Strategic Management Journal
Strat. Mgmt. J., **35**: 1230–1248 (2014)
Published online EarlyView 29 June 2013 in Wiley Online Library (wileyonlinelibrary.com) DOI: 10.1002/smj.2155
Received 16 May 2011; *Final revision received 17 October 2012*

DO NON-COMPETITION AGREEMENTS LEAD FIRMS TO PURSUE RISKY R&D PROJECTS?

RAFFAELE CONTI*
Catolica Lisbon School of Business and Economics, Palma de Cima, Lisbon, Portugal

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 be in novel technological areas. This study uses data about U.S. patent applications from 1990 to 2000 and considers longitudinal variation in the enforcement of non-compete clauses. The 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.

INTRODUCTION

For firms competing in knowledge-intensive industries, retaining talent is crucial for building and maintaining a competitive advantage. The departure of key employees leads to leakages of valuable technological know-how to competitors, who could benefit from such knowledge without incurring the costs of creating it (Agarwal, Ganco, and Ziedonis, 2009; Shaver and Flyer, 2000). Thus, companies frequently use non-competition contractual agreements (hereafter, non-competes) to prevent employees from joining a competitor or forming a new company (e.g., Holley, 1998; Kaplan and Stromberg, 2003). Consider the case of Kai-Fu-Lee, a renowned computer scientist and technology executive who worked on revolutionary speech recognition technology for Microsoft. In 2005, he joined Google, and Microsoft immediately went to a court in Washington, D.C., to enforce its non-compete contract with him. The court eventually issued a restraining order, forbidding Lee from working on projects for Google that were similar to those he performed for Microsoft.

The protection granted by non-competes might change the very nature of firm R&D strategy if companies choose R&D characteristics that enable them to avoid or minimize the costs due to outbound mobility (Zhao, 2006). This idea echoes the words of Brian Halligan, CEO of Hubspot, who calls his company 'super entrepreneurial' in its persistent development of novel technological solutions, which occur precisely because the non-competes that employees sign 'encourage new thought about the way Hubspot does business' (Psaty, 2010). Thus, non-competes likely affect firms' choices to follow risky but high-potential technological trajectories. The decision to commit resources to a safe or risky R&D path has important implications for the stream of corporate returns and, therefore, has been extensively investigated in previous research (e.g., Cabral, 2003; Henderson, 1993; March, 1991). The role played

Keywords: appropriability; R&D strategy; innovation; technological breakthroughs; non-competition agreements
*Correspondence to: Raffaele Conti, Catolica Lisbon School of Business and Economics, Palma de Cima, 1649–023 Lisbon, Portugal. E-mail: raffaele.conti@ucp.pt

by things like non-competes, which can determine the knowledge appropriability regime, has been largely neglected, though. I propose that firms choose their degree of R&D riskiness according to the extent of non-compete enforcement they confront. Specifically, I argue that mobility-induced knowledge leakages imply that the firm shares its profits, but not its losses, from R&D with rivals. Therefore, a stronger enforcement of non-competes should make high-risk R&D projects relatively more valuable than low-risk ones, such that in regions in which non-competes are enforced more strictly, firms likely undertake R&D paths whose outcomes have a higher probability of being both extremely valuable (i.e., breakthroughs) and extremely poor (i.e., failures). Moreover, to the extent that non-competes create incentives to undertake riskier R&D paths, they also affect the direction of research efforts and induce firms to undertake projects in new technological areas.

To test this prediction, I have gathered data on U.S. patent applications by public companies from 1990 to 2000. I identify the impact of non-competes by considering longitudinal variation in U.S. non-compete enforcement. The findings indicate that in states where non-compete enforcement becomes stricter, 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 appear in novel technological areas. Accordingly, this study provides several contributions to strategy literature.

First, this work contributes to the growing stream of research into the relationship between business activity and the institutional context (e.g., Furman, 2003; Ingram and Silverman, 2002; Pe'er and Gottschalg, 2011). This link is of central interest to practitioners, especially because government policies, such as those that I investigate, are more likely to affect companies' economic values than actions by any other group of stakeholders, except customers. In addition, government impacts are expected to increase (McKinsey Global Survey, 2010). In this respect, I show how a firm's R&D strategy depends strongly on the institutional environment in which it is embedded.

Second, this study clarifies how the impact of knowledge leakages on firm profitability varies with firm characteristics. Shaver and Flyer (2000) argue that unintended knowledge spillovers to competitors are asymmetric with respect to the quality of firm technologies; losses are higher for firms that possess better technologies. In an extension of their contribution, this work theorizes that mobility-induced knowledge leakages are asymmetric with respect to the riskiness of the firm's R&D strategy, in that losses are higher for firms that pursue riskier research projects.

Third, in the framework of strategic entrepreneurship literature, this study points out that non-competes, though they reduce the formation of new companies (e.g., Samila and Sorenson, 2011), might stimulate corporate entrepreneurship by encouraging managers to explore novel and potentially pathbreaking technological solutions. From a policy perspective, non-competition agreements, thus, may create, at the regional level, a trade-off between entrepreneurship and intrapreneurship.

Fourth, the study extends a growing stream of research that examines the determinants of inventive breakthroughs (e.g., Ahuja and Lampert, 2001; Fleming and Singh, 2010; Phene, Fladmoe-Lindquist, and Marsch, 2005). Interest in breakthroughs is mainly motivated by the skewed distribution of inventions' value, such that a small minority of inventions account for a disproportionate share of value (Gambardella, Harhoff, and Verspagen, 2008). In this context, non-competes enhance the likelihood that any inventive outcome will be extremely profitable.

BACKGROUND AND THEORY DEVELOPMENT

Non-competes, appropriability, and the incentive to invest in R&D

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 leaks to rivals (Agarwal *et al.*, 2009; Shaver and Flyer, 2000). If proprietary knowledge cannot be protected at all and complementary assets are freely available, innovative firms suffer a constant disadvantage; competitors simply imitate their knowledge without incurring the costs of creating it. Thus, companies use different mechanisms to limit unintended knowledge spillovers to rivals (Levin *et al.*, 1987), including protections granted by patent or

DOI: 10.1002/smj

copyright laws. Tacit knowledge can also 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 leaving the company, such as through non-competes. These non-competes significantly limit outbound mobility to competitors (Garmaise, 2011; Marx, Strumsky, and Fleming, 2009) and are widely used in contracts with scientists, engineers, and technology executives. In the United States, almost 70 percent of entrepreneurs receiving venture capital financing are required to sign non-competition clauses by the venture capital firms (Kaplan and Stromberg, 2003), and about 80 percent of newly hired IT professionals are asked to sign non-compete contracts (Holley, 1998).

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 spread to the United States in the nineteenth century; by the start of the twentieth century, U.S. courts generally considered non-competes enforceable if they met 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 varies by jurisdiction. In most states, a non-compete contract cannot specify a time restriction greater than two years, but Pennsylvania courts routinely accept three-year covenants.

