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Do non-competition agreements lead firms to pursue path-breaking inventions?

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Abstract

Non-competition agreements are contracts signed by employees and firms that prohibit employees from joining or forming a rival company after splitting from the firm. Stricter enforcement of such contracts may induce firms to undertake riskier R&D projects, leading to technological breakthroughs or dead ends. Specifically, non-competition agreements reduce the risk that the firm loses the fruits of inventive activity by its employees, such that when the enforcement of non-compete covenants is stricter, firms grant corporate inventors more freedom to explore risky but high-potential research paths. This study uses data about U.S. patent applications between 1990 and 2000 to identify the impact of non-competition agreements and considers both cross-state and longitudinal variation in the enforcement of non-compete clauses. The empirical findings are mainly consistent with theory and show that in states with stricter enforcement, companies are more likely to undertake risky and potentially path-breaking R&D projects than in states that do not enforce non-compete agreements as strictly.

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Do non-competition agreements lead firms to pursue risky R&D projects?

ABSTRACT

This study investigates the impact of non-competition agreements on the type of R&D activity undertaken by companies. Non-competition agreements, by reducing outbound mobility and knowledge leakages to competitors, make high-risk R&D projects relatively more valuable than low-risk ones. Thus, they induce companies to choose riskier R&D projects, such that corporate inventions are more likely to lie in the tails of the inventions' value distribution (as breakthroughs or failures), and to be in novel technological areas. This study uses data about U.S. patent applications between 1990 and 2000 and considers both cross-state and longitudinal variation in the enforcement of non-compete clauses. Results indicate that in states with stricter enforcement, companies undertake riskier R&D paths than in states that do not enforce non-compete agreements as strictly.

Keywords: Appropriability, R&D strategy, innovation, technological breakthroughs, non-compete agreements.

INTRODUCTION

Any competitive advantage created by introducing an innovation would be transitory if proprietary knowledge could easily spill over to competitors. Companies can use different mechanisms to safeguard their idiosyncratic technological competences (Levin et al. 1987), including the protections granted by patent or copyright laws. Yet, some technological know-how sticks to individual employees, and the most effective way organizations can retain such knowledge is by preventing researchers from joining a rival, such as through non-competition contractual agreements (hereafter, non-competes). Non-competes are in fact widely used in contracts of scientists, engineers and technology executives. In United States, almost 70% of entrepreneurs receiving venture capital financing are required to sign non-competition clauses with the venture capital firms (Kaplan and Stromberg 2003), whereas about 80% of newly hired IT professionals are asked to sign a non-compete contract (Holley 1998).

Given the importance and diffusion of non-competes, a natural question is how they affect firms' R&D strategy. One might expect that a stronger enforcement of non-competes should induce companies to invest more in R&D. However, previous research does not find any significant relationship between the enforcement of non-competes and the amount of company R&D expenditure (Garmaise 2009). Extant work does not focus on the *type* of R&D though. Designing an R&D strategy actually means choosing not just how much to invest but also *how* to invest, and a crucial choice concerns the degree of riskiness of the R&D activity (Cabral 2003). In this respect, anecdotal evidence suggests that non-competes may in fact create incentives to undertake risky but high-potential R&D paths. Brian Halligan, CEO of Hubspot – one of the most successful software companies in Boston –, notes that his company is "super entrepreneurial" and persistently develops novel technological solutions precisely because the non-competes that employees sign "encourage new thought about the way Hubspot does business"[1]. Consistent with this evidence, I propose that in regions in which such non-competes are enforced more strictly, firms likely undertake riskier R&D paths, implying a higher likelihood of achieving extremely valuable inventions (i.e., technological breakthroughs), but also a higher probability of failure. I also propose that non-compete enforcement affects the direction of research efforts, inducing firms to undertake projects in new technological areas. I predict these effects because non-compete contracts reduce outbound mobility and knowledge leakages to competitors. As a result, a stronger enforcement of non-competes makes high-risk R&D projects relatively more valuable than low-risk ones.

To test this prediction, I gather data about U.S. patents applications by public companies during 1990–2000. I identify the impact of non-competes by considering both cross-state and longitudinal variation in U.S. non-compete enforcement. The findings indicate that in states with stricter non-compete enforcement companies choose riskier R&D paths, such that corporate inventions are more likely to lie in the tails of the inventions' value distribution (as breakthroughs or failures), and to be in novel technological areas.

The contribution of this study to the strategy literature is threefold. First, the study shows how the strategy and competitive advantage of firms depend on the institutional environment in which they are embedded (Ingram and Silverman 2002; Furman 2003). In particular this work theorizes and provides evidence that in regions where non-competes are enforced more strictly, companies tend to choose riskier R&D paths, eventually leading to technological breakthroughs. Second, within the strategic entrepreneurship literature, this study points out that non-competes, while reducing the formation of new companies (e.g., Samila and Sorenson 2010), might stimulate corporate entrepreneurship, as they induce managers to explore novel and potentially path-breaking technological solutions. From a

[1] http://bostinnovation.com/2010/03/08/are-non-compete-contracts-helping-hot-companies-like-hubspot-become-grand-slams-for-boston/.

policy perspective, this implies that non-competition agreements may create, at the regional level, a trade-off between entrepreneurship and intrapreneurship. Finally, the study extends the growing stream of research that examines the determinants of inventive breakthroughs (e.g., Ahuja and Lampert 2001; Phene, Fladmoe-Lindquist and Marsch 2005; Fleming and Singh 2010). The interest in breakthroughs is mainly motivated by the skewed distribution of inventions' value, with a small minority of inventions accounting for a disproportionate share of value (Gambardella, Harhoff and Verspagen 2008). In this context, non-competes enhance the likelihood of any inventive outcome being extremely profitable.

BACKGROUND AND THEORY DEVELOPMENT

For firms competing in knowledge intensive industries a key strategic problem involves capturing the value created by investing in R&D and limiting unintended knowledge leakages to rivals (e.g. Shaver and Flyer 2000; Agarwal, Ganco and Ziedonis 2009). If proprietary knowledge cannot be protected at all, innovative firms suffer a constant disadvantage, because competitors simply imitate their knowledge without incurring the costs of creating it. However, companies use different mechanisms to limit unintended knowledge spillovers to rivals (Levin et al. 1987), including the protections granted by patent or copyright laws. Tacit knowledge also can be protected by embedding it in organizational practices and routines (Nelson and Winter 1982). Yet some knowledge may be inherent to individual members of the organization, in which case it is difficult to share throughout the organization. The most effective way firms can retain such knowledge is by restricting the possibility of employees to leave the company, such as through non-competes. These contracts, signed by employees and firms, forbid employees to join a competitor or form a new company, usually for a specified period of time or geographic location. The functioning and effectiveness of non-competes are well exemplified by the case of Kai-Fu-Lee, a renowned computer scientist and technology executive. In 2005 he left Microsoft for joining Google. In order to prevent knowledge leakages to its competitor's advantage, Microsoft immediately went to a court in Washington, to enforce the non-compete contract signed with Kai-Fu-Lee. The court eventually issued a restraining order, forbidding Dr. Lee to work on projects for Google similar to those he performed for Microsoft. Some systematic empirical evidence points in the same direction as the previous example: non-competes significantly limit outbound mobility of inventors and executives to competitors (Germaise 2009; Marx, Strumsky and Fleming 2009).

The historical origins of modern non-competes stem from England. In 1711, a court allowed partial restraints on workers' mobility in certain circumstances. This "partial restraint logic" seemed spread in the United States in the nineteenth century; by the start of the twentieth century, U.S. courts generally considered non-competes enforceable, if they were within the boundaries of "reasonableness standards." Although most U.S. states thus allow some form of non-competition contracts, their enforcement varies substantially. For example, in California non-compete agreements are not enforceable, and in Texas they are valid only if employees receive some ancillary compensation for entering into them. The geographical reach and duration of a non-compete also vary in different jurisdictions. In most states, a non-compete contract cannot specify a time restriction greater than two years, but Pennsylvania courts routinely accept three-year non-compete covenants.

