# The Impact of Restricting Labor Mobility on Corporate Investment and Entrepreneurship

Jessica S. Jeffers[*]

January 3, 2018

## Abstract

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. The estimates in my sample suggest that, in such sectors, there is roughly \$2 million of additional capital investment from publicly-held firms for every lost new firm entry.

# 1    Introduction

Recent research and policy proposals have renewed the debate over agreements that prevent workers from leaving their employers ("labor mobility restrictions") (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, Haltiwanger, Jarmin and Miranda 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 important because it can help shed some light on these trends. Moreover, these two components of growth intuitively trade off: it is a tension underlying both the concepts of creative destruction (**?**) and boundaries of the firm (**?**).

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. Since randomly assigning mobility restrictions to employees is not possible, this means finding a source of variation in labor mobility that is otherwise uncorrelated with capital investment and entrepreneurship. The second challenge is observing mobility for a large and diverse set of workers, including information on their employment before and after moving, and the entry of new firms that capture growth (as opposed to subsistence) entrepreneurship.

To address the first challenge, I focus on a particular restriction on labor mobility, non-

compete agreements (NCs). NCs 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, my identification strategy relies on a new setting of seven state supreme court rulings and one law that modified the enforceability of NCs 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 as legislators, 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, NCs affect mobility for a large number of workers: Starr, Prescott and Bishara (2016) estimate that 18% of all labor force participants are currently subject to a NC, with rates as high as 35% among tech workers and engineers.

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 focus on workers and firms for which NCs matter the most, namely those engaged in and relying on knowledge-intensive activities (Starr et al. 2016). Moreover, I observe company-level information such as industry and year founded for both origin and destination firms. As a result I am able to isolate moves to competitors and to new businesses, and the entry of new businesses. Importantly, the type of new firm that I will capture on LinkedIn is more likely to be the kind of growth entrepreneurship that is of interest here, as opposed to new establishments or new subsistence entrepreneurship that census-type data capture. Another important advantage is that the data encompass a wide range of workers in all fifty states.[3] Looking 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.[4,5]

---

[1] I find similar results whether I limit the analysis to the seven court rulings only, or include the law change in Georgia.

[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.

[3] The data include workers in foreign countries as well, though I do not use these in this paper.

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

[5] Active members are defined as members who have visited LinkedIn in the past sixty days at time of retrieval.

The paper contains three main results. First, I establish the internal validity of my approach by verifying that NC enforcement has a significant impact on labor mobility. In my setting, an increase in NC enforceability leads to a 0.7 percentage point drop in the total departure rate. This drop is economically large, representing 8% of the average departure rate. For workers in knowledge-intensive occupations, I estimate a 17% decrease in the average departure rate. Consistent with the language in NC agreements, declines are particularly pronounced for within-industry departures, and for departures to more or equally senior positions, which are most likely to be voluntary departures and build on previous experience. I further focus on a subsample of knowledge-intensive occupations which I call "knowledge workers."[6] Because of the knowledge involved in their occupations, the mobility of these workers is both more likely to be restricted by NCs and to be costly to firms (Starr et al. 2016). I find that it is these knowledge workers in particular who drive the departure impact of NC enforceability.

Next, I estimate the economic impact of these changes in labor mobility by considering two sets of outcomes: entrepreneurship and capital investment. I approximate departures to entrepreneurship by counting departures to newly founded businesses, and find that departures from knowledge-intensive firms to entrepreneurship decrease 9% relative to the average rate following stronger enforceability of NCs. I am also able to look specifically at individuals who leave to become the founder of a new company, and although the results are less consistently statistically significant, I do consistently find a drop of 6-8% relative to average. In turn, the entry of new firms in knowledge sectors declines by 17% relative to average.[7]

Lastly, I examine the impact of NC enforceability on capital investment. Entrepreneurship declines when NCs are more enforceable, yet it is possible there is an economic benefit for existing firms. In particular, 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

---

[6]Knowledge worker occupations are arts & design, business development, consulting, education, engineering, entrepreneurship, finance, information technology, media & communication, operations, product management, program & project management, and research.

[7]Knowledge sectors correspond to the occupations identified as knowledge-intensive, excluding Finance. Specifically, there are three knowledge sectors: Technology; Professional, scientific & technical services; and Education & training.

more highly dependent on human capital, the net capital investment rate rises. Knowledge-intensive firms increase investment by $8-10k for every $100k of capital, or roughly $22-24 million for the average firm and $2.5-3 million for the median firm. Finally, I do a back-of-the-envelope calculation to understand the trade-off these estimates imply between increased capital investment on the one hand and decreased firm entry on the other. The estimates for my sample suggest about $2 million of capital investment from publicly-held knowledge-intensive firms is added for every lost new knowledge-intensive firm entry.

The results stand up to a range of robustness analyses. An important assumption underlying the empirical approach is that treated and untreated observations are trending similarly before the change in NC enforceability, and would have continued to do so absent the change. I show graphically that when I break out the difference estimates by years to and from treatment, the estimates are close to zero and fairly stable prior to the NC changes, and the effects occur *after* the NC changes. 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. Results are also robust to including firm-year or state-year fixed effects whenever possible (e.g., in triple difference specifications). This addresses concerns that unobserved local conditions could drive both court rulings and mobility and investment outcomes. Finally, I test robustness to different levels of "footprint concentration" for firms. To assign the law changes to firms, I use the state in which a firm has the most employees as that firm's "main state." I show that the greater the fraction of a firm's employees who are in this main state (i.e., the less noisy is the treatment assignment), the more significant the results are, both statistically and economically.[8]

The main contribution of this paper is to document a trade-off between entrepreneurship and capital investment as a result of tighter mobility restrictions, and to provide more granular and broad-based evidence for the mechanism than previously possible. As such, the paper fits in with three related but distinct sets of literature. First, this paper builds on questions raised by Zingales (2000) about corporate finance in the context of firms' increasing dependence on

---

[8]The reason I report both sets of results is that there is a trade-off: by requiring a greater fraction of employees to be in the main state, I obtain a less noisy treatment assignment, but I also lose some observations. That said, more than three quarters of sample firms have 40%+ of their employees in their main state.

human capital, and picked up in a growing literature relating frictions in human capital to firm value (Chen, Kacperczyk and Ortiz-Molina 2011, Eiling 2013, Donangelo 2014). More specific to my context, a few studies relate these frictions to capital decisions (Autor, Kerr and Kugler 2007, Garmaise 2011). Garmaise (2011), in particular, also considers the role of NCs, though the paper focuses on CEOs rather than knowledge workers, and finds that investment relative to labor *decreases* after NCs become more enforceable in three states. In contrast, I find in my setting that investment increases with NC enforcement, and argue that this is due to complementarities between human and physical capital in knowledge-intensive firms.[9] At the same time, I find that there is a substantial trade-off between this investment increase and entrepreneurship. My findings thus 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).

Second, this paper contributes to the growing literature on labor mobility restrictions. Other papers studying the relationship between NCs and employee departures include Marx, Strumsky and Fleming (2009) (inventors), Garmaise (2011) (executives), Lavetti, Simon and White (2014) (physicians), and Starr, Balasubramanian and Sakakibara (2015) (survey).[10] Conti (2014) also considers the impact of NCs on patenting and Kang and Fleming (2017) on market structure. A related set of papers looks at the Inevitable Disclosure Doctrine (IDD), a judicial doctrine that restricts mobility for certain workers with access to trade secrets (Png and Samila 2013, Liu 2016, Qiu and Wang 2016). The contribution of this paper is the focus on the entrepreneurship-investment trade-off, and the use of LinkedIn data to provide detailed evidence on different types of mobility and entrepreneurship.

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 NC enforcement has a large impact on firm entry, but also present a potential drawback of limiting NC agreements. Quantifying this trade-off is particularly important in light of recent proposals to limit the use of NCs at both the state and

---

[9]This is not necessarily contradictory with the result from Garmaise (2011) since I look at investment-to-capital and he looks at investment-to-labor, but it adds some nuance. In my setting I do not find a result statistically different from zero when I look at the investment-to-labor ratio.

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

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 my setting, mobility restrictions generate substantially higher capital investment at existing knowledge-intensive firms. NCs are a form of private ordering, and therefore government intervention into this arrangement needs to justify reallocating resources from one set of agents (e.g. existing firms) to another set of agents (e.g. entrepreneurs). Such justifications could include frictions such as asymmetric information between the agents, or inefficient externalities generated by the arrangement – e.g. if there are externalities to entrepreneurship. While this is beyond the scope of this paper, the aim of this analysis is to provide a stepping stone to answer these important questions.

The remainder of the paper is structured as follows. Section 2 explains the NC policy setting. Section 3 describes the data. Section 4 outlines the hypotheses and empirical approach. Section 5 presents the main results, and section ?? covers robustness. Section 6 concludes.

