including a review of the language and a history of cases citing the laws, see Geczy et al. (2015).

Firms are subject to OC laws if they are incorporated in a state which has such a law. As a result, it is important to properly identify the state of incorporation. To do this I use the data assembled by Gormley and Matsa (2014). These data track the historical state of incorporation through 2006, after which I use state of incorporation from Compustat when necessary.

2.2.2. Measures of corporate social responsibility

CSR is "generally understood as being the way through which a company achieves a balance of economic, environmental and social imperatives... while at the same time addressing the expectations of shareholders and stakeholders."[6] Below are ways in which I capture firms' CSR.

Employee safety and health

A broad index of CSR is not available during the main period when legislatures pass OC laws (1987-1990). To verify the effect of the law's passage, I first focus in on a specific category of stakeholders for which I can observe strengths and concerns within the same time period: employees.

I collect workplace safety inspection and violation records from the Occupational Safety and Health Administration (OSHA), the US federal agency responsible for monitoring workplace safety.[7] Table 20 provides an overview of the variables I collect as well as summary statistics for my sample.

OSHA has jurisdiction over almost all non-governmental U.S. workers.[8] Inspections are

[6]Definition from the United Nations Industrial Development Organization, http://www.unido.org/en/what-we-do/trade/csr/what-is-csr.html

[7]I obtain detailed inspection and violation data on OSHA from the Department of Labor's online data catalog available at http://ogesdw.dol.gov/views/data_catalogs.php

[8]Those not covered by OSHA include self-employed workers, immediate family members, farm employers, and workers whose hazards are regulated by another federal agency https://www.osha.gov/OSHA_FAQs.html

always conducted without advance notice, although in special circumstances OSHA may give a notice of less than 24 hours to the employer.[9] While a business can refuse an OSHA inspection without a warrant, almost none do, as requiring a warrant tends to invite more scrutiny and result in more citations.[10]

Alternative measure: KLD database

In section 2.5 I use the Kinder, Lydenburg, Domini & Co (KLD) index as an alternative measure of CSR to test the robustness of my results to alternative specifications. The KLD index is arguably the most widely used tool for measuring CSR. However, KLD only begins tracking companies in 1991, which is too late to compare pre- and post-law levels for my sample. As a result in section 2.5 I examine cross-sectional differences between OC and non-OC states.

KLD scans public databases and news reports to track company strengths and concerns along multiple dimensions of CSR. For the purpose of this paper I focus on five categories of CSR: community, diversity, employee, environment and product. Each category is broken down further into strengths and concerns that are coded as 1 (for presence of trait) or 0 (for absence). For example, the community category includes charitable giving, support for housing, support for education and volunteer programs as strength traits. Concern traits include investment controversies, negative economic impact and tax disputes. Following Cheng et al. (2013), I construct an annual aggregate CSR score for each firm by adding the firm's strengths and subtracting its concerns across all five categories. Table 21 provides summary statistics for these data.

[9]OSHA may also inspect businesses at random, but the agency typically prioritizes situations of imminent dangers, accident investigations, and complaints or referrals. See https://www.osha.gov/Publications/osha2098.pdf

[10]http://www.pedersenhoupt.com/newsroom-publications-46.html

2.2.3. Government Contracts

To proxy for public goodwill, I look at public contract awards. The data I collect come from Department of Defense contract records kept by the National Archives. The Department of Defense is the single largest contractor within the U.S. government, with contracts representing a wide variety of product markets and suppliers, from food to services to manufacturing parts. These records are available as undelimited text files, and require substantial work to read in and clean. For parsimony, I focused on the ten most important years of data, 1984-1993, which covers 29 of the 33 laws passed.

I match these data to Compustat using fuzzy matching on names and other available information. I expect some observations from the contract data not to match to Compustat (contracts associated with private companies) and some observations from Compustat not to match to the contract data (public companies without a contracting relationship with the government). However, it is also likely that I am losing some observations. Because I consider the lack of a contract to be relevant information, I keep observations from Compustat which do not match to the contract data, setting the number of contracts and total contracting dollars for these firms to zero. As long as there is no systematic bias in my matching algorithm, that is, as long as unmatched firms are no more or less likely to be incorporated in states that pass OC laws, this should only widen my standard errors but not bias my results. My final sample contains 61,498 firm-year level observations, of which 4,675 (or about 8%) indicate one or more contracts were signed for that firm in that year.

