How innovating firms manage knowledge leakage: A natural experiment on worker mobility*

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Abstract

Innovating firms face a risk of knowledge leakage as their workers can exit employment to join competitors. We study worker mobility as the key mechanism through which firms decide on strategies to protect innovation outputs. Our empirical analysis exploits a 1998 court case decision whereby the California Courts of Appeal ruled that out-of-state non-compete agreements are not enforceable in California. Consequently, non-California firms faced a loophole in the enforcement of non-competes for their previously bound workers. When facing the higher mobility of existing workers, firms strategically increase patent filings as a means of knowledge protection. Further tests support our theoretical account that worker mobility plays a crucial role in patenting decisions. The importance of worker mobility and *leakage-by-leaving* problem has significant scholarly and managerial implications.

Keywords: innovation strategy, knowledge management, patents, worker mobility, out-of-state non-competes
JEL Classification: O32, J61, K31, G34

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1 INTRODUCTION

Firms in knowledge-based industries must constantly innovate to create a competitive advantage (Agarwal, Ganco, & Ziedonis, 2009; Almeida & Kogut, 1999; Argote & Ingram, 2000; Barney, 1991; Campbell, Ganco, Franco, & Agarwal, 2012b; Coff, 1997; Gambardella, 1992; Hall, 1992; Helfat *et al.*, 2009; Teece, Pisano, & Shuen, 1997). To sustain their competitive advantage and fuel further innovation, however, firms must also protect their knowledge against leakage to competitors. While both creation and protection of knowledge are fundamental to firm performance, studies have paid relatively little attention to the latter.

Protecting knowledge is challenging because knowledge is carried by individual workers (Grant, 1996; Simon, 1991; Song, Almeida, & Wu, 2003). Innovating firms constantly face the risk of knowledge leakage because workers who possess important knowledge can exit employment to join competitors or start their own business (*leakage-by-leaving*). The mobility of individual workers is thus one of the most important factors that determines knowledge leakage and misappropriation risks faced by a firm. Despite its importance, we do not have a good understanding of how firms protect their knowledge in response to worker mobility.

We study worker mobility as the key mechanism through which firms decide on strategies to protect knowledge, in particular, patent filings. We argue that firms dynamically adjust their knowledge protection strategies as the mobility of their workers changes. If employers can retain their workers, the knowledge embedded in these individuals remains within the firm boundaries (e.g., complete secrecy); in this case, there is little reason for firms to patent an invention with the cost of disclosure. If increased worker mobility deteriorates firms' ability to retain workers, on the other hand, firms should seek alternative measures to appropriate and protect their innovation outputs. Thus, firms will increase the use of patenting, as a knowledge protection strategy, when facing heightened worker mobility.

To establish a causal relationship between worker mobility and firms' strategic choices on patenting, we take advantage of a court decision that exogenously increased the mobility of workers to California. *Application Group, Inc. v. Hunter Group, Inc.*, 61 Cal. App. 4th 881 (1998)—henceforth, *Application v. Hunter* (1998)—provides us with a nearly ideal setting for our experiment. In the United States, many firms prevent their employees from joining competitors by requiring them to sign a non-competition agreement (henceforth "non-competes"), a contract in which an employee agrees not to work with a different firm in direct competition with the current employer for a certain amount of time in a specified area of expertise once their current employment ends (Garmaise, 2011; Marx & Fleming, 2012; Prescott, Bishara, & Starr, 2016; Starr, Prescott, & Bishara, 2019). In *Application v. Hunter* (1998), the California Courts of Appeal refused to enforce *out-of-state* non-competes written between a non-California employer and a non-California employee. This court decision made California emerge as a "loophole" state in non-compete enforcements: *non*-California employees who were bound by non-competes could now move to California employers; the ability of non-California employers to retain their workers via non-competes has since significantly and abruptly decreased.

Using a difference-in-differences methodology, we find that firms, on average, increased patent filings by about 3.7% (and up to 21% for large firms), whereas other qualitative characteristics of patents remain unchanged. Further analyses support that worker mobility is the key mechanism in play. The findings are greater in magnitude and more precisely estimated for firms that are in complex product industries, are in fast-growing industries, are moderately large in terms of the number of inventors, and are in states in which the migration rate to California is high and where the court decision was more salient. Further analysis of public companies confirms that the increased patent filings primarily come from firms that possess trade secrets and do not result from higher R&D investments. The combined findings suggest that firms *strategically* increased patent filings to protect their innovations in response to the heightened mobility of workers between jobs and state lines.

This study contributes to a broad stream of strategy and innovation literature. We highlight that a patent may not be a simple function of innovation inputs or knowledge *creation* as often presumed in prior studies. Our findings reveal that knowledge *protection* strategies can significantly drive patenting decisions and that firms strategically set and dynamically change their patenting strategies in response to various conditions, notably the mobility of workers. In addition, our study investigates a novel mechanism that explains how firms choose between different knowledge protection strategies. Prior research on this topic has primarily focused on institutions, such as trade secret laws, that *directly* cause one more effective than another. We demonstrate that broader contextual factors—namely, the mobility of individual workers—could also substantially change the relative efficacy of knowledge protection devices and subsequently affect how firms choose their knowledge protection strategies. Furthermore, we establish that legal enforcement in one state could have far-reaching consequences outside of the focal state and propose a novel and robust identification strategy that exploits this feature.

2 WORKER MOBILITY, KNOWLEDGE LEAKAGE, AND PATENTING

Firms have a range of options when it comes to the protection of knowledge: patents, secrecy, lead time advantages, and the use of complementary assets or capabilities (Cohen, Nelson, & Walsh, 2000). One of the most effective—yet underemphasized—option is patenting (Anton & Yao, 2004; Arora, 1997; Png, 2017b). Patenting is a form of intellectual property that provides formal legal protection of knowledge for a limited period—up to 20 years from the filing date under the U.S. patent law (Bessen & Maskin, 2009; Galasso & Schankerman, 2014; Green & Scotchmer, 1995; Huang & Murray, 2009). Patents effectively prevent others from using the patented knowledge for their own benefit (Agarwal *et al.*, 2009; Gallini, 1992; Gilbert & Shapiro, 1990; Klemperer, 1990;

Somaya, 2012). A major disadvantage of patenting, however, is in its disclosure risk. In exchange for formal protection, patent applicants must publicly disclose the technical details of the inventions that they seek to protect (and that may trigger imitation and reverse engineering by competitors). Patent registration fees, maintenance fees, payments to patent attorneys, and legal uncertainty are important costs and other risks that incur to patenting firms (Kitch, 1977; Teece, 1986; Williams, 2013). As such, prior research shows that firms also utilize different protection mechanisms, including, but not limited to, secrecy, lead time advantage, complementary sales and services, and complementary manufacturing (Arundel, 2001; Cohen *et al.*, 2000; Hall, Helmers, Rogers, & Sena, 2014; Png, 2017b)

How then do firms choose between patenting and alternative devices when protecting their proprietary knowledge? Studies have shown that firms react to changes that make one more effective than others (Teece, 1986). Contigiani, Hsu, and Barankay (2018) and Png (2017b), for example, examine how firms choose between patents and trade secrecy in response to the expansion of employer-friendly trade secrecy protection. Whereas these studies establish that firms pay keen attention to changing incentives to different knowledge protection devices, little has been studied about the *mechanisms* that indirectly affect a firm's decision to patent their knowledge (for one exception see Kim & Marschke, 2005).

