Screening Spinouts? How Noncompete Enforceability Affects the Creation, Growth, and Survival of New Firms

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Abstract

This paper examines how the enforceability of noncompete covenants affects the creation, growth, and survival of spinouts and other new entrants. The impact of noncompete enforceability on new firms is ambiguous, since noncompetes reduce knowledge leakage but impose hiring costs. However, we posit that enforceability screens formation of within-industry spinouts (WSOs) relative to non-WSOs by dissuading founders with lower human capital. Using data on 5.5 million new firms, we find greater enforceability is associated with fewer WSOs, but relative to non-WSOs, WSOs that are created tend to start and stay larger, are founded by higher-earners, and are more likely to survive their initial years. In contrast, we find no impact on non-WSO entry, and a negative effect on size and short-term survival.

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1. Introduction

A principal avenue of new firm formation is through employees of an existing firm leaving their employment to establish a new firm. An important factor that may affect firm formation through this avenue is the presence and enforceability of covenants not to compete (CNCs). These agreements, commonplace in many industries and in all U.S. states, are routinely signed by many employees, including potential spinout founders (Kaplan and Stromberg, 2003; Marx, 2011; Garmaise, 2011; Starr et al., 2015). Typically, these covenants prohibit employees from joining or establishing a competing firm for a specified period of time, and within a stipulated geographic area. Such prohibitions become particularly critical where these covenants are more likely to be enforceable. For example, Gilson (1999) proposes that the higher growth in Silicon Valley compared with Route 128 is due to CNCs being void in California, but enforceable in Massachusetts. In this paper, we examine the impact of such variation in CNC enforceability ("enforceability" from here on) on the formation and life cycle of new firms, including those of spinouts formed in the same industry as the parent firm ("within-industry spinouts" or WSOs).[1]

Unlike the impact on employee mobility, the impact of enforceability on WSOs and non-WSOs (which include spinouts formed in different industries from parent firms and other types of new firms) is theoretically ambiguous. The intended effect of higher enforceability is to reduce employee mobility to competitors, and policymakers hope that higher enforceability will encourage firms to invest in intangible assets—for example, in industry-specific expertise—that would otherwise be at risk for dissemination to the firm's competitors. Past studies that have examined the impact of enforceability found that in line with expectations, higher enforceability reduces the mobility of employees, including inventors (Marx, et

[1] The term *enforcement* is typically used in the related management literature. However, in the legal literature, enforcement is a choice for agents to pursue (e.g., a parent firm can choose to enforce the CNC), while enforceability relates to whether the enforcement can withstand legal scrutiny in a court of law. Hence, to be more precise, we use the term *enforceability* in this paper.

al., 2009), CEOs (Garmaise, 2011), and workers in California's IT cluster (Fallick et al., 2006). Though no past study has *directly* examined the impact of enforceability on new firms, Samila and Sorenson (2011) find that the elasticity of new establishment formation with respect to venture capital investment is lower in states with high enforceability, which indirectly suggests that enforceability inhibits new firm formation. Similarly, Stuart and Sorenson (2003) show that enforceability mitigates entrepreneurship following a liquidity event such as an IPO or an acquisition. Although a majority of studies point to the potential negative impact of enforceability, more recent studies have found evidence for its positive effects. Conti (2014) finds that enforceability encourages incumbent firms to pursue riskier R&D strategies because it reduces payoff leakages from high-risk R&D projects. Starr (2016) shows that enforceability increases the likelihood that firms provide skill-upgrading training to their employees.

In this paper, we propose a theoretical framework (grounded in a formal model) that links enforceability to the formation and life cycle of new firms. Unlike workers leaving to join a competitor, potential entrepreneurs face three separate effects of higher enforceability—a *screening effect*, which lowers the expected profits for a WSO founder by increasing the probability of losing CNC litigation, a *hiring cost effect*, which increases the cost of hiring for all new firms, and an *investment protection effect*, which reduces the potential loss due to employees leaving the new firm. Our framework predicts that, even though the joint effect of the latter two effects is ambiguous, the negative screening effect unambiguously lowers the entry rate of WSOs relative to non-WSOs. In particular, this reduction in entry occurs because high enforceability dissuades potential WSO founders with low human capital from forming a spinout (over most of the relevant parameter space in our model). Because firm size and human capital are positively correlated (e.g., Cooper et al., 1989), our framework predicts that relative to non-WSOs, at some threshold of quality, higher enforceability will truncate the left tail of the initial WSO size distribution, and thus increase the observed average initial size of WSOs and contribute to observed survival differentials and size differentials later in life.

We test our hypotheses using matched employer-employee data from 1990 to 2008 on about 5.5 million new firms from thirty U.S. states covering all industries, about 8% of which we identify as WSOs.

Because other state-level factors may be correlated with enforceability, we develop a novel within-state difference-in-differences identification strategy that exploits an unusual aspect of law firms: law is the only industry in which courts do not enforce any CNCs in *any state*. The per se prohibition on CNCs for lawyers was first suggested by the American Bar Association in 1961 and later codified in 1983 (Buffkin, 1999). The prohibition has been adopted in every state in the United States (Malsberger, 1996), and hence, law firms form a natural control group to analyze the effect of enforceability.

We identify the screening effect of enforceability for WSOs by comparing them with non-WSOs in the same NAICS four-digit industry-year in the same state, which amounts to a triple-difference approach. That is, we compare how the difference between non-law WSOs and non-law non-WSOs (first difference) and the corresponding difference between within-industry law spinouts and non-WSO law firms (second difference) varies with enforceability (third difference). The difference with law firms nets out unobserved variables correlated with enforceability, and the difference with non-WSOs nets out the investment protection effect and the hiring cost effect, leaving the screening effect that WSOs face.

Based on these analyses, we find that the rate of entry of WSOs is negatively affected by enforceability. In contrast, we find that enforceability has no effect on the entry rate of non-WSOs. However, relative to non-WSOs, WSOs that are established in higher-enforceability states tend to be larger, faster growing, and more likely to survive the first few years. Relative to non-WSOs, a unit (or one-standard-deviation) increase in enforceability is associated with about a 0.13-percentage-point decrease in the mean WSO entry rate and a 1.1% increase in their initial size (as measured by employment). Also, the impact on the 25th percentile of WSO initial size is more than three times higher than on the 75th percentile (1.5% vs. 0.4%). We find no such effects for non-WSOs. Consistent with larger parent firms being more likely to enforce CNCs against departing employees, we also find that WSOs tend to have smaller parents in high-enforceability states. Additionally, we find that enforceability is associated with higher earnings of founders prior to spinout formation, suggesting that higher enforceability dissuades founders with lower human capital from forming WSOs. Together, these results

are in line with our theoretical framework and strongly suggest that enforceability has a screening effect on the process of WSO formation.

Lastly, we examine how CNC enforceability affects the type of employees that are hired. Consistent with enforceability screening out lower-quality employees (for all firms), we find that enforceability is associated with fewer employees being hired by non-WSOs, but that the ones that are hired tend to be more educated and to have earned more previously. There is little relative difference in the types of individuals hired by WSOs and non-WSOs in higher-enforceability states.

Our study makes several important contributions to the literature on new firms and CNCs. To our knowledge, this is the first study to provide direct evidence that enforceability influences the formation and characteristics of new firms. Prior studies either focus on how enforceability affects the mobility of individuals and investments by incumbent firms (Garmaise, 2011), or examine its impact indirectly through interactions of enforceability with other variables—for example, with venture capital investment, as in Samila and Sorenson (2011), or a liquidity event, as in Stuart and Sorenson (2003). More importantly, our study makes the crucial distinction between WSOs and non-WSOs. Clearly, enforceability should affect the former more than the latter due to their greater susceptibility to CNC litigation, and that is indeed what we see in the data. Both of these were possible because our study is at the firm level, unlike many past studies that were at the inventor level (e.g., Marx et al., 2009) or the aggregate metropolitan statistical area (MSA) level (Samila and Sorenson, 2011). Furthermore, we go well beyond entry (the focus of past studies), showing that enforceability influences the size, growth, and survival of new firms, as well as the characteristics of spinout founders and workers hired by spinouts.

Another important contribution is to synthesize the three different effects of CNCs on new firm formation into one integrative model, including hypothesizing and testing screening as a mechanism that links enforceability to WSO formation. We do this theoretically by offering a model that unifies many of the hypotheses around the screening effect, and explicitly considers the ambiguous effect of the hiring cost and investment protection effects. Our empirical results are consistent with enforceability dissuading lower human capital founders from forming WSOs. Importantly, our results do not appear to support the

plausible alternative that higher enforceability dissuades higher human capital founders. Thus, higher enforceability appears to act as a screening device, much like venture capital firms screen potential new firm ideas (Kaplan and Stromberg, 2003), or patent enforcement reduces the mobility of stars but does not prevent the brightest ones from leaving (Ganco et al., 2015).

Moreover, our framework allows us to examine the impact of enforceability beyond labor mobility. Once new firms are formed, the founders face both hiring cost and investment protection effects. Because of the investment protection effect, potential founders may actually *benefit* from being able to impose CNCs on their future employees. This benefit is discussed in the existing literature, typically as a benefit to the firm but not as a motivator to individual mobility (e.g., Conti, 2013; Starr, 2016). In contrast, the hiring cost effect may dissuade potential founders from forming a firm even if they have no CNCs with their parent firm. Together, our theoretical framework and empirical findings provide a significantly more comprehensive and integrated assessment of the effect of enforceability on the life cycle of new firms than the extant literature.

Incidentally, our study also disentangles the unconditional effect of stricter enforceability from the effect conditional on initial firm size. This is particularly important as many prior studies have used firm size as a control when examining enforceability (e.g., Garmaise, 2011; Conti, 2014; Marx et al., 2009). However, as we show, initial firm size is itself an outcome that is influenced by enforceability, and hence, including firm size (highly correlated with initial size) as a control biases causal estimates. Finally, by highlighting the unique role of spinouts, our study also contributes to the broader literature on spinouts (e.g., Agarwal et al., 2004; Klepper, 2001, 2009; Klepper and Sleeper, 2005; Campbell et al., 2012) and sheds light on how a particular policy instrument affects the process of spinout formation.

2. Covenants Not to Compete

2.1. CNCs and Their Enforceability

CNCs are post-employment restraints that prohibit employees from either joining competitors or starting a competing firm for a specified amount of time (typically between six months and two years) (Gilson, 1999) and in a specified geographic region. CNCs are common in the United States for all types

of workers (Stone, 2002). Starr et al. (2015) report a national signing rate of 18.1% among all workers, though it tends to be higher for more-educated and higher-earning workers: 14.3% of those without a bachelor's degree have signed, compared with 25% with such a degree and 30% with a higher degree. This is consistent with past studies that have found the incidence of CNCs to be between 70% and 80% for CEOs (Bishara et al., 2015; Garmaise, 2011), 45% for physicians (Lavetti et al., 2015), 40% for engineers (Marx, 2011), and 70% for firms receiving venture capital (Kaplan and Stromberg, 2003).