Table 22 shows summary statistics for contracts in my sample. Since the outcomes are right-skewed, I use a log transformation in regressions. In order to keep observations with zero contracts, I add one to contract dollars and number before taking the log when considering the full sample.

2.3. Empirical Approach

I wish to test whether an increase in CSR will be followed by greater propensity to benefit from awards that depend on public approval. An important criticism of existing research on CSR is that studies fail to account for potential correlation between CSR and unobserved variables that also affect performance. Reverse causality is another concern: firms that do better may have more resources to spend on CSR.

To address these issues, I propose a novel approach that uses the staggered passage of state laws that relaxed constraints on directors' ability to engage in stakeholder-friendly policies. Specifically these laws, known as Other Constituency (OC) laws, allow directors to consider the interests of non-shareholder constituents when making decisions for the firm. An example of the statutes' language is in the appendix. The laws pass in 33 states over a wide range of time, though concentrated in the late 1980s and early 1990s. I provide more context for the passage and meaningfulness of the laws later in this section when discussing the underlying assumptions.

2.3.1. Specification

The main specification is as follows.

$$y_{it} = \beta_0 + \beta_1 OC_{jt} * post + \beta_2 BC_{jt} * post + \beta_3 x_{it} + \epsilon_{it} \tag{2.1}$$

where y_{it} is the firm outcome (e.g., number of government contracts awarded), x_{it} are control variables including industry-year and state of incorporation fixed effects (which absorb the traditional difference-in-differences indicators "Treated" and "Post"), $OC_{jt} * post$ is an indicator that turns on once a firm's state of incorporation has passed an Other Constituency law and similarly $BC_{jt} * post$ is an indicator that turns on once a firm's state of incorporation has passed a Business Combination law. I control for BC laws throughout because they are laws that pass around the same time and have been documented to affect firm outcomes.[11]

[11]BC laws are the main other director-oriented laws passed around the same time as OC laws.

I also control for size as defined by the number of employees and total assets of the firm. In tabulated results I allow employee and asset controls to vary over time, because they do not appear to be affected by OC laws and because restricting them to 1984 values unnecessarily decreases the sample size. However, results are qualitatively similar when using 1984 values. I define industry as the four-digit SIC code. Standard errors are clustered at the state of incorporation to account for serial correlation as well as cross-sectional correlation in the state of incorporation. I estimate this equation using OLS.

For outcome variables that capture public goodwill, I use the attribution of federal government contracts. The underlying assumption is that governments have an incentive to reflect the preferences of their voters. In theory, as a representative of the public, the government should reflect constituents' preferences. More practically, the government is likely to be concerned about associating with good corporate citizens (and not associating with bad corporate citizens) for the purpose of being reelected. Anecdotal evidence supports this assumption. The U.S. government has previously stated similar preference mechanisms, such as favoring veteran- and minority-owned businesses. Such contracts do not appear in my analysis, which only considers publicly-owned businesses, but indicate a willingness to consider the social impact of companies in attributing contracts. Local governments have often aligned themselves with local sentiment in opposing relationships with "bad corporate citizens" such as Walmart and Monsanto.[12,13] Finally, other national government such as Italy and the United Kingdom have put in place formal preferences for socially responsible firms, suggesting governments can reflect constituents in caring about corporate social responsibility practices.

2.3.2. Relevance

An important assumption underpinning this natural experiment approach is that OC laws were relevant to companies' social responsibility. CSR can be defined as a company's prac-

[12]Newsday, June 5 2014, "De Blasio: Walmart Unwelcome in New York City."

[13]Joc.com, July 7 1992, "For Monsanto, It's No Fun Trying to Open a Plant that No One Wants."

tice of ensuring the well-being of its non-shareholder stakeholders. These other stakeholders – or other constituents – are typically defined as including community, employees, consumers and environment. Indeed, these are key categories of constituents tracked by the KLD Index. In the main results of this paper, I focus on a particular class of constituents: employees.

As illustrated in the language of the Pennsylvania statute provided in the appendix, OC laws are designed to protect directors engaging in policies oriented toward these other stakeholders. The language typically provides directors with broad discretion to pursue a social agenda. According to Carney and Shepherd (2008) "[these] states effectively give directors *carte blanche* discretion by allowing them to consider other constituencies."

