Trade Secrets, Labor Mobility, and Innovation

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Abstract

We examine whether restrictions on worker mobility achieve their policy goal of stimulating innovation through trade secret protection. Utilizing an economically representative sample of public and private U.S. firms, including startups, we find that, when courts increase labor mobility in a state, trade-secret-reliant firms in that state, relative to peers, significantly reduce innovation and subsequently reduce their self-reported reliance on trade secrets. These results are robust to a battery of tests and alternative explanations. We also find significant positive spillovers to non-trade-secret-reliant firms in the affected states, resolving the seemingly contradictory results in the existing literature studying aggregate effects.

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1. Introduction

Innovation is crucial to firm value creation, and trade secrets are important to a firm's innovation policy. Hall *et al.* (2014) find that a majority of U.S. firms that engage in research and development consider trade secrets to be at least as important to protecting their investment in innovation as is formal intellectual property, such as patents. Trade secrets enable firms to protect innovations that are either too difficult or costly to patent (Driver *et al.* 2024). To protect trade secrets, firms sometimes place labor mobility restrictions on employees and utilize courts to enforce them. The intended policy goal of such judicially enforced labor mobility restrictions is to incentivize firms to innovate by enhancing trade secret protections (Thomas 2014).

Two prominent court-enforced labor mobility restrictions in the U.S., at the state level, are the Inevitable Disclosure Doctrine (henceforth, "IDD") and non-compete agreements (henceforth, "non-compete(s)"). If a state's judiciary adopts the IDD, a firm can enjoin a current or former employee from accepting a job at a competitor if said firm can convince a court that its trade secrets would otherwise be inevitably disclosed (Weisner 2012). A non-compete is a contract between a firm and employee that prohibits the latter from taking a job at a competitor or starting a competing firm. State judiciaries vary in their willingness to adopt the IDD and enforce non-competes. The existing literature, discussed in detail in Section 2, finds that such labor mobility restrictions impact capital structure, capital expenditures, executive compensation, and other policies public firms. Additional studies examine effects on aggregate measures of innovation at the state or county levels, as well effects on individual inventor productivity.

However, no prior study, of which we are aware, addresses whether labor mobility restrictions encourage firms to invest in innovation *through the channel of trade secret protection*. One of the most important court decisions establishing the IDD deems this channel to be of first order importance: "The question of … inevitable [trade secret] misappropriation in this case lies at the heart of a basic tension in trade secret law, [which] serves to …'encourage invention and innovation.'"[1] We thus make an important contribution to the literature by conducting the first direct empirical tests of the proposition that court-imposed labor mobility restrictions achieve their stated policy goal of encouraging "invention and innovation" *through the channel of trade secret protection.*

[1] *PepsiCo v. Redmond,* 54 F.3rd 1261 (U.S. 7th Cir 1995)

The unique dataset we use, collected through the nationally representative Business Research, Development and Innovation Survey (BRDIS), allows us to provide insights prior studies cannot. The BRIDS collects data form public *and private* firms, including startups, whereas prior studies are limited to public firm data. The BRIDS data also allow us to explore a richer set of innovation variables unavailable to prior studies, including disaggregated measures of firm-level innovation inputs, such as research *or* development expenses, and outputs, such as sales from new products. Most importantly, the BRDIS data uniquely allow us to directly observe which firms consider trade secrets to be important to their business (henceforth "trade secret firms") and which do not ("non-trade-secret firms"). We can thus directly link to the trade secret channel any changes in innovation policy that occur after courts exogenously change labor mobility in their respective states.

We find that court rejections of the IDD cause trade secret firms to become significantly less likely to engage in research and development relative to non-trade-secret firms. We also find significant negative relative effects on trade secret firms' investment in physical capital devoted to R&D, the number of employees devoted to R&D, wages paid to R&D employees, the number of patent applications, and sales from new products. We cannot detect significant effects of changes in non-compete enforceability on innovation within our full sample. However, we find that court rulings rendering non-competes harder to enforce decrease R&D at trade-secret-dependent startups relative to the control group.

To conduct our tests, we employ a stacked, triple difference analysis to estimate the average treatment effects of several state court rulings using zero-inflated Poisson (ZIP) regression specifications. This method allows us to account for the high skewness of innovation data, as well as estimate treatment effects along the extensive and intensive margins. Our method requires only a weak identifying assumption to obtain valid causal inferences for *relative* treatment effects. Specifically, we analyze the difference in how trade secret and non-trade secret firms, within treated states, change their innovation policy in response to a court ruling (i.e., treatment). Then we test whether this difference in differences in treated states is different from that of control states. Our method is robust to spurious nationwide shocks impacting trade secret firms differently from non-trade secret firms, so long as this effect is not different in treated than in untreated states. Our method is also robust to spurious shocks that differentially impact treated and untreated states, so long as they do not differentially impact trade secret firms relative to

non-trade-secret firms. We require only the weak identifying assumption that no confounding shocks simultaneously coinciding with, but not caused by a state's treatment (i.e., court rulings), either 1) differentially impact the innovation policy of trade secret firms relative to non-trade secret firms in treated states; or 2) differentially impact the innovation policy of trade secret firms relative to non-trade secret firms in non-treated states (i.e., "bad" controls).

As with all studies utilizing a triple differences research design, we cannot rule out with certainty that some confounding shock violating our identifying assumption exists. Nevertheless, it is difficult to imagine a sensible economic story under which one could arise. Moreover, we provide suggestive evidence that our identifying assumption holds through pre-trend analysis. We find the trend in the difference between a trade secret and non-trade secret firm's innovation policy, in treated states, only begins to diverge from that of control states in the treatment year, and not before. Hence, any hypothetical shocks invalidating our identifying assumption would have to come in the same years as the court rulings we utilize as our exogenous shocks. Further, we note that courts base their rulings on technical legal reasoning, *not* economic conditions. Therefore, IDD and non-compete rulings are unlikely to be systematically related to non-trade-secret-related economic conditions that impact innovation policy.

The treatment effects we find are relative, *not* absolute: they measure a trade secret firm's treatment response *relative* to that of non-trade-secret firm. As labor mobility rulings change the relative competitive advantages of trade secret and non-trade secret firms, we hypothesize that negative absolute treatment effects of relaxed mobility restrictions on trade secret firms positively impact innovation at non-trade secret firms. Supporting this hypothesis, we find non-trade secret firms, located in the same industry and state as their treated trade secret peers, significantly *increase* their innovation activities following court rulings rejecting the IDD.[2] This finding suggests that relaxations of labor mobility restrictions cause a substitution into innovation more easily protected by formal intellectual property and away from innovation less easily so protected. This effect is likely welfare reducing, as prior research finds the two types of innovation are fundamentally different and *not* perfect substitutes (Boot & Valdmirov 2025). Our results on the substitution effect also resolve the seemingly contradictory results in prior literature that mobility restrictions have a small effect on the aggregate quantity of innovation:

[2] The existence of spillover effects implies the Stable Unit Treatment Value Assumption does not hold, so we can only draw inferences about relative, and not absolute, treatment effects from our main results.

the negative effect on trade secret firms' innovation gets offset by the positive effect on that of non-trade-secret firms.

To further test whether labor mobility restrictions impact a firm's innovation policy, we conduct a difference-in-differences analysis estimating the effects of labor mobility rulings on post-ruling intellectual property protection strategies. We find evidence firms located in states rejecting the IDD become relatively less likely to report that trade secrets are important to their business. This effect is not present in startups and is stronger for large firms. These results suggest large firms can find substitutes for trade secrets after protection is weakened, while startups cannot, explaining why we find IDD and non-compete rulings have particularly strong effects on startups.

To supplement our results studying IDD and non-compete rulings, we utilize an alternative removal of labor mobility restrictions following Fich *et al.* (2023). In addition to our IDD and non-compete rulings, in our stacked analysis we examine the effects of a state passing a corporate opportunity waiver (COW) law. Unlike IDD and non-compete rulings, which impact all employees, COW laws only impact managers and directors. We find COW laws reduce the innovation of trade secret firms relative to peers; our results on IDD and non-competes continue to hold in these specifications.