# 2 Empirical Setting: Non-Compete Agreements (NCs)

A non-compete agreement (also known as a covenant not to compete, hereafter NC) is a contract provision between an employer and an employee that precludes the employee from working for a competitor after separating from the employer, usually for a limited time (e.g. 18 months) and for a specific geographic area (e.g. a 15 mile radius from any of the employer's offices).

## 2.1 Prevalence of NCs

In a survey of more than 11,000 U.S. labor force participants, Starr et al. (2016) find that 38% workers have at some time signed a NC, and that 18% are currently subject to one. NCs are more common for more knowledge-intensive positions: Starr et al. (2016) estimate that 39% of college-educated workers and those earning more than $100k have NCs, and 35% of those in architecture, engineering or computer and mathematical occupations are currently subject to a NC. Senior employees and in particular executives are also more likely to be subject to NCs: Garmaise (2011) finds evidence of NCs for top executives in 70% of the public companies he examines. Nonetheless, NCs 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 NC (Starr et al. 2016). Anecdotally, news organizations from the Wall Street Journal to the Washington Post and the Atlantic have reported on the increased prevalence of NCs over the past 15 years, including in relatively unskilled positions such as sandwich-maker.[11]

## 2.2   NC Enforceability

NCs are governed at the state level, and there is wide variation across states in the type of NCs that are permitted and how they are enforced. At one extreme, California bans the use of NCs. At the other extreme, several states allow enforcement of NCs 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 NCs 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 NCs, many states consider continued employment in and of itself to be sufficient consideration; others require any new NC or NC amendment to be paired with a material benefit to the employee, e.g. a promotion (Starr 2016a).

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 NC law, provides a useful example. Consider the following fictional NC:

> "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 NC is simply unenforceable. The court cannot modify it to apply only to Cook County. If the state allows a blue pencil

---

[11] *The Wall Street Journal*, Feb. 2, 2016, "Noncompete Argeements 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".

approach, the court may strike out portions of the agreement to make it enforceable, but they may not make any other changes. In this example, 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.[12] A consequence of the reformation approach is that employers have an incentive to draft NCs 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, Bishara and Martin 2014).

Building on these six dimensions identified by Bishara (2011), Starr (2016a) constructs an index of NC enforceability for 1991 and 2009. In Table 2, I show that treated and control states were at similar levels of NC enforceability in 2009.

## 2.3 Changes in Enforceability

NCs 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 8 outlines the NC 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 verifying all cases using Westlaw.[13] For each case, I also search for what local attorneys wrote about the expected impact of the decision, and verify that it is consistent with my interpretation.[14] 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 NCs entered into before the decision was issued,

---

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

[13]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.

[14]For example, the law firm Jackson Lewis writes of the Colorado decision "The ruling will impact Colorado employers' decision about how and when to implement non-competition agreements." The law firm Beck Reed & Riden writes of the Texas decision "Texas has just become much more favorable to employers than it has been in the last two decades." The law firm Seyfarth Shaw writes of the Virgina decision "There can be no doubt that the [Virginia] decision will invigorate employers to enforce noncompete agreements and make employers more reluctant to hire employees who are subject to noncompete provisions."

as well as new NCs.

Two of the 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 NCs. To reflect this, I code Illinois as an increase in 2012 but no longer an increase in 2013.[15] In Montana, the decision concerns the applicability of NCs to terminated employees and is therefore a more narrow change. I verify that my results are robust to excluding Montana.

During my sample period, in 2011, Georgia also passes 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 NCs with their employees in order to benefit from modification. I include the Georgia change in my main specifications, but also verify that all of my results are robust to removing the state.

## 2.4 State Supreme Court Composition

Table 9 shows the composition of the state supreme courts for the seven states in which I observe the relevant rulings. 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. Judges on these courts 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 4 shows the geographic location of these states. In Section 4, 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

---

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

control group in the years leading up to the decisions.

## 2.5   Impact on Mobility

The main mechanism through which NC enforcement is likely to dissuade labor mobility is deterrence. There are several (complementary) ways this can happen. 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 5 shows that since 2008, courts have decided 900-1,000 NC 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 NC breach has increased.[16] Third, employers can and do remind workers of their obligations.[17] Fourth, prospective employers refrain from hiring workers who are likely subject to NCs.[18] Fifth, in the case of modification, employers have incentives to draft broader NCs going forward (Thomas et al. 2014). Finally, there could also be a peer effect: As fewer colleagues move to or establish a new firm, this could reduce the motivation for an employee to follow that path.

For prosecution of NCs, the relevant jurisdiction is typically the place of performance of the economic activity (Lester and Ryan 2010). I gather information on employee location from LinkedIn, and assign firms to the state in which they have the most employees. I also show that the effect is stronger when I restrict to more concentrated firms, e.g. where a minimum of 20% or 40% of employees are located in the assignment state.

---

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

[17] 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."

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

# 3 Data

## 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 to the website at least once in the 60 days before the snapshot was created. 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 the history of where they have worked and in what capacity, as well as where they went to school and other interests. I use the start years and end years from this history, along with information on the employers, to code employee movements between firms. In this way an individual does not have to be a LinkedIn member at the beginning of my sample period to be present at the beginning of my sample. For example, an individual could have become a LinkedIn member in 2015 but posted her employment history going back to 2005; the individual would then be in my sample starting in 2005.

LinkedIn standardizes employer information, and 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 properly aggregate all individuals working for the same firm, and to identify within-industry moves and 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 6 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

11

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 NCs (see Figure 7). 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. Combined with the requirement that they be active on the site in 2016, this should minimize the number of individuals with incorrect information about their employment before 2014.

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 employed elsewhere in year t+1. The departure rate of a firm is its number of departures scaled by the number of employees as measured on LinkedIn. In untabulated results I verify that the results are robust to using the Compustat number of employees in the denominator instead, to verify the results are driven by the numerator.

Table 1 Panel A contains summary statistics for departure rates. All variables are scaled by the same number of employees, so the total departure rate is mechanically higher than all other departure rates. The total average departure rate is 9.27% (median 8.00%), and the average knowledge worker departure rate is 4.71% (median 1.42%). In other words, knowledge workers represent roughly half of all departures. The former number is relevant in interpreting results from the generalized difference-in-differences specification, while the latter is useful for the triple differences specification. Panel A also shows the average departure rate to newly founded firms, and to newly founded firms with a "founder" title. The numbers are small, but the results still show a systematic effect for these outcomes (and no other available data source contains this information.)

I also use LinkedIn company pages to measure the entry of new firms. Using year founded,

location, and industry, 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.[19] To make numbers comparable across states, I scale the number of newly founded firms by population. In Table 1 Panel C, I show summary statistics for both scaled and unscaled observations, for easier interpretation. The average number of firms entering per year per industry is 2.53, while the median is 1.

I believe these data for new firm entry improve on BDS data for several reasons. First, I believe firms that we observe as start-ups on LinkedIn are more likely to capture the kind of growth entrepreneurship that we care about than Census numbers of new firm starts. Moreover, the BDS statistics available by state and year are aggregated in very broad sectors, with nothing that would map to a technology sector. Finally, BDS counts the number of opening establishments from new firms, which could over- or under-count the number of new firms.

## 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 the firm's investment decision occurs each period, and so is conditional on the depreciated stock of capital in that period. However, results are qualitatively similar when I scale by gross capital or assets instead.

---

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

Table 1 Panel B presents summary statistics for this sample. The average net capital investment rate is 0.30, and the median 0.22. Knowledge firms – firms employing a higher than median fraction of knowledge workers – have slightly higher but similar investment rates, and tend to be slightly smaller.

# 4 Hypotheses and Empirical Approach

## 4.1 Hypothesis Development

*A) Labor Mobility and Entrepreneurship*

Just as NCs 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 NCs 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 NCs will be seized by individuals not limited by these agreements (e.g. college students). In that case there would be no overall impact on the creation of new firms. 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). As a result, I hypothesize that new firm entry will also decline in response to stronger enforceability of NCs. 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 NCs.

*B) Labor Mobility and Firm Investment*

Consider a typical firm with two inputs to production: physical capital and human capital. Here, human capital combines both the number and quality of the workers - the same way that physical capital captures not the number of machines but their value, we can think of human capital as capturing the knowledge and skills embodied in the firm's workforce. I assume that 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. (This is especially true in more knowledge-intensive occupations.)

14

We can thus think of a depreciation rate for human capital as well, which is negatively related to tenure. Intuitively, high turnover in the workforce is costly for firms because it prevents knowledge – human capital – from accumulating. There is also a potential effect of uncertainty: since it can be hard to properly gauge the quality of prospective hires, high turnover can also increase uncertainty about the quality of human capital tomorrow. In this setting, making it easier for workers to leave (e.g. by decreasing NC enforceability) is akin to increasing the average and volatility of the depreciation rate of human capital. Conversely, increasing NC enforceability, which increases workforce tenure, represents a decrease in the average and volatility of the human capital depreciation rate.

The impact on the firm's 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.