However, laws sometimes stand on the books without being used, or are interpreted in ways that set precedents potentially different from the original intent. Geczy et al. (2015) construct a history of cases citing OC laws in the years since they were passed and review the ways in which the laws were enforced. The authors find 47 relevant cases, of which 21 cases address the expansion of directors' rights to consider non-shareholder interests. (Other cases address the standing of other constituents to *require* consideration by directors – typically not recognized – or cite to the statutes without directly addressing the expansion of directors' rights.) Out of these 21 cases, 17 affirm the expansion of directors' rights, while 4 decline to recognize expanded authority. The number of cases is not large, but it is enough to indicate that courts recognized the presence of the laws. Moreover, shareholders and directors often reach agreement before taking matters to court, which would not show up in case records. The authors conclude that the laws were largely implemented as intended.

Even if the laws are not misconstrued or forgotten, they may not have much impact on firms' behavior. To address this, I use employee safety and health as a measure of social responsibility. Employees are an important category of stakeholders, one of the categories specifically tracked by KLD. I consider a range of variables pertaining to firms' behavior toward employees: I first look at number of violations, number of serious violations, and number of instances a violation was found (e.g., whether the violation appears to be a one-time omission or systematic). Since labeling an infraction as a violation may be subjective, I also look at measures of firms' behavior which are not subject to inspector discretion, such as number of complaints received about the firm, number of instances a hazardous substance was found, and number of accidents. In section 2.4.1, I show that all of these measures decline after passage of the laws, suggesting companies improved their policies. To be clear, I do not argue that absent the laws, companies are deliberately harming employees. Instead I argue that expanded discretion to pursue pro-employee policies results in even lower violations than expected in a standard place of business. In section 2.5.1, I also verify that being incorporated in a state with an OC law is predictive of a higher KLD score.

2.3.3. Parallel Trends

The second main assumption underlying this natural experiment is that firms incorporated in different states are experiencing relatively similar trends prior to the passage of the law and would have continued to experience similar trends but for the passage of the laws. Table 2.6 provides summary statistics of firm characteristics for both groups of observations as of 1984 (before most laws pass). There are no statistically significant differences between the two groups.

Many states passed these laws as a response to rulings that favored shareholder primacy over other constituents,[14] with the intent that companies be responsible to their other constituents, making the timing of the law plausibly exogenous.

2.4. Results

I first use OSHA outcomes as a proxy for CSR to verify that OC laws have an impact on CSR. Across the board, violations and other measures of bad behavior appear to decline. These results are consistent across a number of specifications, including controls for employees, assets, and firm fixed effects.

I then use government contract awards as a proxy for public goodwill and find that these

[14] See *Revlon, Inc. v. MacAndrews & Forbes Holdings, Inc.* (Del. 1986)

awards increase following the passage of OC laws. Again, results are consistent across a number of specifications, and I discuss robustness in section 2.5.

2.4.1. Workplace Safety Violations and Complaints

Table 24 presents the results of the difference-in-differences specification from section 2.3.1 with measures of employee environment quality as the dependent variables. In Panel A, I consider whether violations declined following the passage of OC laws. The coefficient estimates suggests that the presence of an OC law resulted in 0.223 fewer violations, 0.560 fewer instances of violations and 0.0843 fewer serious violations for the average firm incorporated in the state. These numbers are small because most firms have zero violations – relative to the averages reported in Table 20, the estimates appear relatively large, at 20-25% of average levels (for violations the estimate represents 24% of an average level of 0.94, for number of instances 23% of an average of 2.43 and for serious violations 23% of an average of 0.40). One reason for the size of these estimates may be the skewness of the outcome variables. Since I want to consider firms that go from having some violations to zero violations, I include all firms incorporated in the state, including ones with zero violations.

Panel B presents the results of the regression analysis for other behavior outcomes. The coefficient estimates suggest that following the passage of OC laws, firms have 0.0266 fewer complaints, 0.0167 fewer instances of hazardous substance found, and 0.00530 fewer accidents. Again, for complaints and accidents, the effect is proportionally similar, with the estimated decline in complaints representing 24% of an average of 0.11 and the estimated decline in accidents representing 27% of an average of 0.02. For hazardous substances, the magnitude of the coefficient suggests a larger effect, representing 56% of the average of 0.03. Relative to standard deviations, coefficient estimates in both panels hover around 1-3% of standard deviation, again pointing to the large variation in the distribution of outcomes.