The social desiderability of non-competes is on debate. On one hand, Gilson (1999) argues that Silicon Valley's entrepreneurial growth mainly reflects California's proscription of non-competes. Stuart and Sorenson (2003) confirm that liquidity events, such as acquisitions or initial public offerings, increase the number of new firms, especially in areas where non-compete covenants are forbidden. Along similar lines, Samila and Sorenson (2010) show that the positive impact of the supply of venture capital on both the number of new firms, inventions and employment is significantly greater in regions that do not enforce non-compete agreements strictly. On the other hand, the knowledge protection provided by non-competes may be essential in the emergent stages of a new industry for stimulating both entrepreneurship and innovation (Franco and Mitchell 2008).

The macro implications of non-competes for regional growth and performance have been extensively dealt with the literature. However, their implications for companies' R&D strategies have been largely neglected, despite the fact that firms use widely non-compete clauses in contracts of scientists, engineers and technology executives. In United States, almost 70% of entrepreneurs receiving venture capital financing are required to sign non-competition clauses with the venture capital firms (Kaplan and Stromberg 2003), while about 80% of newly hired IT professionals are asked to sign a non-compete contract (Holley 1998).

How do non-competes affect company R&D strategy? A straightforward economic argument would suggest that companies should invest more in R&D if non-competes are enforced more strictly, as non-competes increase the ability to capture the value created by innovating. Yet, it does not exist any significant relationship between non-compete enforcement and the amount of company R&D expenditure (Garmaise 2009). This finding is less surprising than it might initially seem if we consider the conflicting results produced by the literature about the impact of Intellectual Property Rights (IPR) on company expenditure in R&D (e.g., Ginarte and Park 1997; Sakakibara and Branstetter 2001; Kanwar and Emerson 2003; Qian 2007). While Kanwar and Emerson (2003) show that firms in countries with stronger IPR tend to invest more in R&D, Sakakibara and Branstetter (2001), does not find any significant relation between IPR rights and corporate R&D expenditure.

Rather than focusing on the sheer amount of R&D as the relevant outcome (an aspect already treated in the literature), I attempt to understand how non-competes affect the *type* of R&D activity pursued by companies. As Cabral (2003) convincingly argue, designing an

R&D strategy actually means choosing not just how much to invest but also *how* to invest. In particular, companies will cope with potential knowledge leakages by strategically choosing the R&D project characteristics in order to avoid or minimize the costs due to knowledge outflows to rivals. For instance, Zhao (2006) and Alcacer and Zhao (2007) finds that multi-location companies facing appropriability risks usually choose projects characterized by strong linkages with other corporate proprietary knowledge, because high interdependence between the focal project and firms' organizational expertise create knowledge that is hard to replicate by competitors. Instead of looking at the degree of internal linkages, similarly to Cabral (2003) I consider another R&D characteristic firms may choose: the degree of riskiness.

I argue that non-competes might stimulate the experimentation with riskier R&D paths because the profit decrease due to outbound mobility is relatively higher for riskier projects. Hence non-competes, by reducing outbound mobility, make high-risk R&D projects relatively more valuable than low-risk ones. To make the point clear, consider the following situation. There are two R&D projects with the same initial expected value. The first R&D project *A* is safe and produces a positive profit a with probability 1. The second R&D project *B* is risky and it will generate a positive profit b with a probability of p, but it will produce an economic loss L with a probability of $(1 - p)$. In principle a risk-neutral firm is indifferent between projects A and B, as they have the same expected value a, that is $a=pb-(1-p)L$. That preference changes when non-competes are not enforceable. In this scenario, once a project turns out to be profitable, researchers working on it may leave the firm with probability λ, in which case the company loses a share γ of profits. Since profits decrease but losses do not, the expected value of the risky project *B*, $(1-\lambda\gamma)pb-(1-p)L$, falls lower the expected value of the safe project *A*, $(1-\lambda\gamma)a$. It is easy to show that such reasoning can be generalized to projects with different degree of riskiness (see Appendix 1). To sum up, when companies cannot enforce non-competes, a high-risk project becomes less valuable than a low-risk one with the same initial expected value. In other words, passing from a situation where non-competes are forbidden to another one where non-competes can be enforced, high-risk R&D projects become relatively more valuable than low-risk ones. Using again the previous example, without non-competes a risk-neutral company definitely prefers the safe project A; with the possibility of enforcing non-competes it would be indifferent between the safe project A and the risky project B. Hence, non-compete enforcement induces firms to invest more resources in riskier R&D projects.

High-risk projects are more likely to generate technological breakthroughs compared with low-risk ones. Greater riskiness in the outcome distribution appears preferable in the quest for extremely valuable outcomes (March 1991; Fleming 2007), because more variance fattens the right-hand tail of inventions' value distribution, increasing the likelihood of a breakthrough. Yet greater riskiness implies an increase in the mass of both tails of the distribution. That is, a greater probability of breakthrough outliers will be accompanied by a greater probability of dead-ends and failures. Therefore, I formulate the following hypotheses:

H1: The stricter the enforcement of non-competes, the greater the likelihood that corporate inventions are breakthroughs.

H2: The stricter the enforcement of non-competes, the greater the likelihood that corporate inventions are failures.

The enforcement of non-competes also affects the direction of research endeavors. Firms can choose whether to undertake projects closely related to their preexisting knowledge base, or to pursue projects distant from the current technological know-how. This choice has implications for the distribution of rewards to the inventive activity, as the exploration of novel technological competences is usually riskier than the exploitation of existing know-how. As March puts, it "compared to returns from exploitation, returns from exploration are systematically less certain" (March 1991, p.73). Hence, to the extent that non-competes create incentive to undertake riskier R&D paths, they should also have an impact on the direction of research efforts, inducing firms to undertake projects in new technological areas. Thus I hypothesize that:

> *H3: The stricter the enforcement of non-competes, the greater the likelihood that corporate inventions occur in new technological areas.*

Concluding, a stronger enforcement of non-competes should be reflected in undertaking riskier R&D projects, i.e. projects whose outcome has a higher probability of *both* being a breakthrough and a failure. Moreover, a stronger non-compete enforcement should also lead companies to produce inventions in technological domains distant from the current technological know-how of the organization.

METHODS

Sample and data

To investigate how the enforcement of non-competes affects firms' inventive outcomes, I gathered a data set that includes all granted patents whose application was filed in the United States by a public firm during 1990–2000. In particular, I focused on patented inventions whose first inventor resides in a U.S. state; similar to prior work (e.g., Thompson 2005), I assigned each patent to that state of residence of the first inventor. Information about patents came from the most recent update of the National Bureau of Economic Research (NBER) patent database (www.nber.org/patents), which makes available the citations for all U.S. patents granted from 1976 to 2006. To ensure I could assign each patent to an organization, I considered only public firms, for which I could identify subsidiaries relatively easily over time. I used the concordance file provided by Bessen (2009) to connect the assignee identification number of the NBER patent data set to the Compustat GVKEY identification number. These connections reflected the firms and subsidiaries identified in the "Who Owns Whom?" database. Ownership may change through mergers, acquisitions, or spinoffs, and when an organization is acquired/merged/spun-off, its patents likely go to the new owner. These changes have been tracked using data on the mergers and acquisitions of public companies reported in the SDC database. In total, I gathered 337,054 U.S. patents, whose first inventor resides in the United States, applied for during 1990–2000 and eventually granted to public companies, which therefore represented the sample used in the empirical analysis.

