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

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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 $1.5 million of additional capital investment from publicly held firms for every lost new firm entry.

Friday, November 10, 2017, 14:00-15:30
Room 118, Extranef building at the University of Lausanne

The Impact of Restricting Labor Mobility on Corporate Investment and Entrepreneurship

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September 18, 2017

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 $1.5 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 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. 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, 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.

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Specifically, my identification strategy relies on a new setting of 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, Prescott and Bishara (2016b) 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.

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. 2016b). Moreover, I observe company-level information such as industry, year founded and size for both origin and destination firms. As a result I am able to isolate moves to competitors and to new businesses. I use the latter to proxy for departures to entrepreneurship, and also use company-level information to capture the entry rate of new firms. Another important advantage is that the data encompass a wide range of workers in all fifty states and foreign countries. 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.[3,4]

The paper contains three main results. First, 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

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[1]I find similar results wether 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]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.

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. 2016b). Indeed, I find that departure rates in these occupations are particularly affected by increases in CNC 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 by 0.75 following stronger enforceability of CNCs. This represents a large drop, 20%, relative to the average departures to entrepreneurship captured in my sample. The relative drop is even larger when looking at small new businesses. In turn, the entry of knowledge-intensive firms declines by 17%.

Entrepreneurship declines when CNCs 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 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 $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 $1.5 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. 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

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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 capital to firm value (Chen, Kacperczyk and Ortiz-Molina 2011, Eiling 2013, Donangelo 2014). More specific to my context, a few studies have begun relating these frictions to capital decisions (Autor, Kerr and Kugler 2007, Garmaise 2011). Garmaise (2011) finds that investment relative to labor decreases after CNCs become more enforceable in three states. In contrast, I find in my setting that investment increases with CNC enforcement, and argue that this is due to complementarities between human and physical capital in knowledge-intensive firms.[6] 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 CNCs and employee departures include Marx, Strumsky and Fleming (2009) (inventors), Garmaise (2011) (executives), Lavetti, Simon and

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[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.

[6]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.

White (2014) (physicians), and Starr, Balasubramanian and Sakakibara (2015) (survey).[7] Conti (2014) also considers the impact of CNCs 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 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.

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 my setting, mobility restrictions generate substantially higher capital investment at existing knowledge-intensive firms. CNCs are a form of private ordering, and therefore 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 2 explains the CNC policy setting. Section 3 describes the data. Section 4 outlines the hypotheses and empirical approach. Section 5 presents the main results, and section 6 covers robustness. Section 7 concludes.

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 after separating from the employer, usually for a limited time (e.g.

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

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 CNCs

In a survey of more than 11,000 U.S. labor force participants, Starr et al. (2016b) 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 positions: 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. 2016b). 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]

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 burdern 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

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[9] *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".

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 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, 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.[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, Bishara and Martin 2014).

Building on these six dimensions identified by Bishara (2011), Starr (2016a) 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.

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

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 search for what local attorneys wrote about the expected impact of the decision, and verify that it is consistent with my interpretation. 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 of these 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.

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 CNCs 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.

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[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."

2.4 State Supreme Court Composition

Table 2 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 1 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 control group in the years leading up to the decisions.

2.5 Impact on Mobility

The main mechanism through which CNC 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 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 re-

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[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."

frain 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 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 CNCs, 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. In untabulated results, I verify that the effect is stronger when I restrict to more concentrated firms (where more of the employees are located in the assignment state).

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

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

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 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. In this paper I use the Compustat number of employees rather than the LinkedIn number 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 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.

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

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

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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.

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

4 Hypotheses and Empirical Approach

4.1 Hypothesis Development

A) 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). 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). 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.

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

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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.)

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 CNC enforceability) is akin to increasing the average and volatility of the depreciation rate of human capital. Conversely, increasing CNC 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, 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 (2016b). 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.

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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 I 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

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[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 (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.

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 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 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. 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 6 I verify that results are robust to allowing for different trends by market, asset and employee size.