Further, our results concerning IDD and non-compete rulings remain robust across several additional tests. We do not find results are driven by the Uniform Trade Secrets Act (UTSA) adoption, the failure of legislation banning non-competes in Massachusetts in 2014, or "spillovers" to firms located in neighboring, untreated states. Further, while the ZIP methodology is the most appropriate given our data's skewness, we find consistent evidence utilizing linear ordinary least squares (OLS) fixed effects regressions. Additionally, we do not find evidence that firms are more likely to move to a new state following rulings. Finally, our results remain unchanged when we determine a firm's location by the proportion of its payroll expenses in a given state rather than by the firm's survey address.

Many previous studies use IDD rulings in a quasi-natural experimental design, so we consider whether our inferences are affected by the tendency of quasi-natural experiments to increasingly over-reject the null hypothesis the more they are re-used (Heath *et al.* 2023). For reasons discussed in Section 5.3, we argue this inference problem does not apply to our study.

Nevertheless, most of our inferences are unaffected if we adjust critical values for natural experiment reuse as suggested by Heath et al.

Our results have important policy implications. The question of whether firms should be allowed to restrict labor mobility to protect trade secrets is controversial and of interest to policy makers. While it is thought trade secret protection encourages firms to innovate, and innovation is socially beneficial, restricting labor mobility adversely impacts worker welfare (Rowe 2005) and reduces new firm entry into impacted states (Jeffers 2023). Our results can help inform policy makers who face this tradeoff.

We also contribute to the literature on trade secrets. There is extensive research on patents but substantially less on trade secrets, which are the means most used by firms to protect their investments in innovation (Hall *et al.* 2014). The question of how trade secret protection impacts innovation at the firm-level has been less studied than patent protection's effects. Our results provide some of the first quantitative estimates of the impact of two specific trade secret protection policies: The IDD and non-competes. Our finding that IDD rejection impacts patent applications also implies that trade secrets and patents are sometimes complements.

2. Literature review

The existing literature finds important empirical relations between the strength of trade secret protections and innovation (and other firm outcomes), primarily utilizing states' adoptions of the Uniform Trade Secrets Act (UTSA).[3] We provide new evidence on two additional trade secret protection policies not considered in the trade secret literature: the IDD and non-competes.

Almost all scholarly studies in economics, finance, accounting, and management on the IDD examine treatment effects on firm policies other than innovation. The most cited, Klasa *et al.* (2018), finds that firms significantly increase leverage following exogenous decreases in labor mobility restrictions from IDD rulings, while most others examine the impact on other non-innovation outcomes.[4]

[3] Bradley *et al.* (2023), Glaeser (2018), Guernsey (2019), Guernsey *et al.* (2022), Png (2017a), Png (2017b), and Wang (2023).

[4] Gao *et al.* (2023) on operating leverage; Ee *et al.* (2023) on debt maturity; Chowdhury and Doukas (2022) on cash holdings; Chen *et al.* (2021) on mergers and acquisitions; Dey and White (2021) on anti-takeover provisions; Flammer and Kacperczyk (2019) and Jia *et al.* (2023) on costly public relations efforts; Cao and Chen (2018) on hiring; Rowe (2005), (Na 2020), Canil *et al.* (2022), Li and Li (2020), Lin *et al.* (2019), Chen *et al.* (2022), and Yang and Zhang (2023) on executive compensation; Ali *et al.* (2019), Amore (2020), Arslan-Ayaydin *et al.* (2020),

A handful of studies examine the effect of IDD rejections on firm-level innovation outcomes. Nguyen *et al.* (2022) find that IDD rejections tend to decrease sales growth and the rate of new product introductions at smaller public firms. Kannan *et al.* (2022) use a difference-in-differences analysis to study the effects of the IDD on capital expenditures and research and development (R&D) expenses at public firms. Though they find an effect on capital expenditures, they fail to find one on R&D. However, none of these studies can link treatment effects on innovation to trade secret protection because, unlike us, they cannot observe which firms consider trade secrets to be important to their business.

The literature on the enforceability of non-competes is less vast. Jeffers (2023) finds rulings making it easier to enforce non-competes increase net capital expenditures at the firm-level, but she cannot detect a significant effect for R&D. [5] Jeffers (2023) argues that the insignificant labor mobility ruling treatment effects on R&D expenses could be misleading because such rulings are likely to impact wages as well as the number of employees devoted to R&D. Our unique data, which includes the number of R&D employees at a firm, allows us to overcome this limitation. Moreover, our unique ability to observe which firms are and are not trade secret dependent allows us test whether trade secret protection is the channel through which the treatment effects operate, rather than other channels.[6]

Several studies examine the effects of labor mobility restriction rulings on innovation, but not at the firm-level. Patel and Devaraj (2022) test for and fail to find evidence that IDD adoption significantly affects the rate of new business formation at the county-level. Jeffers (2023) finds court rulings making non-competes easier to enforce *decrease* the rate of new business formation at the state-level. Castellaneta *et al.* (2016) find IDD adoption *increases* aggregate venture capital funding at the state-level. Contigiani *et al.* (2018) find IDD adoption

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Callen *et al.* (2020), Callen *et al.* (2020), and Li *et al.* (2018) on disclosure policy; Peng and Yin (2021), Gao *et al.* (2018), and Oh and Park (2023) on earnings management; Li *et al.* (2022) and Ding *et al.* (2021) on corporate tax avoidance; Canil *et al.* (2023) and Cai *et al.* (2025) on payout policy. Qiu and Wang (2018) on stock price levels and overhead; Kim *et al.* (2021) on stock price synchronicity; Hu *et al.* (2023) and Li and Jian (2023) on crash risk; Dai *et al.* (2023) on analyst forecast accuracy; Li *et al.* (2025) on the bank efficiency and loan terms.

[5] Other studies on non-competes include Marx *et al.* (2009), Marx *et al.* (2015), and Younge and Marx (2016), who find a pro-firm non-compete ruling in Michigan impacted employee mobility, skilled worker outmigration, and the value of listed firms. Kini *et al.* (2021), Lin *et al.* (2022), and Starr *et al.* (2018), respectively, find that non-compete enforceability rulings impact executive compensation, CEO turnover, and employees leaving to form startups.

[6] For example, Seo and Somaya (2022) find that inventors are more likely to coauthor patent applications with coworkers when their ability to leave and form startups is hampered in states where courts apply the IDD. Loss of such incentives for within-firm cooperation, unrelated to trade secrets, could plausibly explain why court rulings increasing labor mobility might decrease firm innovation.

reduces the rate at which individual inventors produce patents. Mueller (2023) finds that, when inventors constrained by non-competes change jobs, they end up at less well-matched employers and become less productive. Like the studies on firm-level innovation, the above studies cannot speak to whether the treatment effects they find operate through the channel of trade secret protection, a limitation we overcome.

3. Institutional background and hypothesis development

In this section, we provide an institutional background on the law related to the IDD and the enforceability of non-competes. We also discuss how changes in legal doctrine related to the IDD and non-competes are likely to impact a firm's innovation policy.

3.1 The inevitable disclosure doctrine

If a firm is incorporated, or does business, in a state that has adopted the IDD, it can stop its former employees from accepting jobs at competitors, or starting a competing firm, if it can convince a court there is significant risk its trade secrets will be disclosed. The status of the IDD in a state is not determined by legislation, but by judges' interpretations of their state's trade secret laws, which is either a version of the UTSA (which all but a handful of states have adopted) or common law. Variation in IDD application exists across states and time because judges disagree on the correct interpretation of the law.[7]

Because IDD enforcement reduces the likelihood a former employee will disclose trade secrets to competitors, it plausibly reduces the odds that competition will erode a firm's returns on investments in innovations protected by trade secrets. Therefore, it is reasonable to hypothesize that the IDD encourages investments in innovation best protected by trade secrets. Because judges base their interpretations of state law on legal reasoning, and not economic conditions, rulings changing or clarifying IDD precedent constitute shocks to trade secret protection that are credibly exogenous to economic factors that impact a firm's innovation policy.

We gather information on IDD court rulings during our sample period (2008–2018) from various sources. From the second half of 2008 through 2011,[8] we use the comprehensive list in

[7] See Klasa *et al.* (2018) and Weisner (2012) for details on how the IDD is applied and for how differences in judicial interpretation create variation in its adoption across states and time.

[8] Because our sample period begins in 2008, for any treatment event in the first half of 2008 or earlier, we would only have post-treatment observations. Hence, we can only use rulings from the second half of 2008 or later.