Even though CNCs are signed in every state (Starr et al., 2015), the degree of enforceability varies considerably across states. CNCs in California and North Dakota are almost entirely unenforceable in a court of law. Most other states enforce CNCs according to some version of the "Rule of Reason," which balances the protection needed by the firm with the harm done to the employee and society (Blake, 1960). However, states differ on both what is reasonable and how to handle unreasonableness. For example, although many states modify overly broad contracts to be reasonable and subsequently enforce them, Wisconsin voids the entire contract if any part is deemed overly broad. Hence, two workers in two different states confronting CNC litigation may find themselves facing very different outcomes even if they had identical CNC contracts and otherwise identical situations. Specifically, for a given set of facts, workers in states with greater enforceability will find it harder to win CNC-related litigation than workers in states with lower enforceability. Studies of enforceability capture this difference by constructing an index that factors various dimensions of enforceability. Section 3.2 discusses the index used in this study.

2.2. Effects of CNC Enforceability on New Firms

We use a formal model of the new firm formation process to develop our hypotheses. The model has some elements of Franco and Mitchell (2008) and is similar to, but simpler than, those in Hopenhayn (1992) in which the dispersion of firm size (or productivity) in an industry is determined by a threshold that defines the least capable firm that will stay in the industry. Given our empirical focus, we discuss the key elements of the model here, and leave the technical derivations to the Appendix. Figure 1 presents a simplified overview of the model.

In our model, potential WSO and non-WSO founders vary on their human capital, θ, which is distributed uniformly. Founders get an idea and deal with CNC-related issues before formation. Then, in the first period (formation), they potentially form a new firm by paying a fixed cost, $F > 0$, and start operating. In the second period (growth), they randomly perceive an investment opportunity that requires them to hire an employee.

In line with the literature, we assume that the new firm's profits and size are increasing in the founders' human capital. For instance, Cooper et al., (1989) and subsequent studies have found such a positive correlation between founder human capital and size, suggesting that high human capital founders are better able to assemble more and higher-quality resources for their venture. We do not explicitly model survival, but in generating our hypotheses, we assume survival is increasing in size. We assume that a share λ of the new firm's profits comes directly at the expense of the parent firm and the rest $(1 - \lambda)$ from other sources not subject to CNC restrictions, where $0 \leq \lambda \leq 1$. Hence, a WSO will have $\lambda > 0$, while a non-WSO, which does not hurt the parent firm's profits, will have $\lambda = 0$.

In the absence of enforceability (the "control" group denoted by the dotted line in Figure 1), a new firm expects a certain level of profits in the formation period, and a higher level of profits in the growth period. Because the human capital of founders is a distribution and not a single number, one can think of the line in Figure 1 as representing the expected profits for some specific level of human capital. We assume that founders form the new firm if the sum of the expected profits in the two periods (that is, the total area under the expected profit line) exceeds the cost F.

As enforceability increases, the likelihood of a parent firm winning a lawsuit against its employees for violating CNC increases. The lawsuit entails not only litigation costs for the founders but also may force them to stop operating if the lawsuit is decided in favor of the parent firm. If founders face litigation, with probability $\rho > 0$ the founders lose their case. Here, ρ is a measure of enforceability, with greater enforceability associated with a higher value of ρ. With probability $(1 - \rho)$, the founders win and operate their new firm. Irrespective of the outcome, the litigation costs the founders L (e.g., to cover lawyer fees etc.). As an alternative, we assume that the founders can negotiate with the parent firm by

compensating it for its losses, and form their spinout. This element is similar to Rauch and Watson (2014), for example, who model a monetary transfer from an employee to the firm to buy out his or her CNC contract and start a new firm.[2]

Together, as shown in the Appendix, these assumptions imply that the surplus available for settlement (as opposed to litigating) is higher for high human capital founders because the expected profits from forming a spinout are higher for such founders. Importantly, the minimum compensation that the parent firm will accept also increases with enforceability. Hence, as enforceability increases, founders have to pay a higher compensation to their parent firm in order to form a WSO (denoted by the continuous line in Figure 1-A).[3] This reduces their expected profits, which we term the *screening effect*. Founders of a non-WSO (depicted by the dashed line in Figure 1-B) do not (or in practice, are less likely to) face CNC litigation, and hence follow the same line as the controls in the formation period.

Enforceability also plays a role in the growth (second) period, when the spinout perceives investment opportunities and hires an employee. However, unlike the screening effect, which is unambiguously negative for a WSO, enforceability in the growth period has two opposing effects. First, enforceability makes it more difficult to hire because the employee may also be subject to CNC-related litigation, and may thus be reluctant to move. In our model, this is reflected as a higher wage that the spinout must pay in order to make it attractive for the employee to move, which reduces the profitability of investment opportunities (*hiring cost effect*).

The other consequence of enforceability arises directly from the intended effect of CNCs. By reducing employee mobility to competitors, enforceability not only prevents the dissipation of valuable

[2] The probability of signing CNCs and the awareness among founders about their CNC signing and state policies also affect the decision. Given the lack of evidence on these, we have simplified the process here.

[3] We assume that the negotiation is "take it or leave it," where the founders make an offer and the parent firm accepts it as long as the offer is better than its best alternative to a negotiated agreement. The results are not affected if the minimum or maximum compensation is used.

information, but also increases the incentives for firms to invest in human capital through training (Meccheri, 2009; Starr, 2016), R&D, and intra-firm information sharing (Grossman and Hart, 1986; Hart and Moore, 1990; Garmaise, 2011). We refer to these effects as the *investment protection effect.* Specifically, in our model, the returns from the second-period investment opportunity depend on whether the employee hired to exploit that opportunity stays or leaves. In line with prior studies such as Marx et. al. (2009) and Garmaise (2011), stricter enforceability reduces the probability that the employee leaves, which in turn increases the value of the second-period investment to the founders. Figure 1 shows the opposing nature of the investment protection effect and the hiring cost effect.

To summarize, enforceability affects a spinout's expected profits in three ways: (i) a negative screening effect that applies directly only to WSOs, (ii) a negative hiring cost effect that applies to both WSOs and non-WSOs, and (iii) a positive investment protection effect that applies to both WSOs and non-WSOs. Together, these three effects determine the expected profits from forming a new firm, which in turn determines the threshold level of human capital required to cover the fixed cost *F*. Broadly, the lower the expected profits for a given level of human capital, the higher the threshold. Based on this setup and additional assumptions (see Appendix), our model predicts that above a certain level of enforceability ($\bar{\rho} = \frac{L}{F}$), when all potential WSO founders are at risk of litigation, the following hypotheses hold.[4] For brevity, we primarily focus on the effect of CNCs on WSOs relative to the effect on non-WSOs. Though these relative effects can be explained by the screening effect alone, we discuss the investment protection and hiring cost effects in the model in order to provide a richer theory about the formation of spinouts and allow a more complete interpretation of the empirical results, particularly relating to non-WSOs.

[4] The model predicts two regions below $\bar{\rho}$. Close to zero, there is no effect of enforceability because the low probability of winning makes it unattractive to litigate. In the next region, enforceability reduces relative WSO entry, but by dissuading founders with midrange human capital. Under reasonable assumptions, these two regions are likely to be very small. See Appendix for further discussion.

2.2.1. Entry

Focusing first on non-WSOs, they have the same expected profits as the controls in the formation period. However, in the growth period, the expected profits for non-WSOs may be higher or lower than the controls, depending on whether the hiring cost or investment protection effect dominates. Thus, compared with the case when there is no enforceability, the total expected profits for non-WSOs may be higher or lower, which in turn implies that the threshold human capital for entry may be lower or higher. This implies that the effect on entry is ambiguous. Furthermore, because size (and survival) are increasing in human capital, the effect on those variables is also ambiguous.

In the growth period, WSOs face the same opposing effects as non-WSOs. However, unlike non-WSOs, WSOs face an unambiguously negative screening effect because founders must make a payment to a parent firm in order to start a WSO.[5] Hence, for any given level of founder human capital, compared with non-WSOs, the expected profits from forming WSOs are lower. This implies that the threshold human capital required for forming WSOs is higher than for forming non-WSOs, which lowers their entry rate relative to that of non-WSOs. Moreover, the reduction in expected profits increases as enforceability increases (because the payment to the parent firm increases), which implies that the relative entry rate of WSOs decreases with enforceability. This leads to the following hypothesis:

Hypothesis 1*: Relative to non-WSOs, the entry rate of WSOs decreases with enforceability.*

2.2.2. Formation

Initial size is determined by the founders' human capital. Because higher enforceability increases the threshold human capital required for WSO entry, the minimum initial size of WSOs increases with enforceability. In contrast, founders at the upper end of the human capital distribution are not affected, and hence, the upper end of the distribution of initial size is not affected. As before, non-WSOs do not

[5] The screening effect may also indirectly affect non-WSOs through its effect on WSOs. We discuss one such possibility (founders substituting non-WSO ideas with WSO ideas) in Section 5.3.

directly face the screening effect, and their minimum initial size is determined by whether the investment protection or hiring cost effect dominates. These arguments imply the next three hypotheses.

Hypothesis 2a*: The difference between the minimum initial size of WSOs and non-WSOs increases with enforceability for all* $\rho \geq \bar{\rho}$*. The corresponding difference between the maximum initial sizes will not be correlated with enforceability.*

Hypothesis 2b*: The difference between the average initial size of WSOs and non-WSOs increases with enforceability for all* $\rho \geq \bar{\rho}$.

Hypothesis 2c*: Assuming that founders' earnings at the parent firm are positively correlated with their level of human capital, then the difference between the mean pre-founding earnings of WSO founders and that of non-WSOs increases with enforceability for all* $\rho \geq \bar{\rho}$.

Because larger firms are more likely to win CNC litigation due to their resource advantage, the litigation threshold is lower for larger parent firms. Also, as enforceability increases, this threshold falls. Consequently, founders from larger parent firms have to pay a larger compensation to form WSOs, which makes WSO formation unattractive for founders from larger parent firms. Hence, it follows that:

Hypothesis 3*: The difference between the average parent-firm size of WSOs relative to non-WSOs decreases with enforceability for all* $\rho \geq \bar{\rho}$.

2.2.3. Growth

After founding, all new firms face opposing hiring cost and investment protection effects related to enforceability. Hence, the impact on investment in employees is ambiguous for both WSOs and non-WSOs. In particular, if the hiring cost effect dominates, then our framework predicts that fewer firms will hire, though the average human capital of the employees hired will be higher (it predicts the opposite if the investment protection effect dominates). The intuition is that enforceability makes it worthwhile for firms to hire only high human capital individuals, but higher wages rule out many investment opportunities. This ambiguity also extends to the expected profits from the second-period investment; specifically, the expected profits are increasing in enforceability if the investment protection effect dominates and decreasing in enforceability if the hiring cost effect dominates.

However, because average founder human capital is higher for WSOs, their subsequent size will also be higher than that of non-WSOs (due to initial size difference). Furthermore, conditional on the initial size, which factors out the screening effect, these effects are identical for both types of new firms. Hence,

Hypothesis 4a*: Unconditional on initial size, the difference in later-life size and survival probability between WSOs and non-WSOs increases with enforceability for all* $\rho \geq \bar{\rho}$.

Hypothesis 4b*: Conditional on initial size, the difference in later-life size and survival probability between WSOs and non-WSOs will not vary with enforceability for all* $\rho \geq \bar{\rho}$.

3. Data and Empirics

The data for the study come from two micro-level datasets at the U.S. Census Bureau: the Longitudinal Business Database (LBD) and the Longitudinal Employer Household Dynamics (LEHD). The LBD contains employment and payroll information from 1976 to 2010 for all establishments in the United States with at least one employee. The LEHD is a state-level matched employer-employee dataset, and contains employment history of all individuals, employment and payroll information on all employers, and some individual information on employees. The first year the LEHD data are available for three states (the minimum required for any analysis) is 1990. The geographical coverage increased over time as more states began to participate in this program. Our study was based on thirty states (AR, CA, CO, FL, GA, HI, IA, ID, IL, IN, LA, MD, ME, MT, NC, NJ, NM, NV, OK, OR, RI, SC, TN, TX, UT, VA, VT, WA, WI, and WV).