It is possible that the nature of this relationship (complement or substitute) depends on the nature of the job, or 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, NCs are more common in occupations that are knowledge-intensive, since their goal is to preserve valuable human capital. Figure 7 shows the incidence of NCs by occupation from Starr (2016b). I expect firms that employ more workers from high-NC occupations to have a greater positive response to NC enforcement increases, both because they are more likely to employ NCs and because their human capital may be less substitutable.

## 4.2    Methodology

I use a generalized difference-in-differences approach with NC 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.[20] I include company fixed effects $\gamma_{it}$ and industry-year fixed effects $\theta_{jt}$ 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 I include log market capitalization, assets, and employee size.

The main assumption underlying this approach is that absent the NC 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 NC enforcement.

In Figures 1-3, I show that there was no anticipation or differential trend between both groups prior to the treatment, in terms of departures. For Figure 1, 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 1 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

---

[20] 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 (2016a) 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. If I decompose $treated_i * post_t$ into $increased_i * post_t$ and $decreased_i * post_t$ – a separate indicator for states where enforcement increases and where it decreases – I obtain very similar estimates for $increased_i * post_t$ as for $treated_i * post_t$, and for $decreased_i * post_t$, the estimate is of similar magnitude but with the sign flipped and not statistically significant, which is not surprising given there are much fewer observations.

departure rate in the treatment group drops significantly relative to the departure rate in the control group. The omitted variable is the difference three or more years prior to treatment.[21]

In Table 2, I report political and economic characteristics for eventually treated and control states, 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 2. The first shows the ex-ante mean for states in which NC enforcement does not change in my sample period (Never Treated). The second shows the ex-ante mean for all states in which NC enforcement changes during the sample period (Eventually Treated). I then break this group into two groups, based on whether NC enforcement increases or decreases. The last group contains only Montana and South Carolina experience decreases in the enforceability of NCs.[22]

Looking at these political and economic characteristics, treated and control states seem overall similar, although Montana and South Carolina have lower average GDP per capita and are more conservative than the rest. Regression results are robust to excluding these two states. States in which NC enforceability 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.

Finally, using Starr et al. (2016) I separate out occupations that are most likely to be affected by NCs. Figure 7 reproduces the incidence of NCs by occupation found in their survey. I call individuals in the highest NC 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

---

[21]Only Illinois and Virginia have information more than three years prior to treatment, as the changes there occur in 2012 and 2013. Similarly, only Wisconsin has information more than three years after treatment (the change there occurs in 2009), so I group these years together.

[22]Although I include p-values for the t-test of differences in Table 2, these are only marginally informative given the small size of the sample involved.

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.[23]

# 5 Results

## 5.1 Employee Mobility

To verify that NC enforcement impacts mobility, I first look at departure rates to determine whether NCs have an impact on employee mobility. Table 3 Panel A shows results from a difference-in-differences regression of departure rate on an indicator for increased NC enforceability. The sample universe is public companies matched between LinkedIn and Compustat. Following an increase in NC enforceability, the departure rate drops by 0.73 percentage points, which represents 8% of a 9.3% average departure rate. As expected, this effect is pronounced for within-industry moves, which drop by 0.4 percentage points or 12% relative to the average within-industry departure rate. In contrast, the drop in departures to jobs in different industries is not statistically significant, and the magnitude is lower (5% relative to average). Next, I break out employee moves by the seniority level of the new position relative to the old position. Moves where employees increase in seniority or state at the same seniority level are more likely to be voluntary than moves to less senior positions. Moreover, moves where seniority increases might capture situations where a new position builds on the employee's prior experience (to justify the higher seniority). Indeed, increased enforceability of NCs leads departures to more senior positions to decrease substantially, by 39 basis points or 15% relative to the average rate. Departures where employees stay at the same seniority level decrease 28 basis points, or 6% relative to average, while the estimate for departures to lower seniority is not statistically significant.

In Table 3 Panel B, I show that these results are driven by workers in knowledge-intensive occupations by estimating difference-in-difference-in-differences (or "triple differences") regressions. These regressions compare the impact of higher NC enforceability on all occupations

---

[23]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.

to the impact on the subsample of occupations that Starr et al. (2016) identify as more prone to NCs. The coefficient estimate on *treated\*post\*knowledge worker* represents how much more knowledge-intensive occupations respond to the NC enforcement changes. For all categories except departures where seniority decreases, this estimate is negative, showing decreased departure rates are driven by knowledge workers. Taken together, these results indicate an important economic impact of increased NC enforceability. For an average firm size of 4,054 employees, 0.73 percentage points represent 30 individuals.

## 5.2    Entrepreneurship

One of the main costs to NCs 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).

I proxy for entrepreneurship by looking at departures to newly founded businesses. Specifically, a business is new if it is founded within a year in either direction of the employee's departure. I also look specifically at individuals who move to new firms where they list themselves as founders of the new firms.

In Table 5, I look at the total number of departures to newly founded businesses as well as departures to "founder title." I again separate out the effect on knowledge workers, as in the previous section and following the discussion in Section 4.

The results suggest that an increase in NC enforceability discourages knowledge workers from leaving to start or join small new firms. Summing the two coefficient estimates to get the total effect on workers from knowledge firms, the results suggest a 1.5 basis point drop in departures to new firms, and 0.1 basis point drop to founder title at a new firm. Since these rates are very low to begin, the estimates are in fact fairly large relative to average: a 9% decrease in departure rate to new firms and 6% decrease to founder title at new firms. The latter result is not statistically significant in the full sample of firms, but becomes statistically significant at the 90% level when the sample is limited to firms with a more concentrated footprint (column 4). The numbers are quite small, so should be interpreted with some caution, but provide additional evidence of the channel through which NCs end up affecting new firm entry.

In Table 6, I turn to the number of new businesses founded per industry-year. Again, I find a more pronounced effect in knowledge sectors, defined as firms in technology, professional, scientific & technical services, or education & training.[24] 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 following higher NC enforceability, the knowledge sector experiences a 17% decrease in new firms. To better understand the economic interpretation of the results, I break them out by sector in Table **??**. New firm entry declines 6% in the technology sector, 12% in professional, scientific & technical services, and 28% in education & training.

## 5.3   Capital Investment

Stronger NCs lead to a decline in entrepreneurship, but do they lead to an increase in investment elsewhere? In Table 7, I show that tighter restrictions on labor mobility consistently lead to a higher investment rate. Following the discussion in Section 4.1 and the results in Section 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 NC 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 $10k for every $100k of net capital, and for firms with high R&D intensity, $8k. At first, the effect appears extremely large: Comparing to the average investment rate, the coefficient estimates imply an increase of 21%, and even more in knowledge and high R&D firms.

However, the estimates make more sense in an economic context. Multiplied by the average net capital stock, the estimates indicate a total $24 million increase in investment in knowledge firms and a total $22 million increase in high R&D intensity firms. Put differently, the median

---

[24]I use Starr et al. (2016) as a reference for industries in which NCs are more frequently used. I exclude finance because of the prevalence of "garden leave" (compensated NCs) in finance, and the time period which includes the crisis.

knowledge firm increases investment by \$3.2 million, and the median R&D intensive firm by \$2.7 million.

## 5.4 Trade-off

Both results taken together point to a tension in the impact of labor mobility restrictions: on the one hand, stronger enforcement of NCs leads to a decline in new firm entry, but on the other hand it leads to an increase in capital investment, at least at publicly-held firms. While these are only partial outcomes, it seems nonetheless valuable to understand how at least these two effects trade off against each other.

I take advantage of the fact that I observe both responses within the same natural experiment setting to get an approximation of the trade-off in my sample. First, I do a back-of-the-envelope exercise to gauge the aggregate loss of knowledge-intensive entrants in my sample of treated states. In Table **??**, the observations are number of new entrants scaled by state population, at an industry-state-year level. I adjust by number of industries (for each sector) and population to get an estimated loss over all treated states, across these knowledge-intensive sectors. I also have to account for the fact that my sample only captures private firms for which I observe year founded on LinkedIn. To get a better approximation of the aggregate loss of knowledge-intensive entrants, I infer the coverage rate of my sample from the Business Dynamics Statistics (BDS) database of the Census (about 5%).[25] Overall, my estimates imply an aggregate loss of close to 3,000 knowledge-intensive entrants across treated states.

Second, I repeat the back-of-the-envelope approach to gauge the aggregate increase in capital investment in treated states. In Table 7 column 3, the estimated increase in investment rate for a treated high R&D firm is the sum of the two coefficients, i.e. 0.0786. This number represents a dollar increase in capital investment scaled by net capital, at a firm-year level. I adjust by average net capital for high R&D firms, and also aggregate over the number of high R&D firms in the treated states. [26] Overall, my estimates imply an increase of a little over \$6

---

[25] I do not use the BDS data directly for my regressions for several reasons that I discuss in the data section. However, for the purpose of the back-of-the-envelope aggregation exercise, the BDS data provide the best available comparison point of which I am aware.