Government Contracts

Table 25 presents estimates of the difference-in-differences with $ln(1 + \textit{contracting dollars})$ as the dependent variable. The log transformation brings the distribution closer to a normal distribution, while adding one allows me to keep observations with zero contracting relationship, which are important for this analysis.

The use of this transformation means that the interpretation of estimates depends on the level of the dependent variable. Given an average contracting dollar amount of 19,439 (in thousands of USD), the estimates in Table 25 suggest an increase in government contract amounts of 6-7% for the average observation following the passage of OC laws. Note that as the dependent variable decreases, the percentage increase implied by the estimate rises at the rate of $\frac{1+y}{y}$, where y is the dependent variable. As a result the estimate is harder to interpret for observations with zero contracts. Nonetheless, this analysis reveals a robust positive relationship between contracts obtained from the government and the presence of OC laws.

To dig into the drivers of this relationship, I look at whether firms had more contracts approved or more valuable contracts. Table 26 presents the results of the same estimation as above, but with $ln(1 + \textit{number of contracts})$ as the dependent variable. Estimates of the coefficient on the presence of an OC law are positive, but only statistically significant above a 90% confidence threshold in the last specification when clustering standard errors at the state of incorporation.[15] As with the previous specification, the interpretation of the magnitude of the coefficient depends on the level of the variable. The estimates suggest an increase in the number of contracts awarded of 2% for the average observation.

Finally, I look at the average value of contracts. The dependent variable in Table 27 is $ln(\textit{mbox dollars per contract})$. The sample universe for this regression is substantially smaller since it include only firms with one or more government contracts. Nevertheless,

[15] If clustered at the firm level, estimates throughout are statistically significant, but this does not allow for cross-correlation across firms incorporated in the same state.

controlling for size (by employees) the estimates suggest an increase in value per contract of 10-13% that is significant above a 95% confidence level.

2.5. Robustness and discussion

2.5.1. Instrumental variable approach in later sample (2000-2009)

One of the drawbacks of the difference-in-differences approach is that there are limited options to measure CSR and government contracts in the 1984-1993 period. Starting in 2000, usaspending.gov tracks all government contracts (not just Department of Defense contracts), and more detailed CSR data are available from the KLD index. To verify that results hold when using these measures of government contracts and CSR, I conduct an instrumental variable analysis. In this analysis, incorporation in a state with an OC law is an instrument for CSR, where CSR is measured by the KLD score.

In a naive OLS regression of contract outcomes on KLD score, contract outcomes appear to be strongly positively related with CSR (KLD) score, even when controlling for profitability and size. The magnitudes implied by the regression are large: 2% greater probability of having any contract, 13% more contracts, and 37% more contracting dollars for the average observation. Most importantly, the regressions support a positive relationship even when looking at a different measure of contracts and CSR.

Next, I employ a two-stage least-squares approach to analyze the relationship between CSR and public contracts, with incorporation in an OC state as an instrument for CSR score. Results are shown in Table 29. In the first stage, presence of an OC law is associated with a 0.210 higher KLD score, or about 10% of a standard deviation in the KLD score. Results from the second stage show a positive and statistically significant relationship between CSR score and contract outcomes. The implied magnitudes of the effect of incorporation in an OC state are again large: 0.4% greater probability of having any contract, 28% more contracts (12% at the lower bound), and 255% more contracting dollars for the average observation (44% at the lower bound). The magnitude of the latter is perhaps implausibly large. Nonetheless, in this alternative setting we again observe a positive and statistically significant relationship.

2.5.2. Alternative interpretations

A potential alternative explanation for the results described in this paper is that companies use CSR and OC laws as a means of lobbying politicians. An example of this would be a company spending money to renovate a park in a politician's neighborhood, rather than renovating a park in a less well-connected, low-income neighborhood. The two can be hard to disentangle, but the fact that OC laws are associated with more pro-employee behavior suggests there is more at play than catering only to politicians' private preferences. In untabulated results, I verify that OC laws are positively correlated with all five KLD categories, providing further support that companies are engaging in CSR activities that are unlikely to benefit politicians only (e.g., diversity). Nonetheless, it is possible that while companies are engaging in both types of behavior, it is only the behavior which benefits politicians directly that results in more public contracts. One way to test for this would be to control for companies' spending on political campaigns as a proxy for their propensity to lobby politicians. Another would be to compare companies with similar CSR levels but different levels of public reputation for CSR. If the channel is public goodwill, companies with better public reputation all else equal should be more likely to reap benefits. At this stage, these analyses are beyond the scope of the paper, but deserve attention in the future. Finally, I look at federal contracts, which should be harder to lobby (at least via CSR) than local contracts.