Klasa *et al.* (2018), which is based on law review articles that include Weisner (2012), as well as legal treatises. According to Klasa *et al.* (2018) and Weisner (2012), there are no rulings that change or substantially clarify IDD precedents from the second half of 2008 through 2011.

To update the list of precedent-changing or clarifying IDD rulings through 2014, we turn to Qiu and Wang (2018), who state their list is current through 2014. We also consult Flammer and Kacperczyk (2019), who attempt to compile all IDD rejections (but not adoptions) through 2013. Surprisingly, the two lists are inconsistent where they chronologically overlap, as well as inconsistent with Klasa *et al.* (2018). To select the appropriate rulings for this study, we are conservative with respect to the existing literature. We carefully review all rulings on the Qiu and Wang and Flammer and Kacperczyk lists that fall between the second half of 2008 and 2014. After reviewing commentary in the legal literature about these cases, as well as the court opinions themselves, we verify that two rulings rejecting the IDD found on both lists, Massachusetts in 2012 and Georgia in 2013, constitute true exogenous shocks likely to change economic agents' priors about whether the IDD is likely to be applied in the state.[9] We determine the remaining cases on the two lists after mid-2008 do not change or clarify precedent, so we do not utilize them in this study. We discuss our reasoning on which cases to retain and discard in detail in Section IA1 and Table IA-1 in the Internet Appendix.

Our verification of the above rulings makes an important contribution to the literature because several studies rely on these Flammer and Kacperczyk (2019) and Qiu and Wang (2018) lists of rulings, and thus include cases that do not constitute true IDD shocks.[10] Studies with sample periods ending in 2011 or earlier generally rely on IDD rulings listed in Klasa *et al.* (2018), which we do not verify because they all occur before our sample period begins.

To obtain all relevant IDD rulings after 2014, as well as ensure the two more recent lists did not omit any rulings changing precedents from the second half of 2008 through 2014, we examine all state supreme and appellate court rulings returned from a LexisNexis full text search

[9] While it is unclear that the Massachusetts case, included by both papers and Klasa et al., reversed an IDD adoption, it brought legal clarity about the status of the IDD in the Commonwealth of Massachusetts, whose courts had previously applied it inconsistently (Weisner 2012). Georgia's IDD rejection is included in Flammer and Kacperczyk (2019) but excluded in Qiu and Wang (2018).

[10] For example, Kannan *et al.* (2022) include some of the cases listed in Flammer and Kacperczyk (2019) that are not true IDD rejections. Cai *et al.* (2025) use the Flammer and Kacperczyk (2019) list through 2013, and they add the alleged 2014 North Carolina IDD rejection from the Qiu and Wang (2018) list that we determine does not change IDD status in North Carolina. As far as we are aware, all other empirical studies, with sample periods extending beyond 2011, on the effects of changes in IDD status, use either the Flammer and Kacperczyk (2019) or Qiu and Wang (2018) list as their source for rulings after 2011, or some combination of the two.

from 2008–2018 that contain keywords "inevitable disclosure" or "inevitably disclose." In addition, as Jeffers (2023) does for non-compete rulings, we consult several prominent professional legal blogs that specialize in employment, trade secret, or intellectual property law.[11] Finally, we consult the section on the inevitable disclosure doctrine in each state's chapter in the authoritative Malsberger (2022) treatise on trade secret law. We find no additional IDD adoptions or rejections before 2018, leaving two IDD treatments during our sample period: the rejections of the IDD in Massachusetts in 2012 and in Georgia in 2013.

For our triple difference analysis of the IDD, we rely on two shocks, whereas recent studies utilizing the triple difference method typically include more. However, our relatively low number of shocks does not invalidate the analysis. At worst, it reduces statistical power. Moreover, we supplement IDD shocks with rulings on non-competes and corporate opportunity waiver (COW) laws.

3.2 Non-competes

In some states, courts will readily enforce virtually any non-compete. In other states, such as California, courts refuse to enforce any. Most states fall somewhere between these two extremes, allowing the enforcement of non-competes only if they are written sufficiently narrow and meet other conditions for reasonableness. As with the IDD, judicial disagreement on how to correctly interpret the relevant common and statutory law creates variation in non-compete enforcement precedents across states and time. Unlike the IDD, however, states also sometimes change non-compete enforceability through legislation, which is potentially endogenous to economic shocks that might impact innovation.[12]

To identify non-compete cases and legislation from 2014–2018, we follow Jeffers (2023) and consult professional legal blogs, as discussed in the prior section. We also consult an up-to-date state-by-state summary on non-compete enforcement practices published by a respected law firm.[13] We independently verify and use the list of court rulings and legislation that change non-compete's enforcements from Jeffers (2023), which spans from the latter half of 2008 through 2013.

[11] The blogs are restrictivecovenantreport.com by Jackson Lewis; employmentlawspotlight.com by Baker Hostetler; Seyfarth.com; and non-competes.com.
[12] See Jeffers (2023) for details on how non-competes function and how court rulings change their enforceability.
[13] https://beckreedriden.com/50-state-noncompete-chart-2/, accessed 12/31/2023.

We find three precedent-changing non-compete rulings after 2013: Kentucky in 2014, Pennsylvania in 2015, and Nevada in 2016. We do not utilize Nevada's ruling because we cannot observe firm trade secret status as of treatment because our Census survey did not ask the trade secret question in the relevant year.[14] We also find four instances of non-compete legislation: Arkansas and Alabama in 2015; Nevada in 2017; and Utah and Idaho in 2018. We follow Jeffers (2023) in *not* utilizing legislative changes in our research design because of their possibly endogenous nature. We classify changes affected by court rulings that make non-competes easier and harder to enforce as "pro-firm" and "pro-labor," respectively. Table 1 describes the non-compete court rulings we use in our analysis, as well as the IDD rulings.

The court-imposed changes to enforceability of non-competes during our sample period seem *a priori* less likely to have a strong effect on trade secret protection than rejections of the IDD, especially for large, established firms. We thus expect non-competes to have a weaker impact on innovation. Four of the ten rulings we consider change the requirement that employers must offer additional compensation for the non-compete to be enforceable. While these rulings plausibly affect a firm's cost of utilizing non-competes, the effect is unlikely to be large so long as labor markets are reasonably competitive. Two of the ten rulings either allow or reject so-called "blue penciling." This legal doctrine, "[g]ives courts the authority to either (1) strike unreasonable clauses from a non-compete, leaving the rest to be enforced, or (2) actually modify the agreement to reflect the terms that the parties could have, and probably should have, agreed to" (Pivateau, 2007). A change in "blue penciling" precedent surely impacts the care taken in drafting non-competes, which likely impacts legal fees, but it is hard to imagine it having a large impact on larger firms. The remaining four of the ten rulings are unique in how they change non-compete enforcement. In each case, the rulings plausibly change how costly it is for firms to use non-competes to protect trade secrets. The cost changes are likely to be modest, however, so we do not expect to see large effects for most firms. For startups, however, for whom the cost changes are likely to be more important, non-negligible effects are plausible.

[14] We exclude Nevada from the control sample to ensure our inferences are not impacted by its lack of inclusion in our treatment sample.

4. Data sources, sample construction, and descriptive statistics.

In Section 4.1, we discuss our data sources and how we construct our sample. In Section 4.2 we discuss how we scale and adjust raw data, followed by the presentation of descriptive statistics.

4.1 Data and sample construction

We begin constructing our dataset by considering all observations collected by the Business Research, Development, and Innovation Survey (BRDIS) from 2008–2016 and the Business Research and Development Survey (BRDS) from 2017–2018, for a total sample of firms from 2008–2018. For brevity we refer to this combination of BRDIS/BRDS henceforth as BRDIS. The BRDIS is conducted annually by the National Science Foundation and U.S. Census Bureau. We exclude observations for firms that report they are majority owned by another firm. We also exclude observations based on survey forms BRD-FED, BRD-1S, or BRDI-1S because they did not include the question asking a firm to report the importance of trade secrets to its business. We also exclude observations where the field indicating which form was being used is missing. Within this set of observations, wherever the value of a variable is missing, we reasonably assume it is zero, as firms are legally required to respond and can be fined can be fined for providing incorrect information.