[26] I count high R&D firms even if I am not able to match them to LinkedIn data. Almost all publicly-held firms have LinkedIn pages, but because I have to do the matching mostly based on name identifiers, I lose almost 60%

billion in capital investment coming from publicly-held high R&D firms across treated states. The resulting trade-off is approximately $2 million of additional capital investment for every lost firm entry, in knowledge-intensive sectors. An important caveat is that this counts only investment from publicly-held firms. I do not attempt to generalize the result to privately-held firms because I have no point of information for the investment behavior of these firms, or the number and size of these firms in my treated states. However, if privately-held firms respond in a similar way to publicly-held firms, we can think of the trade-off number here as somewhat of a lower bound on the actual trade-off.

# 6    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 (NCs), mostly for its potentially negative effects on knowledge spillovers and entrepreneurship (Samila and Sorenson 2011, Matray 2014). However, NC 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 NC 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 NCs 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 NCs 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-

---

currently in my matching. Since the purpose here is to gauge the aggregate effect, I multiply by all publicly-held high R&D firms.

intensive firm increases its investment rate by an estimated $2.3-3 million annually. At the same time, the rate of entry of new firms in knowledge sectors declines by 17% 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 among knowledge-intensive firms at around $1.5 million in capital investment (just from publicly-held firms) for every foregone entrant. 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.

# References

**Acemoglu, Daron and Robert Shimer**, "Holdups and Efficiency With Search Frictions," *International Economic Review*, 1999, *40* (4), 827–849.

**Autor, David H., William R. Kerr, and Adriana D. Kugler**, "Does Employment Protection Reduce Productivity? Evidence from US States," *The Economic Journal*, 2007, *117*.

**Babina, Tania**, "Destructive Creation at Work: How Financial Distress Spurs Entrepreneurship," *Working Paper*, 2015.

**Bhide, 1994**, "How Entrepreneurs Craft Strategies That Work," *Harvard Business Review*, 1994.

**Bishara, Norman D**, "Fifty Ways to Leave Your Employer: Relative Enforcement of Covenants Not to Compete, Trends, and Implications for Employee Mobility Policy," *University of Pennsylvania Journal of Business Law*, 2011, *13* (3).

**Chen, J., M. Kacperczyk, and H. Ortiz-Molina**, "Labor Unions, Operating Flexibility, and the Cost of Equity," *Journal of Financial and Quantitative Analysis*, 2011, (46), 25–58.

**Conti, Raffaele**, "Do Non-Competition Agreements Lead Firms to Pursue Risky R&D Projects?," *Strategic Management Journal*, 2014, *35*, 1230–1248.

**Decker, Ryan, John Haltiwanger, Ron Jarmin, and Javier Miranda**, "The Role of Entrepreneurship in US Job Creation and Economic Dynamism," *Journal of Economic Perspectives*, 2014, *28* (3), 3–24.

**Donangelo, Andres**, "Labor mobility: Implications for Asset Pricing," *The Journal of Finance*, 2014, *69* (3).

**Eiling, Esther**, "Industry-Specific Human Capital, Idiosyncratic Risk, and the Cross-Section of Expected Stock Returns," *The Journal of Finance*, 2013, *68* (1).

**Franco, April M. and Matthew F. Mitchell**, "Covenants not to compete, labor mobility, and industry dynamics," *Journal of Economics and Management Strategy*, 2008, *17* (3), 581–606.

**Furman, Jason**, "Business Investment in the United States: Facts, Explanations, Puzzles, and Policies," in "Reviving Private Investment" Remarks at the Progressive Policy Institute Washington, D.C. 2015.

**Garmaise, Mark J.**, "Ties that truly bind: Noncompetition agreements, executive compensation, and firm investment," *Journal of Law, Economics, and Organization*, 2011, *27* (2), 376–425.

**Gilson, Ronald J**, "The Legal Infrastructure of High Technology Industrial Districts: Silicon Valley, Route 138, and Covenants Not to Compete," *New York University Law Review*, 1999, *74* (3), 575–629.

**Hall, Bronwyn H. and Josh Lerner**, "The Financing of R&D and Innovation," in "Handbook of the Economics of Innovation" 2010.

**Kang, Hyo and Lee Fleming**, "Large Firm Advantage and Entrepreneurial Disadvantage: Non-competes and Market Concentration," *Working Paper*, 2017.

**Lavetti, Kurt, Carol Simon, and William White**, "Buying Loyalty: Theory and Evidence from Physicians," *Working Paper*, 2014.

**Lester, Gillian and Elizabeth Ryan**, "Choice of Law and Employee Restrictive Covenants: An American Perspective," *Comparative Labor Law & Policy Journal*, 2010.

**Liu, Zack**, "Intellectual Property Allocation and Firm Investments in Innovation," *Working Paper*, 2016.

**Marx, Matt, Deborah Strumsky, and Lee Fleming**, "Mobility, Skills, and the Michigan Non-Compete Experiment," *Management Science*, 2009, *55* (6), 875–889.

**Matray, Adrien**, "The Local Innovation Spillovers of Listed Firms," *Working Paper*, 2014, pp. 1–61.

**Png, I.P.L. and Sampsa Samila**, "Trade Secrets Law and Engineer/Scientist Mobility: Evidence from "Inevitable Disclosure"," *Working Paper*, 2013.

**Qiu, Buhui and Teng Wang**, "Does Knowledge Protection Benefit Shareholders? Evidence from Stock Market Reaction and Firm Investment in Knowledge Assets," *Working Paper*, 2016.

**Samila, Sampsa and Olav Sorenson**, "Noncompete Covenants: Incentives to Innovate or Impediments to Growth," *Management Science*, 2011, *57* (3), 425–438.

**Saxenian, AnnaLee**, "Regional Advantage: Culture and Competition in Silicon Valley and Route 128," *Cambridge, Mass.: Harvard University Press*, 1994.

**Starr, Evan**, "Consider This: Training, Wages and the Enforceability of Covenants Not to Compete," *Working Paper*, 2016.

———, "Redirect and Retain: How Firms Capitalize on Within and Across Industry," *Working Paper*, 2016.

———, **J.J. Prescott, and Norman Bishara**, "Noncompetes in the U.S. Labor Force," *Working Paper*, 2016.

———, **Natarajan Balasubramanian, and Mariko Sakakibara**, "Screening Spinouts? How Noncompete Enforceability Affects the Creation, Growth, and Survival of New Firms," *Working Paper*, 2015.

**The White House**, "Non-Compete Agreements: Analysis of the Usage, Potential Issues, and State Responses," Technical Report, The White House 2016.

**Thomas, Randall S., Norman Bishara, and Kenneth J. Martin**, "An Empirical Analysis of Non-Competition Clauses and Other Restrictive Post-Employment Covenants," *SSRN Electronic Journal*, 2014.

**Zingales, Luigi**, "In Search of New Foundations," *The Journal of Finance*, 2000, *55* (4).

# 7 Tables

## Table 1: Summary Statistics

This table reports summary statistics for key variables. Panels A and B contain information for publicly-held firms matched between LinkedIn and Compustat. Panel C contains information at the industry-state level for new firm entry, as measured by looking at privately-held firms with a LinkedIn page founded during the sample period 2008-2014.