The 1990-2000 time period selection was mainly determined by practical reasons. First, the enforcement index elaborated for U.S. states by Garmaise (2009), which I used in my empirical analysis, also refers to this time period[2]. Moreover, choosing this relatively short window of time enabled me to estimate the effects of a change in non-compete regulation while keeping other possible state-level changes constant. I ended the data collection with 2000, to ensure sufficient future time to measure the patented inventions' value, according to the number of forward citations received. Limiting the analysis to the 1990-2000 time period has some shortcomings, as two important longitudinal changes in non-compete enforcement (occurred in Michigan in 1985 and in Louisiana in 2004) have been excluded from the analysis. However, the alternative solution of using a longer time window was even worse for at least two reasons. First, using data before 1985 would mean the impossibility to assign any patent to the correct organization, because SDC, used in order identify the ownership structure of public companies over time, is reliable only from 1986[3]. Second, the last version of the NBER patent database includes information about all patents

[2] To be precise, the index elaborated by Garmaise (2009) refers to 1992-2004. However, no changes in non-compete enforcement occurred in 1990-1991. According to Samila and Sorenson (2010, p. 427), "only four states have experienced meaningful changes over the last 30 years", and none of these states during 1990-1991.
[3] This information is directly reported by Thompson Reuters.

granted until December 2006. Thus it is impossible to get a reliable measure of the number of forward citations received by patents applied after 2004.

Measures

The empirical analysis pertains to the invention level. Therefore, I estimated the impact of the strength of non-compete enforcement in a certain state on the likelihood that an invention produced by an inventor residing in that state is a breakthrough (H1), a failure (H2), or in a new technological area (H3).

Dependent variables

Breakthroughs are extremely valuable inventions, so I measured *inventive breakthroughs* according to the number of forward citations received by a patent since the year of its application. The number of citations correlates with several measures of technological and economic value, including consumer surplus generated (Trajtenberg 1990), expert evaluations of patent value (Albert et al. 1991), patent renewal rates (Harhoff et al. 1999), contribution to an organization's market value (Hall, Jaffe, and Trajtenberg 2005), and inventors' assessments of economic value (Gambardella et al. 2008).

Similar to previous studies (Phene, Fladmoe-Lindquist and Marsch 2005; Fleming and Singh 2010), I employed a dichotomous variable that takes a value of 1 if the patent is in the top 5% in terms of forward citations received, with respect to all patents applied for in the same year (by application date) and in the same technological class (i.e., four-digit IPC classes). The variable equals 0 otherwise.

In line with Fleming and Singh (2010), I measured a *failure* as an invention that receives 0 forward citations. Therefore, I used a dummy variable that takes the value of 1 if a patent receives no citations and 0 otherwise.

Finally, I coded *invention in new technological areas* for a company as a 1 if the patented invention fell in a primary patent class different from the primary classes of patents applied for by that organization in the previous five years, and 0 otherwise (Gilsing et al. 2008). The patent class referred to the first four digits of the International Patent Classification (IPC) system. Consistent with prior research (Argote, Beckman, and Epple1990), I considered a five-year window, to acknowledge the rate of organizational forgetting.

Independent variable

I took advantage of an index that measures the *enforcement of non-compete covenants* in U.S. states, as elaborated by Garmaise (2009) and based on 12 questions proposed by Malsberger (2004). This index assigns one point for each dimension for which the jurisdiction's enforcement exceeds a given threshold, so total scores range from 0 to 12. A complete list of questions, thresholds, and state totals appears in Appendix 2. Although the laws governing the enforcement of non-competition agreements are largely static over time, two states (Texas and Florida) exhibited significant shifts in the enforcement of these covenants during the sample period. In June 1994, in *Light v. Centel Cellular Co.*, the Texas Supreme Court issued a new set of requirements for enforcement of non-competition agreements. Therefore, whereas the non-competition enforcement index score for Texas was 5 before 1994, it fell to 3 after the decision. The Florida law change instead resulted from actions by the state legislature, which in May 1996 replaced the state's existing law regulating non-competes. As a result of this change, its enforcement index increased from 7 to 9.

Control variables

At the patent level, more recent patents are less likely to have received forward citations, so to control for this and other temporal effects, I included a dummy variable for each *calendar application year*. At the firm level, I took into account the *number of employees* and the *size* *of the firm's knowledge base* [4], measured as the number of patents granted to the firm, applied for in the five-year window previous to the year of observation. Both variables aim at capturing the impact of firm scale, which is clearly important for R&D activity though findings about the sign of this effect remain controversial (for a survey, see Ahuja, Lampert and Tandon 2008). To address the diversity of firm technological knowledge, which may prevent routine thinking and increase the chances of a breakthrough (Ahuja and Lampert 2001), I controlled for the *specialization of the firm's knowledge base*, according to the indicator $\mathrm{specialization}_{it} = \sum_k \left(\frac{n_{kt}}{n_t}\right)^2$, where n_t is the total number of patents applied for by the firm in the five years preceding year t, and n_{kt} is the number of patents in the IPC (four-digits) technological class k, applied in the same period of time. The indicator measures the concentration of a firm's knowledge stock within some technology classes in the five years before year t.

At the state level, I considered the *number of Technological Competitors* in the state, that is the number of firms patenting in the same application year, state and technological category of the focal patent[5]. Such variable may affect company R&D strategy and performance, as conducting R&D in an agglomerated region means both benefiting from knowledge spillovers (e.g., Audretsch and Feldman 1996), and contributing to them (Shaver and Flyer 2000). To control for other time-invariant characteristics that might correlate with the enforcement of non-competes and affect the inventive performance of companies (e.g., presence of universities, cultural factors), I included a *state dummy variable*.

Table 1 summarizes the operationalization of the variables for the analysis.

[4] I did not include firm's R&D expenditure for two reasons. First, its correlation with the size of firm's knowledge base is higher than 0.8. Second, Garmaise (2009) shows that non-competes do not influence R&D expenditures. Thus excluding this variable from the empirical analysis should not create a bias in the estimated impact of non-compete enforcement.

[5] I used the categorization provided by Hall, Jaffe and Trajtenberg (2001) that classify the patents into six macro-technological classes: Chemicals (excluding drugs), Computer and Communications, Drugs and Medical, Electrical and Electronics, Mechanicals, and Others.

Insert table 1 about here

Empirical strategy

To identify the impact of non-compete agreements on inventive outcomes, I adopted two methods. First, I used the longitudinal and cross-sectional variation in the non-compete enforcement index elaborated by Garmaise (2009) to assess the economic and statistical significance of an increase in the enforceability of non-competition agreements. Second, I exploited the quasi-natural experiments provided by Texas and Florida and adopted a difference-in-differences regression method, such that I separately estimated the impacts of an *increase* of non-compete enforcement (Florida in 1996) and a *decrease* of such enforcement (Texas in 1994).

Variation in non-compete enforcement index

I first only considered longitudinal variations in non-compete enforcement, by including as controls state dummies. With H1, I posited that non-compete enforcement should increase the chance of any invention being a breakthrough. Because this dependent variable is binary, I used a logistic regression, with the assumption that there is a latent variable $y* \in (-\infty, +\infty)$. I did not observe $y*$ directly but can observe a binary outcome y, such that $y = \mathbf{1}(y* = x\beta + u > 0)$, where $\mathbf{1}$ is an indicator function that takes the value of 1 if the condition within parenthesis is satisfied, x is a vector of variables that influence $y*$ linearly, β is a vector of parameters, and u represent a logistically distributed stochastic component. Using a logistic model, I estimated the impact of the enforcement of non-competes on the probability that a certain invention *j*, generated by company *i* in state *s* at time *t*, will be path-breaking. Thus,

$$\text{Prob}\ (\text{Breakthrough}_{\text{jist}} = 1|\text{X}) = \text{Pr}\ (\alpha(\text{Enforcement}_{\text{st}}) + \beta\text{Z} + \text{e}_{\text{jist}} > 0). \quad (1)$$

where X is the vector of all covariates; Enforcement_{st} the strength of non-compete enforcement in a certain state *s* at time *t*; Z is the vector of control variables, including state fixed effects; and e_{jist} is the stochastic component. If H1 is supported, α should be greater than 0. Because the use of micro-data to estimate the impact of a variable that affects a group of observations may produce spurious predictions of the statistical significance of the variable of interest, I followed Moulton (1989) and clustered the errors at the state level to allow for intra-group correlations in the disturbances of observations that refer to the same state.

