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# Trade secrets and innovation: Evidence from the "inevitable disclosure" doctrine

Andrea Contigiani | David H. Hsu | Iwan Barankay

Management Department, The Wharton School, University of Pennsylvania, Philadelphia, Pennsylvania

**Correspondence**
David H. Hsu, Management Department, The Wharton School, University of Pennsylvania, 3620 Locust Walk, 2000 Steinberg Hall-Dietrich Hall, Philadelphia, PA 19104-6340.
Email: dhsu@wharton.upenn.edu

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Mack Institute for Innovation Management, Wharton School, University of Pennsylvania

***Research Summary:*** Does heightened employer-friendly trade secrecy protection help or hinder innovation? By examining U.S. state-level legal adoption of a doctrine allowing employers to curtail inventor mobility if the employee would "inevitably disclose" trade secrets, we investigate the impact of a shifting trade secrecy regime on individual-level patenting outcomes. Using a difference-in-differences design taking unaffected U.S. inventors as the comparison group, we find strengthening employer-friendly trade secrecy adversely affects innovation. We then investigate why. We do not find empirical support for diminished idea recombination from suppressed inventor mobility as the operative mechanism. While shifting intellectual property protection away from patenting into trade secrecy has some explanatory power, our results are consistent with reduced individual-level incentives to signaling quality to the external labor market.

***Managerial Summary:*** While managers often list trade secrecy protection as their most important appropriation mechanism form and basis of competitive advantage (even more often than formal intellectual property protection), researchers have a hard time studying the effect of such secrets. We use a changing trade secrecy legal environment in some U.S. states (the introduction of the inevitable disclosure doctrine [IDD]) to study its effect on inventor-level innovation. We find that a strengthened employer-friendly trade secrecy regime adversely affects inventor-level innovation. While part of the effect is due to substituting trade secrecy protection for patents, we also find that inventors' diminished external labor market prospects may dampen their innovation output. Consequently, while employers may wish for strengthened trade secrecy protections, the results may paradoxically be against their innovation interests.

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**KEYWORDS**
innovation, inter-firm mobility, knowledge workers, labor markets, trade secrets

## 1 | INTRODUCTION

When surveyed about the most common modality for appropriating returns from both their product and process innovations, industrial managers most frequently respond that trade secrecy and the closely related mechanism of lead time advantage are by far the most important channels (e.g., Cohen, Nelson, & Walsh, 2000). Despite the stated importance of the secrecy channel, the empirical social science literature has predominately focused on patents, which are rated as far less important in the surveys as a means of appropriating returns from innovation. The likely reason for this mismatch is observability, a prerequisite for empirical analysis: Patent protection is granted in exchange for detailed disclosure, while managers have an economic incentive to keep trade secrets secret (discovering trade secrets via legal channels such as accidental disclosure, independent discovery, and backward engineering invalidates them).[1] The empirical literature on trade secrecy has therefore been understandably sparse,[2] even while the theoretical literature suggests that the most important commercial business ideas are likely to be protected via trade secrets, especially in weak property rights regimes (Anton & Yao, 2004).

Law and economics perspectives of trade secrecy law suggest its evolution arising from the need to balance efforts to protect trade secrets (e.g., Friedman, Landes, & Posner, 1991), and more specifically, the interests of employers (in appropriating returns from their human capital investments) with those of employees (in self-determining career choice) (e.g., Fisk, 2001). This balance likely affects innovation processes and incentives. However, it is not theoretically clear how shifts in trade secrecy protection strength might impact innovation outcomes.

On one hand, a more employer-friendly trade secrecy regime may incentivize employers to increase their investments in employee firm-specific human capital since the frictions to employee mobility are enhanced. This may boost innovation outcomes. On the other hand, the innovation literature has stressed the need for idea recombination for innovation outcomes (e.g., Fleming, 2001). To the extent that employer-friendly trade secrecy regimes place frictions on the circulation of individuals and ideas across technical, organizational, and geographic boundaries, innovation outcomes may consequently be dampened (e.g., Hellmann & Perotti, 2011). Another set of theories, rooted in labor market dynamics, also predicts a negative relation between a more employer-friendly trade secrecy regime and innovation outcomes. This is due to, among other things, muting employees' ability to use the external labor market to signal their quality (which could otherwise be useful in obtaining improved employment terms). Higher thresholds for labor market mobility stemming from an

[1]Patent-based intellectual property protection is a useful contrast. To obtain a patent in the United States, an invention should be novel (compared to the "prior art"), non-obvious, and useful. The quid pro quo of 20 years of patent protection of an invention is granted in exchange for detailed, codified invention disclosure. By comparison, trade secrets can apply beyond technical domains, and may include a broad spectrum of dimensions of business competitiveness (and so the scope of what could be protected by trade secrecy is much wider). Trade secrets protection against unlawful procurement can extend indefinitely and is adjudicated at the U.S. state level, as compared to patents, which are of fixed length and enforced at the federal level.

[2]See Moser (2012), Castellaneta, Conti and Kacperczyk (2017), and Png (2017b) for recent exceptions.

employer-friendly trade secrecy regime may therefore be associated with diminished employee innovation effort and outcomes.

Because the theoretical relationship between trade secrecy regime and innovation outcomes is ambiguous (in large part because the production function of innovation is complex, multidimensional, and uncertain), we seek to examine it empirically. We do so by exploiting a context afforded by shifting trade secrecy legal regimes (outside of contractual agreements) in a set of U.S. states to estimate the effect of trade secrecy environment on innovation outcomes. The "inevitable disclosure" doctrine (IDD) holds that courts may enjoin employees from switching employers for a certain period of time if plaintiffs can show that it would not be possible for the employees to perform their job without *inevitably disclosing* their respective prior employer's trade secret (more on this in the next section). The prior literature on inventor human capital and organizations has tended to focus on contract law, either nondisclosure agreements or covenants not to compete, though that literature has not clearly established the effect on innovation outcomes. By contrast, we examine trade secrecy law, in part, because there are a range of circumstances tied to competitive situations in which contractual agreements are typically not struck between or among parties. This is due to, among other things, differences in corporate strategies and possible difficulties in governing such arrangements.

Using a difference-in-differences empirical design, we find that the average effect of IDD on innovation quality, as measured by forward-citation-weighted patent counts, is negative, both at the inventor-year and U.S. state-year levels of analysis. We then explore the mechanisms that may be driving this effect. We first examine whether this negative effect is driven by a substitution away from the less effective appropriation channel (patenting) and into the strengthened channel (trade secrecy). We then analyze the salience of two theories predicting a negative relationship, mitigated inventor idea circulation resulting in lower idea recombination and muted inventor incentives arising from frictions to labor market mobility. By presenting empirical evidence on a subsample of data in which substitution between patenting and trade secrecy is unlikely, we suggest that the negative effect is not entirely driven by changing intellectual property protection strategy from patenting to secrecy.[3] While we are unable to provide a sharp test of the operative mechanisms, our additional analyses are most consistent with the dampened labor market incentives mechanism, not the recombination channel.

The remainder of the article takes the following form. Section 2 discusses the institutional context: U.S. trade secrecy law and IDD. Section 3 summarizes the theoretical perspectives that may impact the relationship between IDD and innovation outcomes. Section 4 describes the data set. Section 5 presents the main analyses and empirical tests of possible mechanisms. A final section concludes by discussing limitations and implications.

## 2 | U.S. TRADE SECRECY LAW AND THE INEVITABLE DISCLOSURE DOCTRINE (IDD)

A critical period in the evolution of U.S. legal thought on trade secrecy law was in the 1890 to 1920 time frame. The norm in the antebellum United States was to presume that workers owned the rights to their ideas, unless there was an express contractual covenant to the contrary, under the thought that employer property was confined to physical manifestations of employer knowledge such as

[3]We also contextualize the other part of the sample that may be subject to intellectual property substitution in the face of altered trade secrecy business environments, and discuss (in the Appendix) why the decline in innovation outcomes in the remainder of the sample is unlikely to be predominantly driven by secrecy substitution for patenting.

laboratory notebooks and physical equipment (Fisk, 2001). This sentiment started to change in the 1890s, and eventually, shifted so that employer trade secrets expanded from discrete items to more inchoate employee know-how (Fisk, 2001).