The social desirability of non-competes remains a topic of debate. On one side, 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. And Samila and Sorenson (2011) show that the positive impact of venture capital on the number of new firms, inventions, and employment is significantly greater in regions that do not enforce non-compete agreements strictly. On the other side, the knowledge protection provided by non-competes may be essential during emergent stages of a new industry to stimulate both entrepreneurship and innovation (Franco and Mitchell, 2008). Although the macro implications of non-competes for regional growth and performance have been dealt with extensively, their implications for companies' R&D strategies have been neglected, despite their widespread use in contracts for scientists, engineers, and technology executives.

How do non-competes affect company R&D strategy? Companies might invest more in R&D if non-competes were enforced more strictly. Non-competes encourage firms to allocate resources to the innovation process because they protect the firms from the loss of their R&D investments that would ensue if researchers depart. Moreover, R&D investments benefit from the presence of highly skilled researchers, but companies likely spend money developing researchers' skills only if they can secure their employee human capital within firm boundaries (Garmaise, 2011). In contrast, non-competes could decrease firms' R&D investment because they prevent competitors from eliciting rents from previous innovations, which reduces the incentive to innovate. Furthermore, fewer mobility-induced knowledge spillovers might diminish the returns to R&D investments to the extent that R&D across firms becomes complementary. Finally, non-competes can reduce the average quality of the match between an employee and the employer, which likely reduces R&D productivity (Samila and Sorenson, 2011). Theoretically then, the impact of non-compete agreements on the amount of corporate R&D expenditure is ambiguous. Empirically, it appears to be null (Garmaise, 2011), which aligns with the results that have been produced by research into the impact of intellectual property rights (IPR) on R&D expenditures (e.g., Sakakibara and Branstetter, 2001).

Therefore, rather than analyzing again the relationship between non-compete enforcement and the sheer amount of company R&D expenditure, I attempt to determine how non-competes affect the *type* of R&D investments. In particular, I focus on the riskiness of the R&D undertaken by companies that operate under different non-compete enforcement regimes.

DOI: 10.1002/smj

Non-competes and the choice of R&D riskiness

As Cabral (2003) convincingly argues, designing an R&D strategy means choosing not just how much to invest, but also *how* to invest, which entails a choice of R&D riskiness. Committing firm resources to a safe versus a risky research path has important implications for corporate returns. Consider, for example, a pharmaceutical firm that performed research in the antidepressant market in the early 1990s (see Cabral, 2000). Broadly speaking, it faced a choice of two strategies: (1) it could invest in developing an antidepressant based on a serotonin inhibitor, a relatively well-known method which would provide a high probability of success but a limited payoff considering the incremental nature of the innovation; or (2) it could invest in developing an antidepressant that used a substance P receptor blocker approach. For this relatively risky strategy, which had not been used by previous drugs, there was a significant probability of complete failure, but also the promise of a substantial payoff in the case of success: it would represent a radical innovation.

Similar R&D riskiness choices for firms depend on their organizational and environmental characteristics, as well as their relative competitive position. Among the organizational factors, substantial literature suggests that incumbency may hamper a firm's incentive and ability to undertake riskier technological trajectories. This is because the research efforts of incumbents seeking to exploit novel and uncertain technological trajectories are significantly less productive than those of new entrants (e.g., Henderson, 1993). Environmental factors also play important roles. As March (1991) shows, organizations operating in winner-take-all environments and facing a multitude of rivals likely undertake strategies designed to enhance performance variance, rather than mean performance. As the number of competitors increases, 'the contribution of the variance to competitive advantage increases,' whereas 'the mean becomes irrelevant' (March 1991, p. 83). Moreover, in winner-take-all environments, the initial position of the company with respect to competitors matters. Cabral (2003) argues that market leaders likely invest in safe R&D strategies, whereas market laggards pursue risky R&D because the latter have a lot to gain from reversing their relative position and little (or nothing) to lose from just remaining a laggard.

However, previous research has not considered how the decision about R&D riskiness may depend on the appropriability regime faced by companies. That is, firms cope with potential knowledge leakages by strategically choosing R&D project characteristics that enable them to avoid or minimize the costs of knowledge outflows to rivals. Zhao (2006) and Alcacer and Zhao (2012) find that multilocation companies facing appropriability risks usually choose projects characterized by strong linkages with other corporate proprietary knowledge—because this interdependence creates knowledge that is hard for competitors to replicate. The degree of R&D riskiness provides another characteristic that might minimize knowledge leakages costs because mobility-induced knowledge leakages should have asymmetric impacts on R&D projects of varying riskiness. In particular, the profit decrease should be relatively higher for riskier projects. Hence, by reducing outbound mobility, non-competes may make high-risk R&D projects relatively more valuable.

As an illustration, imagine the following situation: two R&D projects have 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 generates a positive profit b with a probability p, but it produces an economic loss L with a probability $(1-p)$. In principle, a risk-neutral firm is indifferent between Projects A and B because they have the same expected value a, that is, $a = pb - (1-p)L$. Their preference changes, though, when non-competes are not enforceable because researchers working on a profitable project leave the firm with probability λ, in which case the company loses a share γ of profits. Its profits decrease but losses do not, so the expected value of the risky Project B, or $(1-\lambda\gamma)pb - (1-p)L$, falls to below the expected value of the safe Project A, which is $(1-\lambda\gamma)a$. Such reasoning generalizes to projects with varying degrees of riskiness (see Appendix 1). Therefore, when companies cannot enforce non-competes, a high-risk project becomes less valuable than a low-risk one with the same initial expected value. If firms pass from a situation in which non-competes are forbidden to one in which the competes can be enforced, their high-risk R&D projects should become relatively more valuable. Using the previous example, without non-competes, a risk-neutral company prefers

DOI: 10.1002/smj

Project A, but if the possibility of enforcing non-competes exists, it is indifferent between projects. In this case, the non-compete enforcement induces firms to invest more resources in riskier R&D projects.

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

Hypothesis 1 (H1): The stricter the enforcement of non-competes, the greater the likelihood that corporate inventions are breakthroughs.

Hypothesis 2 (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 pursue projects distant from their current technological know-how. This choice has implications for the distribution of rewards, in that the exploration of novel technological competences is usually riskier than the exploitation of existing know-how, so 'compared to returns from exploitation, returns from exploration are systematically less certain' (March, 1991: 73). To the extent that non-competes create incentives to undertake riskier R&D paths, they also affect the direction of research efforts and induce firms to undertake projects in new technological areas. I hypothesize:

Hypothesis 3 (H3): The stricter the enforcement of non-competes, the greater the likelihood that corporate inventions occur in new technological areas.

In summary, stronger enforcement of non-competes should encourage riskier R&D projects whose outcomes have a higher probability of being both breakthroughs and failures. Moreover, stronger non-compete enforcement should lead companies to produce inventions in technological domains that are distant from their current technological know-how.