I also have predicted that non-compete enforcement increases the probability of an invention being a failure (H2) and in a new technological area (H3). The dependent variables again are dichotomous, so I used a logit model, with standard errors clustered at the state level, to estimate the predicted impacts:

$$\text{Prob}\ (\text{Failure}_{\text{jist}} = 1|\text{X}) = \ \text{Pr}\ (\alpha(\text{Enforcement}_{\text{st}}) + \ \beta\text{Z} + \text{e}_{\text{jist}} > 0), \tag{2}$$

and

$$\text{Prob}(\text{NewArea}_{\text{jist}} = 1|\text{X}) = \ \text{Pr}\ (\alpha(\text{Enforcement}_{\text{st}}) + \ \beta\text{Z} + \text{e}_{\text{jist}} > \tag{3}$$

In both Equations (2) and (3), α is expected to be positive. To take unobserved firm heterogeneity into account, as a robustness check, I also estimated equations (1), (2) and (3) using a linear probability model with firm fixed effects[6]. As Horrace and Oaxaca (2006) show, the linear probability model gives unbiased results if the predicted probabilities lie between zero and one.

The problem with using just the longitudinal variations in non-compete enforcement is that only few changes are taken into account. Nevertheless, using the non-compete enforcement index without state fixed effects would make it impossible to distinguish the effect of non-compete enforcement from the impact of other time-invariant factors at the state-level. Thus, similarly to Garmaise (2009) and Samila and Sorenson (2010) I adopted an

[6] Computing constraints, due to the large number of observations, drove my choice of a linear probability model instead of a conditional logit model.

alternative empirical strategy. More in details, I included state fixed effects and I considered the interaction between non-compete enforcement and the extent of intra-state technological competition. The rationale of such interaction is quite straightforward. Non-competes induce companies to undertake risky R&D project by reducing outbound mobility and knowledge outflows to competitors. Therefore the effect of non-compete enforcement should be even stronger when the number of competitors, and thus the possibility that employees leave the firm, increases. Since "during the sample period, it was considerably more difficult to enforce a noncompetition agreement across state boundaries than within a state" (Garmaise 2009, p. 17), only competitors within the state of the focal patent are taken into account. Thus I estimated the following regressions, where $\text{NumberTechCompetitors}_{st}$ represent the number of technological competitors in the state *s* at time *t:*

$$\text{Prob}\ (\text{Breakthrough}_{jist} = 1|\text{X}) = \ \text{Pr}\ (\alpha(\text{Enforcement}_{st} * \text{NumberTechCompetitors}_{st}) + \ \beta\text{Z} + \text{e}_{jist} > 0) \quad (4)$$

$$\text{Prob}(\text{Failure}_{jist} = 1|\text{X}) = \ \text{Pr}\ (\alpha(\text{Enforcement}_{st} * \text{NumberTechCompetitors}_{st}) + \ \beta\text{Z} + \text{e}_{jist} > 0) \quad (5)$$

$$\text{Prob}\ (\text{NewArea}_{jist} = 1|\text{X}) = \ \text{Pr}\ (\alpha(\text{Enforcement}_{st} * \text{NumberTechCompetitors}_{st}) + \ \beta\text{Z} + \text{e}_{jist} > 0). \quad (6)$$

Garmaise (2009) and Samila and Sorenson (2010) assume that, once the interaction term is included in the regression, non-compete enforcement does not have any direct impact on the outcome variables: Thus they exclude non-compete enforcement as a stand-alone variable from the regression. Since this assumption might not hold, I test that α is greater than zero in the previous equations (4), (5) and (6), both including and excluding non-compete enforcement from the vector of controls Z. Moreover, as a robustness check, I also used linear probability models with firm fixed effects.

Difference-in-differences approach

With a difference-in-differences methodology, I exploited the quasi-natural experiments provided by Texas and Florida to estimate separately the impact of two opposite "treatments": a decrease of non-compete enforcement in Texas in 1994 and an enforcement increase in Florida in 1996. To the extent that changes in non-compete regulation are neither influenced nor predicted by individuals, such treatments can be considered truly exogenous. For Texas, this consideration is likely true, because the change in non-compete enforcement was generated by a Texas Supreme Court decision. It is therefore reasonable that companies were not aware of the decision the Court was going to make. The change in Florida, in contrast, resulted from the actions of the state legislature, so companies probably were aware of the possible change, because it had been widely debated (Marx, Strumsky and Fleming 2009). Yet even in this case, endogeneity did not seem to be an issue. If managers expected the change in regulation, the R&D organization could have started changing its practices prior to the approval of the new law, and the coefficient would underestimate the impact of the change in enforcement. Therefore, I would perform a conservative test.

Using the difference-in-differences technique, I can estimate the effect of the treatment on an outcome variable by comparing what happened to the treatment group before and after the treatment, to what happened to a group that was *not* subject to the treatment (control group), again before and after the treatment. In principle, it might seem sufficient to investigate the treated group alone to deduce the effect of the treatment. Nevertheless, without the counterfactual (i.e. what would happened to the treated group *without* the treatment) the impact of the treatment may be confounded with the impact of other factors that affect the outcome variable at the same time. A control group enabled me to take these other factors into account, with the assumption that they affect the treatment and control groups equally (Wooldridge 2002).

Therefore, the inventions generated in Texas and Florida represent the treated group, whereas inventions in other U.S. states constitute the control group. To estimate the effect of decreased non-compete enforcement in Texas in 1994, I excluded the Florida observations and estimated the following logit models, in which the dependent variable is the probability of invention *i* generated by firm *j*, in a certain state *s* at time *t*, being in a new technological area (Equation 7), a breakthrough (Equation 8), or a failure (Equation 9):

$$\text{Prob}(\text{Breakthrough}_{\ ijst} = 1|\text{X}) = \text{Prob}(\alpha(\text{TX} * \text{Post1994}) + \beta\text{TX} + \gamma\text{Post1994} + \delta\text{Z} + \text{e}_{\text{jist}} > 0) \qquad (7)$$

$$\text{Prob}(\text{Failure}_{\ ijst} = 1|\text{X}) = \text{Prob}(\alpha(\text{TX} * \text{Post1994}) + \beta\text{TX} + \gamma\text{Post1994} + \delta\text{Z} + \text{e}_{\text{jist}} > 0). \qquad (8)$$

$$\text{Prob}(\text{NewArea}_{\text{ijst}} = 1|\text{X}) = \text{Prob}(\alpha(\text{TX} * \text{Post1994}) + \beta\text{TX} + \gamma\text{Post1994} + \delta\text{Z} + \text{e}_{\text{jist}} > 0)\ . \qquad (9)$$

In these equations, (TX*Post1994) is the treatment, in that TX is a dummy variable that takes the value of 1 for inventions in Texas and 0 otherwise, and Post1994 is a dummy that takes the value of 1 for inventions applied for in the period after 1994 and 0 otherwise. Furthermore, Z is the vector of controls. The α estimator involves the following interpretation: Suppose that Equation (7) were a linear, rather than logistic, regression. Let $\overline{\text{PrBr}}^{\text{Texas}}_{\text{Post1994}}$ denote the sample average probability that inventions generated in Texas after 1994 were breakthrough inventions. Let $\overline{\text{PrBr}}^{\text{Other}}_{\text{Post1994}}$ represent the same probability for inventions generated in the rest of the United States. Finally, let $\overline{\text{PrBr}}^{\text{Texas}}_{\text{Pre1994}}$ denote the average probability that inventions generated in Texas before 1994 were path breaking and $\overline{\text{PrBr}}^{\text{Other}}_{\text{Pre1994}}$t is that value for other states. Then:

$$\alpha = \left(\overline{\text{PrBr}}^{\text{Texas}}_{\text{Post1994}} - \overline{\text{PrBr}}^{\text{Texas}}_{\text{Pre1994}}\right) - \left(\overline{\text{PrBr}}^{\text{Other}}_{\text{Post1994}} - \overline{\text{Pb}}^{\text{Other}}_{\text{Pre1994}}\right). \qquad (10)$$