To construct our sample, we first selected from the LBD, all single-establishment firms that were born in the states and during the years for which we had LEHD data. This sample consisted of about 5.5 million firms formed between 1990 and 2008. We then identified a subset of these new firms as WSOs, the details of which are provided below. The remaining firms were classified as non-WSOs.

3.1. Identifying WSOs

Because the LBD data do not directly identify WSOs, we used employee-movement data from the LEHD to identify WSOs. In this regard, our approach broadly draws on the discussion in Benedetto et

al. (2005) about identifying various firm events from these data. As with any classification under uncertainty, we had to trade off Type I errors (misclassifying some true WSOs as not being WSOs) with Type II errors (misclassifying some true non-WSOs as being WSOs). Given the critical importance of WSOs in our study, we focused on minimizing the latter, so that we are confident that the WSOs we identified are unlikely to be non-WSOs.

We began by identifying "founding clusters" of one or more employees moving from one establishment ("the predecessor establishment") to another ("the successor establishment") within the same state during a one-year period. We chose one year because the time duration in many CNCs is one year, and only in-state spinouts because out-of-state spinouts are less likely to be litigated and are a small fraction of all spinouts. Of these clusters, we restricted our attention to clusters that had fewer than twenty employees as potential spinouts because clusters with large numbers of employees are more likely to be data errors or administrative problems such as name changes rather than true new firms. From these clusters, we excluded clusters where the predecessor establishment was too small relative to cluster size; specifically, we imposed a condition that the cluster size be at most 50% of employment at the predecessor establishment. This condition also aimed to reduce the likelihood of events like ownership changes and firm deaths being identified as spinouts. As Benedetto et al. (2005) discuss, most of the predecessor-firm employees moving to a new firm likely represent firm deaths. We also excluded clusters where the successor establishment was too large relative to cluster size; specifically, we imposed a condition that the cluster size be at least 75% of employment at the successor establishment. This cutoff requires that a substantial part of the new firm worked together previously and also reduces the likelihood of simple identifier changes being classified as a spinout (Benedetto et al., 2005). We also excluded clusters where the successor establishment was more than one year old at the time of the employees moving to the establishment. Broadly, then, these clusters represent groups of employees moving from an existing firm to join a new firm. Employees in this cluster were classified as founders of the new firm. The predecessor establishment was defined as the parent.

This preliminary list was refined by using data from the LBD to exclude firms present in multiple states. From this, we identified WSOs as those that have the same four-digit NAICS code as their parent firm.[6] Those that had a different code were classified as out-of-industry spinouts (OSOs). Thus, at the end, we had three types of firms: WSOs, OSOs, and other new firms, comprising all others identified in the first step but not identified as WSOs or OSOs. We refer to the latter two types together as non-WSOs. About 8.4% of all new firms were classified as WSOs. Descriptive statistics are provided in Table 1. More details on the classification process are provided in the Online Appendix, Figure A4.

3.2. Quantifying CNC Enforceability

Ideally, an enforceability index would measure the probability of CNC enforcement (by courts) if a randomly selected CNC signer joined a competitor and the original employer sued the employee. To generate such an index, one would require data on both the circumstances under which a CNC would be enforced in a given state and the likelihood that such circumstances apply to a randomly selected CNC signer. Without knowing the latter, numerous scholars have used Malsberger's (1996) series *Covenants Not to Compete: A State by State Survey* to estimate the first component (Stuart and Sorenson, 2003; Garmaise, 2011). We utilize the index developed in Starr (2016), which applies factor analysis to seven dimensions of enforceability quantified by Bishara (2011) to create weights for each dimension, which approximate how important each component is for overall enforceability. Relative to other indices, this index is both more finely coded and less subjectively weighted. Table A1 in the Online Appendix reports the means for the seven dimensions of enforceability and the factor analysis weights, and Figure A1 in the Online Appendix presents the resulting enforceability scores by state for 1991 and 2009. The enforcement index, normalized to be mean 0 with a standard deviation of 1, is a continuous measure running from –4.17 to 1.15. We use the 1991 index.

[6] If there were multiple parent firms, we chose the parent firm according to the following criteria: (i) best industry match with parents, (ii) largest cluster size of moving employees, (iii) earliest observation, and (iv) largest parent firm. If these did not resolve the issue, we randomly assigned one parent firm.

The enforcement of an employee's CNC by courts is, of course, the final stage of a longer process: the focal employee must have signed a CNC, joined a competitor, and been sued by his former employer for violating the CNC, and the case must have made it to a final judgment. We provide some details of this process in Figure A2 of the Online Appendix. In Section 4.6.5 we discuss potential strategic actions by employees and firms during this process and what they mean for our estimates.

3.3. Identification

State enforceability is not determined randomly. It is potentially influenced by pro-firm, pro-worker, or other interests in the state. In particular, we are concerned that states with stricter enforceability may pass other policies that affect new firm formation and growth. One such example is corporate tax rates: states with stricter enforceability may have lower tax rates (indeed, tax rates are much higher in California than in Florida, which has the strictest enforceability of CNCs). Another example is the adoption of wrongful discharge laws (Autor et al., 2006), which make it harder to fire employees: California was the first to adopt these laws, while Florida has yet to adopt any. Though we can control for tax rates and wrongful discharge laws, there may be other policies that we cannot control for—for example, states also vary in the number of hours or years of schooling required to enter a particular occupation. If states with greater enforceability also have greater requirements, then they will also have similar effects to enforceability. Hence, enforceability is likely to be correlated with such unobserved state-level policies, and any cross-sectional analysis is likely to be contaminated by omitted variables.

To address the nonrandom selection of enforceability, other studies, such as Marx et al., (2009), Garmaise (2011), and Conti (2014), treat CNC law changes in Michigan in 1985, Texas in 1994, Florida in 1996, and Louisiana in 2001 as natural experiments and perform a difference-in-differences analysis before and after the changes. Due to the timespan of our data and restrictive disclosure requirements from the LEHD, we cannot pursue this longitudinal identification strategy.

Instead, we use a novel identification strategy that relies on the fact that for law firms, CNC enforcement is prohibited in *every* state. The primary argument for such a prohibition is that limiting a lawyer's ability to practice is equivalent to limiting a client's choice of attorney (Buffkin, 1999; Stroud,

2002). The American Bar Association (ABA) introduced the issue with Formal Opinion 300 in 1961, created Disciplinary Rule 2-108(A) in 1969, and finally codified it in 1983 as Rule 5.6 of the ABA's Model Rules of Professional Conduct, effectively establishing a prohibition on the enforcement of covenants not to compete for lawyers (Buffkin, 1999).[7]

Thus, to the extent that law firms are not affected by enforceability but are just as affected by all unobservable state-level variables that are correlated with enforceability as non-law firms, law firms form a natural control group for identifying the treatment effect of enforceability. This forms the basis of our difference-in-differences identification strategy. Controlling for industry-year, the within-state difference in firm-level outcomes between non-law and law firms differences out confounding state-level correlates, leaving the effect of enforceability. Furthermore, because we consider how enforceability affects WSOs and non-WSOs separately, we form a separate control group for each firm type. The control group for WSOs are law WSOs, and the control group for non-WSOs are law firms that are not identified as WSOs.

In our empirical analysis, we primarily run variants of the following specification:

$$Y_{jist} = \beta_0 + \beta_1 WSO_j + \beta_2 NonLaw_i + \beta_3 NonLaw_i * WSO_j \quad (1)$$
$$+ \beta_4 ENFC_s + \beta_5 ENFC_s * WSO_j + \beta_6 ENFC_s * NonLaw_i + \beta_7 ENFC_s * NonLaw_i * WSO_j + \alpha Z_{jist} + \nu_{jist}$$

where $ENFC_s$ refers to the level of enforceability in state s, $NonLaw_i$ refers to an industry being "not law,"[8] Y_{jist} refers to the outcomes of interest for firm j in industry i and state s in year t: log firm employment at years 0, 3, 5, and 7; dummies for surviving one, two, and three years; founder characteristics; and characteristics of employees hired in years 2 and 3. The variable WSO_j refers to a dummy for firm j being a WSO. In the preferred specification, state fixed effects subsume the main effect of enforceability, $\beta_4 ENFC_s$, while $\beta_2 * NonLaw_i$ is subsumed by industry-year fixed effects. The controls included in Z_{jist} are the log of the number of firms and total employment in the state-year, the log of the

[7] Stroud (2002) reviews this development of case law.

[8] We identify law firms as the five-digit NAICS code 54111 (Offices of Lawyers). Because we use four-digit industry-year fixed effects, we treat "Offices of Lawyers" as its own four-digit NAICS category.

number of firms and total employment in the industry, and the industry share of firms and employment, all of which are meant to capture both the size of the state and the industry. We also include main effects for state-level variables and interactions with $NonLaw_i$ for the following variables: dummies for whether the state has adopted the public policy, implied contract, and good faith exceptions to at-will employment (Autor et al., 2006), top state-level corporate tax rates (Seegert, 2012), and a dummy for whether the state has adopted right-to-work laws. These variables control for the possibility that any of these characteristics differentially affect law and non-law firms. Standard errors are clustered at the state level.

To help understand what each coefficient represents in Equation (1), we provide a graphic depiction in Figure 2 of how the predicted values of a given outcome for the four types of firms vary by enforceability. The coefficients β_0, β_1 β_2, and β_3 all shift the intercept term, estimating the mean of the outcome variable among our four groups: Law Non-WSOs (β_0), Law WSOs ($\beta_0 + \beta_1$), Non-Law Non-WSOs ($\beta_0 + \beta_2$), and Non-Law WSOs ($\beta_0 + \beta_1 + \beta_2 + \beta_3$). The coefficients β_4, β_5, β_6, and β_7 are all slope shifters, capturing how enforceability differentially affects the four groups.

To understand the role of law WSOs and other law entrants in our empirical analysis, consider coefficients β_4 and β_5. These measure the association between a unit increase in enforceability and the outcome variable for law non-WSOs (β_4), and the differential effect of enforceability on law WSOs (β_5). If Equation (1) was appropriately specified such that all omitted state-level and law-specific confounders were accounted for, then we should find that $\beta_4 = \beta_5 = 0$, because CNCs are not enforceable against departing lawyers. If, alternatively, enforceability is associated with unobserved state-level factors, then $\beta_4 \neq 0$, and if law WSOs are differentially affected by such confounders, then $\beta_5 \neq 0$.

Turning to the other coefficients, the causal impact of enforceability for non-WSOs is given by β_6, and the differential effect of enforceability on WSOs is given by β_7. The overall impact of enforceability on WSOs is $\beta_6 + \beta_7$. The focus in this paper is on the potential screening effect of WSOs, and so our primary emphasis is on β_7, though we discuss in detail the effects on non-WSOs.

To empirically examine entry rates, we aggregate the data to the state-industry-year level and calculate the entry rate of new firms, WSOs, and non-WSOs as the number of new firms of each type divided by the number of firms in the state-industry-year. We run variants of the following specification:

$$Y^c_{ist} = \gamma_0 + \gamma_1 NonLaw_i + \gamma_2 ENFC_s + \gamma_3 ENFC_s * NonLaw_i + \alpha Z_{ist} + \nu_{ist} \qquad (2)$$

where Y^c_{ist} represents the entry rate of all new firms, WSOs, and other entrants.