The Uniform Trade Secrets Act (UTSA) of 1979 (and amended in 1985) was an attempt to bring some degree of national uniformity to the law of trade secrets (encompassing the definition of a trade secret, the requirements for protection, and the remedies for misappropriation) since the historical origins of the U.S. law arises from English common law—and so there was little uniformity across states in the definition and application of trade secrecy (National Conference of Commissioners on Uniform State Laws, 1985). According to the UTSA, which as of 2013 had been ratified by 47 state legislatures (albeit over a prolonged time span), a trade secret is a piece of information (including formulas, patterns, compilations, programs, devices, methods, techniques, or processes) that (a) derives economic value, actual or potential, from not being generally known to persons outside the organization; and (b) is the subject of efforts that are reasonable under the circumstances to maintain its secrecy. While both parts of the definition are subject to interpretation, the second part is particularly so. For example, the standard for misappropriation is *not* whether a trade secret was discovered via illegal means.[4]

An early expression of the legal theory of "inevitable disclosure"—the idea that a departing employee will "inevitably" disclose trade secrets, even without the intent of disclosing trade secrets (due to the inherent difficulty of compartmentalizing know-how), was a ruling by an early 20th century court: "Equity has no power to compel a man who changes employers to wipe clean the slate of his memory." (*Peerless Pattern Co. v. Pictorial Review Co.*, 132 N.Y.S. 37, 39 [App. Div. 1911] cited in Fisk, 2001, p. 494).

The key contemporary case applying the inevitable disclosure doctrine is *PepsiCo, Inc. v. Redmond* (1995), which is widely-acknowledged as a legal turning point in the application of IDD. The *PepsiCo* case is interpreted as broadening the scope of the IDD doctrine beyond technical trade secrets, which was the traditional domain (e.g., Kahnke, Bundy, & Liebman, 2008; Wiesner, 2012). In the case, William Redmond, a sports drink manager at PepsiCo in the early 1990s, accepted a job at a competing sports drink company, Quaker, in 1994. PepsiCo filed suit in the 7th District Court in Illinois, arguing that Redmond had access to its strategic and operating plans (trade secrets related to pricing, distribution, packaging, and marketing), and that he could not perform his new job at Quaker without inevitably disclosing PepsiCo's trade secrets. In the words of the court: Unless "Redmond possessed an uncanny ability to compartmentalize information, he would necessarily be making decisions about Gatorade and Snapple by relying on his knowledge of trade secrets."[5] Redmond did not have a noncompete clause in place with PepsiCo (he was an employee at-will), and Redmond had an explicit confidentiality agreement in place with Quaker prohibiting him from disclosing others' trade secrets. Despite these contractual agreements, in December 1994, the court enjoined Redmond from taking the new position through May 1995.

Before describing our data and analysis on the impact of IDD on innovation, we first review the theories that may impact the relationship between an altered trade-secrecy regime and individual-level innovation.

[4]One notable case, *DuPont v. Christopher* (431 F.2d 1,012, 5th Circuit, 1970), involved a low-flying aircraft taking aerial pictures of an under-construction DuPont methanol manufacturing plant that was fenced-off on the ground. The court held that while not illegal to take aerial pictures, the defendant had wrongfully misappropriated DuPont's trade secrets through its photographic industrial espionage.

[5]U.S. Court of Appeals for the Seventh Circuit (54 F.3d 1,262, 7th Circuit, 1995).

## 3 | THEORETICAL BACKGROUND

The theoretical literature relating trade secrecy strength to innovation outcomes suggests opposing predictions. A positive relation may arise if employers are incentivized to enhance their investments in employee skill development and human capital as a result of the greater cost of employee departure under an employer-friendly trade secrecy regime. Firms may be differentially incentivized to invest in their employee's human capital development depending on the redeployability of the resulting skills. On the one hand, if skills are largely *specific* to the focal organization (in that re-use of those skills in another organization is difficult or ineffective), employers may be willing to incur such investments in general (Becker, 1964; Gibbons & Waldman, 1999). Furthermore, this investment incentive might be bolstered when there are barriers to employee mobility in the labor market. On the other hand, employers may be hesitant to invest in *general* employee human capital, skills that are interchangeable and equally applicable across organizations. Without labor market frictions, it may be difficult for firms to appropriate the returns from general human capital investment (Becker, 1964). However, the presence of labor market frictions, which raise the expected returns from these investments, may create organizational incentives to invest, even in the case of general human capital. Since these two forms of human capital investments may be critical inputs to the innovation process, and because employer-friendly trade secrecy regimes heighten frictions in the employee labor market, if this mechanism is salient, we would expect to observe a *positive* relation between an employer-friendly IDD regime and innovation outcomes. Consistent with this explanation, Png (2017b), for example, found that R&D expenditures for larger firms that are in more technology-intensive areas are positively related to state-level enactment of the UTSA previously discussed in our institutional background.

On the other hand, a more employer-friendly trade secrecy regime may dampen innovation through two channels: reducing the potential of idea recombination and diminishing employees' incentives to innovate. First, the much-discussed mechanism of idea recombination as an important precursor to innovation may result not only because of the possible need to gather feedback about idea quality from diverse knowledge sources (e.g., Hellmann & Perotti, 2011), but also because the recombination process itself may generate novel configurations (e.g., Fleming, 2001). Since an employer-friendly trade secrecy regime raises the employee costs of circulating in the labor market, thus diminishing the potential for idea recombination, innovation might be consequently hampered.

The second channel is less explored by the management literature. Individual-level innovation incentives stem in part from the competitive labor market because employment terms are also driven by the employee's value, including innovation potential, in the labor market (e.g., Bulow & Summers, 1986; Doeringer & Piore, 1971). An employer-friendly trade secrecy environment may hamper the operation of that competitive external labor market, and so raises the possibility of dampened individual incentives for innovation. In Becker's (1964) framework, strengthened employer trade secrecy protection through legal doctrines such as IDD converts potentially general employees' skills into the firm-specific domain in function. As a consequence, employees may be less incentivized to acquire such skills in the first place, in part due to the diminished ability of employees to use the secondary labor market to bargain for better terms with the current employer. The net result is diminished inventor productivity.[6] Acharya, Baghai, and Subramanian (2013), for example, found that

[6]To understand this mechanism more precisely, consider the behavior of inventors in the context of a signaling game (Spence, 1973) with incomplete information in which the workers differ in their productive ability, that is, their ability to generate high-quality patents, and face a market of potential employers who set wages (Gibbons, Katz, Lemieux, & Parent, 2005). Here, talented inventors have an incentive to produce patents of high quality that could not be generated by lower ability inventors (either for their lack of skill or because the investment would be inefficient). The empirical prediction of these signaling models is that firms of different types emerge,

strengthened employee-friendly wrongful-discharge employment protection acts to spur innovation through reducing the potential of employer holdup.

An additional response of individual inventors to an employer-friendly trade secrecy regime would be to shift the *nature* of their innovation efforts. Importantly, the preceding theory pertains only to the situation of employees wishing to continue working within the domain they have worked within under their respective original employer (consistent with the remedies available under IDD). If the employees were hired to conduct work in a different domain, the employers' arguments for applying IDD to enjoin the employees would be considerably less compelling in court. Yet, at the same time, it is likely that new potential employers are interested in hiring the employees precisely because of their specialized knowledge and experience within a domain (for otherwise, there would be a high degree of human capital substitution available—and likely without the potential legal encumbrances). We therefore posit that IDD incentivizes inventors to signal their skills and quality in other, noncompeting markets and domains. An empirical prediction is that inventors under an employer friendly trade secrecy regime will be more likely to produce innovations that can be used across a wider spectrum of purposes (general-purpose technologies) relative to before exposure to such a regime.

In summary, while the firm-specific investment channel predicts a positive effect of IDD on innovation, both the idea recombination and the labor market signaling views predict a negative relation.

## 4 | DATA, MEASURES, AND EMPIRICAL STRATEGY

To empirically assess these theories, we follow the classification of IDD rulings published in Castellaneta, Conti, Veloso, and Kemeny (2016), which in turn, is based on a number of legal sources. These rulings are made at the U.S. state level, and are coded as rulings for and against the IDD (with one case being equivocal).[7] The 1995 Illinois *PepsiCo* case previously discussed was a watershed case, and the first in the sequence of pro-IDD rulings. Figure 1 graphically illustrates the cumulative number of states with each type of ruling over time.