Therefore, if Equation (8) were a linear regression, α would estimate how much the probability of breakthrough inventions in Texas changed after the court decision to decrease non-compete enforcement, compared with the equivalent change in the rest of the U.S. states. The problem is that the model represented by Equation (7) is logistic, and the parameter α is a coefficient of the interaction term between the group (TX) and time (Post1994) dummies. Ai and Norton (2003) suggest that in nonlinear models, the coefficient of the interaction term is not a meaningful indicator of the real impact of the interaction variable. However, Puhani (2008) proves that in a nonlinear difference-in-differences model with a strictly monotonic transformation function of a linear index (e.g., probit, logit, or tobit), the treatment effect is 0 if and only if the coefficient of the interaction term between the group and time dummy is 0. Moreover, the sign of the treatment effect is equal to the sign of the interaction term. Therefore, even if in Equation (7) α does not represent the impact of the treatment precisely, it is appropriate to focus on it to verify the sign of the treatment effect. In Texas, the treatment involves a reduction of non-compete enforcement, so I expect α to be negative in Equations (7), (8) and (9), consistent with H1–H3.

For Florida, which experienced increasing enforcement in 1996, I excluded observations referring to Texas and estimated the following regressions:

$$\mathrm{Prob}(\mathrm{Breakthrough}_{ijst} = 1|X) = \mathrm{Prob}(\alpha(FL * Post1996) + \beta FL + \gamma Post1996 + \delta Z + e_{jist} > 0) \quad (11)$$

$$\mathrm{Prob}(\mathrm{Failure}_{ijst} = 1|X) = \mathrm{Prob}(\alpha(FL * Post1996) + \beta FL + \gamma Post1996 + \delta Z + e_{jist} > 0). \quad (12)$$

$$\mathrm{Prob}(\mathrm{NewArea}_{ijst} = 1|X) = \mathrm{Prob}(\alpha(FL * Post1996) + \beta FL + \gamma Post1996 + \delta Z + e_{jist} > 0). \quad (13)$$

In these equations, FL is a dummy that takes the value of 1 for inventions in Florida and 0 otherwise, and Post1996 is a dummy that takes the value of 1 for inventions applied for in the period after 1996 and 0 otherwise. For Florida, the treatment entails an increase in non-compete enforcement, so I expect α to be positive in Equations (11), (12), and (13).

One potential pitfall of difference-in-differences estimation is inconsistency in standard errors, due to serial correlation among observations, which may be extremely high if the analysis includes several periods of time. This issue may lead to an indication of spurious statistical significance in the treatment. Therefore, I adopted the strategy suggested by Bertrand, Duflo and Mullainathan. (2004) and clustered the errors at the level of the treatment, that is, the state level.

RESULTS

Descriptive statistics

Tables 2 and 3 contain the descriptive statistics and pairwise correlations among variables. Consistent with prior research (Stuart and Sorenson 2003), I find a negative correlation between the enforcement of non-competes and the degree of technological competition, as measured by log of the number of firms inventing in the same technological class, year and state of the focal patent. The correlation between non-compete enforcement and the probability that an invention is a breakthrough is negative; however, this result may reflect other variables at the state level that correlate negatively with the degree of non-compete enforcement but positively with inventive performance. As a concrete example, California forbids non-competes, but its culture, which promotes knowledge exchanges and risk taking, allows many California companies to produce path-breaking inventions (Saxenian 1994). Ignoring other state-level variables would mistakenly attribute to non-competes a negative impact on the probability of achieving technological breakthroughs.

There is a strong correlation between the size of firms' knowledge stock (log of the number of patents), firm technological diversification and the number of employees. However, potential multicollinearity problems are lessened by the large number of observations in the sample.

Insert tables 2 and 3 about here

Variation in non-compete enforcement index

The results pertaining to H1 and H2 appear in Tables 4 and 5. Specifically, in support of H1, enforcement of non-competes significantly increases the probability that an invention will be a breakthrough according to the logistic model (model a, Table 4). Keeping the covariates at their mean, a one standard deviation (2.151) increase in non-compete enforcement enhances the probability of any invention being a breakthrough from 7% to almost 9%. This increase in non-compete enforcement is similar in magnitude to the actual change in Florida, where enforcement increased from 7 to 9, and in Texas, where enforcement fell from 5 to 3 on the index. A jump in the enforcement index from 0 (minimum) to 9 (maximum in the sample) would raise the probability of a breakthrough from 7% to approximately 14%. The sign of non-compete enforcement remains positive and statistically significant even when controlling for firm fixed effects in the linear probability model (model d, Table 5). It also is interesting to note the results of the models considering the interaction between non-compete enforcement and intra-state technological competition (model b, c, e and f, Table 5): The effect of non-competes enforcement on the likelihood of any invention being path-breaking is even stronger when the number of technological competitors increase.

Also in support of H2, greater non-compete enforcement raises the likelihood that any invention will fail, based on the results of the logistic model (model a, Table 5). A one standard deviation increase in non-compete enforcement raises the probability of failure by almost 2.5%, such that at the sample mean of all variables the probability of an extremely poor outcome increases from almost 8.7% to 11.2%. The linear probability model confirms that non-competes significantly increase the probability of extremely poor outcomes (model d, Table 5). Also the models considering the interaction between non-compete enforcement and the number of technological competitors in the state suggest that non-competes increase the probability of producing worthless inventions, especially in states with many technological competitors (model b, c, e and f, Table 5).

Insert tables 4 & 5 about here

To sum up, results corroborate the idea that non-competes create incentive to invest in riskier R&D projects, i.e. projects that *ex post* turn out to be breakthroughs or failures. However, the ex ante distribution observed by managers when they choose the R&D project type may somehow differs from the ex post distribution of the inventive outcome. Thus I also explore the impact of non-compete on some *ex ante* measure of riskiness, such as the exploration of novel technological area. Accordingly to H3, I find that non-competes increase the explorative nature of corporate inventions. In particular, the logistic model reveals that non-compete enforcement significantly increases the likelihood that any invention occurs in a new technological area for a company (model a, Table 6). When the non-compete enforcement index increases from 0 to 9, the likelihood of an invention appearing in a new technological domain rises about 5.5% (covariates at their mean) to 7.5%. For a more realistic prediction, a one standard deviation increase of non-compete enforceability increases the probability of an invention being in a new domain from 5.5% to about 6% (a 9% relative increase). In the linear probability model controlling for firm fixed effects the association between non-compete enforcement and the explorative nature of an invention remains positive but is not significant (model d, Table 6). However, models considering the interaction between non-compete enforcement and intra-state technological competition, (model b, c, e and f, Table 6), corroborate the hypothesis that non-competes induce firms to undertake project in novel technological areas.

Insert table 6 about here

Difference-in-differences approach

The results from the difference-in-differences estimation provide additional support for my proposed theory. A crucial assumption underlying the difference-in-differences technique is that differences in the outcome variables between the treated and the control group would have remained constant without the treatment. Both visual inspection of trends and a t-test of differences in trends before the treatment indicate that this assumption is viable.

Table 7 contains the results for Texas. Consistent with H1 and H2, the decrease in non-compete enforcement led to a lower likelihood of any invention being path-breaking and a failure. Moreover, in line with H3, when non-compete agreements were enforced less strictly, the probability of any invention being occurring in a novel technological area for a company declined.

For Florida, the results in Table 8 again confirm the predicted outcomes. Specifically, the greater non-compete enforcement after 1996 augmented the likelihood of any invention being path-breaking (H1), a failure (H2), and in a new technological domains (H3).