4. Results

4.1. Creation

Entry-related results (Equation (2)) are shown in Table 2, which builds up the difference-in-differences results. For each dependent variable, the first column shows the overall relationship without the law difference, the second column introduces a difference between law and non-law firms, and the third column includes state fixed effects. Though the overall entry rate of new firms is unaffected by enforceability, the entry rate of WSOs is negatively affected: a unit (or one-standard-deviation) increase in enforceability reduces WSOs' entry rate by 0.13 percentage points. The point estimate for non-WSOs is positive, but insignificant. Overall, in line with Hypothesis 1, these results show that enforceability reduces the entry of WSOs but does not have a significant impact on the entry of non-WSOs.[9]

4.2. Initial Size

Table 3 presents the results on how initial size, as measured by employment in the first year, is influenced by enforceability. To provide insights into the workings of our empirical specification, the table provides a detailed breakdown of the triple difference (Equation (1)). Starting with the basic correlations that include only the industry-year fixed effects (Column 1) and other controls (Column 2), we find that a unit increase in enforceability is associated with about a 2% decrease in the initial size of the firm. Column (3) groups all new firms together and shows that new non-law firms in high-

[9] As checks, we (i) estimated an establishment-level regression of a WSO dummy on *ENFC*NonLaw* interaction and controls (ii) re-estimated Equation (2) with number of new firms in the state-industry-year as weights. These results (Table A20 of the Online Appendix), indicate a screening effect for WSOs.

enforceability states tend to be smaller than law firms, but not significantly so. Columns (4) and (5) break down the effect in Column 3 separately for WSOs and other entrants. They show that WSOs tend to be larger in high-enforceability states (in line with Hypothesis 2b), but that other entrants tend to be smaller, though the difference for both is not statistically significant. Column (6) presents results without separately identifying law firms and shows that, as suggested by (4) and (5), non-WSO entrants are initially smaller in high-enforceability states, while WSOs tend to be relatively larger. Column (7) presents our triple-interaction specification without state fixed effects, while Column (8) presents our preferred specification with state fixed effects (Equation (1)).

The results of the full model (Column 8) indicate that a unit increase in enforceability reduces the observed average initial size of non-WSO entrants by 1.4% (β_6, the coefficient on *ENFC*NonLaw*). In contrast, relative to non-WSOs, a unit increase in enforceability increases WSO size by 1.1% (β_7, the coefficient on *ENFC*NonLaw*WSO*), almost completely undoing the negative effects on non-WSOs. These results strongly support Hypothesis 2b.

To test Hypothesis 2a, we aggregated the data to state, year, industry, and new firm type level and calculated the 25th and 75th percentiles of the initial size distribution in each cell, again as measured by initial employment. Table 4 shows the results of estimating Equation (1) with the log of the 25th and 75th percentiles of the initial size distribution as the dependent variables. Columns (4) and (8) show that enforceability has no noticeable impact on the 25th and 75th percentiles of the initial size distribution for non-WSOs. Relative to non-WSOs, however, the 25th percentile of the WSO distribution increases by 1.5% for a one-unit increase in enforceability, while the 75th percentile is unaffected. These results strongly support Hypothesis 2a, and show that enforceability truncates the lower portion of the initial size distribution for WSOs, but leaves the upper tail unchanged.

4.3. Founder Characteristics

Next, we consider which types of individuals choose to found new firms in high-enforceability states. In particular, we examine whether in high-enforceability states, founders are more likely to come from smaller firms (Hypothesis 3) and whether founders are more likely to have earned more prior to

founding (Hypothesis 2c). The sample for these regressions includes only WSOs and OSOs—that is, for whom we have parent data. Table 5 shows the results of estimating Equation (1).

Columns (1), (2), and (3) show that parent firm size (whether measured by log employment, Column 1, or by the log number of establishments, Column 2, or as the size of the founding cluster relative to the parent, Column 3) is largely unaffected by enforceability for non-WSOs. Relative to founders of non-WSOs, in high-enforceability states, those who found WSOs are more likely to come from smaller parents (6.4% smaller for a unit increase in enforceability), come from a parent with fewer establishments (4.2% smaller for a unit increase in enforceability), and found larger WSOs relative to their parents (0.5% larger for a unit increase in enforceability). Columns (4) and (5) show that those who found non-WSOs in high-enforceability states earn the same at their parent as those in lower-enforcing states. The relative effects for WSOs, however, show that founders in a one-unit-higher enforceability state earned 1.7% more on average in their parent companies.

To summarize, these results show that the characteristics of non-WSO founders are not correlated with enforceability, but those who form WSOs in high-enforceability states tend to form them from smaller parents and earn more before founding. Together, these results support Hypotheses 2c and 3.

4.4. Later-Life Size and Survival

Showing only our preferred specification, columns (1), (2), and (3) in Table 6 examine how enforceability influences the log of employment three, five, and seven years after formation. Focusing first on non-WSOs, these results show that enforceability is associated with smaller firm sizes, measured using employment, even up to seven years after creation. In particular, these results are consistent with our theoretical framework (which predicts a persistent size difference), and imply that a unit increase in enforceability is associated with a 1.3%, 1.7%, and 1.5% smaller firm for non-WSO new entrants three, five, and seven years post founding, respectively. Relative to non-WSOs, and as in the initial size regressions, a unit increase in enforceability increases the observed average WSO size at years three, five, and seven, by 1.4%, 1.2%, and 0.8%, respectively (in line with Hypothesis 4a). These relative effects almost completely offset the negative effects on size for non-WSOs. That the initial size difference

between WSOs and non-WSOs persists for several years provides strong evidence that the WSOs that do enter in higher enforceability states are of significantly higher quality than those in other states.

In our theoretical framework, the difference between WSOs and non-WSOs is largely due to the *screening effect*. Hence, once we control for the initial size (employment) differences that capture the *screening effect*, enforceability should not differentially affect WSOs and non-WSOs later in their life because they both face the same *hiring cost* and *investment protection effects* (Hypothesis 4b). Columns (4), (5), and (6) in Table 6 add log initial employment as a control in order to compare the growth of firms that start at the same size. The results show that the negative impacts associated with firm size for non-WSOs are almost completely explained by the initial size effect. That is, controlling for initial size, the impact of enforceability on firm size is small and insignificant. In contrast, even controlling for size, WSOs grow faster in their first three years in high-enforceability states relative to other entrants, but that after three years there is no further effect. Broadly, these results support our predictions.

Because the above specifications use the sample of survivors, any differential effect of enforceability on those that did not survive would not be captured, and our estimates may not reflect the complete impact of enforceability. To directly examine how enforceability affects the probability of survival, Table 7 presents Equation (1) with a dummy for surviving one, two, and three years as the dependent variables. Columns (1), (2), and (3), which do not include a control for initial firm size, show that a unit increase in enforceability reduces the probability of survival for non-WSOs by 0.21, 0.34, and 0.35 percentage points in years 1, 2, and 3, respectively. Relative to non-WSOs, a unit increase in enforceability increases the probability of WSO survival by 0.12, 0.15, and 0.07 percentage points in years 1, 2, and 3, respectively (supporting Hypothesis 4a). To examine the extent to which these results are driven by the effect of enforceability on initial size, in columns (4), (5), and (6) we control for initial size. These columns show that the while the effects of enforceability on non-WSOs fall relative to the case without log initial employment as a control, the effect is still negative and significant in each of the first three years. The relative effects for WSOs mostly disappear, consistent with Hypothesis 4b.

4.5. Hiring

To examine how enforceability affects hiring by new firms, we collected age, imputed education, industry experience, and prior earnings data on all employees hired between quarters 5 and 12 of the firm's life from the LEHD.[10] Table 8 reports regressions using our preferred specification without initial size controls (Table A19 includes these controls). A unit increase in enforceability increases the average education of a new hire by a non-WSO by 0.057 years and the log mean prior earnings by about 3%. The estimates for age and industry experience are insignificant, though the coefficients suggest that non-WSOs hire younger individuals or those with more industry experience in high-enforceability states. WSOs appear to hire similar individuals as non-WSOs, except that they hire higher-earning individuals.

4.6. Robustness Checks

4.6.1. Alternative Measures of CNC Enforceability

Our baseline analysis uses the factor-analysis-weighted index constructed from the 1991 data in Bishara (2011). To ensure that our results were not driven by the choice of this index, we repeated our analyses with the Garmaise (2011) indices for 1992 and 2001.We also re-estimated our results using the 2009 data in Bishara (2011) instead of 1991. Tables A2–A5 in the Online Appendix present these results for entry, initial size, growth, and survival. These results are qualitatively and quantitatively similar to those from our baseline Tables 2, 3, 6, and 7. In addition to ruling out potential issues with our index, the results using Garmaise (2011) indices, which have a lower variance than our baseline index, also indicate that the baseline results are not unduly influenced by the states at the ends of the enforceability index.

4.6.2. Alternative Specifications and Samples

We re-estimated and confirmed our Equation (1) results with state-industry-year fixed effects that account for potential state-industry-specific policies to encourage entry and growth during our sample timeframe (Table A2). We build up our main specification for size and survival in Tables A6–A17 of the Online Appendix. The last column in these tables, which estimate Equation (1) without state fixed effects,

[10] Choosing five quarters reduces the chance that we mistakenly capture any founders who joined later.

are similar to our baseline results. This shows that the estimates are robust to not using state fixed effects. We also dropped early years to have a balanced panel and found results similar to our full panel results.[11]

4.6.3. Law Firms as Control Group

The use of law firms as a control raises a concern about the extent to which law firms may represent the counterfactual outcome for non-law firms in the absence of enforceability, and whether factors unique to lawyers are driving the observed results. We address this here.

Though law firms differ in many ways from other industries—law firms tend to be smaller, be more focused on client relationships, and rely more on human capital—our examination of the data shows their entry behavior appears to be highly correlated with the entry behavior of other firms in very low CNC enforceability states. Furthermore, these industry-specific differences are picked up by industry-year fixed effects. Thus, any potentially confounding variables must not only differentially affect law and non-law firms, but also be correlated with enforceability. One such possible reason is that lawyers actually help enforce CNCs. However, given that *all* employment disputes in 2005 made up only 2.1% (Langton and Cohen, 2008) of all civil state court cases, this potential effect is likely to be small. A related possibility is that law firms make indirect attempts at restricting the mobility of their employees (e.g., by writing and litigating more complicated employment contracts). To the extent that high-enforceability states also enforce these indirect attempts at restricting the ability of lawyers to practice, this will attenuate our results because it makes law firms similar to other firms. Lawyers also must pass bar exams in a given state, which may inhibit their mobility—however, we only consider within-state transfers, and state fixed effects should absorb any state-specific bar exam requirement effects.

Furthermore, we effectively rely on a triple difference to estimate the screening effect: we compare how $|Y_{WSO} - Y_{Other}|_{Non\ Law} - |Y_{WSO} - Y_{Other}|_{Law}$ changes with enforceability. Hence, even if the unobserved variables are unique to law firms and correlated with enforceability, the second term in the

[11] We are unable to present some results discussed here due to disclosure constraints. Relevant codes will be made available to researchers interested in replicating these results inside the Census environment.

above expression will difference out those variables, as long as those variables affect both law WSOs and non-WSO law firms similarly. We also confirm our estimates of the screening effect using a more saturated model with state-year-industry fixed effects.