We keep the following annual BRDIS variables measuring firm-funded (as opposed to government funded) domestic innovation inputs (which are also used in Driver *et al.* (2024)): combined research and development expenses (i.e., R&D), research expense (disaggregated from development expense), development expense (disaggregated from research expense), labor costs (wages) associated with R&D, non-labor-related R&D, expenditures on capital equipment used in R&D, and the count of employees devoted to R&D. We focus on domestic inputs only because the rulings we study can impact domestic operations.

We keep several additional annual BRDIS variables measuring R&D outputs: sales attributable to products or services new to a firm's markets in the year; sales attributable to products or services new to the firm but not its markets; and the count of patents a firm applied for at the USPTO. If a firm indicates that trade secrets are somewhat or very important to its business, we set the value of our trade secret importance variable to one, and zero otherwise. Hence, unlike, Glaeser (2018), we need not rely on noisy proxies for trade secret dependence. We use a firm's real sales as our proxy for a firm's size. Finally, we keep the state the firm

reports as its primary address and designate it as the firm's location for the year. In robustness tests, described in Section 5.7, we find consistent results when we require the firm to have at least 50% or 100% of payroll expenses in a treated state.

We supplement our BRDIS-based sample with the Longitudinal Business Database with Revenue (LBDREV) dataset using internal Census firm identifiers specific to each year. From the LBDREV data we keep the total domestic employee count and total domestic payroll. We drop firm-year observations with less than three employees to ensure our results are not driven by cottage industry firms. We use the sales from the BRDIS rather than revenue from LBDREV to construct our firm size measure because the BRDIS variable is better populated. We use the age of the firm's oldest establishment to measure a firm's age. The firm identifiers in LBDREV do not always correctly longitudinally link observations for the same firm, whereas Census establishment identifiers are more longitudinally reliable. Therefore, we follow the algorithm in Driver (2023) that uses establishment identifiers to create longitudinally consistent firm identifiers in our merged panel dataset.

We next construct our master sample from our merged dataset. It consists of the union of treatment and control samples (defined below) for each year where there is at least one treatment listed in Table 1. As it takes time for firms to adjust their R&D budgets to changes in the legal environment, we designate the year after the ruling as the "treatment year" for rulings that occur after June 30. If the ruling takes place in the first half of the year, we designate the year of the ruling as the "treatment year."

Though Nevada had a court ruling related to non-compete agreements in the second half of 2016, making 2017 Nevada's treatment year, we exclude all Nevada firms from our sample because the BRDIS did not include the trade secret question in 2016. In addition, we exclude observations from firms in the states that adopted endogenous legislation on non-compete enforcement in the years indicated, and all years following (Arkansas, Hawaii, and Alabama in 2015; Utah and Idaho in 2018). Finally, we exclude observations for Georgia before 2011 because the state legislature passed endogenous legislation in that year. A state experiencing one of the treatments in Table 1 is referred to as a "treated state," and a firm that lists that state as its primary address on its BRDIS form in the treatment year is henceforth referred to as a "treated firm."

There is at least one treatment sample for each year there is at least one court decision (2010–2016). For treatment years 2010 through 2013, inclusive, there are multiple treated states, so there are multiple treatment samples. The treatment sample corresponding to a treated year includes all observations in our merged dataset for firms located in treated states as of the year of treatment. For all treated firms except those located in Illinois in 2012 and 2013,[15] we also include in the treatment sample corresponding to a given treatment year all available BRDIS observations for these firms in the two years before treatment, the year of treatment, and two years after treatment. However, we exclude observations for treated firms in the years before treatment for which a firm was in a state different from its treatment year's state.

For each treatment year, in addition to the treatment sample, there is a control sample. This sample consists of all observations in our merged dataset, from two years prior to two years after the treatment year, inclusive, for all firms in BRDIS that are never located in a treated state over our entire sample period (i.e., 2008–2018). Within each treatment and control sample, if a firm answers on its BRDIS form that trade secrets are somewhat or very important to its business in the year before treatment, we designate the firm as susceptible to treatment. We use the year before treatment because firms may endogenously change their reliance on trade secrets in response to a court ruling.[16]

We classify a firm as treated but not susceptible to treatment if it is in a treated state as of the year of treatment but reports trade secrets are not important to its business as of the year before treatment. We classify firms as susceptible to treatment but not treated if they report that trade secrets are somewhat or very important in the year before treatment but are not in a treated state as of the year of treatment. Finally, firms are treated and susceptible to treatment if they are in a treated state as of the year of treatment and say that trade secrets are important to their business in the prior year.

[15] Treated firms located in Illinois in 2012 or 2013 have different inclusion criteria because Illinois experienced back-to-back treatments, each different in nature, in 2012 and 2013. To prevent the 2013 Illinois treatment from contaminating our inferences on the 2012 treatment, for the 2012 treatment sample, we only include observations for firms located in Illinois as of 2012, as well as all observations for these firms over 2010–2011 for which the firm was located in Illinois. In the 2013 treatment sample, we only include observations for firms located in Illinois in both 2013 and 2012, for the years 2012–2015, to ensure the 2012 treatment does not confound our inferences about the effect of the 2013 treatment.

[16] If the field is missing in the year before treatment, we assign susceptibility status based on the last non-missing value of the trade secret field the firm has reported up to that point within two years. If the trade secret field is missing for all two years before treatment, we exclude the firm.

The master sample consists of the union of treatment and control samples for treatment years 2010–2016, inclusive. As a result, a BRDIS firm-year case can appear multiple times in the master sample. We thus define the variable "cohort," which is set equal to the treatment year that defines either the treatment or control sample to which the observation belongs. In this way, we differentiate an individual observation belonging to one control sample from other observations drawn from the same case that are part of other control samples. The following combination of variables uniquely defines each observation: cohort, the longitudinally consistent firm identifier, and the observation year. We include cohort-by-firm and cohort-by-state-by-year fixed effects in all regressions discussed in Section 5.

Additionally, we exclude some observations for the following two reasons. First, to be either in a treatment or control sample for a given treatment year, the firm must have at least one observation in the two years before the treatment year and at least one either in the treatment year or within the two years afterward. This restriction ensures we identify firm-level effects of the judicial decisions. Second, two states adopted the USTA during our sample period: New Jersey in 2012 (Milligan 2013) and Texas in 2013 (Mason 2013). To ensure that these potentially endogenous legislative acts are not confounding our inferences on the effects of IDD and non-compete rulings on innovation, we exclude firms in these states in the years surrounding their passage from our analysis.

4.2 Variable adjustments and descriptive statistics

Table 2, Panel A presents descriptive statistics for the innovation input and output dependent variables, as well as a firm's age, a firm's real 2018 USD sales, and a firm's total employee count. In Panel B we present statistics on the frequency with which our indicator variables take the value of one. Note that Census disclosure rules require rounding reported values.

We use the annual consumer price index for all urban consumers, as published by the Bureau of Labor Statistics, to convert yearly, nominal USD to real 2018 USD. Due to the nature of the data, we utilize zero-inflated Poisson regressions (ZIP), discussed in detail below. As such, we use unscaled, real 2018 USD values of the innovation measures, as well as unscaled counts of R&D employees and patent applications as the dependent variables. For specifications where outcomes are measured in real USD, we include the natural logarithm of real firm total payroll, averaged over the two years before treatment, as a covariate to control for size (sales are

sometimes zero, so we cannot use the logarithm of sales). For non-monetary outcome variables, the natural logarithm of total employees is our size control.

Panel A reveals a great deal of variation in firm size across our sample. The median observation has real 2018 USD sales of approximately $36 million, whereas the 25th percentile is at approximately $4.1 million. The 90th percentile firm has real 2018 USD sales of nearly $1.5 billion. Similar patterns are apparent in real payroll and employee counts.

Panel B describes indicators for whether a firm is ever subjected to a given treatment type, an indicator for whether the firm is susceptible to treatment (*Trade*), and finally, an indicator for whether the observation in a given cohort comes in or after treatment year corresponding to its cohort ($Post$). Just over half of our observations correspond to firms that report trade secrets are important in the year before treatment. Just slightly over half the observations are post-treatment. The fraction of firms that are treated is not large, which is expected given the relatively small number of states that experienced treatments during our sample.

5. Research design, results, and robustness tests

In this section, we first describe our research design and our identifying assumptions. We then discuss the results for our main analyses and from additional robustness tests.