Insert tables 7 & 8 about here

Robustness checks

The difference-in differences analysis described in the previous section may raise some concerns. First, using all US states as control group may be problematic, as such a control group might not be able to reproduce the counterfactual (that is how the treated state would have evolved in the absence of any change in the enforcement of non-competes). Second, due to the non-linearity of the empirical models, the coefficient of the interaction term does not provide a good estimate of the real impact of the treatment (Puhani 2008). Third, even if I found that the impact of the treatment is statistically significant, I cannot totally rule out the possibility that results are somehow driven by chance. In particular I am not able to say how often I would obtain results of this magnitude by choosing a state at random. The issue concerning the statistical significance of the treatment is exacerbated by the use of a very large sample, which may provide even small economic effects with statistical significance.

The synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmueller (2010), can be promisingly applied in order to tackle the previous issues. This method constructs a weighted combination of potential control US states (namely, the synthetic control) in order to approximate the most relevant characteristics of the states affected by the intervention (that is Texas and Florida). The weights are chosen so that the pre-treatment outcome and the covariates of the synthetic control are, on average, very similar to those of the treated states. In order to understand how SCM works, assume that there is a panel of N+1 regions over T periods. Only state *i* receives a treatment at T0<T. The treatment effect for region *i* at time *t* is:

$$\tau_{it} = Y_{it}^{I} - Y_{it}^{N} \tag{14}$$

where Y_{it}^{N} is the outcome that would be observed for region *i* at time *t* in the absence of the intervention, and Y_{it}^{I} is the actual outcome for region *i* at time *t* after the intervention.

Suppose that Y_{it}^{N} is given by a factor model:

$$Y_{it}^{N} = \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it} \tag{15}$$

where Z_i is a vector of relevant observed covariates that are not affected by the intervention and can be either time-invariant or time varying; θ_t is a vector of parameters; μ_i is a state-specific unobservable; λ_t is an unknown common factor varying over time and ε_{it} are transitory shocks with zero mean.

Define $W = (w_1 + \cdots + w_N)$ as a generic vector of weights such that $w_j > 0$ and $\sum w_j = 1$.

Further, define $\overline{Y}_j = \sum_{s=1}^{T_0} k_s Y_{js}$ as a generic linear combination of pre-treatment outcomes.

Abadie, Diamond and Hainmueller (2010) show that, as long we can choose W^*such that

$$\sum_{j=2}^{N+1} w_j^* Z_j = Z_i \qquad \text{and} \qquad \sum_{j=2}^{N+1} w_j^* \overline{Y}_j = \overline{Y}_i \tag{16}$$

then:

$$\hat{\tau}_{it} = Y_{it}^{I} - \sum_{j=2}^{N+1} w^* Y_{jt} \tag{17}$$

is an unbiased estimator of the treatment τ_{it}.

The main advantages of SCM are that it gives an unbiased estimate of the impact of the treatment, and it also successfully deals also with the endogeneity problem caused by the presence of time-varying unobservable state heterogeneity (Abadie, Diamond and Hainmueller 2010). In order to implement the synthetic control method, I aggregated the data at the state level. Therefore the outcome variables I took into account are the proportion of inventions in new technological areas, of breakthroughs and of failures. As covariates, I used a dummy indicating the U.S. macro geographic region to which the state belongs[7]; the degree of non-compete enforcement in the pre-treatment period; the level of gross domestic product averaged over the pre-treatment period, and, finally, the lagged outcome variables in the pretreatment period. I considered a longer time span (1986-2000) with respect to the empirical analyses presented in the previous sections: extending the pretreatment period improves the fit between the treated states and their synthetic counterparts. Moreover, I restricted the pool of potential comparison units by picking only those 41 states where at least 10 inventions per year have been generated in any year of the time period taken into account[8]. The average impact of the treatment in the post-treatment period, computed according to equation (17), is reported in table 9. The results of the SCM corroborate the theory, as the decrease in non-compete enforcement in Texas has produced a lower proportion of inventions in new technological areas, of breakthroughs and failures. On the contrary, the increase in non-compete enforcement in Florida has determined a higher proportion of inventions in new technological areas, of breakthroughs and failures.

The synthetic control approach also allows to assess the statistical significance of the treatment through a series of "placebo tests". I applied the synthetic control method to every potential state in the sample. This iterative procedure provided with a distribution of estimated effects of "placebo" treatment for states where no actual treatment took place, such

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[7] I used the regional division defined by the United States Census Bureau, which divides the US in four macro regions: Northeast, South, West, Midwest (http://www.census.gov/geo/www/us_regdiv.pdf).

[8] The rationale is excluding those states with a null proportion of inventions in new technological areas (or of breakthroughs and failures) merely due to a small number of observations. Different cutoffs (20, 30, 40 inventions) give similar results.

that it is possible to examine whether or not the estimated effect of the real intervention is large relative to the distribution of the "placebo" effects for the regions not exposed to the intervention. Following Abadie, Diamond and Hainmueller (2010), I looked at the distribution of the ratio of post/pre-treatment mean squared prediction error (MSPE), i.e. the average squared discrepancies between the outcome of a certain state and its synthetic counterpart. Results of the "placebo tests" procedure to assess the statistical significance of the treatment are reported in table 10. In general, the change in non-competes enforcement in Texas seems to have exerted statistically significant effects on the outcome variables of interest. For instance, considering the impact of non-compete enforcement on the proportion of breakthroughs, only 2 states out of 41 display a greater ratio of post/pre MSPE. This means that the probability of obtaining a greater magnitude by chance is less than 5 per cent. The effect on the proportion of failures and of inventions in new technological areas is about 10 per cent significant. Instead, in the case of Florida, I cannot rule out the possibility that the results seemingly produced by the change in non-compete enforcement are instead driven by chance.

Insert tables 9&10 about here

As a further robustness check, I also considered the extent to which the results might be sensitive to different measures of the dependent variables. I therefore replicated the empirical analyses using a measure of breakthrough that indicated the patent was in the top 1% or 3% (rather than 5%) of the value distribution of patents applied for in the same year and in the same IPC four-digit class. The results were similar (details are available on request). The findings also were robust to a different measure of a new technological domain, namely, a measure with respect to patented inventions produced by the company in the previous three or four (rather than five) years.

DISCUSSION AND CONCLUSIONS

While previous research has extensively studied the implications of non-compete agreements for regional growth and performance, far less is known about the impact of such contracts on firms' strategies. This study investigates the impact of non-competition agreements on the type of R&D activity undertaken by companies. I showed that in areas where non-compete agreements are enforced more strictly, the likelihood that corporate inventions will be explorative and path-breaking is greater. However, I also have found that a greater probability of achieving great inventive successes is accompanied by a greater probability of extremely poor outcomes.

This work accordingly offers several key contributions to prior literature. First, I provide relevant insights into how the strategy and competitive advantage of firms depends on the institutional environment in which they are embedded (see also Ingram and Silverman 2002; Furman 2003). With regard to innovative performance, Hall and Soskice (2001) suggest that in liberal market economies (e.g., U.S., U.K.), due to more labor turnover companies innovate more radically than they do in coordinated-market countries (e.g., Germany, France), where firms instead specialize in incremental, less risky innovation. However, this study provides evidence that in regions where non-competes are enforced more strictly, and thus mobility is limited, corporate inventions actually tend to be radical and path-breaking.

Second, this study offers interesting findings for entrepreneurship literature, which previously has considered non-competition agreements mainly as barriers to the formation of new companies, seemingly decreasing technological variety and risk-taking in a region. My study suggests that the strong appropriability regime determined by non-competes stimulate corporate entrepreneurship, inducing managers to experiment and explore risky and potentially path-breaking technological solutions. Thus non-competes, by increasing the degree of technological exploration and risk-taking *within* companies, might indirectly increase the degree of exploration and risk-taking within regions that host such companies. This last result is consistent with some recent research that reevaluates the situation and shows that non-competition agreements, by providing entrepreneurs with protection of their ideas, actually can foster regional innovation and growth (Franco and Mitchell 2008).