Another possible interpretation is that OC laws have other effects beyond permitting companies to engage in more stakeholder-friendly policies, and it is these other effects which are leading to more public contracts. For example, several OC laws were passed in the same period as takeover protection legislation. If takeover protection leads to more public contracts, this could be captured by coefficients in the difference-in-differences analysis. However, I control for the presence of BC laws throughout the difference-in-differences analysis (BC laws are direct anti-takeover laws passed in the same time period), and these laws appear to have the opposite relationship not only with contracting outcomes but also with employee environment outcomes. Moreover, section 2.5.1 provides supporting evidence that the relationship moves at least in part through the level of CSR.

The results of the analysis bring up a larger question: if CSR is a profitable venture, why don't all companies do it? After all, profitable business strategies should be protected in all states via business judgment rules. Albuquerque et al. (2012) provide a model in which CSR engagement is not equally costly for all firms, so that even though there is some benefit from CSR, firms will not all make the same choice. Another way to view this is that it takes only one disgruntled shareholder to threaten litigation which is reputationally costly for a director. If OC laws reduce the standing of a director to bring such a complaint, they reduce the cost of engaging in CSR. We would thus expect different levels of CSR across firms, and a correlation between OC laws and CSR, as we observe. Moreover, as with other types of investment, directors may have different beliefs about the expected benefit or risk of CSR. This would also lead to cross-sectional variation in the appetite for CSR.

2.6. Conclusion

Corporate social responsibility has an increasingly important and public place in firms' activities. Among firm managers, the consensus appears to be that doing good is good for business. Yet the literature has failed to provide a compelling – and empirically convincing – explanation for this phenomenon. In this paper, I explored the possibility that CSR is an investment in public goodwill, where goodwill is attractive to companies because it increases their chances of obtaining awards or approval from the public, among other benefits.

To address omitted variable and reverse causality concerns in the relationship between CSR and firm outcomes, I employ a novel identification strategy that uses the staggered passage of state constituency laws (OC laws) as a natural experiment for improved CSR. OC laws, passed mainly in the 1980s and early 1990s, provide protection for directors wishing to pursue stakeholder-friendly policies, such as pro-environment, pro-community, pro-employee policies that are an important part of CSR.

In this setting, I show that increases in CSR are accompanied by increases in public awards in the form of government contracts. I first document that OC laws have a positive impact on CSR, by showing that employee environment systematically improves following the passage of the laws. In later years when the measure becomes available, the presence of an OC law is also positively related to a broad index of CSR. Concurrently, firms obtain more public contracts and more money from public contracts following the passage of the laws.

The contribution of this paper is to explore a specific channel through which socially responsible behavior can benefit companies: accumulating public goodwill. This does not preclude the existence of other channels. Future work should address alternative channels, such as ability to attract and retain talent, in particular in more recent years.