5.1 Research design for estimating the effect of labor mobility rulings on innovation policy

We employ a stacked, triple difference research design, where treatment and susceptibility to treatment are defined in Section 4.1. We first compare how innovation policy changes, from before to after treatment, for treated trade secret (susceptible) firms relative to non-trade secret (not susceptible) treated firms. We then compare this difference-in-differences in treated states with the analogous, contemporaneous difference-in-differences in control states. If we find that innovation outcomes change differently for trade secret firms relative to non-trade secret firms in treated states, compared to the analogous difference for firms in non-treated states, we infer the court rulings have causal relative treatment effects via the channel of trade secret protection.

By comparing the changes in innovation policy of trade secret firms to that of non-trade secret firms within treated states, we can rule out the possibility that our inferences are being confounded by economic shocks coincidental to court rulings that similarly impact the

innovation policy of trade secret and non-trade secret firms within treated states. The third difference in our triple difference analysis makes inferences robust to any national confounding events, coincidental with but unrelated to the court rulings, that differentially impact the innovation policy of trade secret and non-trade secret firms.

To draw causal inferences, we must assume that there are no economic shocks, only coincidental to the court decisions, that differentially impact the innovation policy of trade secret firms and non-trade secret firms, and *that also have a stronger effect in treated states.* As with all studies utilizing a quasi-natural experiment research design, our identifying assumption cannot be proved true. However, it is difficult to imagine a sensible economic story under which it would be violated. Moreover, in Section 5.4, we provide suggestive evidence our identifying assumption holds: we do not detect any treatment effects in the years before rulings, only afterward. We can thus rule out confounding shocks that occur close to, but not in the same year as, exogenous court decisions.

5.2 Zero-inflated Poisson regressions

Table 2, Panel A reveals our innovation variables take the value of zero for a significant number of observations and are highly skewed. As a result, linear models are almost surely misspecified and would produce biased and inconsistent estimates. When dependent variables are highly skewed and non-negative, it is now common practice to use Poisson regressions (Cohn *et al.* 2022), even for dependent variables that are not counts.[17] The large number of observations with zeros in our data, however, suggests Poisson regressions are also misspecified, and a model that accounts for excess zeros is required.

Zero-inflated Poisson regressions are particularly well-suited for our data. The nature of some firms' business models is that they never engage in research and development (R&D) activities. Other firms engage in intense innovation during some parts of their lifecycle and none in others, while still others are always innovating. Thus, for some firm-year observations, innovation variables are almost sure to be zero, but for others some innovation activity is plausible (even if it is zero in a given year). The zero-inflated Poisson regression explicitly models this reality. We can thus parameterize treatment effects on a firm's decision to pursue

[17] Silva and Tenreyno (2006) show that Poisson regressions are valid for continuous and other non-Poisson variables, so long as the conditional expectation has the exponential functional form.

innovation (extensive margin), as well as effects on the magnitude of innovation conditional on pursuit (intensive margin).

5.2.1 Zero-inflated Poisson specification details

Formally, the zero-inflated Poisson regressions are specified by the following system:

$$P\left(Y_{i,k,t} \sim \text{Poisson}|\mathbf{X}_{\text{i,k,t}}\right) = \Lambda\left(\alpha + \boldsymbol{\beta}'\mathbf{X}_{\text{i,k,t}}\right) \quad (1)$$

$$\text{E}(Y_{i,k,t}|Y_{i,k,t} \sim Poisson, \mathbf{X}_{\text{i,k,t}}) = \exp(\alpha + \mathbf{B}'\mathbf{X}_{\text{i,k,t}}) \quad (2)$$

Where $Y_{i,k,t}$ is one of our unscaled innovation input or output variables, $\Lambda(\,)$ is the logistic cumulative distribution function, $\mathbf{X}_{\text{i,k,t}}$ is the vector of covariates, while $\boldsymbol{\beta}$ and $\mathbf{B}$ are coefficient vectors. The subscripts *i*, *k*, and *t* index the firm, cohort treatment year, and the observation's year, respectively. We assume that $Y_{i,k,t}$ is either Poisson distributed or that it just takes the value of zero. Equation (1) models the probability that $Y_{i,k,t}$ is Poisson distributed, conditional on the covariates (the extensive margin), while equation (2) models the expected value of $Y_{i,k,t}$ conditional on the covariates and on $Y_{i,k,t}$ being Poisson distributed (the intensive margin).

Vector $\mathbf{X}_{\text{i,k,t}}$ includes the following elements: $IDD_{i,k}$, an indicator for whether firm i experiences an inevitable disclosure doctrine ruling in cohort *k*; $NC_{i,k}$, a trinomial indicator, defined following Jeffers (2023), taking the value of +1 or -1 if, respectively, firm *i* experiences a pro-labor or pro-firm non-compete ruling in cohort *k*, and zero otherwise; interaction terms $Trade_{i,k}IDD_{i,k}$ and $Trade_{i,k}NC_{i,k}$, where $Trade_{i,k}$ is an indicator variable for whether firm *i* reports trade secrets are somewhat or very important to its business in the year *before* cohort *k*'s treatment year; interaction terms $IDD_{i,k}Post_{k,t}$ and $NC_{i,k}Post_{k,t}$, where $Post_{k,t}$ is an indicator for whether the observation comes in or after cohort *k*'s treatment year*;* triple interaction terms $Trade_{i,k}IDD_{i,k}Post_{k,t}$ and $Trade_{i,k}NC_{i,k}Post_{k,t}$; variables $Post_{k,t}$ and $Trade_{i,k}$, and their interaction $Trade_{i,k}Post_{k,t}$; $\ln\left(RealPay_{i,k}\right)$, the natural logarithm of average real total payroll for firm *i* over the two years prior to its cohort *k*'s treatment year (to control for firm size for models where outcome variables are measured in real 2018 USD); $\ln\left(DomEmp_{i,k}\right)$, the natural logarithm of the average count of domestic employees at firm *i* over the two years prior to cohort *k*'s treatment year (to control for firm size for models where outcome variables are measured in counts); the natural logarithm of one plus firm age and its square to control for firm life cycle effects. To control for unobserved, time-invariant firm heterogeneity, as well as time varying unobserved common factors impacting all firms, we follow what Wooldridge (2021) calls the

"Mundlak approach" and include as additional covariates the within-year and within-firm means of all other covariates, except $Trade_{i,k}$, $IDD_{i,k}$, $NC_{i,k}$, $Post_{k,t}$, and their interactions. We thus eliminate the need to include time- and firm-fixed effects, which can create biases in non-linear models due to the incidental parameters problem.[18]

We estimate the parameters in equations (1) and (2) for our stacked sample using the quasi-maximum likelihood method of Cameron and Trivedi (2005). We compute standard errors by clustering on firm, the state-year, and the firm's four-digit primary NAICS industry, making our inferences robust to serial correlation, as well as robust to within-state and within-industry cross-sectional correlation. Relative treatment effects are parameterized by the coefficients on the triple interaction terms.

5.2.2 Zero-inflated Poisson regression results

We estimate equations (1) and (2) on the full sample in Table 3. We control for firm size using total payroll or employee count, based on whether the dependent variable is a monetary value or physical count. We do not control for total assets or total expenses, as is common in the literature, because these variables are not available for most firms in our sample. In all cases, we only consider domestic R&D inputs, as U.S. state court rulings do not impact employees at foreign R&D operations (less than 1% of the firms in our sample have foreign R&D.) Columns 1, 3, and 5 report the impact of independent variables on the odds a firm reports non-zero innovation (i.e., extensive margin); columns 2, 4, and 6 report the impact on the intensive margin of innovation. We first estimate equations (1) and (2) on innovation spending inputs as the dependent variables: research and development (R&D), research, and development. These results are reported in Panel A of Table 3.