Third, I offer insights for the growing stream of research that examines the tails of inventions' value distribution, rather than the average value of inventions (e.g., Ahuja and Lampert 2001; Fleming and Singh 2010; Girotra et al. 2010). The interest in the tails is mainly motivated by the skewed distribution of inventions' value, with a small minority of inventions accounting for a disproportionate share of value (Gambardella, Harhoff and Verspagen 2008). Non-competes enhance the likelihood that any single invention will lie in the tails of the inventions' value distribution, as a breakthrough or a failure. In this sense, this study also contributes to literature pertaining to the impact of legal appropriability regimes on inventive performance (e.g., Ginarte and Park 1997; Sakakibara and Branstetter 2001; Kanwar and Emerson 2003; Qian 2007). Further studies also should consider how intellectual property laws might affect not only the average inventive performance but also the tails of the inventive outcome distribution.

Some limitations of this study are worth noting. First, the study is based on the assumption that companies actually use non-competes. If this was not true, any increase in the enforcement of non-competes would not influence company R&D choices. However, the evidence provided by Kaplan and Stromberg (2003) and Holley (1998) indicate that companies extensively use non-competes whenever it is possible. Moreover, the restriction of the sample to public companies indicates the need to conduct studies with private companies, which likely differ from public companies along several dimensions. For instance, the ownership structure of a firm may influence its corporate risk taking (e.g., Jensen and Meckling 1976; May 1995). As a result, the same degree of non-compete enforcement may exert a different impact on corporate decisions to pursue risky but high potential R&D projects, depending on the private or public ownership of the firm. Furthermore, I measured inventive performance using forward citations to patents, which creates a biased measure of failure. That is, I can only observe patented inventions receiving no forward citations, but I cannot observe "real" failures, such as R&D projects that do not lead to any patented inventions. Using forward citations has also another shortcoming. As non-competes reduce inventors' mobility, they may have a direct negative impact on the amount of forward citations, which are also a proxy for knowledge spillovers. However, this would goes against the finding that in states where non-compete enforcement is stricter corporate inventions are more likely to be extremely valuable.

Despite these limitations, this study offers relevant implications for managers and policymakers. From a firm strategic perspective, in the short run legal institutions are usually beyond the control of firms, but in the long run they may be the object of organizational strategies (Ingram and Silverman 2002). Managers could attempt to modify formal institutions, such as through lobbying activities. Companies operating in highly uncertain technological environments (i.e., where the outcomes of R&D projects is more variable) have more to gain from a stronger appropriability regime, so they should lobby for increasing the enforcement of non-competes.

From a policy perspective, non-competition agreements may create, at the regional level, a trade-off between entrepreneurship and intrapreneurship. Non-competes likely limit the formation of new companies, which might create technological variety in a region.

However, non-competes also increase the degree of technological exploration by companies and the likelihood that corporate inventions will be path-breaking. Therefore, the extent to which policymakers should favor exploration by entrepreneurship rather than exploration by intrapreneurship remains an interesting question for further research.

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Table 1. Operationalization of Variables

Variable	Operationalization
INVENTION IN NEW TECHNOLOGICAL AREAS FOR A COMPANY	Dummy: 1 if the patent is in a new patent class, with respect to patents produced by the organization in the previous five years. *Source: NBER patent database*
BREAKTHROUGH	Dummy: 1 if the patent is in the top 5% of the value distribution of patents invented in the same year (in terms of application date) and IPC four-digit class. *Source: NBER patent database*
FAILURE	Dummy: 1 if the patent receives no forward citations. *Source: NBER patent database*
ENFORCEMENT	Strength in the enforcement of non-competes. *Source: Garmaise (2009)*
FIRM KNOWLEDGE STOCK	Number of patents applied in the previous 5 years by the focal company. *Source: NBER database*.
FIRM SPECIALIZATION	Herfindahl index of concentration, within four-digit IPC classes, of patents produced from t – 1 to t – 5, equal to 1 when the number of accumulated patents is 0. *Source: NBER database*
FIRM EMPLOYEES	Number of company employees. *Source: Compustat*
NUMBER OF TECHNOLOGICAL COMPETITORS	Number of firms patenting an invention in the same technological class, year and state of the focal patent. *Source: NBER database*

Table 2 Descriptive statistics

	Observations	Mean	St. Dev.	Min	Max
Variable					
Invention in a new Technological Area	337054	0.078	0.269	0	1
Breakthrough	337054	0.074	0.262	0	1
Failure	337054	0.105	0.306	0	1
Enforcement	337054	3.565	2.151	0	9
Log Firm Knowledge Stock	337054	6.309	2.110	0	9.744
Firm Specialization	337054	0.186	0.202	0.012	1
Log Firm Number Employees	337054	3.588	1.634	0	7.126
Log Number Tech Competitors	337054	3.867	0.988	0.693	5.843

Table 3. Correlation Matrix

	Variable	1	2	3	4	5	6	7	8
1	Invention in a new Technological Area	1.000							
2	Breakthrough	0.148	1.000						
3	Failure	0.012	-0.09	1.000					
4	Enforcement	0.011	-0.012	0.024	1.000				
5	Log Firm Knowledge Stock	-0.247	-0.028	0.005	0.026	1.000			
6	Firm Specialization	0.081	0.035	-0.007	-0.137	-0.617	1.000		
7	Log Firm Number Employees	-0.170	-0.032	-0.012	0.124	0.773	-0.533	1.000	
8	Log Number Tech Competitors	-0.021	0.006	-0.013	-0.673	-0.021	0.095	-0.093	1.000

Table 4. Probability of any invention being a breakthrough

	Logit			Linear Probability Model		
	a.	b.	c.	d.	e.	f.
Explanatory variable						
Enforcement	**0.115*** **		0.035	**0.006*** **		-0.004
Enforcement*Log NumberTechCompetitors		**0.026*** **	**0.020***		**0.002*** **	**0.003*** **
Log Firm Knowledge Stock	-0.002	0.000	-0.000	-0.013**	-0.013**	-0.013**
Firm Specialization	0.444***	0.453***	0.452****	-0.042**	-0.042**	-0.042**
Log Firm Number Employees	-0.029*	-0.030*	-0.029*	-0.011***	-0.011***	-0.010***
Log NumberTechCompetitors	-0.079	-0.170**	-0.149**	0.001	-0.005**	-0.008**
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	337054	337054	337054	337054	337054	337054
Log-likelihood	-88161.2	-88358.6	-88358.2			
R-square				0.020	0.020	0.020

Notes: Standard errors are clustered by state in the logit models, and by firm in the linear probability models.
* *p* < .10. ***p* < .05. ****p* < .01.

Table 5. Probability of any invention being a failure

	Logit			Linear Probability Model		
	a.	b.	c.	d.	e.	f.
Explanatory variable						
Enforcement	**0.141*** **		-0.084	**0.003***		-0.007*
Enforcement*Log NumberTechCompetitors		**0.042*** **	**0.055** **		**0.001*** **	**0.002*** **
Log Firm Knowledge Stock	-0.005	-0.002	-0.002	-0.001	-0.001	-0.000
Firm Specialization	-0.534***	-0.517***	-0.513***	0.012*	-0.012*	-0.012
Log Firm Number Employees	-0.069*	-0.070**	-0.070**	-0.008***	-0.008***	-0.008***
Log NumberTechCompetitors	-0.386**	-0.540***	-0.590***	-0.015***	-0.019***	-0.025***
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	337054	337054	337054	337054	337054	337054
Log-likelihood	-104357.8	-104335.4	-104333.1			
R-square				0.051	0.051	0.051

Notes: Standard errors are clustered by state in the logit models, and by firm in the linear probability models.
* *p* < .10. ***p* < .05. ****p* < .01.