METHODS

Sample and data

To investigate how non-compete enforcement affects firms' inventive outcomes, I gathered a data set that includes all granted patents whose applications were filed in the United States by a public firm from 1990 to 2000. In particular, I focused on patented inventions whose first inventor resides in a U.S. state; similar to prior work (Thompson, 2005), I assigned each patent to the 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 provides citations for all U.S. patents granted from 1976 to 2006 (Hall, Jaffe, and Trajtenberg, 2001). To ensure that 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 revealed the firms and subsidiaries identified in the 'Who Owns Whom?' database. Ownership may change through mergers, acquisitions, or spin-offs, 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, were applied for from 1990 to 2000, and were eventually granted to public companies, which represented the sample used in the empirical analysis.

The selection of the 1990 to 2000 time period mainly reflected practical reasons. First, the enforcement index elaborated for U.S. states by Garmaise (2011), which I used in my empirical analysis, similarly refers to this time period.[1] Second, choosing a relatively short window of time enabled me to estimate the effects of a change in non-compete regulations while keeping other possible state-level changes constant. Third, I ended the data collection in 2000 to ensure sufficient additional time to measure the patented inventions' value, according to the number of forward citations received. This time period limitation also produces some shortcomings, especially because it excludes two important longitudinal changes in non-compete enforcement (in Michigan in 1985 and in Louisiana in 2004) from the analysis. However, the alternative of a longer time window was even worse, for three reasons. First, the 1985 non-compete enforcement change in Michigan was simultaneous to the introduction of an antitakeover law (cf. Atanassov, 2013) and preceded a branch banking deregulation by just one year (cf. Kerr and Nanda, 2009). This would lead to a spurious estimate of the impact of non-compete enforcement. Second, using data before 1985 would prevent me from assigning patents to their correct organization because the SDC database I used to identify the ownership structure of public companies over time is reliable only after 1986.[2] Finally, the last version of the NBER patent database includes information about all patents granted up to December 2006. Thus, it would be impossible to obtain a reliable measure of the number of forward citations received by patents applied for after 2004.

Empirical strategy

The empirical analysis pertains to the invention level. 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 would be a breakthrough (H1) or a failure (H2) or would refer to a new technological area (H3).

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 (Fleming and Singh 2010; Phene *et al.*, 2005), I employed a dichotomous variable that takes a value of '1' if the patent is in the top 5 percent 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 International Patent Classification (IPC) classes). The variable equals '0' otherwise.

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

Finally, I coded a firm's *invention in new technological areas* as equal to '1' if the patented invention referred to 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 IPC system. Consistent with prior research (Argote, Beckman, and Epple, 1990), I considered a five-year window to account for the rate of organizational forgetting.

To identify the impact of non-compete agreements, I took advantage of an index that measures the *enforcement of non-compete covenants* in U.S. states, as elaborated by Garmaise (2011) and based on 12 questions proposed by Malsberger (2004).[3] Specifically, I exploited the fact that two states (Texas and Florida) exhibited two opposite and almost simultaneous shifts in this index. In June 1994, in *Light v. Centel Cellular Co*., the Texas Supreme Court issued a new set of requirements for enforcement of non-compete agreements. Therefore, whereas the

[1] To be precise, the index elaborated by Garmaise (2011) refers to 1992 to 2004. However, no changes in non-compete enforcement occurred in 1990 to 1991. According to Samila and Sorenson (2011: 427), 'only four states have experienced meaningful changes over the last 30 years' and none of them took place in 1990 or 1991.

[2] The SDC database covers acquisition of non-U.S. targets only from 1985 to present.

[3] 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 appear in Appendix 2.

non-competition enforcement index score for Texas equaled 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 seven to nine. To the extent that changes in non-compete regulations are neither influenced nor predicted by individuals, such treatments can be considered truly exogenous. For Texas, this consideration appears well supported because the change in non-compete enforcement was generated by a Texas Supreme Court decision. It is reasonable to assume that companies were not aware of the impending decision by the Court. However, the change in Florida resulted from the actions of the state legislature, so companies were probably aware of the widely debated possible change (Marx *et al.*, 2009). Even in this case though, endogeneity does 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, my test would be even more conservative.

Using a difference-in-differences technique, I estimated the effect of the treatment (i.e., the exogenous change in non-compete enforcement in Texas and Florida) on the outcome variables by comparing what happened to the treatment group before and after the treatment against what happened to a group that was *not* subject to the treatment (control group), again before and after the treatment. The inventions generated in Texas and Florida represent the treated groups, whereas inventions in other U.S. states constitute the control group. A crucial assumption underlying the difference-in-differences technique is that differences in the outcome variables between treated and control groups would have remained constant without the treatment. Both visual inspection of the trends and a t-test of their differences before the Texas and Florida treatments indicate that this assumption is viable.

To estimate the effect of decreased non-compete enforcement in Texas in 1994 on the probability that an invention i generated by firm j, in state s at time t, is a breakthrough (H1), a failure (H2), or in a new technological area (H3), I excluded the Florida observations and estimated the following logit models:

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{Breakthrough_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{TX} * \mathrm{Post1994}\right) + \beta\mathrm{TX}\right.\\ &\quad \left.+\gamma\mathrm{Post1994} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (1)\end{aligned}$$

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{Failure_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{TX} * \mathrm{Post1994}\right) + \beta\mathrm{TX}\right.\\ &\quad \left.+\gamma\mathrm{Post1994} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (2)\end{aligned}$$

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{NewArea_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{TX} * \mathrm{Post1994}\right) + \beta\mathrm{TX}\right.\\ &\quad \left.+\gamma\mathrm{Post1994} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (3)\end{aligned}$$

In these equations, (TX*Post1994) is the treatment; TX is the dummy variable that takes a 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, including state fixed effects. In Texas, the treatment reduces non-compete enforcement, so I expect α to be negative in Equations 1, 2, and 3, consistent with H1-H3.[4]

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

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{Breakthrough_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{FL} * \mathrm{Post1996}\right) + \beta\mathrm{FL}\right.\\ &\quad \left.+\gamma\mathrm{Post1996} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (4)\end{aligned}$$

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{Failure_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{FL} * \mathrm{Post1996}\right) + \beta\mathrm{FL}\right.\\ &\quad \left.+\gamma\mathrm{Post1996} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (5)\end{aligned}$$

$$\begin{aligned}\mathrm{Prob}\left(\mathrm{NewArea_{ijst}} = 1|\mathrm{X}\right) &= \mathrm{Prob}\left(\alpha\left(\mathrm{FL} * \mathrm{Post1996}\right) + \beta\mathrm{FL}\right.\\ &\quad \left.+\gamma\mathrm{Post1996} + \delta\mathrm{Z} + \mathrm{e_{ijst}} > 0\right) \qquad (6)\end{aligned}$$

[4] Despite some concerns about the interpretation of interaction terms in nonlinear models (Ai and Norton, 2003), Puhani (2008) demonstrates that they are not relevant for treatment effects in nonlinear difference-in-differences models.