Finally, using Starr et al. (2016b) 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

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the following occupations within the LinkedIn classification: arts & design, business develop-
ment, 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]

5   Results

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, Prescott and Bishara (2016a).[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%.

In Table 6, I show that these results are driven by workers in knowledge-intensive occu-
pations by estimating difference-in-difference-in-differences (or "triple differences") regressions.

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[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.

These regressions compare the impact of higher CNC enforceability on all occupations to the impact on the subsample of occupations that Starr et al. (2016b) identify as more prone to CNCs. The coefficient estimate on *treated*post*knowledge worker* represents how much more knowledge-intensive 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.

5.2   Entrepreneurship

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).

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. In addition to the year founded, LinkedIn also provides a current range estimate for the total number of employees at the firm (including employees not on LinkedIn). Unfortunately I only observe the latest size: any company that has grown significantly since it was founded will not be captured as small. It is rare for companies to grow that quickly – and there are few firms starting in the post period with more than 50 employees today – but the group of large new companies could contain the most successful start-ups. With this in mind I provide results for three different categories of companies: 1-10 employees, 1-50 employees, and all sizes.

In Table 7, I look at the total number of departures to newly founded businesses in these three size categories. 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

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focus on departures from knowledge firms as in the previous section and following the discussion in Section 4. 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. Summing the two coefficient estimates to get the total effect on workers from knowledge firms, the results suggest 0.35 fewer workers leave to (currently) 1-10 employee firms, 0.5 to 1-50 employee firms, and 0.75 to new firms of all sizes. Relative to average departures in these categories, the estimates represent a 20-33% drop, with a larger relative drop for smaller new firms.

In Table 8, 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.[20] 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 CNC enforceability, the knowledge sector experiences a 12% decrease in new 1-10 employee firms, a 16% decrease in new 1-50 employee firms and a 17% decrease in new firms of any size. To better understand the economic interpretation of the results, I break them out by sector in Table 9, for new firms with 1-50 employees. New small firm entry declines 6% in the technology sector, 12% in professional, scientific and technical services, and 28% in education.

5.3   Capital Investment

Stronger CNCs lead to a decline in entrepreneurship, but do they lead to an increase in investment elsewhere? In Table 10, 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.

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

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 \$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%. In knowledge firms, the estimated additional increase is 30% relative to an average I/K of 0.35, and in high R&D intensity firms it is 24% 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 a total \$20 million increase in investment in knowledge firms and a total \$18 million increase in high R&D intensity firms. Put differently, the median knowledge firm increases investment by \$3 million, and the median R&D intensive firm by \$2.3 million.[21] Relating back to the results in 5.1, this is equivalent to roughly \$140-190k 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.

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 CNCs 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 9, the observations are number of new entrants scaled by state popu-

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[21]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.

lation, 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%). [22] 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 10 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 (ex-ante) net capital for high R&D firms, and also aggregate over the number of high R&D firms in the treated states. [23] Overall, my estimates imply an increase of a little over $5 billion in capital investment coming from publicly-held high R&D firms across treated states. The resulting trade-off is approximately $1.5 million of additional capital investment for every lost firm entry, in knowledge-intensive sectors. 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.

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[22]I do not use the BDS data directly for my regressions for several reasons. One is that the BDS statistics are aggregated in very broad sectors, with nothing that would map to a technology sector. Another is that BDS counts the number of opening establishments from new firms, which could over- or under-count the number of new firms. I believe my definition of entrepreneurship is better captured by firms that list being founded in a given year than the Census' definition of establishment openings for new firms, so I prefer the former for my regressions. Still, for the purpose of this back-of-the-envelope aggregation exercise, the BDS data provides the best available comparison point that I am aware of.

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

6 Robustness

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 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 28% decrease relative to average for departures to 50 or fewer and 10 or fewer new firms. In turn, Table 12 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 10, indicating that differential trends are not a driver of these results.

Table 13 reports the results 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 10, 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.

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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 15-17 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."

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 move-

ments 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 $2.3-3 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.