Table 6. Probability of any invention being in a new technological area

	Logit			Linear Probability Model		
	a.	b.	c.	d.	e.	f.
Explanatory variable						
Enforcement	**0.045*** **		-0.436***	**0.001**		-0.038***
Enforcement*Log NumberTechCompetitors		**0.046***	**0.120*** **		**0.003*** **	**0.010*** **
Log Firm Knowledge Stock	-0.591***	-0.587***	-0.584***	-0.005***	-0.005***	-0.004***
Firm Specialization	-2.462***	-2.443***	-2.427***	-0.002	-0.000	-0.001
Log Firm Number Employees	0.045	0.043	0.041	-0.021***	-0.021***	-0.020***
Log NumberTechCompetitors	-0.143	-0.310*	-0.576**	-0.0198***	-0.030***	-0.057***
Year Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	337054	337054	337054	337054	337054	337054
Log-likelihood	-81362.7	-81317.6	-81261.6			
R-square				0.042	0.042	0.042

Notes: Standard errors are clustered by state in the logit models, and by firm in the linear probability models.
* *p* < .10. ***p* < .05. ****p* < .01.

Table 7. Difference-in-Differences: Texas reduction of non-compete enforcement

	Breakthrough	Failure	Invention in new technological areas
Explanatory variable			
Texas*Post1994	**-0.285*** **	**-0.237*** **	**-0.080*** **
Log Firm Knowledge Stock	0.0217	-0.0119	-0.591***
Firm Specialization	0.520***	-0.635***	-2.492***
Log Firm Number Employees	-0.0563*	-0.386	0.0389
Log NumberTechCompetitors	0.0199	-0.103*	-0.0640***
Texas	-0.099***	1.046***	-0.077***
Post 1994	0.027	0.029	0.055***
Observations	330497	330497	330497
Log-likelihood	-87281.718	-108644.97	-80328.695

Notes: Standard errors are adjusted for intragroup (state) correlation.
* *p* < .10. ***p* < .05. ****p* < .01.

Table 8. Difference-in-Differences: Florida increase of non-compete enforcement

	Breakthrough	Failure	Invention in new technological areas
Explanatory variable			
Florida*Post1996	**0.137*** **	**0.469*** **	**0.145*** **
Log Firm Knowledge Stock	0.022	-0.380	-0.587***
Firm Specialization	0.516***	-0.720***	-2.500***
Log Firm Number Employees	-0.061*	-0.028	-0.038
Log NumberTechCompetitors	0.019	-0.118**	0.000
Florida	-0.199***	1.132***	-0.335
Post 1996	-0.094***	-0.679***	0.180
Observations	306412	306412	306412
Log-likelihood	-81465.274	-99726.999	-75770.867

Notes: Standard errors are adjusted for intragroup (state) correlation.
* $p < .10$. ** $p < .05$. *** $p < .01$.

Table 9. Synthetic Control Method: Estimation of the impact

	Proportion of inventions in new technological areas	Proportion of Breakthroughs	Proportion of Failures
Estimated treatment			
Texas	**-0.011**	**-0.031**	**-0.028**
Florida	**0.008**	**0.010**	**0.006**

Table 10. Synthetic Control Method: Statistical significance

	Proportion of inventions in new technological areas	Proportion of Breakthroughs	Proportion of Failures
*Statistical significance ** *			
Texas	**5/41=0.123**	**2/41=0.048**	**3/41=0.073**
Florida	**20/41=0.487**	**21/41=0.512**	**25/41=0.609**

* * number of states with a ratio postMSPE/preMSPE greater than the ratio of the treated state, divided by the total number of states

Appendix 1

The profits π_A and π_B of two R&D projects A and B are two random variables, $\pi_A \sim F_A(x)$ and $\pi_B \sim F_B(x)$, where F_A and F_B are two continuous cdf with support (-L,P), where L is the maximum loss a firm can sustain for a project, and P is the highest payoff a firm can achieve. B is riskier than A according to the classical definition by Rotschild and Stiglitz (1970), i.e., B is a mean-preserving spread of A: $\pi_B = \pi_A + z$, where z ~ F(z) is a random variable with the same support of π_A, and with $E(z|\,\pi_A) = 0$ for all π_A. By definition, $E(\pi_B) = E(\pi_A)$.

Assume that, once a project turns out to be profitable, the employee working on it leaves with probability λ, in which case the firm's profits drop of a share γ. The firm's expected profits from project A are:

$$E(\pi_A) = \int_{-L}^{0} x_A dF_A(x) + (1 - \lambda\gamma) \int_{0}^{P} x_A dF_A(x)$$

whereas the expected profits from project B are:

$$E(\pi_B) = E(\pi_A + z) = E(\pi_A) + \int_{-L}^{0} Z dF_Z + (1 - \lambda\gamma) \int_{0}^{P} Z dF_Z.$$

If λ and γ are both greater than 0, then $\int_{-L}^{0} Z dF_Z + (1 - \lambda\gamma) \int_{0}^{P} Z dF_Z < 0$, and $E(\pi_B) < E(\pi_A)$.

Appendix 2

Questions and thresholds to assess non-compete enforcement

The list of questions and thresholds is provided by Garmaise (2009). Each state is granted one point for each question when its laws lie above the threshold.

Question 1. Is there a state statute of general application that governs the enforcement of covenants not to compete?
Threshold 1. States with statutes that enforce non-competition agreements outside a sale-of-business context receive a score of one.

Question 2. What is an employer's protectable interest and how is it defined?
Threshold 2. States in which the employer can prevent the employee from future independent dealings with all the firm's customers, not merely with the customers with whom the employee had direct contact, receive a score of one.

Question 3. What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?
Threshold 3. Laws that place greater weight on the interests of the firm relative to those of the former employee are above the threshold. For example, a law that requires that the contract be reasonably protective of the firm's business interests and only meet the condition of not being unreasonably injurious to the employee's interests would receive a score of one.

Question 4. Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?
Threshold 4. States for which the answer to Question 4 is clearly "Yes" are above the threshold.

Question 5. Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?
Threshold 5. States for which the answer to Question 5 is clearly "Yes" are above the threshold.

Question 6. Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?
Threshold 6. States for which the answer to Question 6 is clearly "Yes" are above the threshold.

Question 7. What factors will the court consider in determining whether time and geographic restrictions in the covenant are reasonable?
Threshold 7. Jurisdictions in which courts are instructed not to consider economic or other hardships faced by the employee are above the threshold.

Question 8. Who has the burden of proving the reasonableness or unreasonableness of the covenant not to compete?
Threshold 8. States in which the burden of proof is clearly placed on the employee are above the threshold.

Question 9. What type of time or geographic restrictions has the court found to be reasonable? Unreasonable?
Threshold 9. Jurisdictions in which three-year statewide restrictions have been upheld receive a score of one.

Question 10. If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenants enforceable?

Threshold 10. States for which the answer to Question 10 is clearly “Yes” are above the threshold.

Question 11. If the employer terminates the employment relationship, is the covenant enforceable?
Threshold 11. States for which the answer to Question 11 is clearly “Yes” are above the threshold.

Question 12. What damages may an employer recover and from whom for breach of a covenant not to compete?
Threshold 12. If, in addition to lost profits, there is a potential for punitive damages against the former employee, the state receives a score of one. States that explicitly exclude consideration of the reasonableness of the contract from the calculation of damages are also above the threshold.

Non-competition enforcement index

State	Score	State	Score
Alabama	5	Montana	2
Alaska	3	Nebraska	4
Arizona	3	Nevada	5
Arkansas	5	New Hampshire	2
California	0	New Jersey	4
Colorado	2	New Mexico	2
Connecticut	3	New York	3
Delaware	6	North Carolina	4
District of Columbia	7	North Dakota	0
Florida 1990-1996	7	Ohio	5
Florida 1997-2000	9	Oklahoma	1
Georgia	5	Oregon	6
Hawaii	3	Pennsylvania	6
Idaho	6	Rhode Island	3
Illinois	5	South Carolina	5
Indiana	6	South Dakota	5
Iowa	6	Tennessee	7
Kansas	6	Texas 1990-1994	5
Kentucky	6	Texas 1995-2000	3
Lousiana	4	Utah	6
Maine	4	Vermont	5
Maryland	5	Virginia	3
Massachussets	6	Washington	5
Michigan	5	West Virginia	2
Minnesota	5	Wisconsin	3
Mississipi	4	Wyoming	4
Missouri	7		

Source: Garmaise (2009)