DOI: 10.1002/smj

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 a 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 4, 5, and 6.

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

In Equations 1 to 6, the vector Z of controls also includes firm-level variables. Specifically, I took into account the *number of employees* and the *size of the firm's knowledge base*,[5] measured as the number of patents granted to the firm and applied for in the five-year window previous to the year of observation. Both variables aimed to capture the impact of the firm's scale, which is clearly important for R&D activity, though findings about the exact sign of this effect remain controversial (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 $\mathrm{specialization}n_{\mathrm{it}} = \sum_{\mathrm{k}} \left(\frac{\mathrm{n_{kt}}}{\mathrm{n_t}}\right)^2$, where $\mathrm{n_t}$ is the total number of patents applied for by the firm in the five years preceding year t, and $\mathrm{n_{kt}}$ is the number of patents in the (four-digit) IPC technological class k, applied for in the same period of time. The indicator measured the concentration of a firm's knowledge stock within some technology classes in the five years before year t. Table 1 summarizes the operationalization of the variables for the analysis.

[5] I did not include firms' R&D expenditures for two reasons. First, its correlation with the size of firm knowledge base is greater than 0.8. Second, Garmaise (2011) shows that non-competes do not influence R&D expenditures. Excluding this variable from the empirical analysis should not create a bias in the estimated impact of non-compete enforcement.

RESULTS

Descriptive statistics

Tables 2 and 3 contain the descriptive statistics and pairwise correlations among variables. 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 pathbreaking 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 stocks (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.

The results of the difference-in-differences approaches, in Tables 4 and 5, refer to each state. In Table 4, the results for Texas are consistent with H1 and H2, such that the decrease in non-compete enforcement led to a lower likelihood of any invention being pathbreaking or a failure. Moreover, in line with H3, when non-compete agreements were enforced less strictly, the probability of any invention occurring in a novel technological area declined. In addition, the results in Table 5 confirm the predicted outcomes for Florida. Specifically, the greater non-compete enforcement after 1996 augmented the likelihood of any invention being pathbreaking (H1), a failure (H2), and in a new technological domain (H3).

I analyzed the time path of the response to variation in non-compete enforcement. As the results in Tables 6 and 7 show, the magnitude of coefficients representing the impact of a change in non-compete enforcement tended to decrease over time. For Texas, for instance, the greatest impact took place in 1996, two years after the change in the non-compete regulation (Table 6). Similarly, for Florida, the greatest impact in most cases (cf. impact on the probability of failures) arose one

DOI: 10.1002/smj

Table 1. Operationalization of variables

Variable	Operationalization
Invention in new technological areas	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 percent 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*
Non-compete enforcement	Strength in the enforcement of non-competes. *Source: Garmaise (2011)*
Firm knowledge stock	Number of patents applied in the previous five years by the focal company. *Source: NBER database*.
Firm knowledge 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*

Table 2. Descriptive statistics

	Observations	Mean	St. dev.	Min	Max
Variable					
Invention in new technological areas	337,054	0.078	0.269	0	1
Breakthrough	337,054	0.074	0.262	0	1
Failure	337,054	0.105	0.306	0	1
Non-compete enforcement	337,054	3.565	2.151	0	9
Log firm knowledge stock	337,054	6.309	2.110	0	9.744
Firm knowledge specialization	337,054	0.186	0.202	0.012	1
Log firm employees	337,054	3.588	1.634	0	7.126

year after the increase in non-compete enforcement (Table 7). These results suggest that any variation in the enforcement of non-competes mainly affected firms' R&D riskiness in the short run. In the long run, organizational changes aimed at protecting the firm's knowledge base likely would compensate for these effects. The analyses exploiting cross-state, rather than longitudinal, differences in non-compete enforcement also indicated no significant impact of non-compete enforcement on the variables of interest (available on request). This finding might be due to the expiration of the non-compete effect over time, in that cross-state differences likely reflect a long-term equilibrium. However, I cannot ignore an alternative explanation: that non-competes do not actually exert any effect on the riskiness of R&D strategy, and the results in Tables 4 and 5 are produced by some idiosyncratic events occurring in Texas and Florida in the same years of the non-compete enforcement changes. Yet, an analysis of the legislation archives for Texas and Florida and of previous literature about changes in U.S. laws potentially affecting innovation, suggest that no other simultaneous shocks may, in fact, drive the findings.[6]

Robustness checks

The previous difference-in differences analysis may raise some concerns. First, despite the statistical significance of the coefficients, results might be due to chance. In order to discard this possibility, I adopted a completely diverse difference-in-differences design. Specifically, I compared the inventions produced in Texas (Florida) by one group of firms affected by non-competes against the inventions produced in Texas (Florida) by

[6] Specifically, all changes in banking deregulation laws (Kerr and Nanda, 2009), antitakeover laws (Atanassov, 2013), labor market laws (Autor, 2003), and trade secrecy laws (Png, 2012) occurred in most U.S. states (including Texas and Florida) well before the time period (1990 to 2000) considered in this work.

DOI: 10.1002/smj

Table 3. Correlation matrix

	Variable	1	2	3	4	5	6	7
1	Invention in new technological areas	1.000						
2	Breakthrough	0.148	1.000					
3	Failure	0.012	−0.097	1.000				
4	Non-compete 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 knowledge specialization	0.081	0.035	−0.007	−0.137	−0.617	1.000	
7	Log firm employees	−0.170	−0.032	−0.012	0.124	0.773	−0.533	1.000

Table 4. Difference-in-differences: Texas reduction of non-compete enforcement

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Texas*post 1994	−0.250***	−0.331***	−0.084***
	0.032	*0.045*	*0.022*
Log firm knowledge stock	−0.006	0.046**	−0.587***
	0.011	*(0.023)*	*0.019*
Firm knowledge specialization	0.424***	−0.328*	−2.455***
	0.110	*0.178*	*0.108*
Log firm employees	−0.025	−0.101***	0.042
	0.019	*0.029*	*0.036*
Post 1994	−0.108***	1.056***	−0.095***
	0.032	*0.045*	*0.023*
State fixed effects	Yes	Yes	Yes
Observations	330,497	330,497	330,497
log likelihood	−86836.401	−107426.224	−79930.643

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

another group of firms not affected by non-competes, before and after the 1994 (1996) change in non-compete enforcement. Therefore, the treated group is represented by inventions produced in Texas (Florida) by firms with at least one competitor (i.e., another firm in the same four-digit standard industrial classification (SIC) code) that had invented in the same state before the non-compete change. The large majority of inventors move within the state boundaries (Marx, Singh, and Fleming, 2010), so an increase in non-compete enforcement would help these firms prevent their employees from migrating to competitors. The control group consists of inventions produced in Texas (Florida) by firms without any rivals in the same state. These companies are only marginally affected by a change in non-compete enforcement, because the possibility of their employees being hired by a competitor is already very low.