As reported column 1, the rejection of the IDD has a large, negative average treatment effect on the extensive margin of a firm's investment in R&D: an IDD rejection leads to a 66% relative reduction in the odds that a trade secret firm invests in R&D. This effect is statistically significant at the 1% level. We also observe negative and significant effects for the extensive margins of both research and development when modeled separately (columns 3 and 5). These effects are also economically significant (relative reductions of 54% and 59% in the odds of

[18] Wooldridge (2021) shows that, under general conditions, that difference-in-difference estimators obtained from linear and non-linear pooled regressions following the Mundlak approach control for unobserved, time-invariant heterogeneity across cross-sectional units, as well as time-varying common factors. It is easy to show that his result extends to the triple difference estimator.

investing in research and development, respectively) as well as statistically significant (both at the 1% level). Despite the significant extensive margin treatment effects on R&D for an IDD rejection, we do not observe a similar significant intensive margin effect. Therefore, it appears IDD rejection primarily changes which firms invest in *any* R&D, but not the amount of R&D investment once they invest. This finding is consistent with the removal of labor mobility restrictions leading firms to undertake significant shifts in innovation activity. Further, we do not find significant effects for rulings on non-competes in the general sample. Finally, we find firm size is positively related to investment at both the extensive and intensive margins, while age is positively, but non-linearly related to investment along the extensive margin.

A large fraction of R&D expenses consist of labor costs. Therefore, Jeffers (2023) argues any labor mobility ruling effects on total R&D expenses could be misleading because such rulings are likely to jointly impact wages and R&D employment. It is plausible that an IDD rejection could result in an increase in R&D worker pay for retention reasons, which, in turn, would result in an increase in R&D expenses, even if there were no real change in the allocation of labor or capital to R&D. The richness of our data allows us to overcome this problem because we observe the number of employees devoted to R&D and wages paid to these employees.

In Panel B, we estimate other versions of equations (1) and (2) where the dependent variable is the real value of R&D capital expenditures, the real value of R&D wages, and the count of employees devoted to R&D activities. Columns 1, 3, and 5 report the coefficient estimates for equation (1), while columns 2, 4, and 6 report coefficient estimates for equation (2).

Consistent with Panel A, results in columns 1 and 2 imply IDD rejection significantly impacts a treated trade secret dependent firm's investment in R&D capital expenditures along the extensive, but not the intensive margin. Treated trade secret firms experience a 57% (significant at the 1% level) relative reduction in the odds of investing in any capital expenditures related to R&D. Columns 3–6 demonstrate IDD rejection also has a real extensive margin impact on a treated trade secret firm's R&D wages. These firms see a 65% relative reduction in the odds of having any R&D wages (significant at the 1% level) and a 75% relative reduction in the odds of having any R&D employees. Along the intensive margin, we find a treated trade secret firm sees a 28% relative increase in R&D wages (significant at the 5% level), but our estimate of the intensive margin effects on R&D employees is statistically indistinguishable from zero. We infer a treated trade secret firm continuing R&D activity post-ruling spends more on R&D wages

without increasing R&D employment. Rulings on non-competes do not have statistically significant relative average treatment effects on capital and labor innovation inputs for treated trade secret dependent firms along the extensive or intensive margins.

We now examine how an IDD rejection affects the following innovation outputs: the number of patents a firm applies for at the USPTO, the sales from products new to a firm, and the sales from products new to a market or just to a firm. We report results in Panel C of Table 3. As shown in Panel C, we find evidence an IDD rejection significantly reduces the innovation outputs of treated trade secret firms. Findings within the extant literature suggest that, at least in aggregate, increases in labor mobility will lead to an increase in patenting due to a substitution effect from innovation through trade secrets to formal intellectual property (Png 2017b; Bradley *et al.* 2023). However, there are inherent and large differences between patentable and non-patentable innovation as discussed by Hall *et al.* (2014). Hence, it is reasonable to hypothesize that decreases in labor mobility will lead treated trade secret firms to reduce innovation inputs (both spending and employees) or stop investment in innovation inputs altogether, leading to a reduction in patenting. Results in columns 1 and 2 support this hypothesis: an IDD rejection significantly reduces patent applications along both the extensive and intensive margins for treated trade secret firms. Specifically, an IDD rejection reduces the odds of a firm filing any patent applications by over 70% (significant at the 1% level) along the extensive margin and reduces the count of patent applications by 39% (significant at the 5% level) along the intensive margin. We also find some evidence that an IDD rejection reduces the likelihood a treated trade secret firm has any sales from products new to its company by 27% (significant at the 10% level) in column 3. Finally, we find evidence that an IDD rejection significantly reduces a treated trade secret firm's sales of products new to their market by over 60% (significant at the 5% level) along the intensive margin. On balance, we find compelling evidence that an increase in labor mobility, via IDD rejection, significantly reduces the innovation outputs of treated trade secret firms.

We also find a marginally significant negative effect of non-compete rulings on sales from products that are new to a firm but not its market along the extensive margin. Specifically, treated trade secret firms reduce the likelihood of having any sales of products new to their firm but not their market by 28% (significant at the 5% level) in response to a weakening of non-

compete agreements. Given we find no other significant effects from these rulings, we caution the reader from drawing strong inferences.

While existing research documents labor mobility restrictions imposed by enforcing the IDD reduces employees' incentives to produce patents (Contigiani *et al.* 2018), we find evidence that the labor mobility restrictions imposed by IDD increase employers' incentives to invest in innovation. A potential source of these differing findings may result from our ability to identify firms where trade secrets are important to a firm's innovation policy. The rejection of the IDD plausibly harms trade secret firms while creating positive spillovers to their non-trade-secret peers. We find evidence consistent with this explanation in Table 3. We find the coefficient on *IDD* × *Post*, representing the effect of removing IDD on the innovation policies of firms not reliant on trade secrets in impacted states, to be positive and significant for most specifications along the extensive margin. These results demonstrate the importance of firm heterogeneity across trade secret reliance on studying how labor mobility restrictions impact firm innovation. We further study these spillover effects in Section 5.6.

To resolve the extensive margin effects concerning IDD rejections on R&D, we compare the proportion of trade secret and non-trade secret firms that have positive R&D in treated and control states, before and after treatment. As reported in Panel A Table 4, we see approximately 90% of trade secret firms in treated states have positive R&D before treatment and this percentage drops to 70% after treatment. For non-trade secret firms in treated states, this percentage increases from 23% before treatment to 53% afterward. These changes are large in magnitude, but do not seem implausible to us. In Panel B of Table 4, we report analogous figures for contemporaneous trade and non-trade secret firms in control states. We also see a decline in R&D for trade secret firms relative to non-trade secret firms in control states after IDD rejection in treated states, but the relative decline is significantly smaller in the control states, consistent with our regression results in Table 3.

We next consider how treated trade secret firms that stop their R&D pursuits post-IDD rejection differ in various characteristics from treated trade secret firms that continue R&D post-ruling. We plot kernel densities for these characteristics and compare them for these two groups in Figure 1. We consider real 2018 USD sales and payroll, averaged over the two years before treatment, as well as firm age in the year before treatment. All three variables are highly skewed, so we transform them: payroll and one plus age using the natural logarithm, and sales, because

they are sometimes zero, using the inverse hyperbolic sine. As is apparent from the plots in Figure 1, treated trade secret firms that stop R&D are somewhat smaller and younger than those that continue it. The plots also suggest these differences are not attributable to outliers, as they are apparent everywhere along a variable's support.

We next consider how reported average treatment effects might differ for large firms and startups who depend on trade secrets. Costs associated with patenting and litigation, as well as labor market benefits associated with increased labor mobility, likely make the effects of changes in trade secret protection different for these two firm types. We define large firms as those that have pre-treatment, average, real 2018 USD sales above $220 million, which is approximately the 75th percentile in sales for the full sample in Table 3.[19] We define startups as all firms that are less than six years old at treatment and do not fit our definition of "large." We report these results in Table 5 ("large" firms) and Table 6 (startups). For brevity, we report coefficient estimates only for our variables of interest; the rest we report in tables in Tables IA-2 through IA-7 in the Internet Appendix.