| Variable | Obs | Mean | SD | 25% | 50% | 75% |
|---|---|---|---|---|---|---|
| **Panel A: Departure Rates** | | | | | | |
| | | | | | | |
| *All workers together* | | | | | | |
| Total departures | 11,524 | 9.27 | 8.37 | 4.55 | 8.00 | 12.11 |
| Same industry | 11,524 | 3.34 | 4.94 | 0.74 | 2.27 | 4.35 |
| Different industry | 11,524 | 5.92 | 6.25 | 2.38 | 4.83 | 7.87 |
| Increase seniority | 11,524 | 2.56 | 3.60 | 0.61 | 2.00 | 3.45 |
| Stay in seniority | 11,524 | 4.95 | 5.39 | 2.10 | 4.13 | 6.54 |
| Decrease seniority | 11,524 | 1.77 | 3.53 | 0.00 | 1.11 | 2.20 |
| To new firm | 11,524 | 0.32 | 1.24 | 0.00 | 0.00 | 0.26 |
| To founder title | 11,524 | 0.04 | 0.23 | 0.00 | 0.00 | 0.00 |
| | | | | | | |
| *Knowledge workers (scaled by total size)* | | | | | | |
| Total departures | 11,376 | 4.71 | 6.04 | 1.42 | 3.45 | 6.15 |
| Same industry | 11,376 | 1.69 | 3.51 | 0.00 | 0.85 | 2.06 |
| Different industry | 11,376 | 3.02 | 4.54 | 0.34 | 2.07 | 3.91 |
| Increase seniority | 11,376 | 1.30 | 2.47 | 0.00 | 0.77 | 1.67 |
| Stay in seniority | 11,376 | 2.61 | 4.20 | 0.00 | 1.77 | 3.39 |
| Decrease seniority | 11,376 | 0.81 | 2.53 | 0.00 | 0.29 | 0.88 |
| To new firm | 11,376 | 0.18 | 0.92 | 0.00 | 0.00 | 0.08 |
| To founder title | 11,376 | 0.02 | 0.20 | 0.00 | 0.00 | 0.00 |
| | | | | | | |
| **Panel B: Investment & Firm Characteristics** | | | | | | |
| | | | | | | |
| *All firms* | | | | | | |
| I/K | 5,030 | 0.30 | 0.29 | 0.12 | 0.22 | 0.39 |
| Investment | 6,048 | 56.68 | 177.82 | 0.56 | 5.56 | 30.23 |
| Net Capital | 6,597 | 475.98 | 1,383.09 | 8.68 | 49.81 | 263.25 |
| Assets | 7,308 | 1,651.47 | 4,169.39 | 49.54 | 280.06 | 1,294.38 |
| Market Cap | 7,308 | 2,163.04 | 7,504.00 | 54.33 | 319.28 | 1,329.30 |
| | | | | | | |
| *Knowledge firms* | | | | | | |
| I/K | 2,361 | 0.35 | 0.30 | 0.14 | 0.27 | 0.48 |
| Investment | 2,998 | 36.80 | 113.70 | 0.37 | 4.03 | 21.29 |
| Net Capital | 2,997 | 229.04 | 737.49 | 6.17 | 30.57 | 143.47 |
| Assets | 3,464 | 1,352.77 | 3,701.82 | 38.02 | 211.24 | 934.03 |
| Market Cap | 3,464 | 2,167.25 | 7,181.87 | 48.41 | 270.92 | 1,356.11 |
| | | | | | | |
| **Panel C: New Firm Entry** | | | | | | |
| | | | | | | |
| New firms | 31,850 | 2.53 | 8.95 | 0.00 | 1.00 | 2.00 |
| New firms all sizes, p. million | 31,850 | 0.37 | 0.88 | 0.00 | 0.10 | 0.37 |
| New firms - Education & training | 1,001 | 1.77 | 3.46 | 0.00 | 1.00 | 2.00 |
| New firms - Professional services | 6,447 | 3.98 | 12.51 | 0.00 | 1.00 | 3.00 |
| New firms - Technology | 5,005 | 4.49 | 16.77 | 0.00 | 1.00 | 3.00 |

## Table 2: Ex Ante Political & Economic Characteristics

This table reports ex-ante (2006-2007) means of political and economic characteristics for states where NC enforcement does not change, and for states where NC enforcement changes during the sample period. Below the means are p-values from a t-test of differences relative to the "never treated" observations, although given the small sample size p-values are only marginally informative. I further break out the treated states into states for which NC enforcement eventually increases (Colorado, Georgia, Illinois, Texas, Virginia, Wisconsin) and those for which it eventually decreases (Montana, South Carolina). The Partisan Voting Index comes from the 2010 Cook Political Report, and the NC enforceability score from the 2009 index constructed in (Starr 2016a).

|  | Never Treated | Eventually 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 |

### Table 3: NC Effect on Employee Departure Rates

Panel A presents the results of difference-in-differences estimations with different departure rates as the dependent variables.

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

Panel B presents the results for corresponding difference-in-difference-in-differences estimations, where each firm is split into "knowledge workers" and other workers, and the treated*post term is interacted with an indicator for the knowledge worker sample.

$$100 * \frac{\# \text{ departures}}{\# \text{ employees}}_{ikt} \quad = \quad \alpha + \beta_1\{\text{treated}_i * \text{post}_t * \text{knowledge workers}_k\} + \beta_2\{\text{treated}_i * \text{post}_t\} + \gamma_i + \lambda_{ks} + \theta_{kt} + \epsilon_{it}$$

The dependent variable is a 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. Column (3) is the complement, and includes only departures where the origin and destination industries are different. In Column (4), the numerator includes only departures to a more senior position than the employee was previously in. In Column (5), it includes only departures to a position of equal seniority, and in Column (6) of lesser seniority. The denominator is the same throughout, so Columns (2)-(6) are subsets of Column (1) – mechanically, the number is highest in Column (1). Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

Panel A: Generalized difference-in-differences at company level

|  | (1) Total departures | (2) Same industry | (3) Different industry | (4) Increase seniority | (5) Stay in seniority | (6) Decrease seniority |
|---|---|---|---|---|---|---|
| Treated*Post | -0.731*** | -0.406*** | -0.317 | -0.392*** | -0.277** | -0.0612 |
|  | (0.248) | (0.143) | (0.196) | (0.129) | (0.128) | (0.128) |
| Observations | 7,063 | 7,063 | 7,063 | 7,063 | 7,063 | 7,063 |
| R-squared | 0.591 | 0.583 | 0.587 | 0.472 | 0.534 | 0.446 |
| Industry-Year FE | Y | Y | Y | Y | Y | Y |
| Company FE | Y | Y | Y | Y | Y | Y |

Panel B: Triple differences at company-knowledge worker (KW) level

|  | (1) Total departures | (2) Same industry | (3) Different industry | (4) Increase seniority | (5) Stay in seniority | (6) Decrease seniority |
|---|---|---|---|---|---|---|
| Treated*Post*KW | -0.540* | -0.152 | -0.352 | -0.264** | -0.356** | 0.0801 |
|  | (0.284) | (0.165) | (0.285) | (0.110) | (0.142) | (0.155) |
| Treated*Post | -0.259 | -0.0802 | -0.198 | -0.132 | 0.149 | -0.276* |
|  | (0.216) | (0.123) | (0.177) | (0.0876) | (0.123) | (0.138) |
| Industry-Year FE | Y | Y | Y | Y | Y | Y |
| Company FE | Y | Y | Y | Y | Y | Y |
| State-KW FE | Y | Y | Y | Y | Y | Y |
| KW-Year FE | Y | Y | Y | Y | Y | Y |
| Observations | 16,012 | 16,012 | 16,012 | 16,012 | 16,012 | 16,012 |
| R-squared | 0.322 | 0.311 | 0.304 | 0.256 | 0.293 | 0.222 |

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

29

Table 4: Departure Rate Estimates by Footprint Concentration

In this paper, the firm's main state is the state which has the most employees, according to LinkedIn. The baseline regressions do not impose a minumum fraction of employees to be in the main state. This table shows the results of the previous regressions when limiting the sample to firms with at least 20% of employees in the main state, or at least 40% of employees in the main state. For brevity, this table reports only the main coefficient of interest for each regression.

Panel A: Generalized difference-in-differences at company level. Estimates for Treated*Post.

| Fraction of workers in main state | N | (1) Total de-partures | (2) Same industry | (3) Different industry | (4) Increase seniority | (5) Stay in seniority | (6) Decrease seniority |
|---|---|---|---|---|---|---|---|
| 0%+ | 7,063 | -0.731*** | -0.406*** | -0.317 | -0.392*** | -0.277** | -0.0612 |
|  |  | (0.248) | (0.143) | (0.196) | (0.129) | (0.128) | (0.128) |
| 20%+ | 6,804 | -0.799*** | -0.426*** | -0.365* | -0.402*** | -0.298** | -0.0983 |
|  |  | (0.259) | (0.148) | (0.209) | (0.133) | (0.125) | (0.141) |
| 40%+ | 5,385 | -0.921** | -0.549*** | -0.358 | -0.494*** | -0.393* | -0.0339 |
|  |  | (0.429) | (0.187) | (0.343) | (0.148) | (0.215) | (0.203) |

Panel B: Triple differences at company-knowledge worker (KW) level. Estimates for Treated*Post*KW.

| Fraction of workers in main state | N | (1) Total de-partures | (2) Same industry | (3) Different industry | (4) Increase seniority | (5) Stay in seniority | (6) Decrease seniority |
|---|---|---|---|---|---|---|---|
| 0%+ | 16,012 | -0.540* | -0.152 | -0.352 | -0.264** | -0.356** | 0.0801 |
|  |  | (0.284) | (0.165) | (0.285) | (0.110) | (0.142) | (0.155) |
| 20%+ | 15,460 | -0.607* | -0.170 | -0.401 | -0.280** | -0.396** | 0.0681 |
|  |  | (0.302) | (0.167) | (0.300) | (0.117) | (0.151) | (0.156) |
| 40%+ | 12,474 | -1.177*** | -0.539*** | -0.594 | -0.387** | -0.842*** | 0.0516 |
|  |  | (0.434) | (0.162) | (0.415) | (0.168) | (0.200) | (0.235) |

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

Table 5: NC Effect on Departures to Entrepreneurship

In Columns (1) and (2), the dependent variable is the departure rate to newly-founded companies. In Columns (3) and (4), the dependent variable is the departure rate to a founder title at a new company. In Columns (2) and (4), the sample of firms is limited to firms with at least 40% of employees in the main state. Standard errors are clustered at the state level. Industry is 4-digit NAICS.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{ikt} = \alpha + \beta_1 \{\text{treated}_i * \text{post}_t * \text{knowledge workers}_k\} + \beta_2 \{\text{treated}_i * \text{post}_t\} + \gamma_i + \lambda_{ks} + \theta_{kt} + \epsilon_{it}$$

|  | To new firm | | To founder title | |
| --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) |
|  | All | Min. 40% in state | All | Min. 40% in state |
| Treated*Post*Knowledge Worker | -0.0691** | -0.0976** | -0.00778 | -0.0115* |
|  | (0.0331) | (0.0369) | (0.00546) | (0.00645) |
| Treated*Post | 0.0846*** | 0.0860*** | 0.00643 | 0.00934 |
|  | (0.0212) | (0.0246) | (0.00680) | (0.00937) |
| Company FE | Y | Y | Y | Y |
| State-KW FE | Y | Y | Y | Y |
| KW-Year FE | Y | Y | Y | Y |
| Observations | 16,012 | 12,474 | 16,012 | 12,474 |
| R-squared | 0.158 | 0.159 | 0.107 | 0.106 |

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

## Table 6: NC Effect on 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), the regression is a triple differences specification with Treated*Post interacted with an indicator for Knowledge Sector (KS), which are: Technology, Professional, scientific & technical services, and Education & training.