We highlight worker mobility as one such mechanism. Individual workers absorb, possess, and cumulate the knowledge and information, even if the whole innovation process is governed by a firm (Grant, 1996; March, 1991). Worker mobility thus is one of the most important factors that facilitate knowledge flows between firms (Arrow, 1972). As Simon (1991; P. 125) puts it: "all learning takes place inside individual human heads," and organizations learn by "ingesting new members who have knowledge the organization didn't previously have." Proliferating studies on learning-by-hiring likewise suggest that firms can leverage hiring as an opportunity to absorb external knowledge (Palomeras & Melero, 2010; Rosenkopf & Almeida, 2003; Singh & Agrawal, 2011; Song *et al.*, 2003; Stolpe, 2002).

From the perspective of employers that lose a worker, however, worker mobility substantially increases a risk to the firm in the form of knowledge leakage and misappropriation (Agarwal *et al.*, 2009; Campbell, Coff, & Kryscynski, 2012a; Carnahan, Agarwal, & Campbell, 2012; Gambardella, Ganco, & Honoré, 2014; Ganco, Ziedonis, & Agarwal, 2015). In particular, worker mobility to competitors is a *double* loss for the prior employer who not only loses (at least partially) its proprietary knowledge as their workers exit employment (Simon, 1991), including business secrets and existing customers, but also gives rise to a competitor's competitive advantage (Agarwal, Campbell, Franco, & Ganco, 2016; Agarwal, Echambadi, Franco, & Sarkar, 2004; Agarwal *et al.*, 2009; Campbell *et al.*, 2012a; Somaya, Williamson, & Lorinkova, 2008; Wezel, Cattani, & Pennings, 2006).

We predict that firms will increase their use of patents when their workers become more mobile for three reasons. First, the efficacy of patents is not dependent on the mobility (or the retention) of workers because the details of invention and the patent assignee firms and inventors are specified in the patent document and protected by law. On the other hand, other protection devices, such as secrecy, generally become more vulnerable to leakage as workers increasingly move between firms. Second, worker mobility increases firms' incentives to *preemptively* file a patent under its own name, before exiting workers do so (often with their new employers). Preemptive patenting minimizes misappropriation risks and potential patent infringement litigations that arise when workers with valuable knowledge leave their employers (Ceccagnoli, 2009; Gilbert & Newbery, 1982). Third, patenting is an effective way to gain bargaining power against mobile workers who possess valuable knowledge. Understanding that they carry valuable knowledge with them, workers may leverage their knowledge to increase their bargaining power and demand higher pecuniary or non-pecuniary benefits. By obtaining formal protection for their inventions by patents, firms could counter workers who try to bargain. All these arguments suggest that firms will increasingly use patents to protect their knowledge (even without any changes in fundamental innovation activities) when facing higher mobility of workers.

3 EMPIRICAL STRATEGY

3.1 Setting: *Application v. Hunter* (1998)

A correlational study on worker movements and patenting would be subject to endogeneity problems. An unobservable confounding factor, such as a firm's ability to identify and attract talented or fitted workers, may be correlated with both worker mobility and patent filings. Reverse causality is another concern; firms that increase their propensity to patent may consequently exert less effort to retain their inventors. Kim and Marschke (2005) formally model worker moves, patenting, and R&D decisions, but, as they note, their empirical analyses (which focus on publicly traded firms and realized worker moves) are correlational and do not rule out the endogeneities.

We exploit the *Application v. Hunter* (1998) decision by the California Courts of Appeal as a naturally occurring experiment[50]. California is known for its strong public policy against the enforcement of restrictive covenants in employment. Since the enactment of the California Business & Professional Code Section 16600 ("Section 16600") in 1872, California has consistently not been enforcing *in-state* non-competes (as agreed on between a California employer and employee). However, *out-of-state* non-competes signed by an employer and employee *outside* of California had been construed as enforceable in California (see Wu, 2003).

Application v. Hunter (1998) was the first legal case to establish that out-of-state non-competes are not enforceable in California (see Kahn (1999) and Online Appendix A for a review). In 1998, the California Courts of Appeal made a final decision that enforcing out-of-state non-competes in California would violate its public policy. The decision significantly affected both non-California employers and employees' *beliefs* about the enforceability of their non-competes in California, particularly employers who pay keen attention to the business environment and have better access to legal counsel and experts. In other words, *non*-California firms faced an unexpected loophole in the enforceability of their non-competes.

An ideal experiment that addresses endogeneity concerns would involve randomly manipulating how firms are affected by the mobility of workers, and *Application v. Hunter* (1998) provides such research setting. First, an individual or a firm could exert little influence on the court decision (as opposed to policy or legislative changes), and the decision was not affected by prior discussions or public hearing, mitigating the enodogeneity concern of the decision. Second, even if the court decision were correlated with local business or legal environments in California (e.g., lobbying), we can circumvent this endogeneity problem by examining firms that do business *outside* of California. Third, most businesspeople and legal practitioners believed that California did not have the rights to refuse to enforce out-of-state non-competes, especially those with a choice-of-law provision (where parties agreed on a particular jurisdiction to resolve their disputes), further ensuring the unexpecptedness and exogeneity of the decision (Wu, 2003).

3.2 Methodology

We estimate the difference-in-differences model by exploiting the *Application v. Hunter* (1998) decision. This unexpected and unprecedented court decision significantly increased the *mobility* (not necessarily realized *moves*) of workers from non-California firms to California firms. Our focus is *not* on firms in California but on firms in all other states in the United States. We compare firms in states that strongly enforce non-competes (treatment group) with those in states that do not or weakly enforce non-competes (control group), before and after the year of the decision, 1998. The main idea of this strategy is that *Application v. Hunter* (1998) only affected firms in the treatment group by creating a loophole in the enforcement of non-competes and did not affect firms in the control group as these firms were unable to enforce non-competes even before the decision. This approach, along with firm and year fixed effects, helps us account for unobservable differences between the two groups. We estimate the following difference-in-differences model:

$$y_{ist} = Enforce_s \cdot Post_t + \delta_i + \gamma_t + \epsilon_{ist} \quad (1)$$

where y_{ist} is the natural log transformation of our outcomes of interest. We use both the Garmaise (2009) and Starr (2019) indices to determine the state-level enforceability of non-competes. We create a state-level indicator, $Enforce_s$, that takes unity if a state's enforceability is above the mean score in both indices ("strong enforcement") and zero if it is below the mean score in both indices ("weak enforcement"). This approach is doubly robust because the two independent indices consistently assigned a high or low score for a state (see Online Appendix B). $Post_t$ is an indicator that equals one after 1998. The remaining terms δ_i and γ_t are firm and year fixed effects.