Patent data are sourced from the NBER (Hall, Jaffe, & Trajtenberg, 2001) and Harvard IQSS Patent Network Dataverse (Li et al., 2014). These sources together allow us to construct firm- and individual-level characteristics. We use utility patents and patent citations to develop measures of inventor-level innovation, inventor location, and inventor affiliation. The SIC-patent class concordance data are obtained from Silverman (1999), while state-level data come from the U.S. Bureau of Economic Analysis. The final sample includes 353,889 distinct inventors from all 50 U.S. states (and Washington, D.C.), observed up to 28 years (1976–2003), for a total of up to 2,772,278 inventor-year observations.[8]

so inventors will move across firms and sort themselves into employment for high or low ability inventors, with a wage premium for high-skill inventors. However, with mobility frictions, as may exist under IDD, highly skilled inventors no longer have an incentive to signal their talent to the market as they cannot sort themselves into employment and wages by skill level, and so invention quality consequently suffers.

[7]The states ruling in favor of IDD are: Illinois (1995), New York (1997), Washington (1997), Utah (1998), Iowa (2002), Delaware (2006), and Pennsylvania (2010). The states ruling against IDD are: California (1944), Louisiana (1967), North Carolina (1976), New Jersey (1980), Minnesota (1992), Massachusetts (1995), Virginia (1999), Florida (2001), and Maryland (2004). The classification we follow interprets the IDD as a substitute for a noncompetition agreement, which accords with legal accounts of the doctrine, especially as exemplified in the *PepsiCo* case.

[8]We build our sample from the patent data. We retain patents for which both assignee and inventor information is available. We then build an inventor-year panel data set where each inventor is present in the time window between his or her first and last patent. We drop inventors who are present only in a single year in order to use inventor-level fixed effects in our empirical analyses.

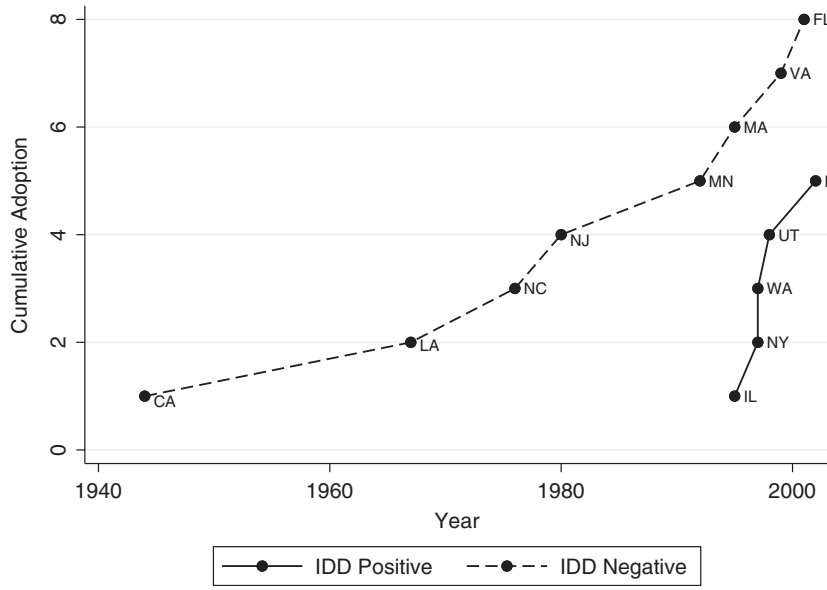


**FIGURE 1** Inevitable disclosure doctrine (IDD) rulings over time. This figure reports the decisions regarding IDD based on Castellaneta et al. (2016). The y-axis is cumulative number of states. The x-axis is time. We use all 13 IDD decisions within our sample time horizon (1976–2003) except New York, which Castellaneta et al. considered unclear. We interpret the equivocal 2003 New York decision as a reversal of a positive IDD decision (1 to 0). For details about the classification, please see Castellaneta et al., 2016, Table B1, p. 539

Our main dependent variable is the count of eventually-granted patents weighted by forward citations (inspired by Trajtenberg, 1990, and follow-on work). As is common in the literature, we use the natural logarithm of these measures, calculated as log $(1 + x)$, to diminish the impact of outliers. Because innovation is a long-term process, we include a temporal lag structure in our analysis to capture the possible delayed effect on innovation following the legal regime shift. The main independent variables are the post-IDD positive and negative regime dummies, which takes the value 1 for individual-year observations when a state had a legal precedent for IDD in place ("IDD positive"), or when a state court had ruled against IDD ("IDD negative"), and 0 otherwise. We use a parsimonious set of time-varying variables to control for other inputs to the innovation production function, including variables at the individual level (log number of years since first patent and log patent stock during the previous four years), organization level (log age and log number of inventors), industry level (log number of firms), and state level (log total wage). Table 1 provides definitions and descriptive statistics of all the variables used in the analysis.

Our main specification is a difference-in-differences design using ordinary least squares regressions on an inventor-year panel data set, in which the first difference is the IDD regime, and the second difference is the year of "treatment." In all specifications at this level of analysis, we use standard errors clustered at the state level[9] to account for potential serial correlation of observations within the same state (Bertrand, Duflo, & Mullainathan, 2004). We also visually check the parallel path assumption prior to treatment through an analysis of year-specific treatment effects, following Acharya et al. (2013, Figure 4), available on request.

[9]Using less conservative approaches, such as individual-level clustered standard errors, enhances the statistical significance of the estimated coefficients.

**TABLE 1** Variable definitions and descriptive statistics

| Variable | Definition (data source) | Mean | Std. dev. |
|---|---|---|---|
| **Inventor-year level of analysis** | | | |
| Log citation-weighted patent count | Log of the number of eventually granted patent applications weighted by number of forward citations within four years from grant (IQSS/NBER) | 0.84 | 1.18 |
| Log mean combinatorial novelty | Log of the mean of the combinatorial novelty of the eventually granted patent applications. Combinatorial novelty measures the degree to which the individual inventions recombine infrequently combined technology areas (Fleming & Sorenson, 2001) | 2.15 | 0.81 |
| Mean patent generality | 1-Herfindahl-Hirschman Index of primary patent classes within forward patent citations (IQSS/NBER) | 0.44 | 0.26 |
| IDD positive | Binary variable taking value 1 when a positive ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.06 | 0.23 |
| IDD negative | Binary variable taking value 1 when a negative ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.29 | 0.45 |
| Log 4-year patent stock | Log of number of patents in the prior four years (IQSS/NBER) | 1.08 | 0.69 |
| Log individual experience | Log of number of years since the inventor appears in the data set (IQSS/NBER) | 1.80 | 0.70 |
| Log organization size | Log of number of inventors with patent assignments to the focal organization (IQSS/NBER) | 4.02 | 2.42 |
| Log organization age | Log of number of years since organization appears in data set (IQSS/NBER) | 2.39 | 0.68 |
| Log industry size | Log of number of inventors in three-digit SIC (IQSS/NBER & Silverman, 1999) | 5.27 | 1.84 |
| Log state total wage | Log of the state sum of wages and salaries in thousands of U.S. dollars (Bureau of Economic Analysis website) | 18.51 | 0.98 |
| **State-year level of analysis** | | | |
| Log citation-weighted patent count | Log of the number of forward citations within four years from grant (IQSS/NBER) | 7.23 | 1.90 |
| IDD positive | Binary variable taking value 1 when a positive ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.02 | 0.14 |
| IDD negative | Binary variable taking value 1 when a negative ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.10 | 0.29 |
| Log state total wage | Log of the state sum of wages and salaries in thousands of U.S. dollars (Bureau of Economic Analysis website) | 17.15 | 1.17 |
| **Invention level of analysis** | | | |
| Log citation count | Log of the number of forward citations within four years from grant (IQSS/NBER) | 1.33 | 1.00 |
| IDD positive | Binary variable taking value 1 when a positive ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.06 | 0.23 |
| IDD negative | Binary variable taking value 1 when a negative ruling for IDD is in place, and 0 otherwise (Castellaneta et al., 2016) | 0.30 | 0.46 |
| Log team size | Log of number of patent team members (IQSS/NBER) | 0.99 | 0.35 |
| Log average experience | Log of average number of years since the inventor appears in the data set (IQSS/NBER) | 1.77 | 0.70 |
| Log average tenure | Log of average tenure in present firm across patent team members (IQSS/NBER) | 1.46 | 0.60 |
| Log average organization age | Log of average organization age across patent team members (IQSS/NBER) | 2.37 | 0.73 |
| Log average organization size | Log of average organization size across patent team members (IQSS/NBER) | 4.26 | 2.24 |

*Note.* This table reports variable definitions and summary statistics for variables used in the analyses. Each section of the table focuses on one of the three levels of analysis employed: inventor year, state year, and invention. When we use the logarithm, it is the natural logarithm of the variable plus 1.