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

Columns (2)-(5) break out the results in difference-in-differences specifications.

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

Standard errors are clustered at the state level. Industry is LinkedIn-defined industry.

| | (1) All new firm entry | (2) Technology | (3) Prof. services | (4) Edu. & training | (5) All other sectors |
|---|---|---|---|---|---|
| Treated*Post*Knowledge Sectors | -0.0626** | - | - | - | - |
| | (0.0245) | | | | |
| Treated*Post | 0.000343 | -0.0388** | -0.0737* | -0.0769* | -0.000370 |
| | (0.00560) | (0.0176) | (0.0414) | (0.0412) | (0.00610) |
| | | | | | |
| Industry-Year FE | Y | Y | Y | Y | Y |
| Industry-State FE | Y | Y | Y | Y | Y |
| Observations | 31,850 | 4,753 | 6,090 | 924 | 20,083 |
| R-squared | 0.768 | 0.814 | 0.811 | 0.511 | 0.540 |

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

## Table 7: Net Investment Scaled by Capital

The dependent variable is net investment scaled by one year-lagged net capital. In Columns (1)-(3), the regressions use the full sample of matched firms, while in Columns (4)-(6) the sample of firms is limited to firms with at least 40% of employees in the main state. In Columns (1) and (4), the regression is a difference-in-differences with all observations pooled. In Columns (2)-(3) and (5)-(6), the regression is a difference-in-difference-in-differences, or triple differences. In Columns (2) and (5), the subsample is the set of firms which employ an above-median fraction of knowledge workers. In Column (3) and (6), it is the set of firms with an above-median R&D intensity. Standard errors in parentheses are clustered at the state level. Industry is 4-digit NAICS.

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

| | All firms | | | Min. 40% in state | | |
| --- | --- | --- | --- | --- | --- | --- |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | I/K | I/K | I/K | I/K | I/K | I/K |
| Treated*Post | 0.0633** | 0.0147 | 0.0256 | 0.0455 | -0.0117 | -0.0325 |
| | (0.0275) | (0.0213) | (0.0252) | (0.0343) | (0.0287) | (0.0297) |
| Treated*Post*Knowledge Firm | | 0.0900*** | | | 0.109** | |
| | | (0.0331) | | | (0.0510) | |
| Treated*Post*High R&D Firm | | | 0.0530* | | | 0.113** |
| | | | (0.0267) | | | (0.0446) |
| Company FE | Y | Y | Y | Y | Y | Y |
| Industry-Year FE | Y | | | Y | | |
| Subsample-Industry-Year FE | | Y | Y | | Y | Y |
| Observations | 4,656 | 4,459 | 4,442 | 3,587 | 3,395 | 3,402 |
| R-squared | 0.569 | 0.594 | 0.575 | 0.572 | 0.596 | 0.578 |

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

# 8 Figures

# Figure 1: Difference in Departure Rate by Years to Treatment

This figure presents the coefficient estimate $\beta_m$ from the equation below, against years to treatment $m$. The estimate represents the difference in departure rate between the treated and control observations, before and after the change in enforcement of NCs.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{it} = \alpha + \sum_m \beta_m \{\text{treated}_i * \text{m years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$
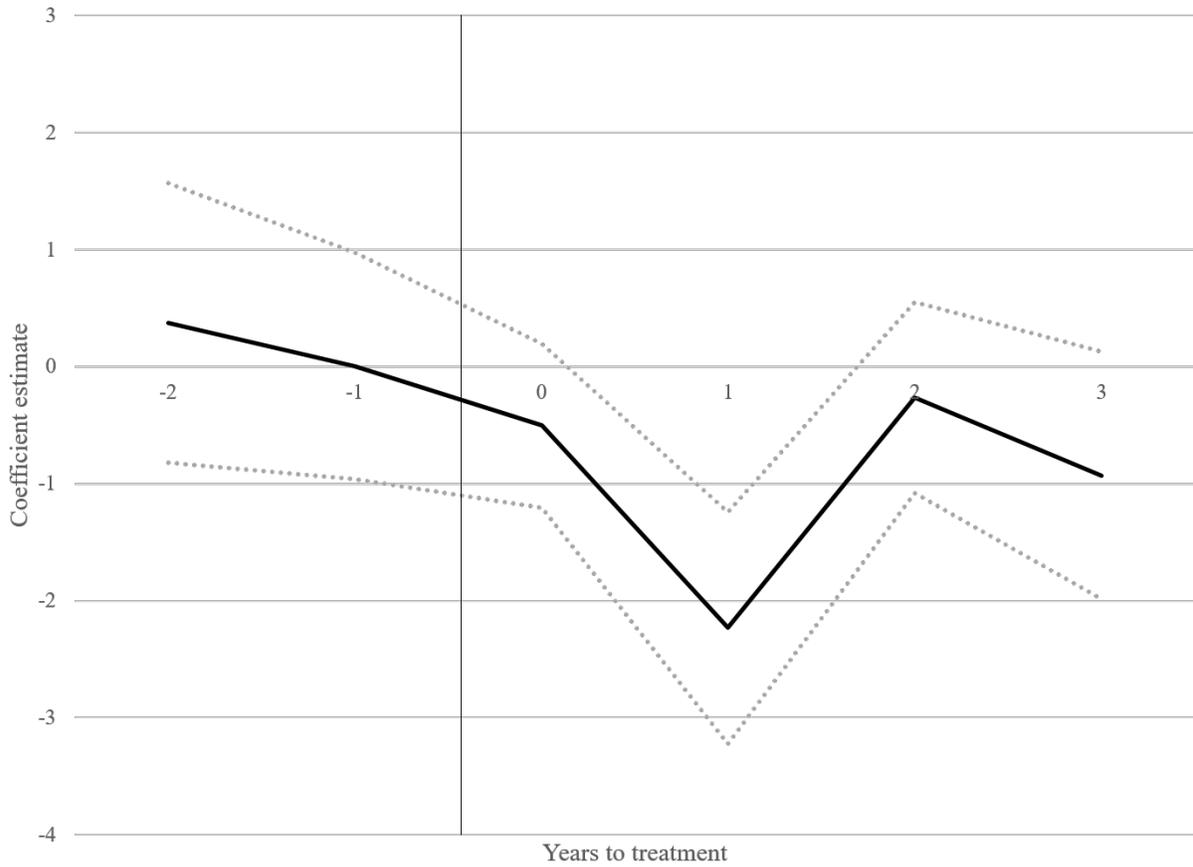
## Figure 2: Difference in Departure Rate to New Firms by Years to Treatment

This figure presents the coefficient estimate $\beta_m$ from the equation below, against years to treatment $m$. The estimate represents the difference in departure rate between the treated and control observations, before and after the change in enforcement of NCs.