We also conduct more flexible econometric analysis by replacing $Post_t$ with year indicators (distributed leads and lags), leaving out a year indicator for 1998 as a baseline. With this flexible estimation in Equation (2), we not only explicitly test the parallel trend assumption for pre-treatment years (1994–1997) but also examine the dynamic patterns of the effects (e.g., one-time adjustment vs. gradual increase) for post-treatment years (1999–2002):

$$y_{ist} = \sum_{k=1994,k\neq1998}^{2002} Enforce_s \cdot \mathbf{1}\{t = k\} + \delta_i + \gamma_t + \epsilon_{ist} \quad (2)$$

3.3 Data and sample

We use PatentsView (December 2019 version), which provides detailed information on patent file and grant dates, technology classes, claims, assignee firms, and inventors with disambiguated identifiers, their location, and citations. Additional analyses of publicly traded companies in Online Appendix E use CRSP/Compustat-Merged data for R&D expenditures and other information.

Our sample selection begins with the universe of patent assignees that have ever filed a patent in the United States from 1994 to 2002. We confine our interest to patent assignees that are companies or corporations and exclude individuals and government institutions as they have been little affected, if any, by *Application v. Hunter* (1998). We further exclude firms in three states that underwent significant changes in the enforceability of non-competes during our sample period: Florida, Louisiana, and Texas (Garmaise, 2011; Kang & Fleming, 2020). Assignee firms in Alaska and Hawaii are also omitted to account for geographic barriers that restrict interstate mobility. Finally, to be included in the sample, assignee firms must have at least one inventor during the five years before the decision (1993–1997). This minimal restriction allows us to filter out firms that have no inventor to retain and thus face a limited threat of worker mobility. The resultant sample consists of 21,989 assignee firms with 367,689 patent filings. Table 1 provides descriptive statistics.

4 RESULTS

4.1 Main results

Table 2, column (1), reports the main results of our difference-in-differences estimation on patent filings. We find that firms in the high-enforcing states increased their patent filling by about 3.7% compared to low-enforcing states, after *Application v. Hunter* (1998). Firms in our sample filed on average 6.43 patents per year during the sample period; the 3.7% increase in patent filings is thus equivalent to 0.242 more patents per year, for every year following the decision. The event-study framework with distributed leads and lags allows for a more flexible and detailed estimation.

Figure 1 illustrates the flexible difference-in-differences results, which reconfirm our findings. A parallel trend persists until 1998, and the treatment group increased its patent filings by, on average, 3.5% right after the decision. Finally, to further deal with the pre-treatment trend, we include interaction terms between each firm's outcome variable (in logs) in each pre-1998 year and a full set of year dummies. This specification absorbs all the pre-1998 differences in patent filings in our analyses and some of the post-1998 variation, making our post-1998 comparisons close to ceteris paribus (Cantoni, Dittmar, & Yuchtman, 2018). Our results from this strict specification, which are reported in Online Appendix C, again confirm that the firms in the treatment group increased their patent filing by about 7.2% after the 1998 decision.

4.2 Heterogeneity by firm size

We expect that firms will respond differently depending on their size (i.e., how many inventors they employ). Larger firms will increase patent filings more than smaller firms. Firms with more inventors face a much higher risk of worker job hopping and knowledge leakage. Furthermore, larger firms face lower marginal costs of patenting, enjoy economies of scale, and have better access to patent attorneys and lawyers. We empirically explore the heterogeneous effects by firm size, measured by how many (unique) inventors a firm employed from 1993 to 1997.

Figure 2 shows the results from split-sample analyses employing the firm size categories used in the Business Dynamics Statistics (BDS). As predicted, the effects are greater for large and medium-sized firms. Large firms (with 50–99 inventors) filed 21% more patents after the decision, equivalent to 3.06 more patents per year per firm. Medium-sized firms (with 25–49 inventors) increased their patent filings by 9%, or 0.74 more patents per year per firm. The remaining firm size categories (tiny, small, and huge) show small effects (0%–3%) that are imprecisely estimated with large standard errors. As already discussed, small firms typically do not achieve the economies of scale to access patent attorneys, and they are likely to have already patented their inventions to send signals to investors and to the market (Agarwal *et al.*, 2009; Conti, Thursby, & Thursby, 2013; Hsu & Ziedonis, 2008). Interestingly, the effects for huge firms (100 or more inventors) were not significantly larger than that for small and tiny firms. One possibility is that huge firms—that face a substantially higher risk of mobility and low marginal costs to patenting—have already filed patents on important inventions even before the decision.

4.3 Robustness checks

To ensure the validity of our findings, we first check the robustness of our model choices against the count data. Poisson quasi-maximum likelihood estimation provides an effective way to model count data that have an excess number of zero counts. The findings remain consistent and robust (see Online Appendix D for detailed estimates with a set of standard error choices). Second, we test the qualitative characteristics of patents to see whether firms begin to patent a different set of inventions in response to worker mobility.

In Table 2, columns 2 and 3, we do not find a meaningful change in the number of backward and forward citations, which are said to be highly correlated with patent quality or the market value of an innovation (Hall, Jaffe, & Trajtenberg, 2005; Lampe & Moser, 2016; Trajtenberg, 1990). We also find from the public firm sample that the average commercial value of patents did not change (see Online Appendix E.2). In addition, the number of patent claims, the number of inventors per patent, and the length of patent examination did not change around the 1998 decision, as shown in Table 2, columns 4, 6, and 7. Interestingly, the number of words used in the first claim—which effectively captures the breadth of patent scope (Kuhn and Thompson, 2019)—decreased by 3.5%, or 5.3 words, in Table 2, column 5 ($p = 0.094$). In other words, firms pursued a broader range of protection, because we interpret fewer words to mean fewer restrictions and a broader scope. This result is consistent with our theoretical account that firms increased their patent filings to protect their knowledge against the heightened mobility of workers. Other than the scope of patents, we do not find evidence that firms changed the qualitative characteristics of the patents they filed.

5 FURTHER ANALYSES OF THE MECHANISMS

Industry product type (discrete vs. complex). The effectiveness of patenting varies across industries, especially by the technological characteristics of different products. Technologies and inventions in *discrete* product industries compose relatively few patentable elements, and patents and alternative protection devices are less substitutable; knowledge in *complex* product industries, on the other hand, consists of numerous patentable elements, making it easier to switch to patents from alternative strategies (Cohen *et al.*, 2000; Contigiani *et al.*, 2018; Png, 2017b). If our theoretical argument is correct, and firms indeed shifted toward patenting to protect their knowledge, we should find a larger effect for firms in complex product industries. The results in Table 3, columns 1 and 2, confirm this prediction.