The results in Tables 8 and 9 reaffirm my previous findings and the underlying theory. Specifically, the 1994 Texas decrease in non-compete enforcement decreased the probability of any invention being a breakthrough, a failure, and in new technological areas for firms with competitors in the same state (Table 8). By contrast, the 1996 Florida increase in non-compete enforcement significantly increased the probability of any invention being a breakthrough, a failure, and in new technological areas for firms with competitors within the state (Table 9). It is worth stressing that since this was a completely different experimental design, it was extremely unlikely to again obtain results consistent with theory, *both* for Texas and Florida, simply due to chance.

An additional concern is that, due to the large sample size, even substantively small effects might show as statistically significant. To address this concern, I aggregated the data at the state and year levels, and I replicated the difference-in-differences models presented in Tables 4 and 5. To control for R&D expenditures at the state

DOI: 10.1002/smj

Table 5. Difference-in-differences: Florida increase of non-compete enforcement

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Florida*post 1996	0.159***	0.405***	0.142***
	0.037	*0.044*	*0.022*
Log firm knowledge stock	−0.007	0.023	−0.583***
	0.012	*0.021*	*0.017*
Firm knowledge specialization	0.416***	−0.404**	−2.461***
	0.118	*0.195*	*0.105*
Log firm employees	−0.030*	−0.098***	0.041
	0.018	*0.028*	*0.039*
Post 1996	−0.100***	1.13***	−0.001
	0.037	*0.048*	*0.013*
State fixed effects	Yes	Yes	Yes
Observations	306,412	306,412	306,412
log likelihood	−81013.430	−98516.559	−75677.024

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
$*p < 0.10$. $**p < 0.05$. $***p < 0.01$.

Table 6. Difference-in-differences: Texas reduction of non-compete enforcement (time path)

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Texas*1995	−0.222***	−0.264***	0.085
	0.023	*0.034*	*0.062*
Texas*1996	−0.475***	−0.380***	−0.358***
	0.033	*0.043*	*0.021*
Texas*1997	−0.261***	−0.376***	−0.081***
	0.035	*0.062*	*0.025*
Texas*1998	−0.318***	−0.238***	−0.185***
	0.047	*0.046*	*0.029*
Texas*1999	−0.278***	−0.271***	0.057*
	0.040	*0.057*	*0.032*
Texas*2000	0.027	−0.252***	−0.012
	0.056	*0.064*	*0.028*
Log firm knowledge stock	−0.004	−0.012	−0.591***
	0.011	*0.026*	*0.017*
Firm knowledge specialization	0.435***	−0.573***	−2.450***
	0.114	*0.195*	*0.104*
Log firm employees	−0.027	−0.069**	0.045
	0.019	*0.031*	*0.034*
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	330,497	330,497	330,497
Log likelihood	−86816.741	−103012.819	−79875.584

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
$*p < 0.10$. $**p < 0.05$. $***p < 0.01$.

level, I included as a regressor (the log of) the number of employees working in R&D in the state, as measured in County Business Patterns (U.S. Census Bureau). To capture relevant time-varying macroeconomic effects, I used the (log of) state GDP.

Because the dependent variable is a fraction (i.e., ratio of the number of breakthroughs, failures, or inventions in new technological domains in state s and year t to the overall numbers of inventions in state s and year t), I adopted the method proposed by Papke and Wooldridge

Table 7. Difference-in-differences: Florida increase of non-compete enforcement (time path)

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Florida*1997	0.247***	0.458***	0.225***
	0.032	*0.057*	*0.018*
Florida*1998	0.180***	0.008	0.136***
	0.044	*0.039*	*0.029*
Florida*1999	0.115***	0.271***	0.100**
	0.039	*0.048*	*0.039*
Florida*2000	0.093	0.564***	0.111**
	0.061	*0.062*	*0.045*
Log firm knowledge stock	−0.005	0.000	−0.583***
	0.012	*0.024*	*0.016*
Firm knowledge specialization	0.435***	−0.491**	−2.426***
	0.120	*0.192*	*0.101*
Log firm employees	−0.032*	−0.081***	0.042
	0.018	*0.030*	*0.036*
State fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	306,412	306,412	306,412
Log likelihood	−81002.448	−81002.448	−75607.331

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Table 8. Difference-in-differences: Texas reduction of non-compete enforcement (treatment group: inventions by firms in Texas with at least one competitor in the same state; control group: inventions by other firms in Texas)

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Firm with competitors*post 1994	−0.838***	−0.089***	−0.245***
	0.003	*0.004*	*0.008*
Log firm knowledge stock	−0.014***	−0.092***	−0.709***
	0.005	*0.014*	*0.024*
Firm knowledge specialization	0.320***	−1.131***	−1.071***
	0.080	*0.138*	*0.253*
Log firm employees	0.034***	0.083***	0.088*
	0.011	*0.008*	*0.046*
Post 1994	0.469***	0.923***	0.032*
	0.004	*0.018*	*0.018*
Firm with competitors	0.927***	−0.108***	−0.120**
	0.005	*0.038*	*0.048*
Observations	30,642	30,642	30,642
Log likelihood	−7349.017	−9071.248	−4762.822

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (firms with/without at least one competitor) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

(1996). To deal with a regression in which the dependent variable is bound between 0 and 1, they propose a quasi-maximum likelihood estimator based on the logistic distribution. This approach has several advantages over alternative solutions. First, a linear functional form of the conditional mean might miss important nonlinearities. Second, a log-odds transformation fails when the variable falls at the corners. The analyses I perform with these variations at the state levels (Tables 10 and 11) fully confirm my previous findings. In Texas, where enforcement decreased, the proportion of breakthroughs, failures, and inventions in new technological domains also decreased (Table 10). In Florida, where the enforcement increased, so did these proportions (Table 11).

Furthermore, the results might not be robust to different definitions of the dependent variables.