Empirically, many of our inferences do not appear to be contingent on using law firms as controls. As can be seen in column (6) in Tables A6–A17 in the Online Appendix, though not all of the specifications without law firms as a control show the same magnitude and significance as those with law firms as a control, the broad inferences based on the specifications without law firms as a control are still consistent with our baseline inferences. Additionally, we re-estimated our specifications based on a sample of professional services industries (which are more similar to law firms), including Professional, Scientific and Technical Services (NAICS 54) and Administrative and Support Services (NAICS 561), and a sample of technology industries defined by Paytas and Berglund (2004). The estimates are qualitatively similar to our baseline estimates, which suggests that the main estimates are not driven by the peculiarities of law firms or the treatment group. Furthermore, we broke the enforceability index into quartiles and re-estimated our main specification. If law firms are a good control group—that they exhibit "parallel trends" in non- or low-enforceability states—then we should not observe significant differences in the second quartile of enforceability relative to the first quartile. This is indeed what we find. The coefficients on enforceability are monotonically increasing, such that there is no significant difference between the first and second quartiles, but that there is between the first and fourth quartiles. These results give us confidence that law firms are accurately capturing the counterfactual outcome.

Nonetheless, we urge readers to be aware that like most identification strategies, using law firms as a control group is not perfect. If high-enforceability (low-enforceability) states adopt pro-incumbent (pro-entrepreneur) policies that do not affect law firms but differentially affect non-law WSOs, then using law firms as a control will not assist in reducing any of the bias from those policies.

4.6.4. Restrictions in the Classification Scheme

The classification process requires that in order to qualify as a spinout (whether WSO or OSO), the founding cluster size be at least 75% of the new firm's employment in at least one of the first four

quarters of its existence in the LEHD. We briefly discuss this restriction (details are provided in the Online Appendix, Figure A4) from theoretical and empirical perspectives.

Theoretically, any categorization of WSOs is inherently subjective, and there is no clear answer to what the cutoff should be. By choosing 75%, we are placing some importance on the group capital of founders and the degree of closeness to the parent, inherent aspects of spinouts (Agarwal et al., 2004; Klepper, 2009). Choosing a lower (higher) cutoff would classify firms farther from (closer to) the parent as WSOs. Empirically, the cutoffs we implemented are the same irrespective of whether the founders moved to the same industry or to a new industry, and are constant across states. Hence, the cutoffs are not inherently correlated with enforceability. Further, the cutoff does not affect the number of firms in the sample but only their classification. If the misclassification is purely classical measurement error, then our estimates will be biased toward zero. For the misclassification to result in upward bias in the enforceability coefficient on size, it must be that in higher enforceability states, "true" non-WSOs that are larger than the average WSO are more likely to be categorized as WSOs (but not in low-enforceability states) or that in higher enforceability states, smaller than average "true" WSOs are wrongly categorized as non-WSOs (but not in low enforceability states). Ex ante, it is not clear why that might happen.

We also performed two empirical tests to assess the robustness of our results to the cutoff restriction. First, we increased the cutoff from 75% to 100% and re-estimated our main specifications. Second, we limited our sample to WSOs and OSOs (out-of-industry spinouts) and did the same. Recall that both WSOs and OSOs are affected by all the classification cutoffs, except for the industry connection to the parent. Both of these tests yield results similar to our baseline estimates.

Together, the theoretical arguments and empirical tests give us some confidence that the cutoffs are not likely biasing our results. Nonetheless, our results are based on a specific cutoff and should be interpreted with this restriction and the associated discussions in mind.

4.6.5. Unobserved Strategic Actions

We are analyzing information at the aggregate level, which raises a concern that unobserved strategic actions by potential founders and firms may somehow bias our results. For instance, potential

founders may avoid CNC-related litigation by convincing individuals in their network who do not face CNC-related concerns to found the firm on their behalf and then join them after their CNC expired. If this was widespread, then WSOs in high-enforcing states would be just as likely to form in lower-enforcing states, and would make it less likely that we would identify any differential effects on WSOs.

Alternatively, the potential founder could leave the geographic area to start the firm. Marx et al. (2015) show that such a brain drain occurred after Michigan started enforcing CNCs in 1985. But such actions do not necessarily result in freeing the employee from the CNC, even if the employee leaves for a low-enforcing state like California (Glynn, 2008). Still, in consideration of this idea, we identified out-of-state spinouts from the LEHD. We found that out-of-state spinouts were such a minuscule proportion of new firms that we decided against studying them explicitly. Indeed, one need not leave the state to start a firm if the county or a set number of miles from the place of work is the relevant geographic constraint.

Firms may also take strategic actions against departing employees. In particular, firms in both low- and high-enforcing states may simply remind employees of their CNC or make threats of a lawsuit. As long as such actions are non-decreasing in enforceability, our predictions hold true. However, if firms are equally likely to threaten employees with CNC-related litigation in high- and low-enforcing states, and employees respond to such threats regardless of the actual enforceability of their CNCs (e.g., due to unawareness; Starr et al., 2015b), then this would attenuate the effects of enforceability.

5. Discussion

5.1. Summary of Results and Their Economic Significance

We find that increased enforceability has no significant effect on the entry of non-WSOs but reduces the entry of WSOs. Enforceability reduces the observed average size and survival of non-WSOs, but relative to non-WSOs, increases observed average WSO survival, size, and the 25th percentile of the initial size distribution. These differentials largely disappear once initial size is controlled for. Further, relative to non-WSOs on whom we observe no effects, enforceability induces higher human capital individuals from smaller parent firms to found WSOs. Finally, although higher enforceability appears to

be associated with lower firm growth, those that hire tend to hire higher human capital employees. This appears to be true among both WSOs and non-WSOs.

Based on our results in Tables 2–8, if the mean enforceability increased by one standard deviation, it would result in a 0.13-percentage-point decrease in the entry rate of WSOs. The average WSO entry rate in the sample is 0.4%, which translates to a substantial 28.7% decline in the number of WSOs that are formed. Relative to non-WSOs that are formed, WSOs that are established would be 1.1% larger, with the 25th percentile of the size distribution being about 1.5% larger. Those WSOs would also be (relative to non-WSOs) 0.12 percentage points more likely to survive the first year, and 0.15 percentage points more likely to survive the second year. The average size of these WSOs' parent firms would be 6.4% lower, and average founder earnings would be 1.7% higher, both relative to the change for non-WSOs. These effects are comparable in terms of magnitude to those found for mobility, for instance, in Marx et al., (2009). Below, we discuss how these results align with our proposed theoretical framework.

5.2. Screening Out: Linking the Empirical Results with Theory

One of the key contributions of our study is to show that stricter enforceability has a screening effect on the formation of WSOs. Above a threshold level of enforceability, the threat of litigation (and the associated higher settlement to the parent firms) reduces the expected profits from forming a WSO. As a result, founders with lower human capital (who are likely to have lower expected profits from forming the WSOs) are dissuaded from entering. Thus, WSOs are less likely to enter, but the average initial size of those that enter is likely to be higher. Furthermore, as long as parents in high-enforcing states are willing to bring CNC-related charges against low-enough-quality potential WSOs, the screening effect also implies that the left tail of the initial size distribution will be truncated, but the upper tail will be largely unaffected. That is indeed our finding. Our results on founder earnings and parent size are also consistent with what one would expect from a screening effect. Furthermore, if this screening mechanism is correct, and if we were to compare WSOs and non-WSOs that had the same initial size, then the differential advantage of WSOs over the longer term should disappear. This is what we find. Together,

these results are consistent with a screening effect. Indeed, Ganco et al. (2015) find a similar effect for patent enforcement, which does not seem to prevent the brightest inventors from leaving.

Turning to non-WSOs, the theoretical impact of enforceability is ambiguous. The investment protection effect increases expected profits by increasing returns from investments in employee human capital. The hiring cost effect reduces expected profits by making it more expensive to hire employees if those employees are also bound by CNCs. Our results on non-WSOs appear to be mixed, with some evidence for the presence of both effects. That the impact of enforceability on entry of non-WSOs is positive (although statistically insignificant), and that the non-WSOs that are created tend to be smaller, is consistent with the investment protection effect, since it lowers the entry threshold. However, conditional on initial size, we do not see a positive effect on growth or survival after founding, which would be expected if firms are benefiting from enforceability. In particular, we do not see any impact on size (at years 3, 5, and 7), but enforceability reduces the probability of survival even after controlling for initial size. The latter is more consistent with enforceability causing hiring challenges. One possible explanation is that non-WSOs systematically underestimate the hiring challenges associated with enforceability when forming the firm. Indeed, this might be the case if founders are unaware of CNC policy (since they founded a non-WSO) until they actually try to hire employees. Better data on to what extent entrepreneurs consider the CNC regime before starting their firm may help resolve this ambiguity.

With regard to hiring, we broadly find that enforceability reduces the new firms' initial and subsequent size, but those that do grow tend to hire higher-quality individuals. In our framework, this is isomorphic to the screening effect for WSOs. New firms pursuing individuals bound by CNCs will not be willing to pay a premium for low-quality employees. As a result, in high-enforceability states fewer firms will hire, but those that do will hire higher human capital employees.

5.3. Alternative Explanations

We briefly discuss some potential alternative explanations, focusing primarily on the differential effect for WSOs. Higher human capital workers are more likely to sign CNCs (Starr et al., 2015). Hence, one may expect that enforceability reduces WSO entry by constraining founders on the upper end of the

human capital distribution. If so, however, we would expect lower human capital founders to form WSOs, which would imply a lower average initial size, a lower 75th percentile of the initial size distribution, lower average founder earnings, and lower average performance unconditional on initial size. All these results are in the opposite direction. Thus, it is unlikely that enforceability is constraining higher human capital founders. A related explanation is that higher enforceability encourages parent firms to provide more industry-specific training to their employees. This would benefit potential WSO founders, who will then likely have better-quality ideas in high-enforceability states. This would explain the positive effect of enforceability on the performance of WSOs, but it would be inconsistent with reduced entry.

Another possible explanation is that potential founders are choosing among multiple ideas, some of which are WSO ideas, and that enforceability makes it more attractive to choose non-WSO ideas. Then, WSO entry would decline, and depending on the payoffs to forming WSOs and non-WSOs, potentially increase the minimum size of WSOs and decrease the size of non-WSOs. Though it is possible that such "substitution" of non-WSO ideas for WSO ideas occurs, we believe that it is not likely to be the dominant mechanism in our context. First, ideas need a lot of resource-intensive development before entrepreneurs can choose among them. Second, studies in the entrepreneurship literature find that in general entrepreneurs do not consider different market options before their first market entry, even when alternative market opportunities are potentially more profitable (Shane, 2000; Grégoire and Shepherd, 2012). Third, this argument would also suggest an increased entry of non-WSOs, and depending on the relative payoffs, a decrease in the minimum and maximum size of the non-WSOs or an increase in the mean and maximum size of non-WSOs, which do not appear to find empirical support. Table A18 outlines how well each of these alternative explanations lines up with the empirical results in this study.