Technology field dynamism (Fast-growing vs. stationary). Fast-growing and expanding industries exhibit a higher rate of innovation. Firms in such industries face higher risks of knowledge leakage via worker mobility to competitors and thus have a greater incentive to protect their knowledge with patents. Firms in stationary industries, on the other hand, have relatively flat and static information and do not compete as fiercely for knowledge. The results in Table 3, columns 3 and 4, shows that the patenting effects are greater and more precisely estimated ($p < 0.01$) for fast-growing industries, where the mobility of workers is much more highly associated with valuable knowledge leakage. The estimate is not distinguishable from 0 for stationary industries ($p = 0.131$).

Interstate mobility (high vs. low migration rate to California). States exhibit different migration rates to California. If worker mobility to California is the underlying mechanism, we should observe stronger patenting effects from firms in states that have a high migration rate to California. We use the County-to-County Migration Flow Files from the 2000 U.S. Census to measure state-level moves to California in 1995. Using these data on the ex ante migration rate of workers to California, we run a split-sample analysis for firms in states that are above and below the median migration rate of workers to California, respectively. We find a larger (4.2%) and statistically significant ($p = 0.002$) effect in the above-median sample (Table 3, columns 5 and 6); in contrast, we find little effect in the below-median sample.

High salience in Maryland. The plaintiff in *Application v. Hunter* (1998), Hunter Group, Inc., is a Maryland corporation headquartered in Maryland. It is thus expected that the decision and the loophole in non-competes enforcement it created is better understood in Maryland. Table 3, column 7, tests this by including only Maryland firms in the treatment group. Maryland firms increased patent filings by about 10%, more than twice as much as other treated firms.

Patents by individuals. We run a placebo test looking at patent filings by individual inventors, who are not affiliated with an organization. We do not find any increase in patent filings by individuals; the point estimate is negative and not distinguishable from zero in column 8, Table 3.

R&D investment of public firms. An alternative explanation is that the increase in patent filings comes from higher R&D investments, not from a need to protect their knowledge against the heightened mobility of workers. We test how firms changed their R&D investments. With the caveat that the sample consists of publicly traded firms, we find that firms increased patent filings by 11.8% but do not find evidence that firms meaningfully increased R&D investment, following *Application v. Hunter* (1998). In other words, our results are not driven by fundamental changes in R&D activities (Png, 2017a; Png, 2017b). We also find that firms with trade secrets increased patent filings more than did firms without, a finding that further supports our theoretical account (see Online Appendix E for additional theoretical developments and results).

6 DISCUSSION AND CONCLUSION

We examine worker mobility as a key mechanism that determines how innovating firms manage knowledge leakage. To causally identify the effects, we take advantage of a milestone court decision in California that created a loophole in the enforcement of non-competes for *non-*California firms. When facing a higher mobility of existing workers, firms—medium and large-sized firms, in particular—increased their patent filings, but most of their other qualitative characteristics remained unchanged. A higher propensity to patent with a broader scope suggests that firms' key motivation for patenting is knowledge protection in the face of workers exiting to work for competitors. Further tests on industry technology type, industry maturity, the salience of the decision in Maryland, worker migration rate to California, and patents by individuals bolster our theoretical account and findings that worker mobility mainly drives protective patent filings. Additional analyses of public firms further confirm that patenting is not the outcome of an increase in innovation inputs (R&D) but a response to the risk of knowledge leakage.

Our empirical analysis adopts a novel identification strategy that merits further discussion. When using an event in California as a naturally occurring experiment and studying its impact on California firms, one may be concerned that factors that affect the event may influence the outcomes of interest. Our empirical approach mitigates this endogeneity concern by comparing outcomes of treated and control firms *outside* of California, which are unlikely to be correlated with the factors that affect a California court's decision.

This study provides several important implications outside of academia as well as further research opportunities. First, we have shown how legal enforcement in one state has far-reaching consequences outside of the focal state. That is, business environments that shape firm strategies are not limited to the local environment but include broader policy and legal institutions. Business managers and policy makers should thus carefully consider how local policies and laws spill over borders. Second, that firms patent strategically implies that the fundamental innovation activities of firms may not well be captured by patent-based proxies. Studies using patent-based proxies to measure innovation rely on an implicit assumption that patent filings are solely determined by knowledge creation considerations, including fundamental R&D investments. Our findings suggest that knowledge protection considerations could also significantly drive patenting decisions. We suggest that researchers carefully examine the validity of such measures. Third, our finding that firms increased their propensity to patent suggests that innovating firms disclose more information on their inventions in exchange for legal protection. An interesting future avenue would be to investigate how disclosures affect the rate and direction of follow-on innovations. We hope that this study connects studies on worker mobility and innovation strategies and leads to a better understanding of how innovating firms create, acquire, and protect knowledge.

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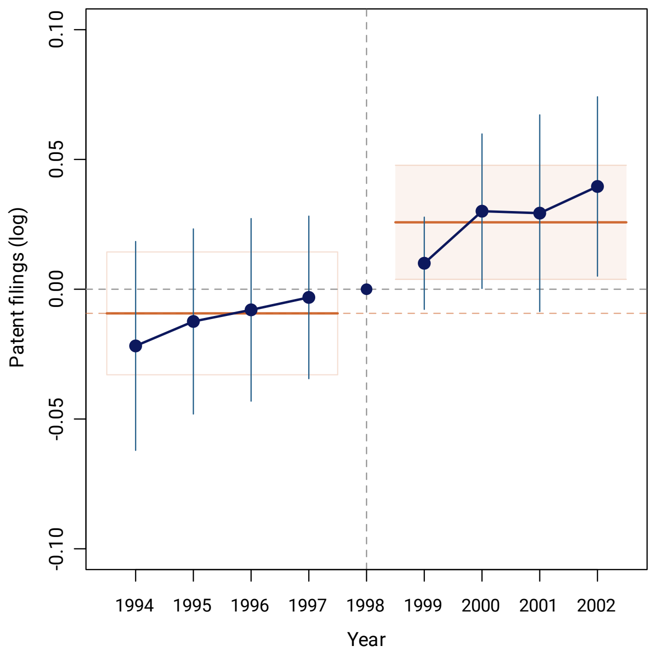
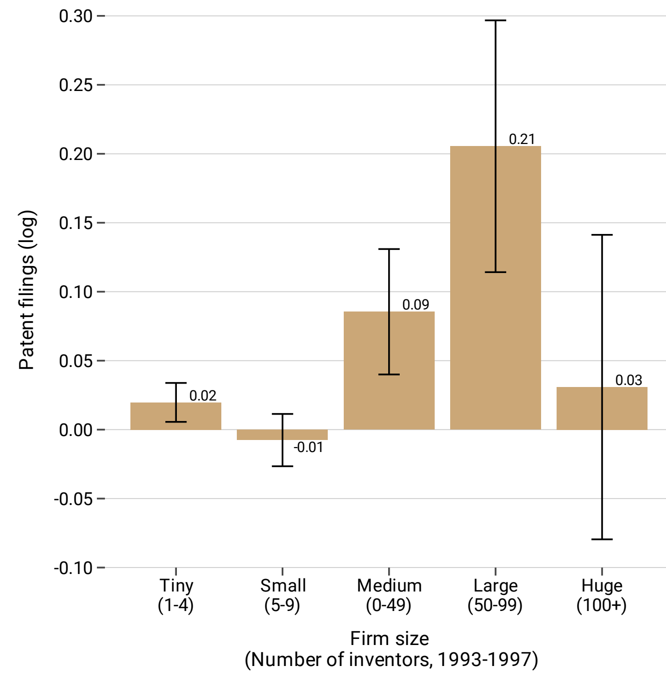
Figure 1.
Effects of worker mobility on patent filings:
Event study approach