The results for "large" firms in Table 5 are consistent with the findings in the full sample in Table 3. In Panel A, we find "large," treated trade secret firms are significantly less likely to invest in R&D, research, or development, following IDD rejections along the extensive margin (relative to control group), with generally similar or larger economic effects. We find similarly large reductions along the extensive margin in Panel B (capital expenditures related to R&D, R&D wage expenses, and R&D employees). In Panel C, we no longer find a significant reduction along the extensive margin of patent applications, sales of products new to the company, and sales of products new to a firm's market. However, we continue to find significant reductions in both patent applications and sales of products new to the market along the intensive margin. Finally, we do not observe any significant responses to innovation inputs or outputs for "large," treated trade secret firms as a result of non-compete rulings. We infer that IDD

[19] It is important to note that we do not use other common measures of size such as book value of assets or the market capitalization of equity since our sample includes private firms where these are not available. In addition, while the definition for a "large" firm as real 2018 USD sales exceeding $220 million is the 75th percentile for our sample, it is relatively smaller than a similar cutoff using only public firms. For example, over the same 2008–2018 sample period, the 75th percentile in real 2018 USD sales for publicly traded non-financial and non-utility firms listed on Compustat is over 5 times as large (>$1.1 billion). Utilizing an alternative higher cutoff is difficult in our setting due to U.S. Census disclosure requirements due to (too) small sample sizes of treated firms.

rejections have a negative effect on innovation inputs and outputs for "large," treated trade secret firms, but there are no analogous detectable effects for non-compete rulings.

We report results for startups in Table 6. While the effects of IDD rejections on the innovation inputs and outputs of trade secret firms are generally similar between the full sample, "large" firms, and startups, some important differences emerge. As shown in Panel A, IDD rejections significantly reduce a startup firm's investment in research along the extensive margin. However, we do not detect significant changes (though the signs remain negative) to investments in development or aggregated R&D. Interestingly, we find that non-compete rulings significantly impact the intensive margin of a treated trade secret startup's investment in R&D and research, reducing investment in R&D by 55% and research by 76% relative to the control group. These effects are significant at the 1% level. Thus, it appears that while non-compete rulings do not significantly impact the innovation policy of "large," treated trade secret firms, they do significantly impact treated trade secret startups.

Turning to Panel B, IDD rejections negatively impact investments in capital expenditures related to R&D along the extensive margin for treated trade secret startups. We also find that treated, trade secret startups reduce the number of R&D employees by roughly 44% along the intensive margin relative to the control group, an effect that is significant at the 5% level. In Panel C, we report the effect of IDD rejections and non-compete rulings on innovation outputs. Unfortunately, due to U.S. Census disclosure requirements, we are unable to report ZIP estimates for startups when studying patent applications. However, we find that rulings weakening non-competes significantly reduce sales of products new to the company. This effect is economically large and statistically significant. On average, treated, trade secret startups realize a reduction in sales of products new to the firm by 92% (significant at the 1% level) in response to rulings that weaken non-compete agreements, relative to the control group. Further, this evidence is consistent with the findings in the general sample in Panel A. Thus, it appears the negative effect of weakening non-competes on new sales to the company along the intensive margin in the general sample is driven by a large reduction by treated, trade secret startups.

In general, we find evidence that increases to labor mobility through IDD rejections negatively impact the innovation inputs and outputs of all treated, trade secret dependent firms, including "large" and startup firms. Non-compete rulings do not have detectable treatment effects on "large," trade secret firms. However, we find evidence that non-competes significantly

reduce treated, trade secret startups' investments in R&D, research, and reduce sales from the introduction of products new to the firm.

5.3 Reuse of Natural Experiments

Heath *et al.* (2023) demonstrate that, under some conditions, hypothesis tests utilizing conventional critical values become increasingly likely to over-reject a null hypothesis as exogenous shocks are increasingly reused in natural experiments. By our count, almost 40 prior studies have utilized IDD rulings in quasi-natural experiments, but the problem of reuse is mitigated in our study for two reasons. First, our set of exogenous shocks is different from that of prior papers. Almost all prior IDD studies end their sample period before ours begins, so we use a completely different set of IDD rulings. As far as we are aware, only five IDD studies predating ours, summarized in Table 7, use a set of rulings that overlap with ours.[20] However, as we show in the Internet Appendix, most of the more recent rulings in these studies are misclassified. Thus, these five studies use only a few IDD rulings that we use. Second, the treatment effect we study, that of encouraging innovation through trade secret protection, is the primary, intended treatment effect of IDD adoption, whereas prior studies test secondary, unintended effects. As Heath *et al.* (2023) argue, "intended treatment effects have a lower statistical hurdle." As adjustments to critical values are only necessary for tests of the secondary effects, we maintain our inferences are sound and require no adjustments. Nevertheless, we examine whether our inferences in the previous section hold if we use the higher critical values derived in Heath et al.

As can be seen in Table 7, the five prior IDD studies that use some rulings in common with ours collectively examine 16 distinct outcomes. According to Table A1 in Heath et al, the critical value of the *t*-statistic for 5% significance is 2.92 when a natural experiment has already been used to study 16 outcomes. All extensive margin IDD treatment effects, apart from New Co. Sales, indicated as statistically significant in Table 3 have *t*-statistics well above 2.92. The intensive margin effects that are indicated as statistically significant in Table 3, however, do not.

Next, we reexamine significance of IDD treatment effects on subsamples. Of the six "large" firm extensive margin IDD treatment effects that are indicated as statistically significant in Table 5, four (research, development, R&D CAPX, and R&D employees) have *t*-statistics

[20] Some of the IDD rejections used in a sixth study, Li *et al.* (2025) overlap with ours, but our study predates theirs.

above the 2.92 hurdle. The extensive margin effects of IDD on research and R&D on startups in Table 6 also have *t*-statistics well above 2.92.

Finally, we re-evaluate the significance of non-compete ruling effects on startups reported in Table 6. While we count four prior studies estimating treatment effects of non-compete rulings, only one, Jeffers (2023), uses a set of rulings that overlaps with ours. Critical values are not significantly higher after only one reuse of a natural experiment, so our inferences for non-competes are unaffected.

5.4 Dynamic treatment effects

The identifying assumption behind our triple difference research design is that, without the state court rulings in question, the trend in the differences between trade secret and non-trade secret firm innovation policy in treated states would have remained parallel to the trend in the differences between trade secret and non-trade secret firms in untreated states. Our finding of a negative relative average treatment effect of IDD rejections on trade secret firm innovation could be spurious if the difference between trade secret and non-trade secret firm innovation were already trending more negatively prior to treatment in treated states than in non-treated states.

To test for pre-treatment trends, we estimate saturated zero-inflated Poisson specifications. These specifications are like those presented in equations (1) and (2), except we replace the variable $Post_{k,t}$ with a vector of four indicator variables $D_{k,t,j}$, for common year values of $j \in \{-2, 0, 1, 2\}$, with the year prior to treatment (i.e., year -1) acting as the reference year. Each $D_{k,t,j}$ takes the value of one if an observation falls j years after (or $-j$ years before) cohort k's treatment year and is zero otherwise. We also replace each term that interacts with the variable $Post_{k,t}$ in equations (1) and (2) with interactions with all four indicators $D_{k,t,j}$. Thus, variable interactions with the $D_{k,t,j}$ indicators represent said variables' effects relative to the -1 reference year. The null hypothesis that there are no differences in pre-trends in the data, between trade secret firms and non-trade secret firms in treated states relative to trade secret firms and non-trade secret firms in control states, implies that coefficients on the triple interaction terms of $IDD_{i,k}$ and $Trade_{i,k}$ with $D_{k,t,j}$ are zero for $j < -1$, as -1 is the reference year and set at zero.

We estimate the saturated specifications of equations (1) and (2) for all outcome variables. We plot point estimates of coefficients on triple interactions with the $IDD_{i,k}$ indicator in Figure 2 as a function of j. We also plot 95% confidence intervals around each point estimate.

Notice that for $j < -1$, the confidence interval for the triple interaction with $IDD_{i,k}$ contains zero for all outcome variables except one: the extensive margin coefficient for patent application counts. Thus, for all outcome variables except patent application counts, we find no evidence of a difference in pre-trends. For patent applications, we find that treated, trade secret firms tend to become relatively more likely to apply for patents two years prior to IDD rulings than in the year just before the ruling. Hence, while we find evidence that differences in patent applications' pre-trends are not zero, they instead move in the opposite direction of the estimated treatment effect. Therefore, if anything, the positive pre-trend in patent applications suggests the negative treatment effects of IDD rejections we estimate are biased toward zero in this case.

Importantly, we continue to find significant negative effects for innovation inputs and outputs in the year of the IDD rejection as well as the two years following the rejection along the extensive margin for R&D, research, development, R&D capital expenditures, R&D wages, R&D employees, patent applications, and sales of products new to a company. We observe similar negative and significant effects on patent applications and sales of products new to a company's market along the intensive margin estimates. We take these results as evidence the parallel trends assumption holds for our analysis, as it implies that any spurious divergence in trends invalidating our inferences would have to perfectly coincide with the IDD rulings. We do not report dynamic treatment effect results for non-compete rulings for brevity since they were largely insignificant. In untabulated results, we also do not find any evidence of diverging pre-trends in these coefficients.