Table 19: Constituency Statutes by Year

State	Statute	Passage Year
Arizona	Ariz. Rev. Stat. Ann. §10-2702	1987
Connecticut	Conn. Gen. Stat. §33-756(d)	1988
Florida	Fla. Stat. §607.0830(3)	1989
Georgia	O.C.G.A. §14-2-202(b)(5)	1989
Hawaii	HRS §414-221(b)	1989
Idaho	Idaho Code §30-1602; §30-1702	1988
Illinois	805 ILCS 5/8.85	1985
Indiana	Burns Ind. Code Ann. §23-1-35-1(d)	1986
Iowa*	I.C.A. §491.101B	1989
Kentucky*	KRS §271B.12-210(4)	1988
Louisiana*	La. Rev. Stat. §12:92(G)	1988
Maine*	13-C M.R.S. §831(6)	1985
Maryland	MD Corps. & Assoc. §2-104(b)(9)	1999
Massachusetts	ALM GL ch. 156D, §8.30(a)(3)	1989
Minnesota	Minn. Stat. §302A.251 subd. 5	1987
Mississippi	Miss. Code Ann. §79-4-8.30(f)	1990
Missouri*	Mo. Rev. Stat. §351.347	1986
Nebraska	Neb.Rev.St. §21-2095	1988
Nevada	NRS 78.138(4)	1991
New Jersey	N.J. Stat. §14A:6-1(3)	1989
New Mexico	N.M.S.A. §53-11-35(d)	1987
New York	NY Bus. Corps. Stat. §717(b)	1989
North Dakota	N.D. Cent. Code, §10-19.1-50(6)	1993
Ohio	Ohio Gen. Corp. Law §1701.59(F)	1984
Oregon*	ORS §60.357(5)	1989
Pennsylvania	15 Pa C.S.A. §1715(a)(b) or §516(a)	1983
Rhode Island*	R.I. Gen. Laws §7-5.2-8(a)	1990
South Dakota*	S.D. Codified Laws §47-33-4(1)	1990
Tennessee*	T.C.A. §48-103-204	1988
Texas	Tex. Bus. Orgs. Code Ann. §21.401	2003
Vermont	11A V.S.A. §8.30(a)(3)	1998
Wisconsin	Wis. Stat. §180.0827	1987
Wyoming	Wyo. Stat. §17-16-830(g)	1989

Table 20: OSHA Summary Statistics 1979-2006

	N	Mean	Std. Dev.
Violations	240,722	0.94	7.46
Serious violations	240,722	0.40	3.41
Number of instances	240,722	2.43	35.96
Number exposed	240,722	26.42	487.34
Accidents	240,722	0.02	0.46
Complaints	240,722	0.11	1.29
Hazardous substance violation	240,722	0.03	0.56

Table 21: KLD Summary Statistics (2000-2009)

	Mean	Std. Dev.	Min	p25	p50	p75	Max
Aggregate CSR Score	-0.19	2.02	-9	-1	0	1	16
Aggregate CSR Strengths	1.17	1.93	0	0	0	2	21
Aggregate CSR Concerns	1.36	1.56	0	0	1	2	13
Community CSR Score	0.04	0.49	-2	0	0	0	4
Diversity CSR Score	0.23	1.23	-2	-1	0	1	7
Employee CSR Score	-0.19	0.85	-4	-1	0	0	5
Environment CSR Score	-0.1	0.66	-5	0	0	0	4
Product CSR Score	-0.18	0.59	-4	0	0	0	2

Table 22: Government Contract Summary Statistics (1984-1993)

	N	Mean	Std. Dev.
Percent contracted	61,498	8%	27%
Total dollars	61,498	19,439	607,809
Total contracts	61,498	6.51	91.54
Dollars per contract	4,721	2,347	12,964

Table 23: Average firm characteristics as of 1984, OC v. non-OC states

	Not Constituency		Constituency	
	Mean	Std. Dev	Mean	Std. Dev
Revenue	4,297.20	14,986.38	3,948.63	15,644.14
Net Income	241.39	1,261.24	273.86	1,606.99
Total Assets	10,563.19	69,818.46	8,356.33	36,763.28
EBITDA	757.23	2,893.59	730.71	3,331.02
Employees (000s)	14.76	62.85	12.29	34.05

All values in $ millions except where otherwise indicated

Table 24: OSHA Outcomes

	Panel A: Violations		
	Violations	Number of instances	Serious Violations
OC*Post	-0.223***	-0.560**	-0.0843**
	(0.0735)	(0.275)	(0.0345)
BC*Post	0.145***	0.0257	0.100***
	(0.0378)	(0.312)	(0.0272)
Industry-Year FE	Y	Y	Y
State of Incorporation FE	Y	Y	Y
Observations	240,722	240,722	240,722
R squared	0.175	0.123	0.19

	Panel B: Other Behaviors		
	Complaints	Hazardous substance found	Accidents
OC*Post	-0.0266**	-0.0167**	-0.00530***
	(0.0103)	(0.00779)	(0.00174)
BC*Post	0.0163	0.00168	0.00163
	(0.0101)	(0.00523)	(0.00272)
Industry-Year FE	Y	Y	Y
State of Incorporation FE	Y	Y	Y
Observations	240,722	240,722	240,722
R squared	0.138	0.133	0.102