## 5 | EMPIRICAL RESULTS

### 5.1 | Main results

Table 2 reports our main estimates of IDD regime and innovation.

Panel A conducts the analysis at the inventor-year level of analysis, and reports the innovation relationship at annual time lags ranging from two to five years after IDD regime onset. While all specifications contain inventor and year fixed effects for each time lag, we report specifications with and without the control variables. We find a robust empirical pattern: An IDD positive regime is consistently negatively associated with log citation-weighted patent counts through the four-year time lag (and becomes nonsignificant at the five-year lag).[10] The estimates suggest a 4–6 % decrease in the inventor-level innovation outcome measure following ascension to an employer-friendly trade secrecy regime.

Meanwhile, the IDD negative regime is not significant in any of the time lags. Because a negative ruling on IDD is essentially not changing the status quo trade secrecy environment, we are not surprised with this nonresult. Naturally, if the labor market environment does not change, there is no reason to expect individual behavioral changes either.[11]

Panel B of Table 2 repeats the analysis at the state-year level of analysis. All specifications include state and year fixed effects, and the standard errors are clustered at the state level. The empirical results remain consistent: Each lag through five years, especially the specifications containing the control variables, is significant and suggests an approximately 25-percentage-point innovation decline for a discrete change into an IDD positive trade secrecy regime. The state level effects are higher since an important source of innovation is individual-level heterogeneity (including the composition of inventors in a state), which can only be captured with inventor fixed effects in the individual-level analysis. In addition, aggregating individual-level data to the state-level involves a variety of sample design choices; these, in turn, may impact the estimates. Finally, a cautionary note: Our ability to control for time-varying state-level effects is limited due to the high degree of collinearity among our available state-level control variables, and the estimate of the *IDD negative* effect can vary depending on which control variables enter the specification. Consequently, we are unable to strongly interpret the IDD negative effect from this state-level analysis. The coefficient on the *IDD positive* variable, however, remains weakly negative throughout the temporal specifications, consistent with our main findings from the more precisely estimated inventor-year analysis (Table 2, Panel A).

### 5.2 | Robustness checks

To verify the effect found in the main analysis, we conduct a variety of robustness checks in Table 3, spanning the domains of sample composition (Panel A) and controls for potential confounding events

[10]Given our use of inventor fixed effects, the IDD effect is a *within* estimate, based entirely on inventors who experienced a change in IDD status. In order to estimate the effect $N$ years after the change in IDD, the inventor had to be in our data prior to the change and $N$ years after as well (please recall that we build our sample including inventors between their first and last patents as is common in the literature). In our data, 23,527 inventors contribute to the $t + 2$ within estimation but the number drops to 11,777 for $t + 5$. Therefore, estimating temporally distant effects implies lower statistical power since fewer inventors contribute to the estimation, thus mechanically increasing the standard errors. Shorter lags are therefore more precisely estimated. In addition, over the longer run, individuals may adjust their behavior and effort to the new legal environment, which can also help explain the temporal effects. Finally, we restrict our analysis to five years post-event since all of the *IDD positive* events take place in the final eight years of our data set.

[11]One might argue that reaffirming that IDD is not in place could itself change inventor behavior. We believe that unlike the extremely visible 1995 landmark Illinois case (an IDD positive decision), IDD negative decisions are not as likely to receive substantial media coverage, a necessary condition for economic agents to act on an updated legal environment.

**TABLE 2** Main analysis of IDD regime and innovation

| DV | Log citation-weighted patent count | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| Time window | t + 2 | | t + 3 | | t + 4 | | t + 5 | |
| | 2B-1 | 2B -2 | 2B-3 | 2B-4 | 2B-5 | 2B-6 | 2B-7 | 2B-8 |
| **Panel A: Inventor-year level of analysis**[a] | | | | | | | | |
| IDD positive | −0.045 | −0.038 | −0.049 | −0.040 | −0.056 | −0.047 | −0.047 | −0.038 |
| | (0.015) | (0.019) | (0.018) | (0.013) | (0.028) | (0.018) | (0.046) | (0.037) |
| IDD negative | 0.024 | 0.023 | 0.015 | 0.016 | 0.013 | 0.014 | 0.009 | 0.010 |
| | (0.023) | (0.025) | (0.021) | (0.022) | (0.021) | (0.019) | (0.021) | (0.016) |
| Controls | N | Y | N | Y | N | Y | N | Y |
| Inventor, state & year FE | Y | Y | Y | Y | Y | Y | Y | Y |
| *N* | 2,067,187 | 2,067,187 | 1,763,496 | 1,763,496 | 1,510,618 | 1,510,618 | 1,297,248 | 1,297,248 |
| Adj. $R^2$ | 0.25 | 0.26 | 0.26 | 0.27 | 0.26 | 0.27 | 0.26 | 0.27 |
| **Panel B: State-year level of analysis**[b] | | | | | | | | |
| IDD positive | −0.188 | −0.260 | −0.206 | −0.266 | −0.206 | −0.249 | −0.233 | −0.253 |
| | (0.201) | (0.142) | (0.176) | (0.123) | (0.165) | (0.118) | (0.154) | (0.114) |
| IDD negative | −0.187 | −0.283 | −0.205 | −0.278 | −0.207 | −0.262 | −0.233 | −0.277 |
| | (0.147) | (0.144) | (0.153) | (0.149) | (0.163) | (0.159) | (0.174) | (0.171) |
| Controls | N | Y | N | Y | N | Y | N | Y |
| State & year FE | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 1,326 | 1,326 | 1,275 | 1,275 | 1,224 | 1,224 | 1,173 | 1,173 |
| Adj. $R^2$ | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 | 0.96 |

[a] This panel reports coefficients and standard errors (in parentheses) from OLS regressions at the inventor year level. The control variables are listed in Table 1. Standard errors are clustered at the state level. The outcome variable is measured at times $t + 2$, $t + 3$, $t + 4$, $t + 5$ relative to focal year $t$. All explanatory variables are measured at time $t$.

[b] This panel reports coefficients and standard errors (in parentheses) from OLS regressions at the state year level. The control variables are listed in Table 1. Standard errors are clustered at the state level. The outcome variable is measured at times $t + 2$, $t + 3$, $t + 4$, $t + 5$ relative to focal year $t$. All explanatory variables are measured at time $t$.

**TABLE 3** Robustness checks

**Panel A: Robustness to sample composition[a]**

| DV | Log citation-weighted patent count | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| **Sample** | **Inventors who do not change state** | | **Inventors who do not change IDD regime** | | **Excludes inventors from California** | | **Inventors from Illinois (treated in 1995) and nontreated U.S. Midwestern states** | | **5-year window around treatment** | |
| **Time window** | ***t* + 2 3A-1** | ***t* + 3 3A-2** | ***t* + 2 3A-3** | ***t* + 3 3A-4** | ***t* + 2 3A-5** | ***t* + 3 3A-6** | ***t* + 2 3A-7** | ***t* + 3 3A-8** | ***t* + 2 3A-9** | ***t* + 3 3A-10** |
| IDD positive | −0.039 | −0.030 | −0.040 | −0.034 | −0.036 | −0.044 | −0.069 | −0.070 | −0.056 | −0.053 |
| | (0.021) | (0.013) | (0.020) | (0.013) | (0.016) | (0.015) | (0.016) | (0.016) | (0.017) | (0.025) |
| IDD negative | 0.013 | 0.007 | 0.017 | 0.013 | 0.019 | 0.011 | N/A | N/A | −0.018 | −0.026 |
| | (0.046) | (0.044) | (0.038) | (0.035) | (0.033) | (0.031) | N/A | N/A | (0.034) | (0.032) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Indiv. & year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 1,581,745 | 1,325,937 | 1,736,865 | 1,465,375 | 1,735,431 | 1,484,942 | 141,072 | 117,248 | 276,624 | 225,242 |
| Adj. $R^2$ | 0.26 | 0.27 | 0.26 | 0.27 | 0.25 | 0.26 | 0.26 | 0.26 | 0.29 | 0.30 |