Notes. The graphs illustrate the results from two different econometric estimations. First, the blue dots represent estimates in flexible difference-in-differences model interacted with year indicators (event study approach). The blue vertical lines represent the 95% confidence interval. Second, the red horizontal lines represent estimates in the difference-in-differences model with aggregated indicators for pre- and post-1998 periods. Boxes around the horizontal lines represent the 95% confidence interval. In both models, the year of the court decision, 1998, is used as a baseline (an omitted category). Standard errors are clustered at the state level. *Source*: PatentsView.

Figure 2.
Effects of worker mobility on patent filings:
Heterogeneity by firm employment size



Notes. This bar plot illustrates estimates from five separate difference-in-differences models by firm size, measured by the five-year inventor stock during 1993–1997. We use firm size categories adopted in Business Dynamics Statistics (BDS) by the U.S. Census. We combine all firms that have more than 100 inventors due to the small sample size. Vertical lines represent the 95% confidence interval. Standard errors are clustered at the state level. The regression estimates, standard errors (in parentheses), and *p*-values (in brackets) are 0.020, (0.014), and [0.172] for tiny firms (N = 28,878); -0.008, (0.019), and [0.692] for small firms (N = 9,110); 0.085, (0.045), and [0.070] for medium-sized firms (N = 9,498); 0.205, (0.091), and [0.033] for large firms (N = 1,539); and 0.031, (0.110), and [0.782] for huge firms (N = 1,922). *Source*: PatentsView.

Table 1. Main variables and summary statistics

	Description	Mean	SD	Min	Max
Patent filings	log of the average number of eventually granted patent applications by a firm	1.159	0.810	0.00	8.398
Backward citations	log of the average number of patents a firm's patents cite ("citing")	2.527	0.793	0.00	7.055
Forward citations	log of the average number of patents that cites the focal patent ("cited by")	2.789	1.118	0.00	7.669
Number of claims	log of the average number of patent claims a firm's patents have	2.649	0.796	0.693	6.501
Number of words in the first claim	log of the average number of words used in the first claim of patents that a firm filed	4.804	0.760	1.386	8.593
Number of inventors	log of the average number of inventors named on a firm's patents	1.079	0.348	0.693	3.434
Examination length (days)	log of the average days of patent examination for a firm's patents	6.617	0.434	3.638	8.945
Industry dynamism (industry level)	The compound annual growth rate of patent filings at the 3-digit CPC level for 1993–1997	0.064	0.068	-0.070	0.534
Migration rate to CA (state level)	The ratio of each state's outflow moves to California between 1995 and 2000 to the state's population in 2000	0.008	0.009	0.001	0.050
Patents filed by individuals	log of the number of eventually granted patent applications by inventors, but not assignee firms	0.883	0.362	0.693	3.611

Notes. This table reports summary statistics for variables used in the analyses. When we use the logarithm, it is the natural logarithm of the variable plus one.

Table 2. Effects of worker mobility on patents

	Dependent variables (log):						
	Quantity	Qualitative characteristics of patents (firm-level average)					
	Patent filings	Backward citations ("citing")	Forward citations ("cited by")	Number of claims	Number of words in the first claim	Number of inventors	Examination length (days)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enforce×Post	0.037	–0.003	–0.016	–0.013	–0.035	0.008	0.005
	(0.012)	(0.030)	(0.057)	(0.031)	(0.020)	(0.011)	(0.011)
	[0.006]	[0.916]	[0.781]	[0.681]	[0.094]	[0.499]	[0.674]
Unit FE	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Time FE	Year	Year	Year	Year	Year	Year	Year
R^2	0.810	0.687	0.743	0.688	0.713	0.705	0.625
Adjusted R^2	0.667	0.451	0.548	0.452	0.496	0.481	0.341
Observations	50,947	50,947	50,947	50,947	50,945	50,947	50,944

Notes. This table reports regression coefficients from seven regressions based on Equation (1). The sample includes all patent assignees that had at least one inventor from 1993 to 1997. The dependent variable consists of the number of patent filings (column 1), the average number of backward citations made (column 2), the average number of forward citations received per patent (column 3), the average number of claims per patent (column 4), the average number of words used in the first claim per patent (column 5), the average number of inventors per patent (column 6), and the average length of patent examination (i.e., the days between patent filing and registration; column 7). The variables *Enforce* and *Post* are absorbed by the firm and year fixed effects. Standard errors, clustered at the state level, are provided in parentheses. *p*-values are provided in brackets.

Table 3. Additional analyses of the key mechanism: Worker mobility and knowledge leakage

	Dependent variables: patent filings (log)							
	By industry product type		By technology field dynamism		By state: Migration rate to CA		MD firms as a treated group	Patents filed by individuals only
	Discrete	Complex	Fast-growing	Stationary	High	Low		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enforce×Post	0.006 (0.009) [0.492]	0.043 (0.016) [0.013]	0.031 (0.011) [0.011]	0.026 (0.017) [0.131]	0.042 (0.011) [0.002]	0.022 (0.019) [0.271]	0.097 (0.008) [0.000]	–0.020 (0.032) [0.536]
Unit FE	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Industry, State
Time FE	Year	Year	Year	Year	Year	Year	Year	Year
R^2	0.783	0.801	0.814	0.788	0.817	0.803	0.808	0.306
Adjusted R^2	0.619	0.651	0.673	0.628	0.678	0.655	0.659	0.254
Observations	50,947	50,947	50,947	50,947	25,795	25,152	12,885	2,144