5.4. Implications of Our Study

Our results have implications for research on new venture formation and entrepreneurship. They suggest that the legal regime affecting employee mobility can have important and nuanced effects not only on employee mobility and the creation of new firms, but also on their growth, survival, and the types of employees they hire. Also, our results indicate that, to understand the overall effect of enforceability,

we need to not only examine individual-level mobility (Marx et al., 2009; Marx, 2011) but also consider firm-level outcomes, because enforceability affects new firm formation and their growth in ways different from those at the individual level. Furthermore, our results highlight how studying the aggregate effect on employee mobility may mask important differential effects across different types of firms.

From an incumbent manager's perspective, a benefit of higher enforceability appears to be lower competition from having fewer WSOs being formed. However, it is not clear that managers will unambiguously benefit from stricter enforceability. Though there are fewer WSOs in such states, the average WSO that is formed is likely to be stronger. Moreover, though stricter enforceability may help managers retain employees for a longer time, it will make it harder for them to hire talented workers. This, in turn, can have negative productivity consequences. Thus, whether to support CNC enforceability may depend on the specific needs of the managers as well as perhaps the specific skills that the potential recruit is expected to bring in or develop. Managers in industries with rapid technological change who rely on outside talent and entrepreneurs who have limited resources to invest in training will likely prefer lower enforceability. In contrast, managers who rely on employees developing skills on the job or have already invested in training programs will likely prefer stricter enforceability.

From a policymaker's perspective, the reduction in WSO entry suggests potential welfare losses due to enforceability. However, because the reduction occurs among lower quality entrants, a more definite determination will require a deeper examination of ideas that are screened out as well as an analysis of incumbent behavior in response to enforceability. We leave these as topics for future research.

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Figure 1: CNC Enforceability and New Firm Formation

Figure 1-A

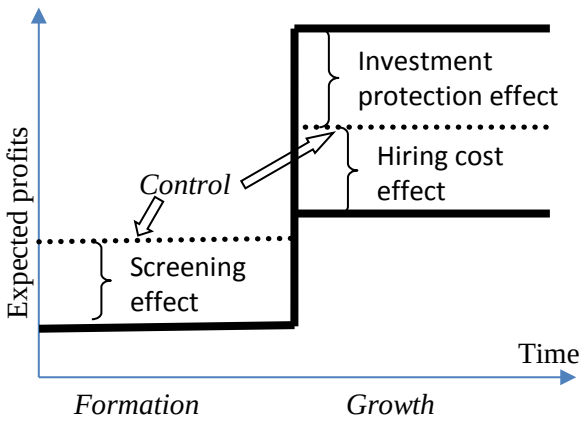
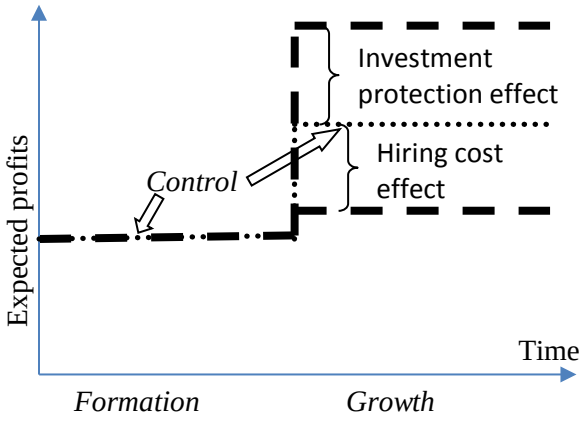


Figure 1-B

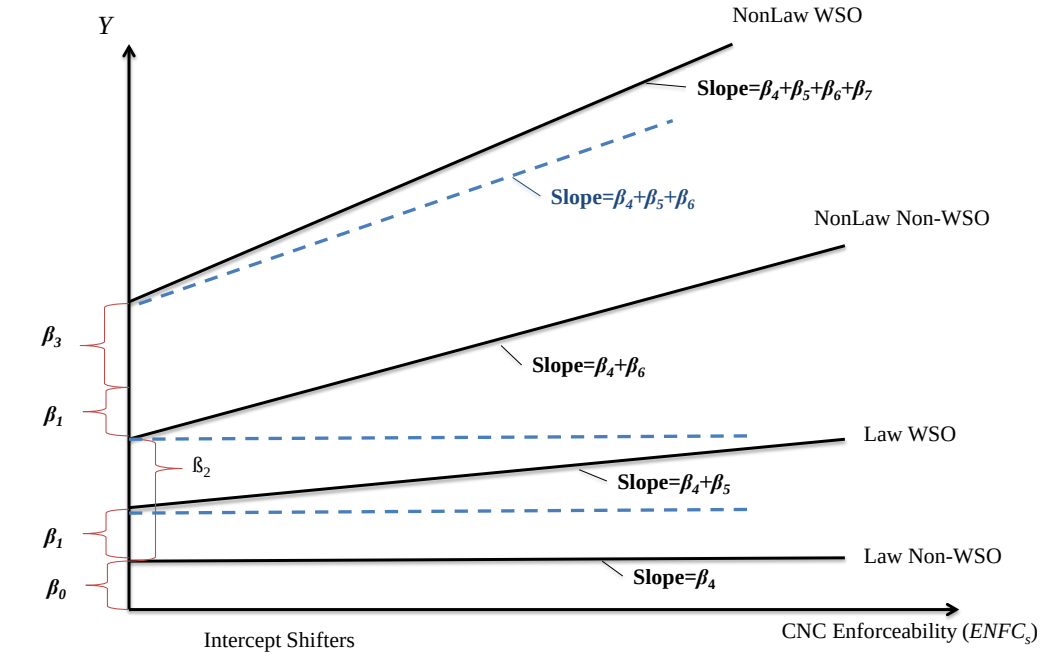


WSO
ρ>0 & λ>0 in the formation period,
ρ>0 & λ≥0 in the growth period

Non-WSO
ρ=0 & λ=0 in the formation period,
ρ>0 & λ≥0 in the growth period

Note: ..... denotes Control: No CNC enforceability (ρ=0).

Figure 2: Our Empirical Approach



$$Y_{jist}=\beta_0+\beta_1 WSO_j+\beta_2 NonLaw_i+\beta_3 NonLaw_i*WSO_j$$
$$+\beta_4 ENFC_s+\beta_5 ENFC_s*WSO_j+\beta_6 ENFC_s*NonLaw_i+\beta_7 ENFC_s*NonLaw_i*WSO_j+\alpha Z_{jist}+\nu_{jist}$$

Slope Shifters

Table 1: Descriptive Statistics

PANEL A: All new firms	**Full sample**		**Law**		**Non-law**	
	Mean	SD	Mean	SD	Mean	SD
WSO dummy	0.08	0.28	0.23	0.42	0.08	0.27
New firm entry rate	0.08	0.09	0.07	0.02	0.08	0.10
CNC enforceability	–0.46	1.77	–0.47	1.77	–0.46	–1.77
Industry employment	44,996	69,052	51,564	39,853	44,849	69,557
Industry number of firms	4,303	5,220	9,696	7,032	4,184	5,109
Industry share of employment	0.01	0.01	0.01	0.00	0.01	0.01
Industry share of firms	0.01	0.01	0.03	0.01	0.01	0.01
Year 0 employment	5.86	60.50	2.92	7.30	5.93	61.20
Year 3 employment	7.66	55.40	3.79	8.08	7.76	56.10
Year 5 employment	8.74	62.60	4.18	7.68	8.87	63.50
Year 7 employment	9.64	73.70	4.51	8.44	9.80	74.80
N	*5,538,000*		*121,000*		*5,417,000*	
PANEL B: WSOs	**Full sample**		**Law**		**Non-law**	
	Mean	SD	Mean	SD	Mean	SD
WSO entry rate	0.004	0.02	0.02	0.01	0.004	0.02
Year 0 employment	3.89	17.4	2.33	3.44	4.00	17.9
Year 3 employment	5.76	50.2	3.14	4.04	5.95	52.0
Year 5 employment	6.67	58.3	3.55	4.04	6.92	60.6
Year 7 employment	7.36	59.2	3.90	6.74	7.67	61.7
Mean founder earnings at parent	7,265	64,124	10,522	23,188	7,055	65,882
Parent establishment size	244	2,235	41	509	258	2,302
Number of parent establishments	8.55	139.39	1.40	18.71	9.01	143.7
Relative size of spinout	0.22	0.18	0.22	0.18	0.22	0.18
N	*466,000*		*28,000*		*438,000*	
PANEL C: Non-WSOs	**Full sample**		**Law**		**Non-law**	
	Mean	SD	Mean	SD	Mean	SD
Non-WSO entry rate	0.07	0.09	0.05	0.01	0.07	0.09
Year 0 employment	6.04	63.02	3.10	8.11	6.10	63.59
Year 3 employment	7.85	55.86	4.01	9.05	7.94	56.44
Year 5 employment	8.95	63.06	4.41	8.37	9.06	63.77
Year 7 employment	9.88	75.06	4.73	8.96	10.01	75.96
N	*5,072,000*		*93,000*		*4,979,000*	
PANEL D: OSOs	**Full sample**		**Law**		**Non-law**	
	Mean	SD	Mean	SD	Mean	SD
Year 0 employment	2.62	17.5	1.61	1.94	2.64	17.6
Year 3 employment	4.14	33.2	2.24	3.66	4.18	33.6
Year 5 employment	5.05	47.1	2.54	3.04	5.11	47.6
Year 7 employment	5.89	55.8	2.71	3.58	5.96	56.5
Mean founder earnings at parent	6,708	36,942	5,645	18,461	6,728	37,611
Parent establishment size	2,210	8,691	2,847	9,642	2,198	8,847
Number of parent establishments	56.35	542.40	55.71	460.51	56.36	552.17
Relative size of spinout	0.10	0.14	0.07	0.13	0.09	0.15
N	*1,078,000*		*19,000*		*1,059,000*	

Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Data for years 3, 5, and 7 based on survivors. WSOs are within-industry spinouts, and OSOs are out-of-industry spinouts.