**Panel B: Robustness to potential confounds.[b]**

| DV | Log citation-weighted patent count | | | | | | | | | | Citation weighted patent count | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Model** | **Trade secrecy index** | | **Time trend** | | **State-specific time trends** | | **Bank deregulation (Kerr & Nanda)** | | **Bank deregulation (Amore et al.)** | | **Poisson** | |
| **Time window** | ***t* + 2 3B-1** | ***t* + 3 3B-2** | ***t* + 2 3B-3** | ***t* + 3 3B-4** | ***t* + 2 3B-5** | ***t* + 3 3B-6** | ***t* + 2 3B-7** | ***t* + 3 3B-8** | ***t* + 2 3B-9** | ***t* + 3 3B-10** | ***t* + 2 3B-11** | ***t* + 3 3B-12** |
| IDD positive | −0.037 | −0.039 | −0.085 | −0.144 | −0.110 | −0.164 | −0.033 | −0.035 | −0.038 | −0.040 | −0.072 | −0.063 |
| | (0.018) | (0.012) | (0.030) | (0.039) | (0.034) | (0.045) | (0.014) | (0.014) | (0.018) | (0.013) | (0.027) | (0.031) |
| IDD negative | 0.023 | 0.016 | 0.033 | 0.013 | 0.038 | 0.008 | 0.017 | 0.012 | 0.023 | 0.016 | 0.026 | 0.012 |
| | (0.025) | (0.022) | (0.042) | (0.042) | (0.070) | (0.076) | (0.023) | (0.021) | (0.025) | (0.022) | (0.020) | (0.021) |
| Controls | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Indiv. FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| Year FE | Y | Y | N | N | N | N | Y | Y | Y | Y | Y | Y |
| N | 2,067,187 | 1,763,496 | 2,067,187 | 1,763,496 | 2,067,187 | 1,763,496 | 2,067,187 | 1,763,496 | 2,067,187 | 1,763,496 | 1,907,452 | 1,627,639 |
| Adj. $R^2$ | 0.26 | 0.27 | 0.25 | 0.26 | 0.25 | 0.26 | 0.26 | 0.27 | 0.26 | 0.27 | N/A | N/A |

[a] This panel reports coefficients and standard errors (in parenthesis) from OLS regressions at the inventor year level of analysis. Standard errors are clustered at the state level. The control variables are listed in Table 1. Columns 1–2 use the sample of inventors who do not change state; Columns 3–4 use the sample of inventors who do not change IDD regime; Columns 5–6 use the sample excluding California; Columns 7–8 use the sample restricted to Illinois and the nontreated U.S. Midwest states (KS, MI, MO, NE, ND, SD, WI); and Columns 9–10 restrict the sample to a five-year window around the event and inventors who do not change state.

[b] This panel reports coefficients and standard errors (in parenthesis) from OLS and Poisson regressions at the inventor-year level of analysis. Standard errors are clustered at the state level in the OLS regressions and robust in the Poisson regressions. The control variables are listed in Table 1. Columns 1–2 include a measure of the prevailing state-level trade secrecy regime (taken from Png, 2017b); Columns 3–4 use a time trend instead of year fixed effects (FEs); Columns 5–6 use state-specific time trends instead of year FEs; Columns 7–8 include the bank deregulation regime from Kerr and Nanda (2009); Columns 9–10 include the bank deregulation regime variables from Amore et al. (2013), and Columns 11–12 use Poisson count models.

and estimation methods (Panel B). In these analyses, we concentrate our attention on the two- and three-year lags post-IDD regime ascension as our main results were most salient in those time periods. All robustness analyses are consistent with our full-sample results.

We start by showing that the main results are robust to various subsamples of the overall data set. We return to the inventor-year level of analysis, with each specification containing the full set of control variables as well as individual and state fixed effects. The first group of robustness checks in Table 3, Panel A, examines whether the results are sensitive to the exclusion of inventors who change states (specifications 3A-1 and 3A-2) and the exclusion of inventors who change IDD regimes (Columns 3A-3 and 3A-4). We wish to dismiss the possibility that changes in the sample composition are driving the main results, which could constitute a violation of the stability of the treatment unit, which in turn, undermines the credibility of the empirical strategy. Our results are robust to both subsample exclusions.

The next two sample composition robustness checks first exclude inventors from California and then focus on the U.S. Midwestern states, where the landmark *PepsiCo* case took place. Since the focal California case placing it in the IDD negative regime took place in 1944, well before the modern trade secrecy environment and the rest of the state-level regimes we consider, and because the case facts also may not have been comparable with the other cases (the defendant was a contractor and not an employee), we verify that the result are robust to excluding California inventors in specifications 3A-5 and 3A-6. In the next two columns, we confine the sample to inventors from Illinois and seven U.S. Midwestern states (Kansas, Michigan, Missouri, Nebraska, North Dakota, South Dakota, and Wisconsin). The treatment is in effect for inventors in Illinois in the post-1995 period. The legal environment shift in this setting was important and swift, and more uniform in case facts (e.g., the importance of bad faith, the absence of a covenant not to compete, and preexisting state-level ratification of the UTSA in applying the doctrine) relative to a multistate analysis. The results are consistent and economically larger (a 7 % innovation decline), even after utilizing a synthetic state control method (Abadie, Diamond, & Hainmueller, 2010; results unreported but available from the authors). The final two specifications in Table 3, Panel A, investigate result robustness to a uniform five-year treatment window around the IDD events,[12] and also find consistent results.

In Table 3, Panel B, we investigate result robustness to a variety of potentially confounding events. We first examine whether differences in noncompete clause enforcement, as described in Garmaise (2011), overturn our main results. We employ Png's (2017a, 2017b) composite index, which incorporates both Garmaise's noncompete clause enforcement data as well as other elements of the state-level trade secrecy regime (with features such as variation in qualifications for trade secrecy protection, civil procedure regarding taking legal action, and remedies in case of violations), as a control variable in the first two columns of Table 3, Panel B. The negative impact of IDD previously reported remains essentially unchanged in magnitude and statistical significance. We next show result robustness to a time trend (specifications 3B-3 and 3B-4) and a state-specific time trend (specifications 3B-5 and 3B-6).

A number of U.S. banking deregulation environment shifts could also be confounders as such deregulation could have implications for, among other things, the decision to undertake projects and make investments. Since these decisions could, in turn, affect innovation rates, even outside of the trade secrecy regime, we control for the banking deregulation regime (by following the Kerr and

[12]We make two sample construction decisions to conduct this analysis because: (a) some inventors could move within the analysis window and so could be treated twice, we analyze inventors who do not move across state; and (b) it is not clear how to construct a time window for an inventor who is never treated, we remove inventors who are never treated from this analysis. This analysis is robust to alternative sampling choices, such as restricting to inventors who are treated only once (regardless of whether they change state) or including never-treated inventors.

Nanda (2009) scheme in Columns 3B-7 and 3B-8, and by following the Amore, Schneider, and Zaldokas (2013) coding in 3B-9 and 3B-10). Our results are robust.

The final two columns of Table 3, Panel B, test the robustness of estimation method. We employ Poisson models, in which the outcome variable is a count of the number of citation-weighted patents. Such models are appropriate for dependent variables that take on nonnegative integer values. Again, the main results hold.

Our main robust finding is a negative relation between patent-based innovation measures and an employer-friendly trade secrecy regime. The remainder of the empirical analyses investigate the potential explanations behind this effect. We do not consider the theoretical explanation centered on enhanced organizational human capital investments, which predicts a positive relation between IDD positive and innovation outcomes. We first examine whether a change in intellectual property rights (IPR) strategy substituting trade secrecy for patenting is driving the main results. We then examine the knowledge recombination and labor market signaling mechanisms in turn, as each theory is consistent with the negative main effect we find.

### 5.3 | Mechanisms analysis

The first mechanism we explore is a shift away from patent protection in the face of a strengthened (from the employer's perspective) trade secrecy environment. This possible intellectual property rights (IPR) protection strategy shift could explain the diminished patenting-based innovation outcome if inventions which were formerly protected by patents are now protected via trade secrecy (Friedman et al., 1991; Png, 2017a), with little or no change in actual innovation. In particular, the negative relation between strengthened trade secrecy and measured innovation (patent output) could result from reduced employee mobility stemming from an employer-friendly trade secrecy environment. This may result in less potential for knowledge spillovers, which in turn, may be manifested in less aggressive patenting (Kim & Marschke, 2005).