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Figure 1: Map of CNC Enforcement Changes
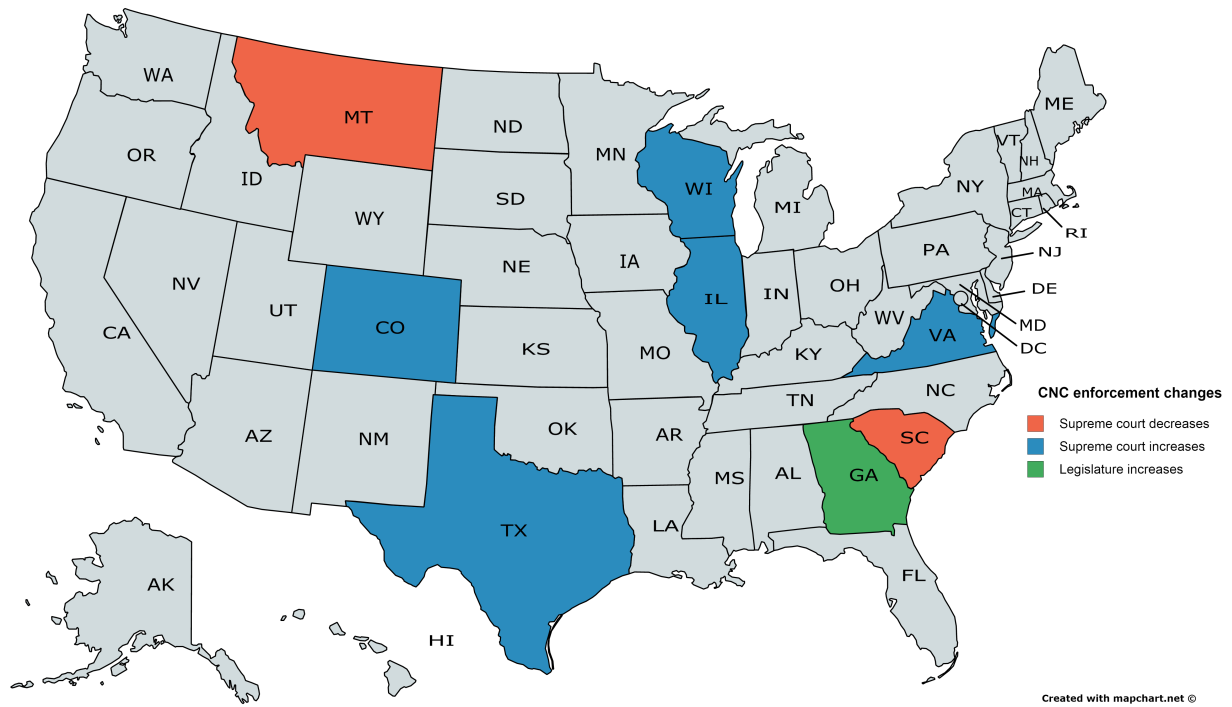
Figure 2: Trade Secret and CNC Litigation from Beck, Reed & Riden, LLC
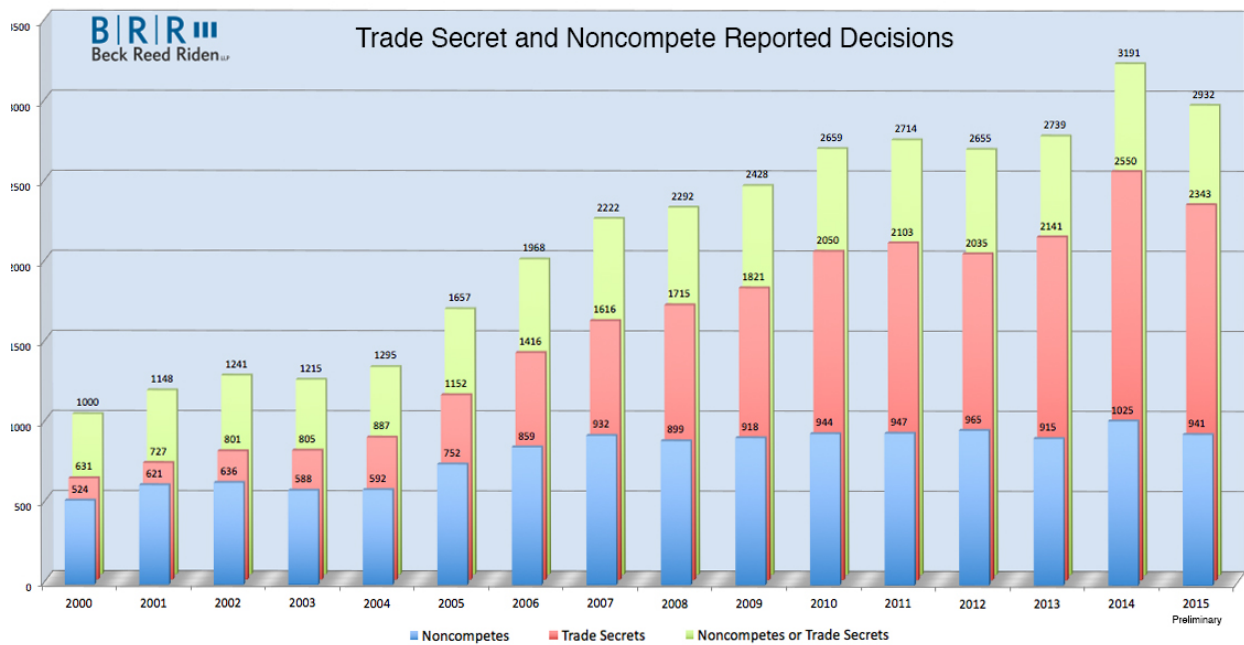
Source: Beck, Reed & Riden, LLC, Jan. 11 2016