Table 2: CNC Enforceability and the Entry Rate of New Firms

	Overall entry rate			WSO entry rate			Non-WSO entry rate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
ENFC	0.0006	0.0014	*n.i.*	0.0002	0.0018**	*n.i.*	0.0005	–0.0000	*n.i.*
	(0.0017)	(0.0017)		(0.0002)	(0.0006)		(0.0016)	(0.0015)	
ENFC*NonLaw		–0.0008	–0.0005		–0.0012**	–0.0013**		0.0005	0.0007
		(0.0012)	(0.0012)		(0.0005)	(0.0005)		(0.0012)	(0.0012)
Relevant sample	All	All	All	All	All	All	All	All	All
Industry-year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
State FE	N	N	Y	N	N	Y	N	N	Y
Other controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	140,000	140,000	140,000	140,000	140,000	140,000	140,000	140,000	140,000

*Notes: The data are aggregated to the state-industry-year level. Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 3: CNC Enforceability and Initial Size of New Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WSO						–0.109***	–0.135***	–0.136***
						(0.008)	(0.008)	(0.008)
NonLaw*WSO							0.027***	0.027***
							(0.007)	(0.007)
ENFC	–0.022***	–0.019***	–0.009	–0.005	–0.008	–0.021***	–0.010	*n.i.*
	(0.005)	(0.005)	(0.007)	(0.008)	(0.007)	(0.005)	(0.007)	
ENFC*WSO						0.026***	0.015***	0.015***
						(0.002)	(0.003)	(0.003)
ENFC*NonLaw			–0.011	0.007	–0.013		–0.012	–0.014**
			(0.009)	(0.009)	(0.008)		(0.008)	(0.005)
ENFC*NonLaw*WSO							0.012***	0.011***
							(0.002)	(0.002)
Relevant sample	All	All	All	WSO	Non-WSO	All	All	All
Industry-year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
State fixed effects	N	N	N	N	N	N	N	Y
Other controls	N	Y	Y	Y	Y	Y	Y	Y
N	5,538,000	5,538,000	5,538,000	466,000	5,072,000	5,538,000	5,538,000	5,538,000

*Notes: The dependent variable in all regressions is the log of employment in the first year. Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 4: CNC Enforceability and the 25th and 75th Percentiles of the Initial Size Distribution

	25th percentile of initial size distribution				75th percentile of initial size distribution			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WSO		0.324***	0.009	0.009		–0.288***	–0.225***	–0.225***
		(0.008)	(0.005)	(0.005)		(0.013)	(0.016)	(0.016)
NonLaw*WSO			0.318***	0.318***			–0.063***	–0.064***
			(0.010)	(0.010)			(0.017)	(0.017)
ENFC	–0.030	–0.016*	–0.035	*n.i.*	0.017	–0.018*	0.011	*n.i.*
	(0.034)	(0.009)	(0.041)		(0.017)	(0.010)	(0.024)	
ENFC*WSO		0.015***	–0.000	–0.000		0.028***	0.024*	0.024*
		(0.004)	(0.004)	(0.004)		(0.007)	(0.013)	(0.013)
ENFC*NonLaw	0.021		0.019	0.019	–0.027		–0.029	–0.029
	(0.036)		(0.044)	(0.044)	(0.019)		(0.025)	(0.025)
ENFC*NonLaw*WSO			0.016**	0.015**			0.005	0.004
			(0.006)	(0.006)			(0.013)	(0.013)
Relevant sample	All	All	All	All	All	All	All	All
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	No	No	Yes	No	No	No	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	159,000	159,000	159,000	159,000	159,000	159,000	159,000	159,000

*Notes: The data are aggregated to the state-industry-year-new firm type level. Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 5: CNC Enforceability, Parent Firm, and Founder Characteristics

	Parent firm			Founder	
	(1)	(2)	(3)	(4)	(5)
	Log. establishment size	Log. number of establishments	Relative size	Mean earnings	Mean relative earnings
WSO	–0.492***	–0.223***	0.066***	0.568***	0.494***
	(0.099)	(0.051)	(0.009)	(0.024)	(0.029)
NonLaw*WSO	–0.368***	–0.097***	0.028***	–0.343***	–0.207***
	(0.110)	(0.032)	(0.006)	(0.028)	(0.022)
ENFC	*n.i.*	*n.i.*	*n.i.*	*n.i.*	*n.i.*
ENFC*WSO	0.043***	0.051**	0.000	–0.004	–0.002
	(0.012)	(0.021)	(0.003)	(0.011)	(0.019)
ENFC*NonLaw	0.015	0.008	–0.001	–0.002	–0.014
	(0.019)	(0.014)	(0.002)	(0.020)	(0.016)
ENFC*NonLaw*WSO	–0.064***	–0.042***	0.005**	0.017**	0.015
	(0.012)	(0.009)	(0.002)	(0.006)	(0.010)
Relevant sample	Spinouts	Spinouts	Spinouts	Spinouts	Spinouts
Log of initial size	No	No	No	No	No
Industry-year FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes
N	1,544,000	1,544,000	1,544,000	1,544,000	1,544,000

*Notes: Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Mean Relative Earnings refers to mean of founder earnings relative to average earnings at the parent in the year-quarter of leaving. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 6: CNC Enforceability and Log of Employment at Years 3, 5, and 7

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Emp Year 3	Log Emp Year 5	Log Emp Year 7	Log Emp Year 3	Log Emp Year 5	Log Emp Year 7
WSO	–0.123***	–0.117***	–0.104***	0.014***	0.023***	0.031***
	(0.010)	(0.010)	(0.013)	(0.005)	(0.005)	(0.008)
NonLaw*WSO	0.032***	0.043***	0.038***	0.017***	0.024***	0.024***
	(0.010)	(0.009)	(0.012)	(0.005)	(0.007)	(0.008)
ENFC	*n.i.*	*n.i.*	*n.i.*	*n.i.*	*n.i.*	*n.i.*
ENFC*WSO	0.016***	0.018***	0.021***	0.002	0.004*	0.010***
	(0.004)	(0.004)	(0.005)	(0.002)	(0.002)	(0.003)
ENFC*NonLaw	–0.013*	–0.017**	–0.015	–0.004	–0.006	–0.003
	(0.007)	(0.008)	(0.009)	(0.003)	(0.004)	(0.006)
ENFC*NonLaw*WSO	0.014***	0.012***	0.008**	0.004**	0.002	–0.002
	(0.003)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)
Relevant sample	3-year survivors	5-year survivors	7-year survivors	3-year survivors	5-year survivors	7-year survivors
Log of initial size	No	No	No	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
N	3,172,000	2,161,000	1,464,000	3,172,000	2,161,000	1,464,000

*Notes: Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 7: CNC Enforceability and Survival

	(1) Survive 1 year	(2) Survive 2 years	(3) Survive 3 years	(4) Survive 1 year	(5) Survive 2 years	(6) Survive 3 years
WSO	0.0512***	0.0658***	0.0717***	0.0573***	0.0737***	0.0792***
	(0.0019)	(0.0047)	(0.0065)	(0.0018)	(0.0046)	(0.0063)
NonLaw*WSO	–0.0004	–0.0172***	–0.0282***	–0.0016	–0.0188***	–0.0297***
	(0.0013)	(0.0033)	(0.0045)	(0.0014)	(0.0034)	(0.0046)
ENFC	*n.i.*	*n.i.*	*n.i.*	*n.i.*	*n.i.*	*n.i.*
ENFC*WSO	0.0004	0.0017	0.0028	–0.0003	0.0008	0.0019
	(0.0006)	(0.0016)	(0.0022)	(0.0005)	(0.0015)	(0.0021)
ENFC*NonLaw	–0.0021**	–0.0034***	–0.0035**	–0.0014*	–0.0026**	–0.0028*
	(0.0008)	(0.0011)	(0.0014)	(0.0008)	(0.0011)	(0.0016)
ENFC*NonLaw*WSO	0.0012***	0.0015*	0.00067	0.0007*	0.0008	0.0001
	(0.0004)	(0.0008)	(0.0013)	(0.0004)	(0.0007)	(0.0012)
Relevant sample	All	All	All	All	All	All
Log of initial size	No	No	No	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes	Yes	Yes
N	5,538,000	5,538,000	5,538,000	5,538,000	5,538,000	5,538,000

*Notes: Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1. n.i.: not identified.*

Table 8: CNC Enforceability and Employee Characteristics

	(1)	(2)	(3)	(4)
	Age	Education	Industry experience	Mean earnings
WSO	0.806***	0.199***	3.000***	0.159***
	(0.138)	(0.023)	(0.303)	(0.010)
NonLaw*WSO	–0.736***	–0.198***	–0.586*	–0.131***
	(0.134)	(0.027)	(0.296)	(0.008)
ENFC	*n.i.*	*n.i.*	*n.i.*	*n.i.*
ENFC*WSO	–0.097**	–0.015**	–0.303**	–0.019***
	(0.036)	(0.007)	(0.125)	(0.003)
ENFC*NonLaw	–0.179	0.057**	0.176	0.029***
	(0.110)	(0.023)	(0.144)	(0.010)
ENFC*NonLaw*WSO	0.025	0.009	0.136	0.018***
	(0.037)	(0.008)	(0.113)	(0.002)
Relevant sample	Hirers	Hirers	Hirers	Hirers
Log of initial size	No	No	No	No
Industry-year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
N	643,000	643,000	643,000	643,000

*Notes: Number of observations rounded to nearest 1,000 in accordance with U.S. Census Bureau disclosure policy. Standard errors clustered by state in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. n.i.: not identified.*

Appendix: Model

The model consists of two periods. In the first period, founders form a spinout, which may be a WSO or a non-WSO. Founder human capital is described by a scalar, $\theta \in U[0, \overline{\theta}]$. Forming a new firm requires a fixed cost $F > 0$ to cover investments in infrastructure such as machinery, websites, warehouses, and so on. A share λ of the spinout's profits comes at the expense of the parent firm, where $0 \leq \lambda \leq 1$. Hence, if the new firm is a more direct competitor to the parent firm (i.e., a WSO), a greater share of its profits comes at the expense of their parent firm.[1] Profits and firm size are increasing functions of founder human capital. In particular, per-period profits and firm size attributable to founder human capital are $\frac{\theta}{2}$.[2] In the second period, the new firm perceives an investment opportunity and possibly grows further by adding an employee.

All CNC-related litigation is assumed to occur prior to formation. Upon knowing the founders' intention to form a spinout, the parent firm may bring CNC litigation at cost L. If it does not litigate or loses the case in court, it loses λ times the spinout's profits after the spinout is formed. Alternatively, founders and the parent firm can negotiate and allow the new firm to form. We assume that the negotiation is "take it or leave it," where the founders make an offer and the parent firm accepts it as long as the offer is better than its best alternative to a negotiated agreement.

If founders face CNC-related litigation, it costs them L to cover lawyer fees and other legal costs. With probability ρ, founders lose the litigation and do not form the new firm. Here, ρ reflects enforceability, with higher values of ρ denoting stricter enforceability. Intuitively, this means that for a given set of case facts, potential founders in a state with stricter enforceability (or equivalently, a higher ρ) are more likely to lose the case than potential founders in a state with lower enforceability. As we show later, increasing levels of ρ screen (or dissuade) certain types of potential founders out of founding a new firm, which we refer to as the *screening effect*.

In the second period, the new firm perceives a random investment opportunity, which requires it to hire an employee with a certain skill level σ and yields a profit of σ, contingent on the employee not leaving the new firm. σ is randomly distributed in $U[0, \overline{\sigma}]$, and hence, different firms may perceive different opportunities with differing skill requirements. Implementing the expansion requires a fixed investment c that may include the search cost of hiring the employee, investments in training the employee, and other expansion-related fixed costs. This employee's wage, $w(\rho)\sigma$, $0 < w(\rho) \leq 1$, is paid at the beginning of the second period and is increasing in enforceability because the worker must be compensated in expectation of potential litigation. This increase in wage due to enforceability reflects the *hiring cost effect*. After being

[1] For instance, a physician leaving an existing practice to start his or her own practice may shrink the parent firm's patient list, but a physician leaving a practice to start a dance studio is unlikely to affect the parent firm's practice.

[2] As long as size and profits are strictly increasing function of θ, our results will hold.

hired (at the beginning of the period), this employee may leave the spinout sometime during the second period with probability $\gamma(\rho)$, where $\gamma'(\rho) < 0$ for all ρ. The decrease in γ with ρ is in line with enforceability leading to lower employee mobility, as found in prior studies such as Marx et al. (2009) and Garmaise (2011). We assume that the investment does not increase firm profits if the employee leaves. Further, we assume that it is more profitable to hire an employee with higher skill than an employee with lower skill for every value of ρ, or equivalently that $(1 - \gamma(\rho)) \geq w(\rho)$ for all $\rho \in [0, 1]$.

We use backward induction to solve for the conditions under which the spinout is formed (rather than the equilibrium in general). So, we solve for the investment decision in the second period, and then use the expected profits from that decision when solving for the first-period firm formation decision. Throughout, we ignore discounting and assume complete information symmetry.