We exploit the heterogeneity of patent effectiveness across industries as perceived and reported in surveys of industrial managers. Among manufacturing industries, Cohen et al. (2000) distinguished discrete from complex technologies. Discrete technologies are those typically composed of a relatively small number of components. Complex technologies are those that tend to be composed of a large number of components.[13] As Png stated: "For discrete technology products, the substitution between patents and secrecy will be relatively weak. So long as a product embeds at least one patent, it gets some of the advantages of a patent—the legal right to exclude, strategic purposes, publicity, underpinning licensing, and access to federal courts. Manufacturers of discrete technology products would be reluctant to switch the single invention from patent to secrecy and lose those advantages. By contrast, in complex technology products, there will be many patents to convey those advantages, even after substitution of some patents for secrecy. Hence, the substitution will be stronger among complex technology products" (2015, p. 2).

Since the actor deciding on the possible IP form is the manager, and he or she is likely examining the invention rather than the inventor in determining the form of IP protection (patents vs. trade

[13]To build these measures, we match patents to SIC codes. We assign a SIC code to each patent building on the method proposed by Silverman (1999). Silverman (1999) constructed a concordance linking the International Patent Classification (IPC) system to the U.S. Standard Industrial Classification (SIC) system, assigning each IPC to one or more four-digit SIC codes where the patent may be used. Using this data set, we assign each IPC to the SIC code with its highest frequency of use, thereby linking each U.S. patent to its most plausible SIC code. Finally, we focus on manufacturing sectors and categorize each SIC as either discrete (SIC between 19 and 33: food, tobacco, textiles, apparel, lumber, furniture, paper, printing, chemicals, petroleum refining, rubber, leather, and varied material products) or complex (SIC between 34 and 39: fabricated metal, industrial machinery, computer and electronic components and equipment, transportation equipment, and measuring/optical/medical goods), as per Cohen et al. (2000).

secrets), we perform the analysis at the invention unit of analysis. Therefore, we regress innovation quality (log of forward citations for each given patent) on IDD regime at the time of patent application and team- and organization-level controls. We find that for the subsample of discrete technologies, which comprises about a third of our overall sample, there is a negative relation between IDD positive ascension and innovation quality, while this relationship does not exist for the sample of complex technologies (see Table 4, Panel A, 4A-3 and 4A-4 as compared to 4A-5 and 4A-6). The same general pattern emerges at the inventor-year level of analysis, though the results are somewhat noisier (regression output available on request).[14] We therefore conclude that IP form substitution does not explain the entire negative innovation output relation arising from an IDD positive trade secrecy regime, particularly for discrete technologies.

Because the theoretical mechanisms explaining the main negative relation between an IDD positive regime and innovation outcomes take place at the inventor level, we now return to that level of analysis in order to assess the mechanisms (Table 4, Panels B and C).[15] We start by examining the knowledge recombination mechanism. This view suggests that because circulating ideas for feedback and recombination may be instrumental for innovation, and because an IDD positive legal regime places frictions on inventor mobility across firms, we might believe that dampening knowledge recombination may be a leading mechanism for the negative innovation result. Since the concept of frictions to labor movement restraining mobility has been well tested in the literature,[16] we do not dwell on it here.

Instead, we concentrate our efforts in directly exploring whether the IDD regime is associated with a validated measure of combinatorial novelty based on the work of Fleming and Sorenson (2001).[17] This measure captures the degree to which a focal invention has recombined elements that are relatively uncommon (a proxy of higher novelty) as compared to historical patents. We extend the measure to inventor-year patent portfolios, and use this as an outcome measure, with the main independent variable of interest the post-IDD positive regime indicator in a difference-in-differences specification. If the knowledge recombination mechanism of explaining the adverse innovation impact of IDD is salient, we would expect that the IDD positive variable would be negatively related to the combinatorial novelty variable. Across a range of empirical specifications, we do not find empirical support for this, for the sample as a whole, and for either the discrete- or complex-technology subsamples (Table 4, Panel B).

A different possible mechanism explaining the negative innovation effect of the IDD positive environment relates to the incentive structure of inventors. Inventors may be incentivized to innovate

[14]While the heart of our mechanisms analysis examines the discrete technology subsample, we contextualize the complex technology subsample with respect to possible IP form substitution in the Appendix.

[15]When we assess these mechanisms, we construct the inventor-year observations using the following procedure: we (a) classify each invention as discrete or complex (using SIC concordance, via the Silverman [1999] method); (b) classify each inventor year as discrete or complex using a majority rule (the cases where an inventor has the same number of discrete and complex inventions in a year are very few, so the results are not sensitive to their inclusion or exclusion); and (c) use an imputation approach to fill in inventor-year observations when there is no patenting, following a common practice in the literature (see, e.g., Marx, Strumsky, and Fleming, 2009, p. 882). The results reported in Table 4, Panels B and C, use the majority rule to classify inventors as discrete or complex (a threshold of more than 50% of the inventor years of a given inventor classified as a given technology type), though the results are robust to alternate thresholds of above 75% and 100% (we report the lower threshold to maximize the number of usable observations).

[16]Several studies found that increased costs of inventor mobility dampen technical employee mobility (we also find the same empirical pattern in our sample). For example, Marx et al. (2009) reported that an unanticipated shift in employee noncompete agreement enforcement in Michigan curtails such mobility.

[17]The combinatorial novelty measure is calculated as follows. We first compute recombination ease E$st$ for technology subclass $s$ at time $t$ as the ratio of number of subclasses combined with $s$ and the number of patents in $s$. Combinatorial novelty C$p$ of patent $p$ is the sum of recombination ease E$st$ for each subclass $s$ associated to patent $p$. Finally, inventor-year combinatorial novelty C$t$ is the average of the combinatorial novelty C$p$ of all patents $p$ applied for in year $t$.

**TABLE 4** Mechanisms

| **Panel A: Potential IP substitution[a]** | | | | | | |
|---|---|---|---|---|---|---|
| **DV** | **Log number of citations (invention level)** | | | | | |
| **Sample** | **Full** | | **"Discrete" inventions** | | **"Complex" inventions** | |
| | **4A-1** | **4A-2** | **4A-3** | **4A-4** | **4A-5** | **4A-6** |
| IDD positive | −0.021 | −0.015 | −0.057 | −0.051 | −0.005 | −0.001 |
| | (0.005) | (0.005) | (0.009) | (0.009) | (0.006) | (0.006) |
| IDD negative | 0.098 | 0.093 | 0.051 | 0.050 | 0.113 | 0.107 |
| | (0.002) | (0.002) | (0.004) | (0.004) | (0.003) | (0.003) |
| Controls | N | Y | N | Y | N | Y |
| SIC2 & year FE | Y | Y | Y | Y | Y | Y |
| N | 792,804 | 792,804 | 238,281 | 238,281 | 554,523 | 554,523 |
| Adj. $R^2$ | 0.17 | 0.18 | 0.10 | 0.11 | 0.17 | 0.18 |

| **Panel B: Knowledge recombination mechanism[b]** | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **DV** | **Log mean combinatorial novelty (inventor-year level)** | | | | | | | | | | | |
| **Sample** | **Full** | | | | **"Discrete" inventors** | | | | **"Complex" inventors** | | | |
| **Time widow** | $t+2$ | | $t+3$ | | $t+2$ | | $t+3$ | | $t+2$ | | $t+3$ | |
| | **4B-1** | **4B-2** | **4B-3** | **4B-4** | **4B-5** | **4B-6** | **4B-7** | **4B-8** | **4B-9** | **4B-10** | **4B-11** | **4B-12** |
| IDD positive | −0.009 | −0.009 | −0.007 | −0.007 | −0.032 | −0.029 | −0.023 | −0.020 | 0.014 | 0.015 | 0.016 | 0.016 |
| | (0.020) | (0.018) | (0.019) | (0.018) | (0.037) | (0.036) | (0.027) | (0.027) | (0.021) | (0.020) | (0.028) | (0.026) |
| IDD negative | −0.008 | −0.007 | −0.009 | −0.005 | −0.023 | −0.022 | −0.021 | −0.017 | 0.011 | 0.015 | 0.006 | 0.011 |
| | (0.011) | (0.012) | (0.007) | (0.007) | (0.013) | (0.013) | (0.012) | (0.011) | (0.009) | (0.009) | (0.008) | (0.009) |
| Controls | N | Y | N | Y | N | Y | N | Y | N | Y | N | Y |
| Ind. & year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 897,582 | 897,582 | 758,575 | 758,575 | 202,644 | 202,644 | 172,706 | 172,706 | 431,906 | 431,906 | 363,382 | 363,382 |
| Adj. $R^2$ | 0.46 | 0.46 | 0.47 | 0.47 | 0.34 | 0.34 | 0.34 | 0.34 | 0.34 | 0.34 | 0.34 | 0.34 |

| **Panel C: Labor market signaling mechanism[c]** | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **DV** | **Mean patent generality (inventor-year level)** | | | | | | | | | | | |
| **Sample** | **Full** | | | | **"Discrete" inventors** | | | | **"Complex" inventors** | | | |
| **Time window** | $t+2$ | | $t+3$ | | $t+2$ | | $t+3$ | | $t+2$ | | $t+3$ | |
| | **4C-1** | **4C-2** | **4C-3** | **4C-4** | **4C-5** | **4C-6** | **4C-7** | **4C-8** | **4C-9** | **4C-10** | **4C-11** | **4C-12** |
| IDD positive | 0.006 | 0.006 | 0.010 | 0.010 | 0.026 | 0.027 | 0.034 | 0.034 | 0.000 | 0.001 | 0.003 | 0.004 |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) | (0.005) | (0.005) | (0.004) | (0.003) | (0.005) | (0.005) |