Table 25: Total Dollars

	(1)	(2)	(3)	(4)
OC*Post	0.0642*	0.0643*	0.0568*	0.0717**
	(0.0359)	(0.0332)	(0.0302)	(0.0329)
BC*Post	-0.0555*	-0.0607*	-0.0640**	-0.0649**
	(0.0292)	(0.0303)	(0.0298)	(0.0303)
Ln(Employees)		0.305***		0.162***
		(0.0151)		(0.0139)
Ln(Assets)			0.278***	0.158***
			(0.0163)	(0.0195)
Industry-Year FE	Y	Y	Y	Y
State of Incorporation FE	Y	Y	Y	Y
Observations	61,498	50,302	55,390	50,297
R-squared	0.141	0.214	0.203	0.216

Table 26: Number of Contracts

	(1)	(2)	(3)	(4)
OC*Post	0.0180	0.0191	0.0164	0.0214*
	(0.0130)	(0.0119)	(0.0108)	(0.0118)
BC*Post	-0.0299**	-0.0331***	-0.0336***	-0.0345***
	(0.0116)	(0.0117)	(0.0116)	(0.0118)
Ln(Employees)		0.102***		0.0561***
		(0.00597)		(0.00395)
Ln(Assets)			0.0922***	0.0507***
			(0.00644)	(0.00670)
Industry-Year FE	Y	Y	Y	Y
State of Incorporation FE	Y	Y	Y	Y
Observations	61,498	50,302	55,390	50,297
R-squared	0.141	0.220	0.208	0.223

Table 27: Dollars per Contract

	(1)	(2)	(3)	(4)
OC*Post	0.0963	0.119**	0.0932	0.121**
	(0.0617)	(0.0582)	(0.0563)	(0.0577)
BC*Post	-0.0494	-0.0133	-0.0259	-0.0127
	(0.0528)	(0.0503)	(0.0543)	(0.0499)
Ln(Emp)		0.143***		0.165***
		(0.0190)		(0.0425)
Ln(Assets)			0.128***	-0.0220
			(0.0184)	(0.0422)
Industry-Year FE	Y	Y	Y	Y
State of Incorporation FE	Y	Y	Y	Y
Observations	4,721	4,569	4,697	4,569
R-squared	0.785	0.805	0.798	0.805

Table 28: Government Contract Awards – OLS Regression with Controls

	(1)	(2)	(3)
	Contracted	Ln(1 + contracts)	Ln(1 + dollars)
CSR Score	0.0162***	0.120***	0.308***
	(0.00425)	(0.0229)	(0.0582)
Revenue	5.09e-06***	3.43e-05***	8.98e-05***
	(6.01e-07)	(4.04e-06)	(1.09e-05)
Total Assets	4.38e-07**	1.19e-06	5.62e-06
	(1.96e-07)	(1.18e-06)	(3.54e-06)
EBIT	-1.31e-06	7.05e-06	4.16e-06
	(7.25e-06)	(3.47e-05)	(0.000123)
Industry-Year FE	Y	Y	Y
Observations	18,011	18,011	17,904
R-squared	0.049	0.105	0.08

*** p<0.01, ** p<0.05, * p<0.1

Standard errors: clustered at state of incorporation

Table 29: Government Contract Awards – IV Regression

	Panel A: First Stage
	(1)
	CSR Score
Constituency State	0.210**
	(0.0803)
Industry-Year FE	Y
Observations	18,199
R-squared	0.002

	Panel B: Two Stage Least Squares		
	(1)	(2)	(3)
	Contracted	Ln(1 + contracts)	Ln(1 + dollars)
CSR Score	0.179*	0.843**	2.564*
	(0.102)	(0.405)	(1.448)
Industry-Year FE	Y	Y	Y
Observations	18,199	18,199	18,092
R-squared	0.103	0.203	0.189

*** p<0.01, ** p<0.05, * p<0.1

Standard errors clustered at state of incorporation

APPENDIX

Example Language of OC Law From Pennsylvania

"In discharging the duties of their respective positions, the board of directors, committees of the board and individual directors of a domestic corporation may, in considering the best interests of the corporation, consider the effects of any action upon employees, upon suppliers and customers of the corporation and upon communities in which offices or other establishments of the corporation are located, and all other pertinent factors." (15 Pa. Cons. Stat. §516(a))

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