Table 9. Difference-in-differences: Florida increase of non-compete enforcement (treatment group: inventions by firms in Florida with at least one competitor in the same state; control group: inventions by other firms in Florida)

	(1) Breakthrough	(2) Failure	(3) Invention in new technological areas
Firm with competitors*post 1996	0.126*	1.322***	0.224***
	0.073	*0.099*	*0.070*
Log firm knowledge stock	−0.030	−0.130***	−0.711***
	0.030	*0.010*	*0.027*
Firm knowledge specialization	0.230***	−0.972***	−3.038***
	0.082	*0.037*	*0.452*
Log firm employees	−0.106**	0.042**	0.162***
	0.045	*0.021*	*0.001*
Post 1996	−0.160*	0.477***	−0.161
	0.086	*0.185*	*0.109*
Firm with competitors	0.536***	−1.116***	−0.329***
	0.026	0.040	0.021
Observations	6,557	6,557	6,557
Log likelihood	−1533.083	−1597.704	−1495.178

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (firms with/without at least one competitor) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Table 10. Difference-in-differences: Texas reduction of non-compete enforcement (state-level analysis)

	(1) Breakthrough 5%	(2) Failure	(3) New technological areas	(4) Breakthrough 1%	(5) Entirely new technological areas
Texas*post 1994	−0.271***	−0.412***	−0.168***	−0.562****	−0.153**
	0.075	*0.085*	*0.060*	*0.097*	*0.070*
Log R&D employees	0.002	0.006	0.015	−0.034	0.000
	0.021	*0.020*	*0.016*	*0.048*	*0.015*
Log GDP	0.319	1.839	−0.701	−0.178	0.161
	0.225	*1.189*	*0.856*	*1.572*	*0.838*
Post 1994	−0.834	1.146***	−0.235	0.011	−0.513
	0.676	*0.422*	*0.292*	*0.489*	*0.314*
State fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	550	550	550	550	550
Log likelihood	−98.956	−123.347	−140.815	−38.440	−138.151

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

Therefore, I replicated the empirical analyses using a measure of breakthrough that indicated whether the patent was in the top 1 percent (rather than 5 percent) of the value distribution of patents applied for in the same year and in the same four-digit IPC class. The results were similar (Tables 10 and 11, Column 4). The findings were also robust to a different measure of a new technological domain that considered all primary and secondary (rather than just the primary) technological classes in which the firm patented in the previous five years (Tables 10 and 11, Column 5).

DISCUSSION AND CONCLUSIONS

Previous research has extensively studied the implications of non-compete agreements for regional growth and performance, but far less is known about the impact of these contracts on firms' strategies. This study investigates the impact of non-competition agreements on the type of R&D activity undertaken by companies. In areas where non-compete agreements are enforced more strictly, the likelihood that corporate inventions are explorative and pathbreaking increases. However, the greater probability of achieving

DOI: 10.1002/smj

Table 11. Difference-in-differences: Florida increase of non-compete enforcement (state-level analysis)

	(1) Breakthrough 5%	(2) Failure	(3) New technological areas	(4) Breakthrough 1%	(5) Entirely new technological areas
Florida*post 1996	0.118*	0.361***	0.471***	0.439***	0.640***
	0.069	*0.052*	*0.048*	*0.103*	*0.061*
Log R&D employees	0.002	0.006	0.015	−0.033	−0.000
	0.021	*0.020*	*0.016*	*0.048*	*0.015*
Log GDP	−0.845	1.843	−0.699	−0.188	0.166
	0.679	*1.190*	*0.857*	*1.575*	*0.840*
Post 1996	0.203	0.905***	0.008	−0.033	−0.160
	0.196	*0.342*	*0.218*	*0.304*	*0.225*
State fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	550	550	550	550	550
Log likelihood	−98.853	−123.083	−141.051	−38.440	−138.408

Notes: Robust standard errors in italics. Standard errors are adjusted for intragroup (state) correlation.
* $p < 0.10$. ** $p < 0.05$. *** $p < 0.01$.

great inventive successes is accompanied by a greater probability of extremely poor outcomes.

Accordingly, this work offers several key contributions to prior literature. First, I provide relevant insights into how the strategies and competitive advantages of firms depend on the institutional environments in which they are embedded (see also Furman, 2003; Ingram and Silverman, 2002; Pe'er and Gottschalg, 2011). With regard to innovative performance, Hall and Soskice (2001) suggest that in liberal market economies (e.g., U.S., U.K.), labor turnover pushes companies to innovate more radically than firms 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 that enforce non-competes strictly, which limits mobility, corporate inventions actually tend to be radical and pathbreaking.

Second, I clarify how the effect of knowledge leakages on firm profitability varies with firm characteristics. Shaver and Flyer (2000) have argued that unintended knowledge spillovers to competitors are asymmetric with respect to the quality of the firm's technologies. This work extends their contribution by showing that mobility-induced knowledge leakages are asymmetric with respect to the riskiness of the firm's R&D, such that profit decreases relatively more when firms pursue riskier research projects.

Third, this study offers interesting findings for entrepreneurship literature, which previously has assumed non-competition agreements are barriers to the formation of new companies that decrease technological variety and risk taking. But my study shows that the strong appropriability regime created by non-competes actually can stimulate corporate entrepreneurship and encourage managers to experiment with and explore risky, potentially pathbreaking 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 across the regions that host these companies. This last result is consistent with some recent research that shows that non-competition agreements, by providing entrepreneurs with IPR for their ideas, can foster regional innovation and growth (Franco and Mitchell, 2008).

Fourth, I offer insights for the growing stream of research that examines the tails of inventions' value distributions, rather than the average value of inventions (e.g., Ahuja and Lampert 2001; Fleming and Singh 2010). The interest in tails is motivated mainly by the skewed distribution of inventions' value; a few inventions account for a disproportionate share of value (Gambardella *et al.*, 2008). Non-competes enhance the likelihood that any single invention appears in the tails of the inventions' value distribution, whether as a breakthrough or a failure. In this sense, this study contributes to investigations of the impact of legal appropriability regimes on inventive performance (e.g., Ginarte and Park, 1997; Kanwar and Evenson, 2003; Qian, 2007; Sakakibara and Branstetter, 2001). Further studies should consider how IPR laws might affect not only the average inventive performance, but also the tails of the inventive outcome distribution.

Some limitations of this study also are worth noting. First, the underlying theory assumes that the decision about whether to undertake a safe or a risky project is made centrally by the firm, rather than autonomously by researchers. Anecdotal evidence suggests that, in general, this assumption holds. Even at Google—famous for providing corporate researchers with autonomy to work on their own projects—employees must work on projects chosen centrally during at least 80 percent of their work time. However, if researchers, rather than the company, choose the type of projects, an alternative explanation might emerge. To the extent that workers move from one state to another, they may choose states that support their job attitudes or provide a preferred degree of non-compete enforcement. Career-oriented researchers, who enjoy moving throughout their career and like to pursue incremental projects, might select and move to states where non-compete enforcement is relatively low. This sorting argument appears theoretically appealing, but it cannot explicate the results of this study for at least two main reasons: (1) the proportion of U.S. inventors who have relocated to states following a change in non-compete enforcement is quite low (Marx *et al.*, 2010); and (2) in unreported analyses, I determined that a change in non-compete enforcement had no significant impact on inventors' skills (as measured by educational attainment) or salary (available on request).

Second, this study relies on the assumption that companies actually use non-competes. If they did not, any increase in enforcement would have little influence on companies' R&D choices. However, evidence provided by Kaplan and Stromberg (2003) and Holley (1998) strongly indicates that companies use non-competes whenever possible.