**TABLE 4** (Continued)

| Panel C: Labor market signaling mechanism[c] | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **DV** | **Mean patent generality (inventor-year level)** | | | | | | | | | | | |
| **Sample** | **Full** | | | | **"Discrete" inventors** | | | | **"Complex" inventors** | | | |
| **Time window** | ***t* + 2** | | ***t* + 3** | | ***t* + 2** | | ***t* + 3** | | ***t* + 2** | | ***t* + 3** | |
| | **4C-1** | **4C-2** | **4C-3** | **4C-4** | **4C-5** | **4C-6** | **4C-7** | **4C-8** | **4C-9** | **4C-10** | **4C-11** | **4C-12** |
| IDD negative | 0.002 | 0.002 | −0.002 | −0.001 | −0.005 | −0.004 | −0.007 | −0.007 | 0.008 | 0.010 | 0.003 | 0.003 |
| | (0.004) | (0.004) | (0.003) | (0.003) | (0.004) | (0.004) | (0.005) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| Controls | N | Y | N | Y | N | Y | N | Y | N | Y | N | Y |
| Ind. & year FE | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| N | 819,229 | 819,229 | 691,408 | 691,408 | 181,036 | 181,036 | 154,091 | 154,091 | 404,530 | 404,530 | 339,892 | 339,892 |
| Adj. $R^2$ | 0.35 | 0.35 | 0.35 | 0.35 | 0.34 | 0.34 | 0.34 | 0.34 | 0.35 | 0.35 | 0.35 | 0.35 |

[a] This panel reports coefficients and standard errors (in parenthesis) from OLS regressions at the invention level. The outcome variable is the log count of forward citations. Control variables are listed in Table 1. Standard errors are robust. All explanatory variables are measured at time *t*. To define "discrete" and "complex" technologies, we follow Cohen et al. (2000) by focusing on manufacturing sectors and categorize each U.S. Standard Industrial Classification (SIC) as either discrete (SICs between 19 and 33) or complex (SICs between 34 and 39). We assign a SIC code to each patent building on the method proposed by Silverman (1999). Silverman (1999) constructed a concordance linking the International Patent Classification (IPC) system to the SIC system, assigning each IPC to one or more four-digit SIC codes where the patent may be used. Using this data set, we assign each IPC to the SIC code with its highest frequency of use, thereby linking each U.S. patent to its most plausible SIC code. Finally, using the SIC code of each patent, we categorize the patent as either discrete or complex. We focus on patents where all inventors are based in the same IDD regime in order to unambiguously assign IDD regime to each patent.

[b] This panel reports coefficients and standard errors (in parenthesis) from OLS regressions at the inventor-year level of analysis. The outcome variable is log mean combinatorial novelty. Control variables are listed in Table 1. Standard errors are clustered at the state level. All explanatory variables are measured at time *t*. To define "discrete" and "complex" technologies, we follow Cohen et al. (2000) by focusing on manufacturing sectors and categorize each SIC as either discrete (SIC between 19 and 33) or complex (SIC between 34 and 39). Inventors with more than 50% of their inventor years classified as a given technology "type" (discrete or complex) are categorized within each category. These results are robust to using 75% and 100% cutoffs.

[c] This panel reports coefficients and standard errors (in parenthesis) from OLS regressions at the inventor-year level. The outcome variable is log mean patent generality. Control variables are listed in Table 1. Standard errors are clustered at the state level. All explanatory variables are measured at time *t*. To define "discrete" and "complex" technologies, we follow Cohen et al. (2000) by focusing on manufacturing sectors and categorize each SIC as either discrete (SICs between 19 and 33) or complex (SICs between 34 and 39). Inventors with more than 50% of their inventor years classified as a given technology "type" (discrete or complex) are categorized within each category. These results are robust to using 75% and 100% cutoffs.

by generating more high quality patents in order to signal their quality to the competitive labor market so as to weaken potential hold-up problems inside a firm and obtain better employment terms. Under an employer-friendly trade secrecy regime such as IDD positive, which raises the cost of within-sector employee mobility, inventors' incentives to make use of the secondary labor market in their focal domain may be dampened. Inventors might instead be incentivized to vary the direction or nature of their innovation efforts in order to signal their skills and quality to organizations other than their direct competitors. We posit that IDD positive rulings incentivize inventors to signal their skills and quality in noncompeting domains, and our empirical prediction is that more general-purpose innovations will be produced by inventors "treated" by IDD positive rulings. The mechanism suggests that if inventors' prospects for labor market bargaining as a result of their productive efforts in their focal technology area is curtailed, then they may be disincentivized in producing in the focal technology area. However, this diminished incentive does not hold in other technology domains since IDD enforcement is specific to a given technology area.

We use the patent generality measure, calculated as 1 minus the Herfindahl-Hirschman index of patent classes in forward patent citations (Hall et al., 2001) to proxy the *nature* of innovation produced, with the idea that while IDD may dampen inventors' within-sector incentives for innovation (for the reasons previously stated), inventors may redirect their efforts to signaling their quality across sectors and technical application areas, where IDD does not apply. Lower values of this measure indicate little use of the focal patent in a broad range of patent classes within the set of forward citations to the focal patent.

We therefore analyze the effect of IDD on inventors' patent generality in using the same difference-in-differences style regression framework we have employed before (using mean patent generality at the inventor-year level of analysis). Note that this analysis does not try to directly link the higher inventor-level patent generality after IDD positive to the same regime shift post-IDD with respect to forward citation weighted patents. Instead, these analyses are complementary in describing possible inventor-level responses to the regime shift making trade secrets protection more employer friendly. The results are presented in Table 4, Panel C. The economically and statistically positive coefficient is confined to the part of the sample in which there is unlikely to be IP substitution: The discrete technology subsample, though an aggregated analysis using the full sample, exhibits the positive relation as well (Table 4, Panel C, Columns 4A3–4A4). We interpret these results as consistent with a labor market signaling perspective in which inventors alter the direction of their inventive efforts to be more general purpose, even if their incentives to produce "quality" patents/innovation in their focal domain (as measured by forward citation weighted patent counts) is dampened.[18]

Our overall conclusions to our analysis of mechanisms are as follows. First, our main evidence for the mechanisms is drawn from the "discrete" technologies as they are unlikely to experience substitution, even in the face of differentially-altered IP regimes. This analysis is consistent with the labor market signaling mechanism and does not find support for the knowledge recombination mechanism. Second, some degree of substitution is likely to be taking place in the other part of our sample of the "complex" technologies. It is hard to quantify this effect, though comparisons of industry composition in light of the existing literature in markets for technology, patent propensity, and patent

[18]An anonymous reviewer suggested exploring the extent to which agents might respond to an *IDD positive* regime by exerting more inventive effort (manifested in patent quality, proxied by forward patent citations) for more general technologies and less such effort in "dedicated" (less general) technologies. Our empirical examination found partial support for this (evidence of the latter effect), though we do not report these results formally because: (a) this analysis divides the sample based on a similar source of information (forward patent citations) that drive the outcome variable we analyze, and (b) we are not entirely comfortable with the premise that inventors can choose both invention quality and generality at the same time, especially if the inventor has previously been producing more specialized technologies.

substitution with trade secrecy (contained in the Appendix) suggest that substitution is unlikely to account for the entire effect.