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.
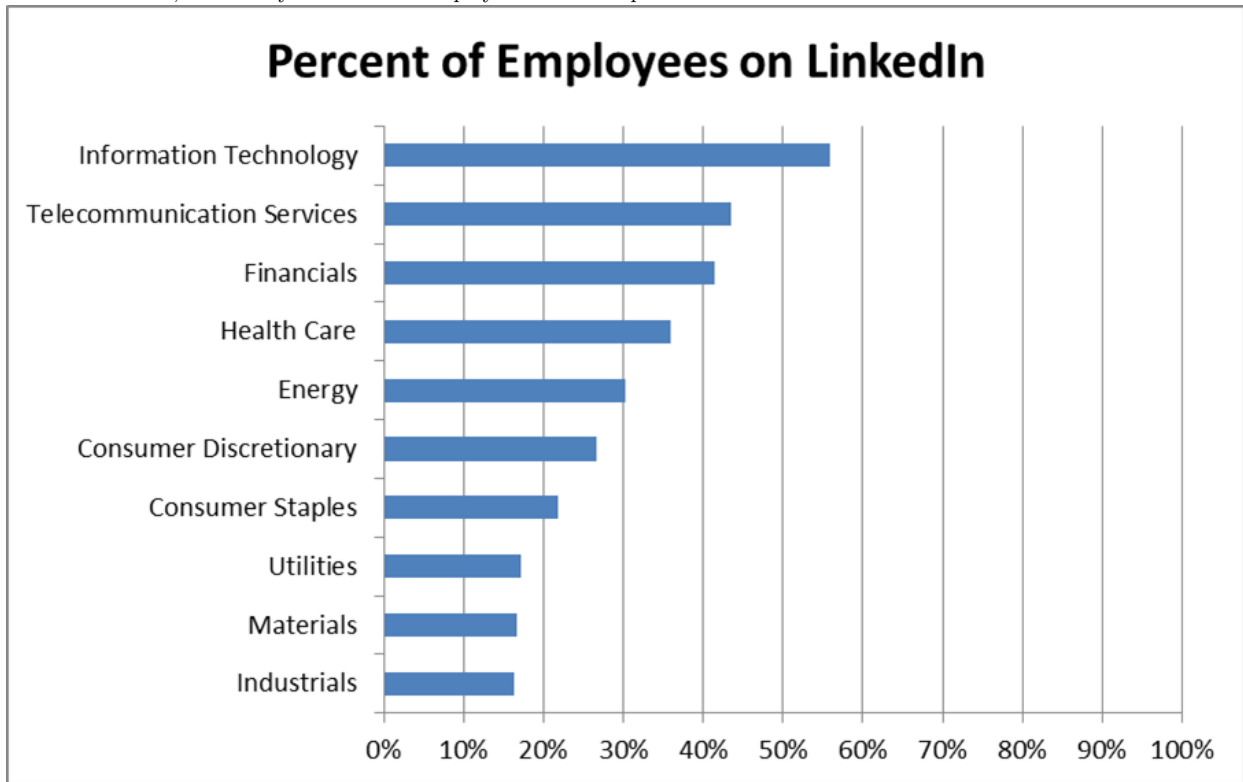
Figure 4: Difference in Departure Rate by Years to Treatment.