Notes. This table reports regression coefficients from eight regressions based on Equation (1). The sample includes all patent assignees that had at least one inventor from 1993 to 1997. The dependent variable consists of the number of patent filings in discrete product industries (column 1), the number of patent filings in the complex product industries (column 2), the number of patent filings in the fast-growing technology fields (column 3), the number of patent filings in the stationary technology fields (column 4), the number of patent filings by firms in states that exhibit high migration rate to California (column 5), the number of patent filings by firms in states that exhibit low migration rate to California (column 6), the number of patent filings with only Maryland firms in the treatment group (column 7), and the number of patent filings by individual inventors only (column 8). For columns 1 and 2, following Vonortas and Kim (2004) and Cohen *et al.* (2000), we code industries with SIC codes less than 35 as discrete product industries; those with SIC codes 35 and above were coded as complex product industries. We identified patents in discrete versus complex product industries using Silverman (2002) IPC-US SIC concordance. For columns 3 and 4, we calculated the compound annual growth rate of patent filings at the 3-digit CPC level for 1993–1997. Technology fields above the median growth rate (5.1%) were coded as fast-growing technology fields; those below the median growth rate were coded as stationary technology fields. For columns 5 and 6, we constructed the migration rate to California variable as the ratio of each state's outflow moves to California between 1995 and 2000 to the state's population in 2000, using the County-to-County Migration Flow Files from the 2000 U.S. Census. Alternatively, we also use the Job-to-Job Flows (J2J) Data for 2000 (the earliest year available) from the Census Longitudinal Employer-Household Dynamics (LEHD). The findings are robust to this alternative measure of interstate job moves. For column 7, we only include Maryland firms in our treatment group. For column 8, we look at patent filings by individual inventors only (i.e., those not associated with any business). The variables *Enforce* and *Post* are absorbed by the firm and year fixed effects. Standard errors, clustered at the state level, are provided in parentheses. *p*-values are provided in brackets.

Online Appendix

Table of Contents

A Litigation Timeline: *Application v. Hunter* (1998)

California is known for its strong public policy against the enforcement of restrictive covenants in employment, including the enforcement of voluntarily entered non-competes (we use the term "non-competes" to refer to non-compete clauses/agreements). The most relevant statute is California Business & Professional Code Section 16600 ("Section 16600"), which states that "except as provided in this chapter, every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void."

Since the 1872 enactment of Section 16600, California has consistently refused to enforce *in-state* non-competes, that is, non-compete agreements between a California employer and employee. However, *out-of-state* non-competes, which are signed by an employer and employee *outside* of California, have been construed as enforceable under California law (for a review, see Wu, 2003).

Application v. Hunter (1998) was the first legal case to establish that out-of-state non-competes are also not enforceable in California, even with the presence of a "choice-of-law" provision in which the contracting parties specify that any dispute arising under the contract shall be determined under the law of a particular jurisdiction (for a detailed review of this case, see Kahn, 1999).

In 1992, Pike, a consultant in computerized human resources management systems, resigned from Hunter Group Inc. ("Hunter") to take a position at a competing firm in California, known as Application Group, Inc. ("AGI"). Pike had signed a non-compete agreement with Hunter prohibiting her from working for a competing firm for one year after the termination of her employment. Their contract also included a "choice-of-law" provision, which specifically stated that the contract should be "governed by and construed in accordance with the laws of the State of

Maryland." As such, this provision allowed Hunter to contend that legal disputes on the contract, including its non-compete agreement, must be decided by a court in Maryland, a state where non-competes are enforceable.

Both firms took instant but separate actions after Pike resigned from Hunter to join AGI. In 1992, Hunter sued both Pike and AGI in the Maryland Circuit Court for a breach of contract and unlawful interference. AGI, on the other hand, filed a complaint to California courts for a declaratory judgement, arguing that California's Section 16600 rather than Maryland law should be applied to this case. The Maryland Circuit Court favored AGI in its decision, noting that Hunter did not provide enough evidence to claim damages. This decision allowed California courts to proceed with their requests with AGI's declaratory relief, which was pending Maryland Court's decision.

In January 1995, the case proceeded to California trial courts. In trial court, Judge Norman originally issued a statement of decision that denied AGI's claims for declaratory relief (January 30, 1995). However, in response to AGI's objections, Judge Norman issued a revised statement of decision that, for the most part, ruled that California law applies to AGI's hiring of Hunter employees (April 5, 1995). On June 15, 1995, the trial court's judgment was entered that California law should indeed apply to the hiring of Pike. The final decision was made by the California Courts of Appeal in February 1998. The decision affirmed the trial court's decision that enforcing out-of-state non-competes in California would violate the state's public policy, even if the contract was signed between a Maryland firm and a Maryland resident and included a choice of law provision (*Application v. Hunter*, 1998).

It is worth comparing the *Application v. Hunter* (1998) with a recent legislative change made by California (Section 925). In January 2017, California added a new statute, Section 925, to the California Labor Code. The key objective of this amendment is to establish a statute that restricts the use of choice-of-law and forum selection clauses by California firms with workers who primarily reside and work in California, in addition to existing restrictions on in-state non-competes (California Business and Professions Code Section 16600: "Code 16600"). Historically, California firms that sign non-competes have tried to strategically avoid being adjudicated under the California law by introducing choice-of-law and forum selection clauses, stating that their choice of law clauses could be governed in courts of their own choice, in their non-competes. The purpose and effect of Section 925, therefore, is fundamentally different from those of *Application v. Hunter* (1998) in the sense that the former prohibits non-competes between a California employer and employee (who is a California resident) from being adjudicated outside California court, whereas the latter concerns a non-California worker moving from a non-California employer to a California employer.

B Non-compete Enforceability Indices: Garmaise (2011) and Starr (2018)

Garmaise (2011) developed an index that quantifies the state-level enforceability of non-competes. Across twelve dimensions of enforceability, Garmaise's assigns 1 point for each dimension if the state's enforcement of non-competes in that dimension exceeds a given threshold. A possible value for the index ranges from 0 to 12 with a higher point indicating stronger enforceability. Building on the work of Bishara (2010), Starr (2019) also developed a state-level non-compete enforceability index. Expanding on Bishara's state-level ranking of seven dimensions of enforceability, Starr further implemented confirmatory factor analysis to reweight different factors and normalized the score to take the standard normal distribution.

Each index has its advantages and disadvantages. To determine the enforceability of state-level non-competes, we use both the Garmaise (2011) and Starr (2019) indices. We create a state-level indicator, $Enforce_s$, that equals one if a state's enforceability is above the mean score in both indices ("strong enforcement") and zero if it is below the mean score in both indices ("weak enforcement"). This approach is doubly robust, because the two independent indices consistently assigned a high (higher than or equal to 5 for Garmaise *and* higher than or equal to 0 for Starr) or low score for a state. We exclude states where Garmaise and Starr indices are conflicting ("unclear"). Table B.1 compares the three—Garmaise, Starr, and ours—indexes.