## 6 | DISCUSSION

Trade secrecy is an inherently difficult phenomenon to study empirically, and theoretical analyses, as we discussed, point to conflicting predictions on shifts in trade secrecy regime and innovation outcomes. Our empirical results, drawn from U.S. state-level trade secrecy environment changes resulting from the IDD, suggest that shifting to an employer-friendly trade secrecy regime may hinder innovation as measured by forward-citation weighted patent grants. Further analysis suggests that this result is more consistent with an explanation rooted in dampened inventor incentives in signaling their quality to the external labor market as compared to the more common mechanism discussed in the management literature, placing frictions on idea recombination by individuals.

These findings suggest that the *nonpatent* intellectual property legal environment (most prior work has focused on patenting) in which individuals and organizations operate can influence innovation outcomes. Paradoxically, our results suggest that what many firms and managers may lobby or wish for—a strengthened trade secrecy environment—may backfire with respect to inventor-level patent output. The topic of appropriation of R&D-based activities is, of course, a long-standing and important theme in the management of innovation/technology literature (e.g., Cohen, 2010). The literature has mainly followed variation in appropriation among industry sectors—for example, the classic Yale and Carnegie Mellon industry surveys organize managerial perceptions of appropriation along industry lines. By contrast, not only do we consider nonpatent-based intellectual property, we also look beyond traditional industry boundaries in appropriability environments. This difference results in part from trade secrecy law adjudication at the U.S. state level. The managerial implication is that there can be differences in appropriability environment within industries, and so more active firm management of IP form (rather than mere consideration of industry membership) may be important.

Beyond the high-level recognition that the knowledge worker labor market environment and the associated incentives generated can impact innovation, our work on the associated mechanisms also hold a number of managerial implications. While one theory consistent with the main empirical pattern is that reduced idea recombination and cross-fertilization may be the operative mechanism, our results are not consistent with this mechanism. On the other hand, the labor market signaling view has not received as much attention in the strategic management literature. Managers should be aware that employee innovative effort, aside from intrinsic motivation, may also rely on expectations of employment terms and career advancement as well as the ability to weaken potential hold-up problems inside a firm. Such advancement, in turn, may depend on both internal-to-the-firm and external-career paths. If frictions are placed on the external path, that may have behavioral implications for technical staff within the focal organization. Our results suggest that this may be manifested in dampened incentives as a result of the threat of internal hold-up as well as a redirection in inventive effort (concentrating on more general-purpose inventions that would not be subject to the restrictions of the IDD).

More generally, since trade secrecy is a commonly used form of intellectual property protection by industrial managers, a deeper understanding of how and when it is used, together with how it operates, should be of managerial interest. Beyond managerial implications, our results also relate to the geography of innovation policy discussions and the debates in that literature regarding the role of regional culture versus legal infrastructure in innovation outcomes (e.g., Gilson, 1999; Saxenian,

1994). Our results do not speak to the role of regional culture, but do support the view that regionally based legal infrastructure can alter innovation incentives and outcomes.

Alongside these implications of our study, it is important to clearly discuss the generality of our findings. We empirically study a particular quasi-natural experiment: an exogenously shifting trade secrecy regime in some U.S. states. In such a setting, our empirical strategy involves estimating how "exposed" individual inventors, who had previously been "untreated" are now affected in innovation outcomes in an altered business environment, as compared to inventors who had never been treated (a difference-in-differences design). This implies that our resulting estimates pertain to contexts in which we are able to observe tangible measures of innovation (via patent data and measures), which otherwise are difficult to observe. Furthermore, to mitigate the role of individual-level heterogeneity, our empirical approach relies on inventor-level fixed effects to assess within-person changes (which also has the effect of reducing the observations). Future work in other empirical settings would ideally triangulate both the main effects we estimate as well as further elaborate the mechanisms we test.

In conclusion, we hope that this study is a beginning of further empirical research deepening our understanding of the relationship between trade secrecy law and innovation.

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## APPENDIX A: TRADE SECRECY/PATENT SUBSTITUTION ANALYSIS IN THE COMPLEX TECHNOLOGY SUBSAMPLE

This appendix further discusses the possibility of intellectual property form substitution (trade secrecy vs. patenting) in the wake of differential strengthening of the employer-friendly trade secrecy regime. The main text concentrates on the case of "discrete" technologies, which are unlikely to face the substitution. For that reason, we rely on the discrete subsample to assess our mechanisms. This appendix discusses the possible substitution in the complex technology subsample, for the sake of contextual understanding rather than for direct evidence of mechanisms.

Substitution is more likely to hold in the context of complex technologies following trade secrecy regime shifts, and so the open question is how to understand and interpret the degree of substitution versus "real" effects of reduced innovation in this subsample. Even though we do not rely on this subsample for empirical assessment of our mechanisms, we wish to qualitatively understand the likely behavior of this subsample, especially since it comprises about two-thirds of our overall sample. Consider Table A1, which lists the two-digit SIC codes of complex technologies appearing in our sample, in descending order of frequency.

The first two columns list each of the two-digit SIC sectors contained within the "complex" technology broad category. The third column describes the relative frequency (as a percentage of the complex technologies) of each sector, and is sorted in descending order of incidence within our sample. The final two columns provide comparisons to two studies related to sector-level activity in the markets for technology (Serrano, 2010) and patenting propensity (Arundel & Kabla, 1998). The table and page references in the header of these final two columns refer to those contained within the cited research.

The markets for technology literature have identified patenting as important in transacting in the market for technology (Arora, Fosfuri, & Gambardella, 2001; Gans, Hsu, & Stern, 2008), and so we compare the industrial composition of our complex technology subsample to the findings in the literature describing the level of activity in the markets for technology along industry lines (Serrano, 2010) in the fourth column. There is a great deal of overlap between the complex industry subsectors in our sample and the Serrano (2010) subsectors with regard to market for technology activity. With the exception of the drugs/medical and chemical sectors, which experienced patent market transactions at a rate of 16% and 14.9%, respectively (*Note*: These two sectors are not categorized as complex technologies), the sectors comprising the complex technology sectors listed in the preceding table were the most frequently transacted in the Serrano sample.

**TABLE A1** Comparison of our subsample of complex technologies, by industry

| Two-digit SIC code | Industry description | Fraction of the complex subsample (within our data) | Serrano (2010) share of "traded" technologies [from Serrano's Table 2, Panel A, p. 693] | Arundel and Kabla (1998) sales-weighted patent propensity [from Arundel & Kabla's Table 1, p. 133. Baseline for all product innovations within their sample: 35.9%] |
|---|---|---|---|---|
| 36 | Electronic and electrical equipment (except computers) | 44.4% | 13.8% | 43.6% ("electronic equipment") |
| 35 | Industrial and communications machinery, and computers | 28.6% | 12.9% | 52.4%–56.8% ("machinery and office equipment"); 46.6% ("communications equipment") |
| 37 | Transportation equipment | 14.8% | 12% ("mechanical") | |
| 38 | Measuring, controlling, and analyzing equipment | 6.8% | | 56.4% ("precision instruments") |
| 34 | Fabricated metal products | 4.8% | | |
| 39 | Miscellaneous manufacturing | 0.6% | | |

Likewise, as shown in Column 5, using a survey of European industrial firms, Arundel and Kabla (1998) described the sales-weighted patent propensity of sectors also contained within our sample of complex technologies. The patenting propensity in the Arundel and Kabla (1998) data were systematically higher in each of the sectors they studied (ranging from 43.6% to 56.8%), which were also present within our set of "complex" industries as compared to their baseline patenting propensity for all product innovations in their sample (35.9%). This suggests that among our complex technologies, patent propensity may also be high.

One important interpretational note to these comparisons is that they are not, in general, made in the face of differential changes in one IP regime over another, as would be the case in IDD positive and negative rulings as compared to patent protection. Png (2015) examined such changes in the wake of state-level adoption of the Uniform Trade Secrets Act (UTSA). He found (his Table 5) that, in two sectors (electronic accessories [SIC3 code: 367] and general industrial machinery [SIC3: 356]), there is a negative and significant relation between UTSA and patent counts. These two three-digit SIC industries seem to belong within our first and third two-digit SIC industries (ranked by frequency of representation in our sample). It is notable that Png (2015) found no significant effect when he analyzed the computer and office equipment industry (SIC3: 357), which is our second most frequent "complex" technology by frequency.

Having examined the degree to which there might be substitution across forms of IP protection, we also verified that inventors were not switching from discrete to complex inventions/technologies (or vice versa) in response to the altered trade secrecy regime. We confirmed that this is not the case for both levels of each type of technology (discrete and complex) as well as the shares of each type (formal regression output available on request).