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.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{it} = \alpha + \sum_k \beta_k \{\text{treated}_i * \text{k years to treatment}\} + \gamma_i + \theta_{jt} + \epsilon_{it}$$
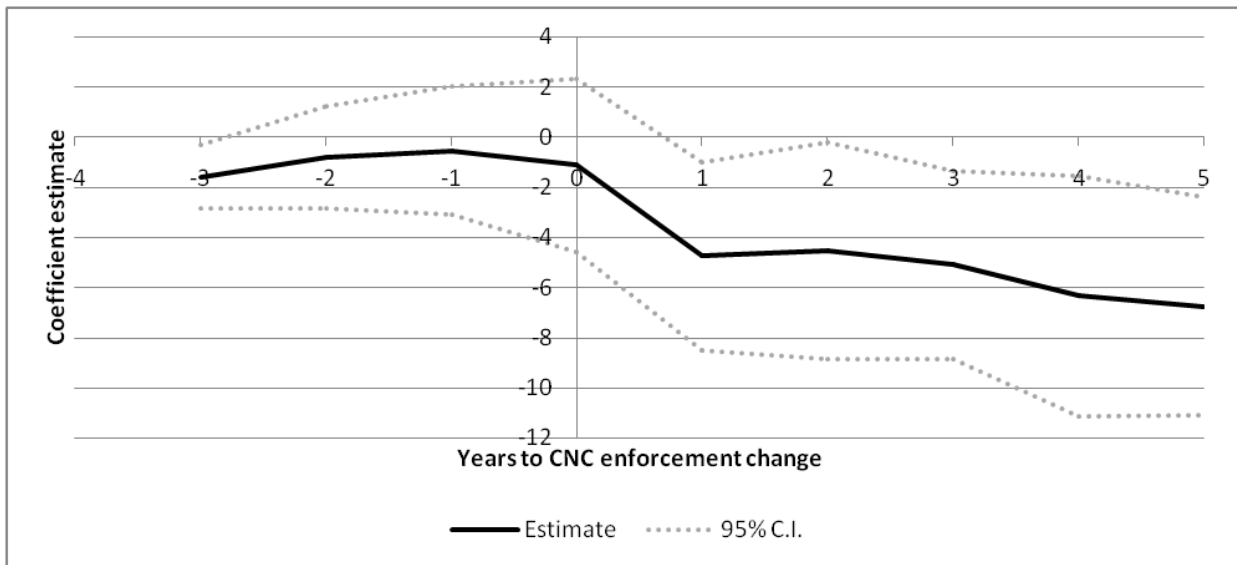
Figure 5: Incidence of CNCs by Occupation from Starr et al. (2016)