$$100 * \frac{\# \text{ departures}}{\# \text{ employees}}_{it} = \alpha + \sum_m \beta_m \{\text{treated}_i * \text{m years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$
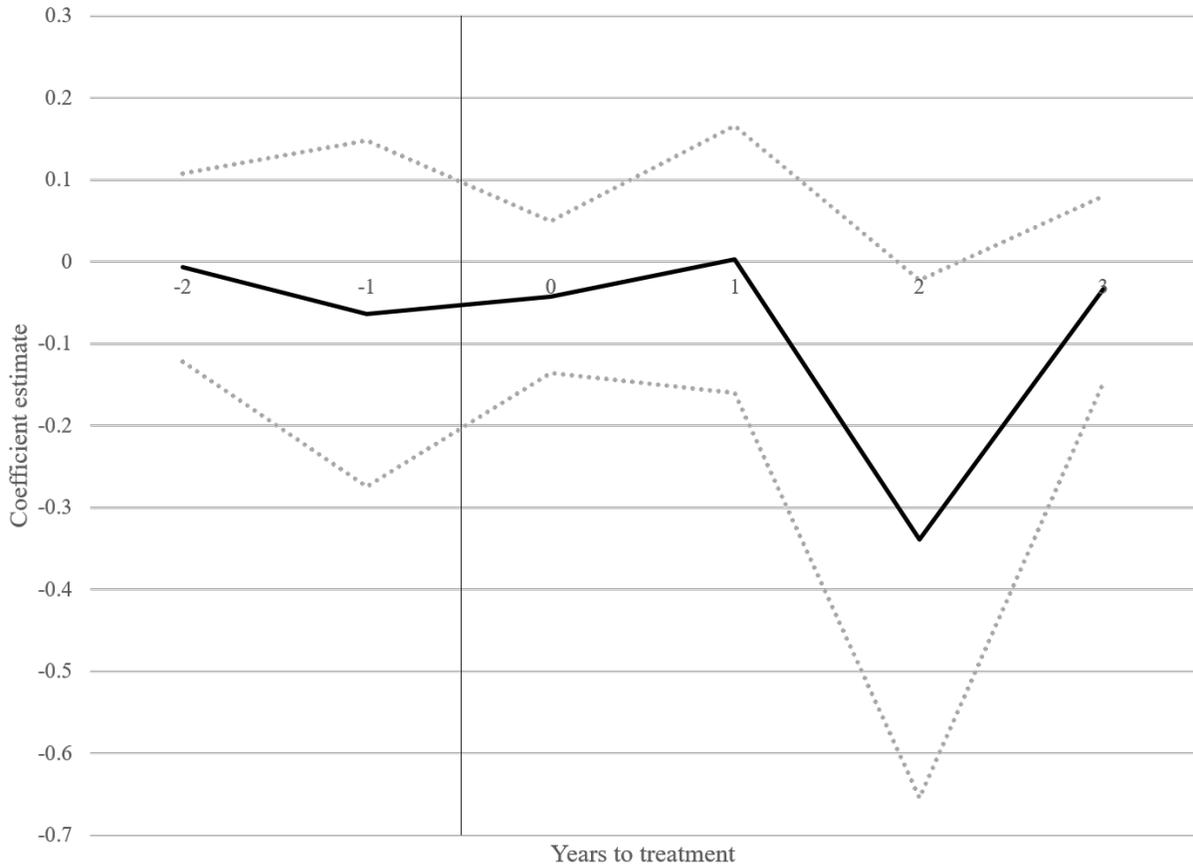
## Figure 3: Difference in Investment Rate by Years to Treatment

This figure presents the coefficient estimate $\beta_m$ from the equation below, against years to treatment $m$. The estimate represents the difference in departure rate between the treated and control observations, before and after the change in enforcement of NCs.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{it} = \alpha + \sum_m \beta_m \{\text{treated}_i * \text{m years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$
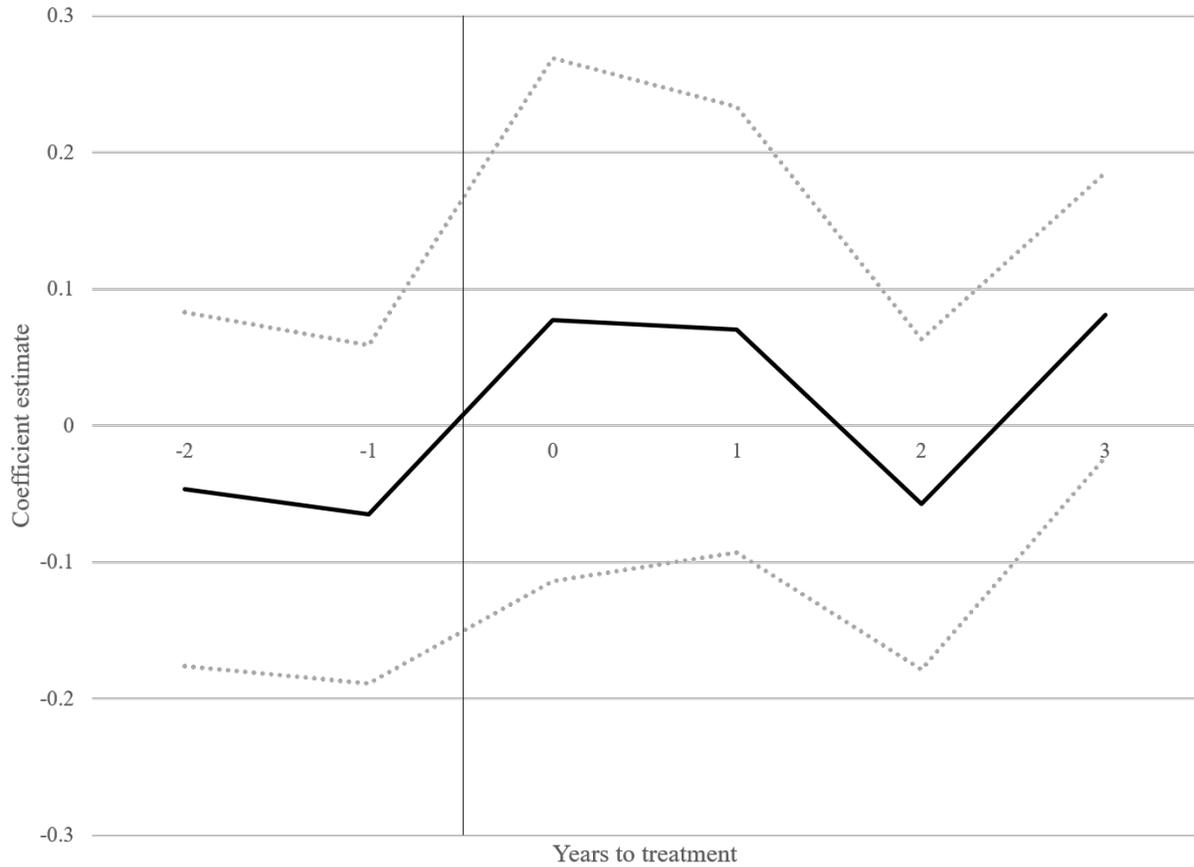
# A    Appendix

Figure 4: Map of NC Enforcement Changes
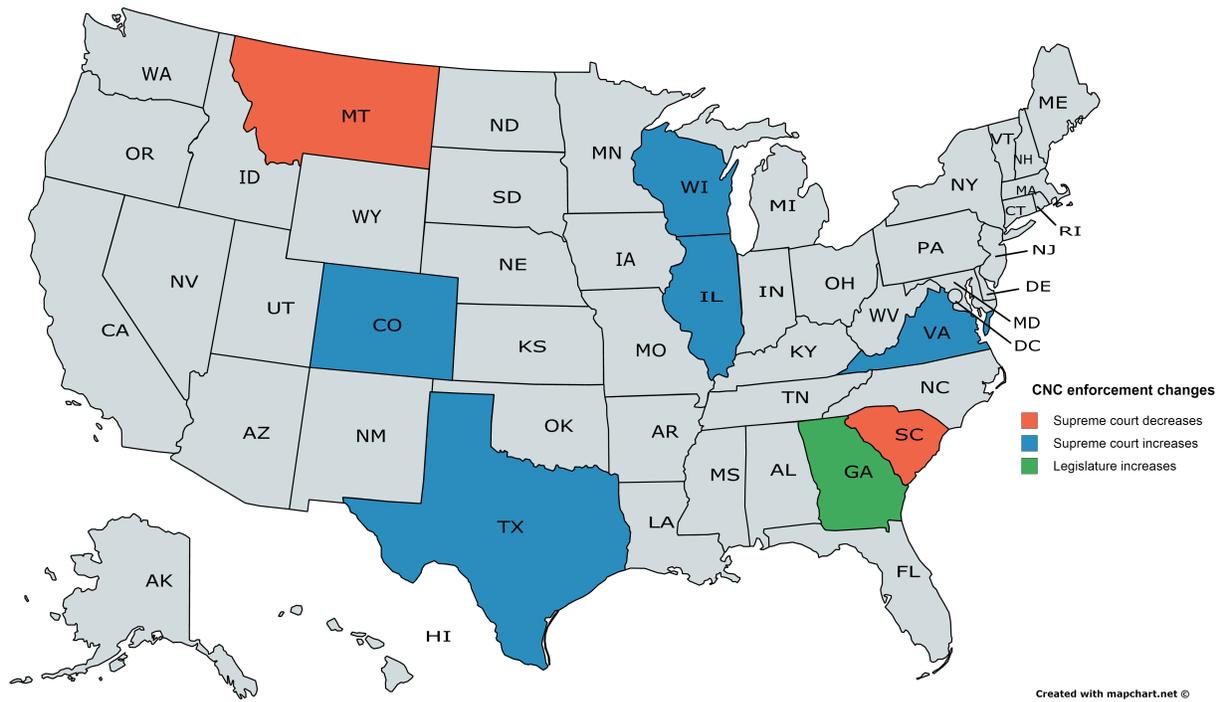
Table 8: NC Enforcement Changes