Investment and hiring in the second period

For any given σ, the investment opportunity is pursued and the employee is hired only if:

$$(1 - \gamma(\rho))\sigma - w(\rho)\sigma \geq c \tag{1}$$

The first term on the left is the expected benefits from the investment were the employee to stay. This expected benefit increases with enforceability and reflects the *investment protection effect*. Given the assumption that $(1 - \gamma(\rho)) \geq w(\rho)$ for all $\rho \in [0, 1]$, it follows that the left-hand side is increasing in σ. Thus, for every ρ, there is a lower bound to the level of human capital hired. This lower bound is given by:

$$\sigma^* = \frac{c}{1 - \gamma(\rho) - w(\rho)} \tag{2}$$

This lower bound may increase or decrease with enforceability depending on which effect dominates. It will decrease if the investment protection effect dominates (specifically, if $\gamma'(\rho) + w'(\rho) < 0$, where γ' and w' are the derivatives of γ and w with respect to ρ) and increase if the hiring cost effect dominates (specifically if $\gamma'(\rho) + w'(\rho) > 0$).[3] Also note that as σ^* increases, fewer but higher-quality investment opportunities (as measured by the average skill level of the employees hired) are pursued. Conversely, a decrease in σ^* will result in more but lower quality investments being pursued.

Then, the expected profit from the second-period investment (at entry in the first period) is given by

$$\pi^*(\rho) = (1 - \frac{\sigma^*}{\overline{\sigma}})\Big((1 - \gamma(\rho) - w(\rho))\big(\frac{\sigma^* + \overline{\sigma}}{2}\big) - c\Big) \tag{3}$$

[3] To see this, note that $\frac{d\sigma^*}{d\rho} = \frac{c(\gamma'(\rho)+w'(\rho))}{(1-\gamma(\rho)-w(\rho))^2}$, which is increasing (decreasing) if the numerator is positive (negative).

The first term of this expression is the probability that the investment opportunity is pursued. The second term is the expected profit if the investment opportunity is pursued, which is the average σ that the firm receives adjusted for wage payments and the expected likelihood the employee leaves, less the cost of pursuing the opportunity. It can be shown that the expected profits from the second period are increasing in enforceability if the investment protection effect dominates and decreasing in enforceability if the hiring cost effect dominates.

Parent firm's litigation decision

The parent firm brings CNC-related litigation if

$$-(1-\rho)\lambda(\theta+\pi^*(\rho))-L \geq -\lambda(\theta+\pi^*(\rho)) \tag{4}$$

The first term on the left-hand side of the inequality is the probability of losing the case multiplied by the potential losses (λ times the spinout's profits) were the case to be lost. The term on the right-hand side is the certain loss incurred by the parent firm if it chooses not to litigate. Solving, we get

$$\theta_L \geq \frac{L}{\lambda\rho}-\pi^*(\rho) \tag{5}$$

Hence, the parent firm litigates only above a certain founder human capital threshold ("litigation threshold"), θ_L, which is decreasing in the extent of damage caused to the parent (λ).

The parent firm may also consider settlement. In this case, the minimum compensation that will make litigation unattractive to the parent firm is determined by the difference in the value between litigating and not litigating: $[-(1-\rho)\lambda(\theta+\pi^*(\rho))-L]-[-\lambda(\theta+\pi^*(\rho))]$. Note that the terms within the brackets are simply the two sides of the previous inequality. This yields the minimum compensation, C_{min}, that the parent firm will accept in return for not litigating: $C_{min}=\rho\lambda(\theta+\pi^*(\rho))-L$, which is increasing in enforceability and founder human capital.

Founders' spinout formation decision

First, consider founders who will not face litigation (because the litigation threshold is higher than their human capital level). Then, their decision to form a spinout is decided by whether the total profits from forming the spinout ($\theta+\pi^*(\rho)$) exceeds the fixed cost F.[4] Solving this for θ yields an entry threshold, $\theta_e^{NWSO}=F-\pi^*(\rho)$, so that no founders below this level of human capital form a spinout. Since non-WSOs never face litigation, we refer to this threshold as the non-WSO entry threshold. Note that even though the founders do not face litigation, the threshold is still a function of ρ because enforceability affects the second-period profits through the investment protection and hiring cost effects.

[4] We assume the founders pay fixed cost F regardless of whether any litigation occurs.

If the founders will face litigation, they can offer a settlement to the parent firm. The maximum compensation that the founders can provide is equal to the surplus value of forming the spinout over litigation, that is, $[\theta + \pi^*(\rho)] - [(1-\rho)(\theta + \pi^*(\rho)) - L]$. The first term is the benefit to the founders from certainly forming the spinout, while the second term is the expected benefit from litigating. This yields the maximum compensation, C_{max}, that the founders can offer: $C_{max} = \rho(\theta + \pi^*(\rho)) + L$, which is increasing in enforceability and founder human capital. Note that $C_{min} < C_{max}$, and so settlement is always preferable to litigation. Although the exact settlement may range anywhere between C_{min} and C_{max}, we assume that it is $\rho\lambda(\theta + \pi^*(\rho))$. This reflects the fact that founders are unlikely to share profits not coming from the parent firm. Since it lies in between C_{min} and C_{max}, we assume the parent firm will accept the settlement and allow the new firm to form.

Based on this, founders who will face litigation (because their human capital is above the litigation threshold) form the spinout when:

$$(\theta + \pi^*(\rho)) - \rho\lambda(\theta + \pi^*(\rho)) \geq F \tag{6}$$

Solving, we get:

$$\theta \geq \frac{F}{1-\rho\lambda} - \pi^*(\rho) \equiv \theta_e^{WSO} \tag{7}$$

Hence, founders that will face CNC litigation form a spinout only when their human capital is above a certain human capital threshold ("entry threshold"), θ_e^{WSO}. This threshold is increasing in the extent of damage caused to the parent (λ), and is always greater than the non-WSO entry threshold, θ_e^{NWSO}. The effect of enforceability is ambiguous because $\pi^*(\rho)$ may increase or decrease in enforceability.[5]

Impact on non-WSOs

Non-WSOs have $\lambda = 0$ and will enter as long as $\theta > \theta_e^{NWSO} \equiv F - \pi^*(\rho)$. Given that θ is distributed uniformly, the non-WSO entry rate is $e_{NWSO} = 1 - \frac{F-\pi^*(\rho)}{\overline{\theta}}$ while the average initial size is $s_{NWSO} = \frac{1}{4}\left[\overline{\theta} + F - \pi^*(\rho)\right]$.[6] If the hiring cost effect dominates, entry falls while average initial size increases with enforceability, and if the investment protection effect dominates, then entry increases while average initial size falls with enforceability.

Impact on WSOs

The entry decisions of a WSO with quality θ can be described as follows: In this case, $\theta < \theta_L$. Therefore, the WSO will enter if $\theta \geq \theta_e^{NWSO}$, since the parent firm will not litigate. If $\theta \geq \theta_L$, then the

[5] Note that the results are not affected if we use C_{max} or C_{min} as the settlement fee. Using C_{max} is equivalent to replacing F in equation (7) with $F + L$ and setting λ=1, while using C_{max} is equivalent to replacing F with $F - L$.

[6] Recall that a founder of quality θ has size and per-period profits of $\frac{\theta}{2}$.

parent litigates and the WSO enters only if $\theta \geq \theta_e^{WSO}$. Below we describe how increases in enforceability affect entry and size depending on the location of the three thresholds $\theta_e^{NWSO}, \theta_e^{WSO}, \theta_L$.

Case A: $\theta_L \geq \theta_e^{WSO}$ (that is, $\rho \leq \frac{L}{\lambda(L+F)}$)

In this case, the litigation threshold is higher than the WSO entry threshold. Hence, all WSO and NWSO founders enter identically and there is no difference between WSO and non-WSO outcomes.

Case B: $\theta_e^{WSO} > \theta_L > \theta_e^{NWSO}$ (that is, $\frac{L}{\lambda(L+F)} < \rho < \frac{L}{\lambda F}$)

In this case, founders of WSOs with $\theta \in [\theta_e^{NWSO}, \theta_L]$ and with $\theta \in [\theta_e^{WSO}, \overline{\theta}]$ will enter, but WSO founders with $\theta \in (\theta_L, \theta_e^{WSO})$ will be dissuaded from entering. That is, low skill WSO founders will enter because they are not affected by litigation and the highest skill founders will find it attractive to enter despite litigation. However, those in the middle of the human capital distribution will be deterred from entering.

In this region, as enforceability increases, WSO entry relative to non-WSOs falls unambiguously with ρ. It can be shown that the net entry rate is $\Delta e_{WSO} = e_{WSO} - e_{NWSO} = \frac{1}{\overline{\theta}}\left[\frac{L}{\rho\lambda} - \frac{F}{1-\rho\lambda}\right]$. Average WSO initial size relative to non-WSOs may increase or decrease depending on where $\theta_L(\rho)$ and $\theta_e^{WSO}(\rho)$ intersect.[7]

Case C: $\theta_L \leq \theta_e^{NWSO}$ (that is, $\rho \geq \frac{L}{\lambda F}$)

The litigation threshold is lower than the entry threshold for non-WSOs. Then all WSOs face litigation and enter if $\theta \geq \theta_e^{WSO}$. In this region, as enforceability increases, WSO entry relative to non-WSOs falls unambiguously with ρ. It can be shown that the net entry rate is $\Delta e_{WSO} = e_{WSO} - e_{NWSO} = \frac{F}{\overline{\theta}}\left[-\frac{\rho\lambda}{1-\rho\lambda}\right]$ (**Hypothesis 1**). Similarly, average initial size is unambiguously increasing in ρ, with $\Delta s_{WSO} = s_{WSO} - s_{NWSO} = \frac{F}{4}\frac{\rho\lambda}{1-\rho\lambda}$ (**Hypothesis 2a-2c**).

Case C is likely to cover most of $\rho \in [0, 1]$ because F is likely to be much larger than L, and WSOs are likely to have λ near 1. Specifically, both $\frac{L}{\lambda(L+F)}$ and $\frac{L}{\lambda F}$ are likely to be small, and Case B is likely to be even smaller, since the length of that region is only $\frac{L}{F}$ of the length of the region for Case A. See Figures A3a and A3b in the Online Appendix for an illustration of these regions along with how entry rate and average initial size changes with ρ for different values of L/F.

Later-life size. The difference between the WSO and NWSO size after the first period is identical to the difference in the first period since both types of firms face the same effects in the second period (**Hypothesis 4a**). This also means that once we condition for initial size, there will be no difference between the WSO and NWSO size after the first period (**Hypothesis 4b**).

[7] If they intersect below $\frac{1}{4}\left[\overline{\theta} + F - \pi^*(\rho)\right]$ (the mean size of non-WSOs), then relative initial size is unambiguously increasing throughout $\rho \in [\frac{L}{\lambda(L+F)}, \frac{L}{\lambda F}]$. If they intersect above that level, then there is always some part of $[\frac{L}{\lambda(L+F)}, \frac{L}{\lambda F}]$ where relative initial size is unambiguously decreasing. The size of that region may be small or large depending on the rate of change of $\theta_L(\rho)$ and $\theta_e^{WSO}(\rho)$ w.r.t. ρ. If the former changes more rapidly for $\rho \in [\frac{L}{\lambda(L+F)}, \frac{L}{\lambda F}]$, then the region will be smaller.