Third, restricting the sample to public companies implies a need to conduct additional studies with private companies, which likely differ in several dimensions. The ownership structure of a firm may directly influence its corporate risk taking (e.g., Jensen and Meckling, 1976; May, 1995). In turn, the same degree of non-compete enforcement could have differential impacts for public versus private corporate decisions to pursue risky but high potential R&D projects. Moreover, focusing on just public companies prevents us from assessing the overall impact of non-competes. Therefore, this study cannot offer any clear-cut social welfare implications regarding changes to non-compete enforcement.

Fourth and finally, I measured inventive performance using forward citations to patents, which creates a biased measure of failure. That is, I can observe only patented inventions that receive no forward citations; I cannot observe real failures, such as R&D projects that never lead to any patents. In addition, forward citations have another shortcoming: non-competes reduce inventors' mobility, which means they could also have a direct negative impact on the amount of forward citations, which are proxies for knowledge spillovers. However, in this case, a stricter non-compete enforcement would decrease the likelihood of extremely valuable inventions, which is the opposite of what results show.

Despite these limitations, this study provides important insights for managers and policy makers. Practitioners have shown growing interest in the relationship between business activity and institutional environment, and government policies have strong influence on companies' economic value (McKinsey Global Survey, 2010). Among the different activities that governments perform, passing laws and enforcing regulations likely have the greatest effect on business management. This study helps clarify the relationship between non-compete regulation and strategic R&D management. In particular, the argument and findings support the idea that mobility-induced knowledge leakages are asymmetric with respect to the degree of risk in R&D strategies. Therefore, R&D managers should choose their risk levels according to the extent to which they can use non-competes and appropriate returns from R&D. They should undertake risky trajectories only if they can prevent employees from leaving and taking their knowledge with them.

Another implication relates to the management of R&D in multilocation corporations. To the extent that a company can choose to allocate projects to different locations, it should strategically move the riskiest R&D projects to areas where non-compete enforcement is strict or, more generally, where IPR protections are strong.

Whereas this study considers the degree of R&D riskiness as something firms can choose, some risk is inherent in R&D and, thus, beyond managerial control. In the long run, even laws may be the object of organizational strategies, though (Ingram and Silverman, 2002). Companies operating in highly uncertain technological sectors (i.e., where R&D is very risky) have more to

DOI: 10.1002/smj

gain from stronger appropriability regimes, so they should proactively interact with the government to encourage increased enforcement of non-competes.

Finally, by offering information that can support decisions by policy makers, this study should indirectly improve the interactions between business and government. Managers complain that regulators do not fully understand the economic impact of government policies (McKinsey Global Survey, 2010). For example, my findings indicate that non-compete agreements create, at the regional level, a trade-off between entrepreneurship and intrapreneurship. They 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 existing companies and enhance the likelihood that corporate inventions will be pathbreaking. The extent to which policy makers should favor exploration by entrepreneurship, rather than exploration by intrapreneurship, remains an interesting question for further research.

ACKNOWLEDGEMENTS

I thank Coeditor Rich Bettis and two anonymous reviewers for their precious help and suggestions. I am gratefully indebted to my dissertation committee Alfonso Gambardella, Anita McGahan, and Giovanni Valentini for their help and encouragement. Thanks to Giovanni Valentini in particular for his patience and guidance through the review process. I thank Ashish Arora, Francesco Castellaneta, Dietmar Haroff, Joseph Mahoney, Myriam Mariani, Marcello Miccoli, Mario Morroni, Elena Novelli, Claudio Panico, Martina Pasquini, Francisco Veloso, and Dennis Yao for useful discussions, as well as seminar participants at Boston University, Catolica Lisbon, ESMT, Harvard Business School, Georgia Tech, Rotman, University of Illinois, and University of Munich, Vlerick, for comments. All remaining errors are mine. I thank Mark Garmaise for graciously providing access to his data.

REFERENCES

Agarwal R, Ganco M, Ziedonis RH. 2009. Reputations for toughness in patent enforcement: implications for knowledge spillovers via inventor mobility. *Strategic Management Journal* **30**(13): 1349–1374.

Ahuja G, Lampert CM. 2001. Entrepreneurship in the large corporation: a longitudinal study of how established firms create breakthrough inventions. *Strategic Management Journal* **22**(6/7): 521–543.

Ahuja G, Lampert CM, Tandon V. 2008. Moving beyond Schumpeter: managerial research on the determinants of technological innovation. *Academy of Management Annals* **2**(1): 1–98.

Ai D, Norton EC. 2003. Interaction terms in logit and probit models. *Economics Letters* **80**(1): 123–129.

Albert MB, Avery D, Narin F, McAllister P. 1991. Direct validation of citation counts as indicators of industrially important patents. *Research Policy* **20**(3): 251–259.

Alcacer J, Zhao M. 2012. Local R&D strategies and multi-location firms: the role of internal linkages. *Management Science* **58**(4): 734–753.

Argote L, Beckman S, Epple D. 1990. The persistence and transfer of learning in industrial settings. *Management Science* **36**(2): 140–154.

Atanassov J. 2013. Do hostile takeovers stifle innovation? Evidence from antitakeover legislation and corporate patenting. *Journal of Finance* **68**(3): 1097–1131.

Autor D. 2003. Outsourcing at will: the contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics* **21**(1): 1–41.

Bertrand M, Duflo E, Mullainathan S. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* **119**(1): 249–275.

Bessen J. 2009. Matching patent data to Compustat firms. Working paper, National Bureau of Economic Research, Cambridge, MA. Available at: www.nber.org/~jbessen/matchdoc.pdf (accessed 15 May 2013).

Cabral L. 2000. *Introduction to Industrial Organization*. MIT Press: Cambridge, MA.

Cabral L. 2003. R&D competition when firms choose variance. *Journal of Economics and Management Strategy* **12**(1): 139–150.

Fleming L. 2007. Breakthroughs and the 'long tail' of innovation. *MIT Sloan Management Review* **49**(1): 69–74.

Fleming L, Singh J. 2010. Lone inventors as sources of breakthroughs: myth or reality? *Management Science* **56**(1): 41–56.

Franco A, Mitchell MF. 2008. Covenants not to compete, labor mobility, and industry dynamics. *Journal of Economics and Management Strategy* **17**(3): 581–606.

Furman J. 2003. Location and strategy: exploring the role of location in the organization of pharmaceutical research laboratories. *Advances in Strategic Management* **20**: 49–88.

Gambardella A, Harhoff D, Verspagen B. 2008. The value of European patents. *European Management Review* **5**(2): 69–84.

Garmaise M. 2011. Ties that truly blind: non-competition agreements, executive compensation and firm investment. *Journal of Law, Economics, and Organization* **27**(2): 376–425.

Gilsing VA, Nooteboom B, Vanhaverbeke WPM, Duijsters GM, van den Oord A. 2008. Network embeddedness and the exploration of novel technologies: technological distance, betweenness centrality, and density. *Research Policy* **37**(10): 1717–1731.

Gilson RJ. 1999. The legal infrastructure of high technology industrial districts: Silicon Valley, Route 128, and covenants not to compete. *New York University Law Review* **74**: 575–629.