| 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 |

Figure 5: Trade Secret and NC Litigation from Beck, Reed & Riden, LLC
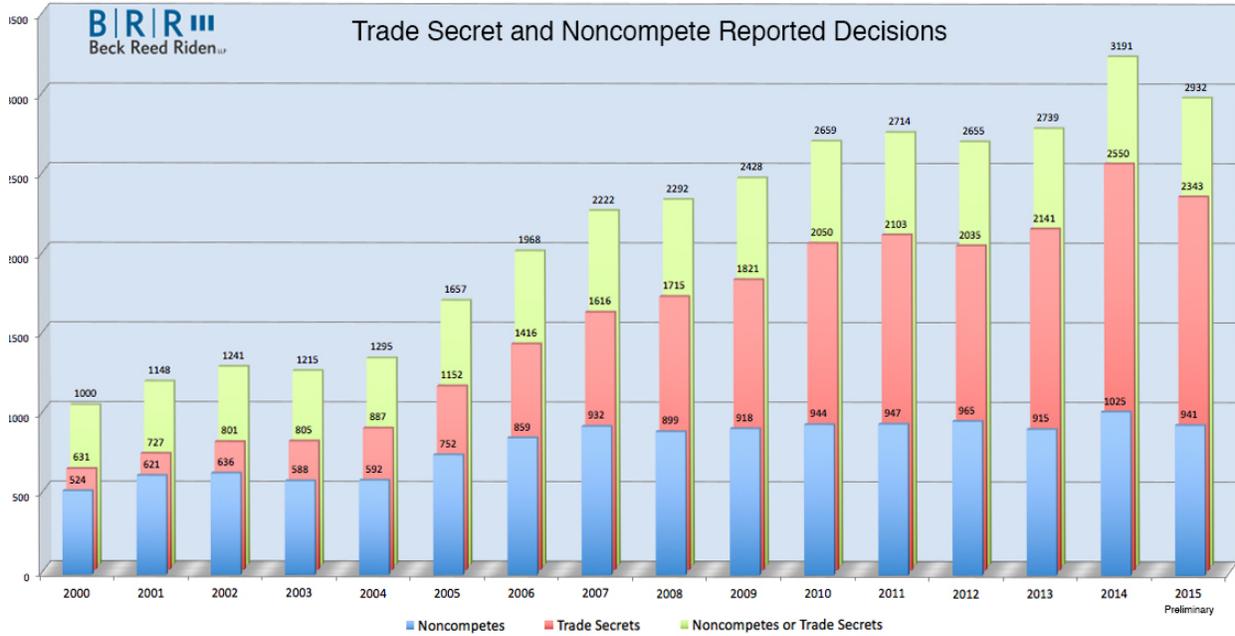


Source: Beck, Reed & Riden, LLC, Jan. 11 2016

Table 9: 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 |

41

## Figure 6: 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.
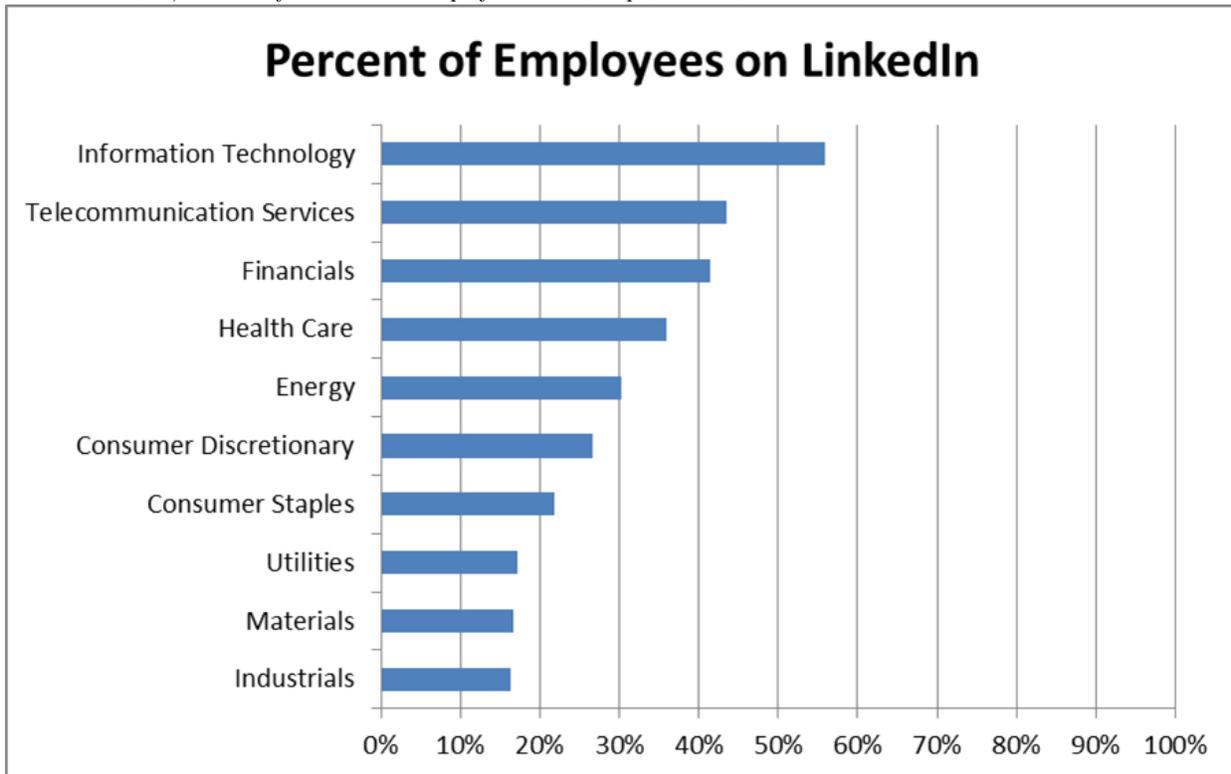
## Figure 7: Incidence of NCs by Occupation from Starr et al. (2016)

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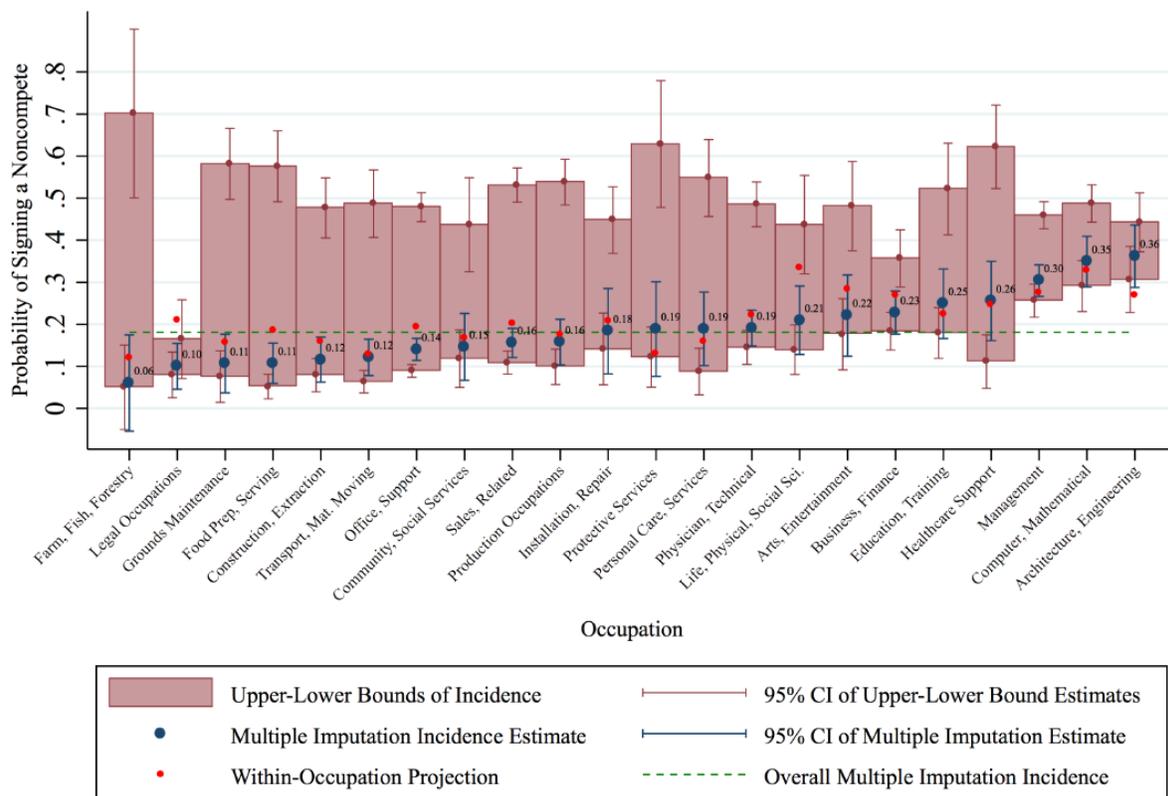


The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.
The projections refer to within-occupation average of the projected proportion of noncompete signers.