Table B.1. Three indices of non-compete enforceability

State	**Garmaise** (score as of 1997)	**Starr** (score as of 1991)	**Combined indicator** ($Enforce_s$)
Alabama	5	0.36	Strong enforcement
Alaska	3	-0.98	Weak enforcement[a]
Arizona	3	0.15	Unclear
Arkansas	5	-0.58	Unclear
California	0	-3.79	Weak enforcement[a]
Colorado	2	0.38	Unclear
Connecticut	3	1.26	Unclear
Delaware	6	0.52	Strong enforcement
District of Columbia	7	0.12	Strong enforcement
Florida	9	1.60	Strong enforcement[a]

Georgia	5	0.02	Strong enforcement
Hawaii	3	-0.17	Weak enforcement[a]
Iowa	6	1.01	Strong enforcement
Idaho	6	0.77	Strong enforcement
Illinois	5	0.95	Strong enforcement
Indiana	5	0.70	Strong enforcement
Kansas	6	1.21	Strong enforcement
Kentucky	6	0.85	Strong enforcement
Louisiana	4	0.50	Unclear[a]
Massachusetts	6	0.48	Strong enforcement
Maryland	5	0.60	Strong enforcement
Maine	4	0.41	Unclear
Michigan	5	0.46	Strong enforcement
Minnesota	5	-0.07	Unclear
Missouri	7	1.08	Strong enforcement
Mississippi	4	0.04	Unclear
Montana	2	-0.65	Weak enforcement
North Carolina	4	0.18	Unclear
North Dakota	0	-4.23	Weak enforcement
Nebraska	4	-0.13	Weak enforcement
New Hampshire	2	0.26	Unclear
New Jersey	4	0.90	Unclear
New Mexico	2	0.74	Unclear
Nevada	5	0.03	Strong enforcement
New York	3	-1.15	Weak enforcement
Ohio	5	0.08	Strong enforcement
Oklahoma	1	-0.94	Weak enforcement
Oregon	6	0.14	Strong enforcement
Pennsylvania	6	0.14	Strong enforcement
Rhode Island	3	-0.33	Weak enforcement
South Carolina	5	-0.27	Unclear
South Dakota	5	1.02	Strong enforcement
Tennessee	7	0.45	Strong enforcement
Texas	3	-0.28	Weak enforcement[a]
Utah	6	1.00	Strong enforcement
Virginia	3	-0.29	Weak enforcement
Vermont	5	0.60	Strong enforcement
Washington	5	0.34	Strong enforcement
Wisconsin	3	-0.09	Weak enforcement
West Virginia	2	-0.80	Weak enforcement
Wyoming	4	0.23	Unclear

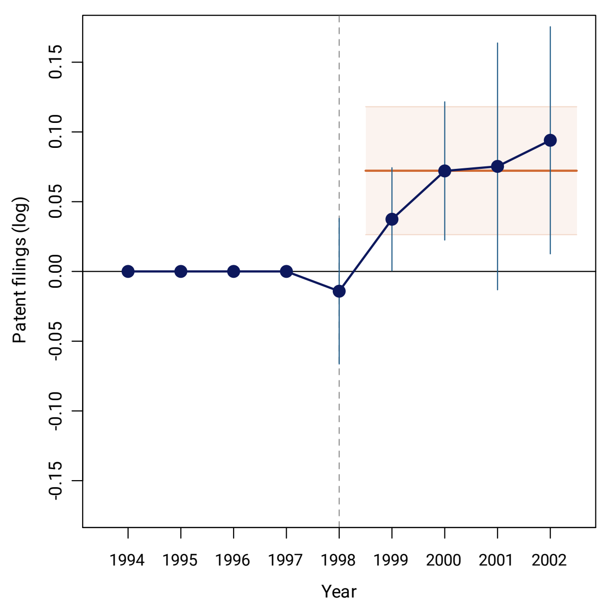
[a] We exclude assignee firms in three states that underwent significant changes in the enforceability of non-competes during our sample period: Florida (1996), Louisiana (2001, 2003), and Texas (1994) (Garmaise, 2011; Kang & Fleming, 2020). Assignee firms in Alaska and Hawaii also have been omitted to account for geographic barriers that restrict interstate mobility.

C Dealing with Preexisting Trends

In the main analyses reported in the paper, we find a parallel trend in patent filings before the year of decision, 1998. In this section, we additionally conduct an analysis that allows the pre-1998 outcome variable to affect the post-1998 outcome variable. That is, we include interaction terms between each firm's outcome variable (in logs) in each pre-1998 year and a full set of year dummies. By absorbing all the pre-1998 differences in patent filings and some of the post-1998 differences, this analysis makes the post-1998 comparisons close to ceteris paribus (for more details on this analysis, see Cantoni, Dittmar, & Yuchtman, 2018).

Figure C.1 illustrates the results for patent filings and R&D expenditures. By design, there are no pre-1998 differences in trends between the treatment and control groups in this specification. We again confirm from this strict specification that the firms in the treatment group increased their patent filing by about 7.2% (SE = 0.023, *p*-value = 0.005) after the 1998 decision.

Figure C.1. Effects of worker mobility on patent filings: Absorbing pre-trends in an event study approach



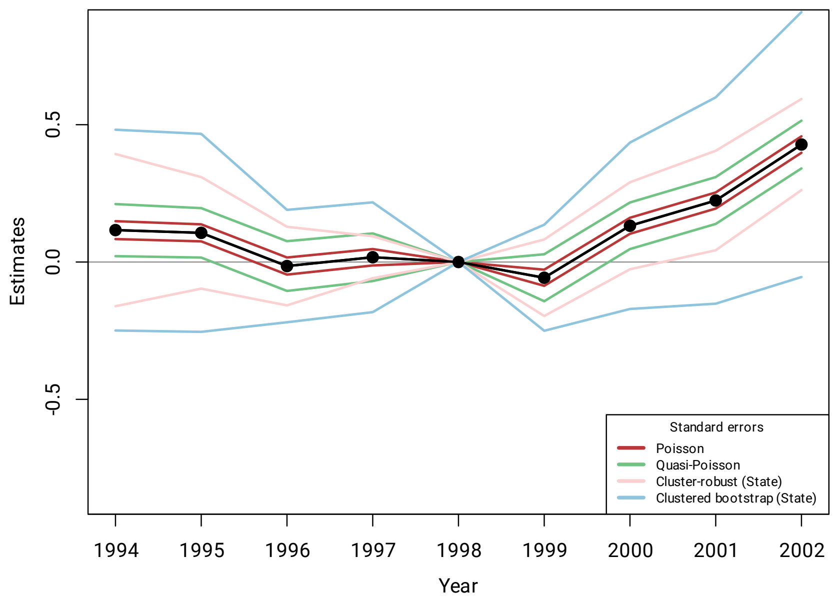
D Poisson Quasi-Maximum Likelihood Estimation

We check whether our results are robust to alternative model choices. The Poisson regression model effectively deals with count data that have an excess number of zero counts. Compared to alternative count models, such as the negative binomial, the Poisson model is more robust to distributional misspecification, even if the data-generating process is misspecified, as long as the conditional mean is correctly specified (Cameron & Trivedi, 2013). The Poisson regression model, however, relies on the assumption that the conditional mean and variance are the same, although in many cases, including our data, the variance is larger than the mean. The Poisson quasi-maximum likelihood estimator (QMLE) relaxes this assumption and estimates the overdispersion parameter (ϕ) from the data.