Ginarte JC, Park WG. 1997. Determinants of patent rights: a cross-national study. *Research Policy* **26**(3): 283–301.

Hall BH, Jaffe AB, Trajtenberg M. 2001. The NBER patent citations data file: lessons, insights and methodological tools. Working paper 8498, National Bureau of Economic Research, Cambridge, MA. Available at: http://www.nber.org/papers/w8498 (accessed 29 May 2013).

Hall BH, Jaffe AB, Trajtenberg M. 2005. Market value and patent citations. *RAND Journal of Economics* **36**(1): 16–38.

Hall P, Soskice D (eds). 2001. *Varieties of Capitalism: The Institutional Foundations of Comparative Advantage*. Oxford University Press: Oxford, U.K.

Harhoff D, Narin F, Scherer FM, Vopel K. 1999. Citation frequency and the value of patented inventions. *Review of Economics and Statistics* **81**(3): 511–515.

Henderson R. 1993. Underinvestment and incompetence as responses to radical innovation: evidence from the photolithographic industry. *RAND Journal of Economics* **24**(2): 248–270.

Holley R. 1998. Looking at the legalities of non-compete agreements. *Computer Reseller News* **800**: 139.

Ingram P, Silverman B. 2002. Introduction: the new institutionalism in strategic management. *Advances in Strategic Management* **19**: 1–32.

Jensen MC, Meckling WH. 1976. Theory of the firm: managerial behavior, agency cost, and capital structure. *Journal of Financial Economics* **3**: 305–360.

Kanwar S, Evenson R. 2003. Does intellectual property protection spur technical change? *Oxford Economic Papers* **55**(2): 235–264.

Kaplan SN, Stromberg P. 2003. Financial contracting theory meets the real world: an empirical analysis of venture capital contracts. *Review of Economic Studies* **70**(2): 281–315.

Kerr WR, Nanda R. 2009. Democratizing entry: banking deregulations, financing constraints, and entrepreneurship. *Journal of Financial Economics* **94**(1): 124–149.

Levin RC, Klevorick AK, Nelson RR, Winter SG, Gilbert R, Griliches Z. 1987. Appropriating the returns from industrial research and development: comments and discussion. *Brookings Papers on Economic Activity* **3**: 783–831.

Malsberger B. 2004. *Covenants Not to Compete: A State-by-State Survey*. BNA Books: Washington, D.C.

March JG. 1991. Exploration and exploitation in organizational learning. *Organization Science* **2**(1): 71–87.

Marx M, Singh J, Fleming L. 2010. Regional disadvantage? Non-competes and brain drain. SSRN working paper. Available at http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1654719 (accessed 20 June 2013).

Marx M, Strumsky D, Fleming L. 2009. Mobility, skills, and the Michigan non-compete experiment. *Management Science* **55**(6): 875–889.

May D. 1995. Do managerial motives influence firm risk-reduction strategies? *Journal of Finance* **50**(4): 1291–1308.

McKinsey Global Survey. 2010. How business interacts with government. Available at: https://www.mckinseyquarterly.com/Public_Sector/Management/How_business_interacts_with_government_McKinsey_Global_Survey_results_2495?pagenum=2#1 (accessed 15 May 2013).

Nelson R, Winter S. 1982. *An Evolutionary Theory of Economic Change*. Belknap Press: Cambridge, MA.

Papke L, Wooldridge J. 1996. Econometric methods for fractional response variables with an application to 401(k) plan participation rates. *Journal of Applied Econometrics* **11**: 619–632.

Pe'er A, Gottschalg O. 2011. Red and blue: the relationship between the institutional context and the performance of leveraged buyout investments. *Strategic Management Journal* **32**(12): 1356–1367.

Phene A, Fladmoe-Lindquist K, Marsch L. 2005. Breakthrough innovations in the U.S. biotechnology industry: the effect of technological space and geographic origin. *Strategic Management Journal* **27**(4): 369–388.

Png I. 2012. Law and innovation: evidence from state trade secrets laws. Available at: http://www.comp.nus.edu.sg/∼ipng/research/2012_r&d.pdf (accessed 15 May 2013).

Psaty K. 2010. HubSpot CEO Brian Halligan: a bro for all the right reasons. Available at: http://bostinno.streetwise.co/2010/02/24/hubspot-ceo-brian-halligan-a-bro-for-all-the-right-reasons (accessed 27 May 2013).

Puhani AP. 2008. The treatment effect, the cross difference, and the interaction term in nonlinear difference-in-differences models. IZA working paper 3478. Available at: http://ftp.iza.org/dp3478.pdf (accessed 27 May 2013).

Qian Y. 2007. Do additional national patent laws stimulate domestic innovation in a global patenting environment? *Review of Economics and Statistics* **89**(3): 436–453.

Rothschild M, Stiglitz JE. 1970. Increasing risk: I. a definition. *Journal of Economic Theory* **2**(3): 225–43.

Sakakibara M, Branstetter L. 2001. Do stronger patents induce more innovation? Evidence from the 1988 Japanese patent law reforms. *RAND Journal of Economics* **32**(1): 77–100.

Samila S, Sorenson O. 2011. Non-compete covenants: incentives to innovate or impediment to growth? *Management Science* **57**(3): 425–438.

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

Shaver JM, Flyer F. 2000. Agglomeration economies, firm heterogeneity, and foreign direct investment in the United States. *Strategic Management Journal* **21**(12): 1175–1193.

Stuart TE, Sorenson O. 2003. Liquidity events and the geographic distribution of entrepreneurial activity. *Administrative Science Quarterly* **48**(2): 175–201.
Thompson P. 2005. Patent citations and the geography of knowledge spillovers: a reassessment. *American Economic Review* **95**(1): 450–460.
Trajtenberg M. 1990. A penny for your quotes: patent citations and the value of innovations. *RAND Journal of Economics* **21**(1): 172–187.
Zhao M. 2006. Conducting R&D in countries with weak intellectual property rights protection. *Management Science* **52**(8): 1185–1199.

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. Project B is riskier than Project A, according to the classical definition by Rothschild and Stiglitz (1970), because B is a mean-preserving spread of A: $\pi_B = \pi_A + z$, where $z \sim F(z)$ is a random variable with the same support of π_A and with $E(z|\pi_A) = 0$ for all π_A. By definition, $E(\pi_B) = E(\pi_A)$.

Once a project turns out to be profitable, the employee working on it leaves with probability λ and, in that 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 (2011). 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 1.

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 1.

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 1.

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

DOI: 10.1002/smj

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 1.

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
Louisiana	4	Utah	6
Maine	4	Vermont	5
Maryland	5	Virginia	3
Massachusetts	6	Washington	5
Michigan	5	West Virginia	2
Minnesota	5	Wisconsin	3
Mississippi	4	Wyoming	4
Missouri	7		

Source: Garmaise (2011).