This figure comes from Starr et al. (2016b). In this paper, I consider knowledge workers to be those in the right-most occupations, starting from Life, Physical, Social Sciences.
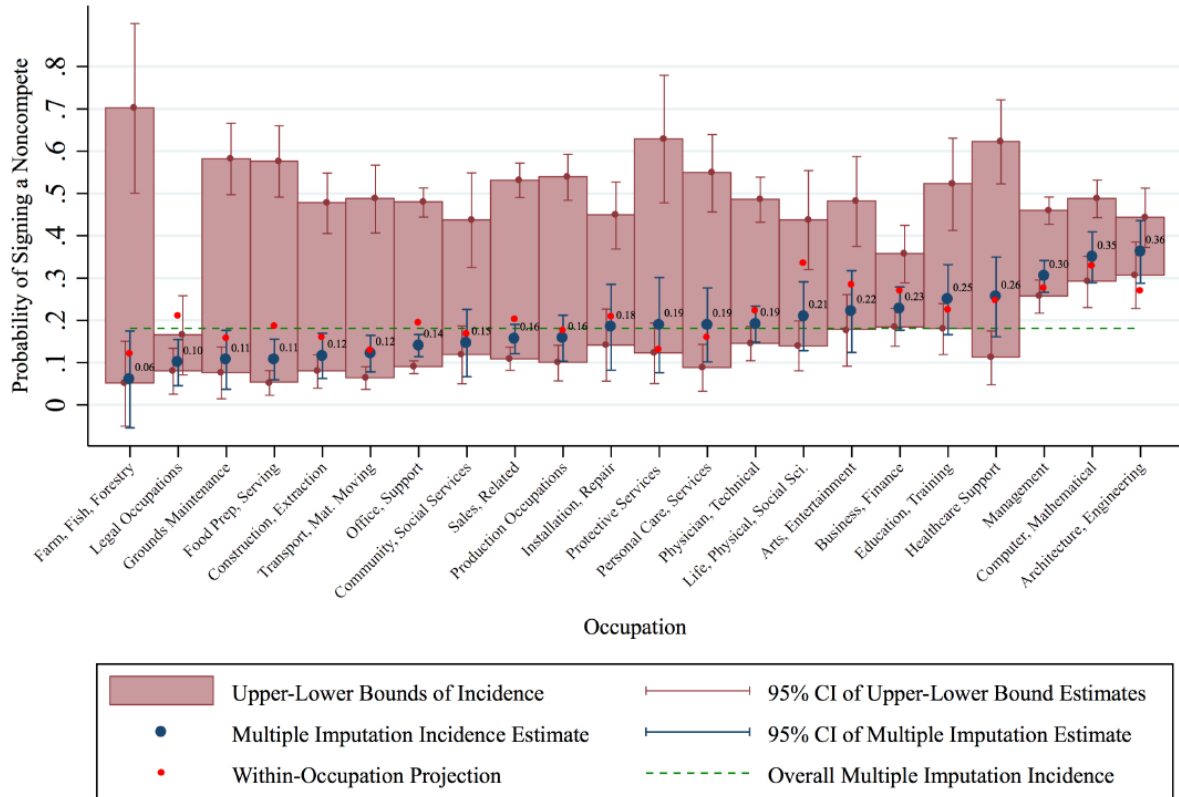
The upper-lower bounds of the incidence of noncompetes assume that those who don't know if they have signed a noncompete did and did not sign, respectively.
The projections refer to within-occupation average of the projected proportion of noncompete signers.

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 2016a). Mobility measures are taken from 2008 only due to data limitations.

| | 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
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.

$$100 * \frac{\text{\# departures}}{\text{\# employees}}_{imt} = \alpha \quad + \quad \beta_1\{\text{treated}_i * \text{post}_t * \text{knowledge workers}_m\}$$

$$+ \quad \beta_2\{\text{treated}_i * \text{post}_t\} + \gamma_i + \lambda_{sm} + \theta_{mt} + \epsilon_{imt}$$

	(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: 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 ≤ 10 Employees	(2) New ≤ 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 8: 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 9: 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 10: 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

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Table 11: 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 12: 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 ≤ 10 Employees	(2) New ≤ 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 13: 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 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)
'Size'-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)
'Size'-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-intensive 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*t+3-5	-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

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

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Table 16: 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)	(2)	(3)
	New ≤ 10 Employees	New ≤ 50 Employees	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*t+3-5	-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 17: 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 $\leq$ 10 Employees	(2) New $\leq$ 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*t+3-5	-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

Table 18: 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*t+3-5	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