The Poisson QMLE estimates coefficients that are identical to those obtained via the Poisson model, but the former model leads to *larger* standard errors, because it accounts for the overdispersion parameter when estimating standard errors (i.e., the standard Poisson model underestimates standard errors in the presence of overdispersion). As such, in the Poisson QMLE model, standard errors need to be adjusted for the clusters in which errors are correlated; otherwise, standard errors tend to overstate estimator precision, leading to absurdly small standard errors (Cameron & Miller, 2015). We ran our main analysis using the Poisson QMLE model, instead of an ordinary least squares (OLS) model, to compare different types of standard errors.

Figure D.1 shows the results. We present different standard errors for comparison, including nonparametric clustered bootstrap standard errors based on 10,000 repetitions. We find a statistically significant increase in patenting intensity for the years after *Application v. Hunter* (1998) across all types of standard errors. However, standard errors based on Poisson and quasi-Poisson are clearly underestimated (these do not account for correlation within clusters), whereas bootstrapping provides more conservative standard errors. In sum, that loglinear OLS estimation and the Poisson QMLE produce similar results assures us that that our findings are not sensitive to our model choices.

Figure D.1. Effects of worker mobility on patent filings: Poisson quasi-Maximum likelihood estimation



Notes. This figure shows difference-in-differences estimates from the Poisson quasi-maximum likelihood estimation. The dispersion parameter for the quasi-Poisson family is 8.452, suggesting the presence of overdispersion in our sample. We provide four different standard errors for comparison. *Source*: PatentsView.

E Analysis of Public Firms

E.1 Sample Comparison: PatentsView versus CRSP/Compustat-Merged Data

In this section, we empirically examine how firms changed their innovation input, namely, R&D investments, around *Application vs. Hunter* (1998). Ideally, we would want to examine the R&D investments of all firms in our sample used for our main analysis. However, because information on R&D investments is often considered confidential information that has important strategic value, it is difficult to obtain such data for all patenting firms, especially for private companies. Using the CRSP/Compustat-Merged Data, we focus on all *publicly traded firms* in the United States that are required to disclose such information. Kogan, Papanikolaou, Seru, and Stoffman (2017) provide the *bridge* between Compustat firms (GVKEY) and their patents (patent ID).

Because there is a hugely significant discrepancy about which firms are covered in each data, we first compare the size of firms, measured by the number of inventor stocks from 1993 to 1997. There clearly exists a huge difference in firm sizes between the two data, as shown in Table E.1. The CRSP/Compustat-Merged data cover a much smaller number of larger firms. The Compustat data cover only 2% of the firms covered by PatentsView. Furthermore, the meaning of a "firm" differs between the two data sets. The assignee firm in the patent data refers to the smallest business unit that file patents under its name, whereas a firm in the CRSP/Compustat-Merged data refers to a company (issue, currency, index) in the CRSP/Compustat file (GVKEY or PERMNO). The latter is generally broader than the former, and a company in the CRSP/Compustat file often holds multiple patent assignee firms. This further complicates the issue because one company could hold patenting assignee firms in different states. Therefore, the high level of aggregation in the CRSP/Compustat data makes these data less desirable for studying state-level outcomes. At a minimum, we note that the results from these two different data sets cannot be compared at the same level, and one should be very careful if linking and interpreting the results.

Table E.1. Comparison of firm sizes in PatentsView and Compustat

Firm size	Mean	SD	First quantile	Second quantile	Third quantile	Observations
PatentsView (All patenting firms)	7.9	86.2	1.0	2.0	4.0	49,319
CRSP/Compustat-Merged (All patenting *public* firms)	85.0	442.2	5.0	13.0	37.0	1,003

Note. Firm size is measured by the number of (unique) inventor stock from 1993 to 1997.

E.2 Patenting and R&D Expenditures of Public Firms

We first examine the patent filings of public firms in the Compustat sample. In Table E.2, column 1, we consistently find that firms increased patent filings by 11.8% (p-value = 0.006). The magnitude of the effect is larger compared with the 3.7% increase for all patenting firms. As discussed in the main paper, large firms have superior resources and capabilities and enjoy economies of scale when filing a patent.

We also find that the firms did not increase their R&D investment, although they increased patent filings. In Table E.2, column 5, we cannot reject the null hypothesis that the estimated coefficient is equal to zero. Note that information on R&D expenditures is not available for every firm; only 50.3% of observations have valid information on R&D expenditures. Some firms do not invest in R&D projects and therefore have no information on R&D expenditures. Some firms have missing information for random years.

Thus, with the caveat that the results from Compustat sample are not readily generalizable to private firms and many observations for R&D expenditures are missing, we conclude that the increased patent filings indeed come from changes in knowledge protection strategies, not from fundamental R&D activities.

E.3 Testing the Mechanism: Trade Secrets

We further compare firm responses in patenting and R&D investment depending on whether or not they have trade secrets. Firms with trade secrets face a higher risk of knowledge leakage when the worker exits employment, so we expect that firms with trade secrets respond more strongly than do those without.

We identify firms with trade secrets from 10-K discussions of trade secrecy (Glaeser, 2018). Regulation S-K requires public firms with valuable trade secrets to discuss the risk of trade secret misappropriation in the 10-K, without revealing the nature of the secret. We consider a firm to have a trade secret if it discusses trade secrets in its 10-K at least once during the period of 1994–2002.

Table E.2, columns 2 and 3, shows that firms with trade secrets increased their patent filings by 16.2% (more than the average effect, which is 11.8%; p-value = 0.061). The estimate for firms without trade secrets, in contrast, is smaller in magnitude (8.2%) and not statistically significant (p-value = 0.14). Columns 6 and 7 explore R&D expenditures. R&D activities do not differ across firms with or without trade secrets. The results from the sample of publicly traded firms additionally support our theoretical argument that the main findings are due to changes in knowledge protection strategies to cope with worker mobility, which poses a risk to the firm in the form of knowledge leakage.

Table E.2. Additional analyses of the knowledge protection mechanisms

	Dependent variables (log):						
	Patent filings			*Patent commercial value*	*R&D expenditure*		
	Full sample	Trade secret: Yes	Trade secret: No		Full sample	Trade secret: Yes	Trade secret: No
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Post×Enforce	0.118	0.162	0.082	0.026	0.055	0.047	0.044
	(0.042)	(0.086)	(0.055)	(0.048)	(0.035)	(0.064)	(0.028)
	[0.006]	[0.061]	[0.140]	[0.585]	[0.119]	[0.468]	[0.125]
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.338	0.361	0.325	0.916	0.980	0.961	0.989
Adjusted R^2	0.170	0.198	0.152	0.894	0.976	0.953	0.986
Observations	10,541	4,469	6,072	9,346	17,323	7,118	11,494

Notes. This table reports regression coefficients from the sample of publicly traded firms. *Source*: CRSP/Compustat-Merged data.

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