5.5 Treatment effects on trade secret reliance

We find evidence that firms reliant on trade secrets significantly reduce their likelihood to innovate in response to the removal of labor mobility restrictions in Tables 3–6. If labor mobility restrictions help firms protect trade secrets, it stands to reason that when such restrictions are weakened by courts, firms (to the extent they are able) will change their business strategy to rely less on trade secrets. We therefore hypothesize firms become less likely to report trade secrets are important to their businesses after labor mobility restrictions are weakened within a state.

We test these hypotheses using a stacked difference-in-differences (i.e., double difference) analysis. We use the stacked sample of treated and untreated firms used for our triple

difference analysis discussed in Section 5.1. To this end, we estimate conditional logit regressions of the form:

$$E(Trade_{i,k,t}) = \Lambda\Big(\alpha_{i,k} + \delta_t + \beta_1 IDD_{i,k} Post_{k,t} + \beta_2 NC_{i,k} Post_{k,t} + \beta_3 ln(age_{i,k,t}) + \beta_4 ln(age_{i,k,t})^2\Big) \quad (3)$$

Where $\Lambda(\ \ )$ is the logistic cumulative distribution function and all covariates are defined as before. If a firm changes its dependence on trade secrets when courts change their ability to stop employees from moving to competitors, we expect $\beta_1 < 0$, $\beta_2 > 0$, and $\beta_3 < 0$. We use the conditional logit to "partial out" the cohort-firm-specific effect $\alpha_{i,k}$, and we include year fixed effects δ_t. Because the conditional logit can only be run on a discordant sample, we exclude from the analysis any observations associated with firm-cohorts for which the value of the trade secret indicator variable is always zero or one.

As with our innovation policy specifications, we run these regressions on the full sample and the subsamples of "large" and startup firms. In Table 8, we present these results. As shown, firms become significantly less likely to report trade secrets are important to their businesses after IDD rejection, but not after non-compete rulings. This result implies that firms adapt to IDD rejections by reducing their reliance on trade secrets. The effects are greater in magnitude for "large" firms but are statistically indistinguishable from zero for IDD rejections and non-compete rulings for startups. We infer startups are less able to adapt to IDD rejections than "large" firms. Economically, all ("large") firms are 20% (31%) (significant at the 10% levels) less likely to depend on trade secrets post-IDD rejection.

5.6. Within-state spillover effects

We find evidence suggesting trade secret firms benefit from labor mobility restrictions protecting their innovations. However, labor mobility restrictions may adversely impact the innovation policies of other firms (Rowe 2005) and reduce new firm entry (Jeffers 2023). In Table 3, we find evidence suggesting this effect is present. Specifically, we find positive and significant effects for non-trade secret firms (the coefficient on *IDD* × *Post*) along the extensive margin for R&D spending, labor inputs, and patent applications, and along the intensive margin for patent applications and sales from products new to the market following an IDD rejection.

To provide further evidence of "spillovers," we conduct a series of additional tests. First, we re-estimate specifications similar to those within Table 3, Panel A, but we exclude variable

Trade, as well as interactions containing *Trade*, in Table IA-8 of the Internet Appendix. We find the net effect of IDD rejections and non-compete rulings to be largely insignificant, suggesting that while labor mobility restrictions tend to worsen the innovation of trade secret dependent firms, non-trade secret dependent firms increase innovation.[21]

Finally, we test whether industry-state peers of trade secret firms significantly change innovation policy following IDD rejections. We include the triple interaction *Peer* × *IDD* × *Post*, as well as its component variables and interactions. We define variable *Peer* as equal to one if a firm that is not reliant on trade secrets is in the same industry, state, and year, as a firm that is reliant on trade secrets and experiences an IDD rejection in the sample and is zero otherwise. We report results of this analysis in Table 9. Consistent with our previous analysis, we find significant "spillover" effects following IDD rejections. While treated, trade secret firms significantly decrease innovation spending and outputs following IDD rejections, their within-state non-dependent industry peers significantly *increase* innovation. An interesting picture emerges when studying within-state spillovers in response to significant changes in labor mobility. Specifically, non-trade secret dependent peers significantly increase research and sales of products new to the market but significantly decrease sales of products new to the company. This is potentially consistent with IDD rejections reducing long-horizon innovation competition, allowing non-trade secret peers to do more innovation in this space while trade secret firms focus on their existing product portfolio. Taken together, these results suggest the importance of not aggregating trade secret dependent firms with non-trade secret dependent peers given the potential for "spillovers"/transfers of within-industry innovation. This finding suggests not only a lack of overall reduction of innovation but also points to a substitution into innovation best protected by formal intellectual property and away innovation less easily so protected. As the two types of innovation are unlikely perfect substitutes, the overall effect is likely welfare reducing (Boot & Valdmirov 2025).

[21] Despite being largely insignificant, we find some significant net effects combined effects across trade secret and non-trade secret dependent firms. Specifically, on net, IDD rejections tend to significantly increase the likelihood of R&D employment on the extensive margin and significantly increase patenting and sales of new products to the market along the intensive margin. We also find that, on net, rulings weakening non-competes reduce R&D employment and sales of new products to the company along the intensive margin.

5.7. Effects of alternate shocks to labor mobility

The purpose of this study is to test whether labor mobility impacts innovation policy. We find that court rulings that remove labor mobility restrictions decrease the innovative activity of a large sample of public and private firms reliant on trade secrets. If such decreases are due to the removal of labor mobility restrictions, and not to some other omitted effect, we would expect similar removals of labor mobility restrictions to lead to similar changes. As discussed in Fich *et al.* (2023), corporate opportunity waivers (i.e., COW) allow firms to waive the legal principle requiring that fiduciaries (directors and other officers) of firms to not take individual business opportunities in which their firm could benefit. One of the motivations for these waivers is to allow startup firms' directors with overlapping business interests flexibility to pursue business interests with other firms. In other words, COW laws removed specific labor mobility restrictions for officers of a firm. Using an ITT, staggered difference-in-differences methodology in a sample of public firms, Fich *et al.* (2023) find that inventors are more likely to move after COW laws are passed, and that firms reduce investment in R&D and patenting.

While these laws do not reflect changes to the *general* labor mobility of a firm's employees and are the result of law changes, rather than court decisions, we utilize COW laws in a manner like our analysis using IDD and non-compete rulings. Specifically, we follow Fich *et al.* (2023) and study the impact of COW law passage on a series of innovation inputs and outputs as an alternative measure of labor mobility restriction removal. We remove any states where COW laws are in place from our control samples in each cohort and study whether passage of COW laws impacts the innovative behavior of firms. We consider two states that passed COW laws during our sample period: Maryland (2014), and Washington (2016). New Jersey also passed a COW law in 2011, but we do not include New Jersey in our analysis because its passage of the UTSA around the same time could confound our inferences. We perform analysis similar to Table 3, but also include the triple interaction, $Trade \times COW \times Post$, as well as its component interactions, and report these results in Table 10.

As shown in Table 10, we do not find evidence that the COW law passage was driving our previous results using IDD and non-competes. We also find that firms that depend on trade secrets significantly reduce total R&D, development, R&D wages, R&D employees, and sales new to the company and market following COW law passage along the intensive margin relative to their peers. However, we find some mixed evidence along the extensive margin. Specifically,

despite significantly reducing R&D spending and new sales to the company, trade secret firms are more likely to report any research and any sales new to the company following COW adoption. This mixed effect may be the result of other economic effects accompanying the law change. Importantly, the removal of labor mobility restrictions specific to directors and officers of the firm reduce the innovative behavior of a large sample of private and public firms reliant on trade secrets. This corroborates our findings using IDD and non-compete rulings and demonstrates the importance of rank-in-file labor mobility to firm innovation.

5.8. Robustness tests

We now briefly discuss a series of additional tests of the robustness of our main results. Details of these tests and their results are presented in the Internet Appendix. In the fall of 2011, just before courts issued a clarifying IDD rejection in Massachusetts, the governor indicated he would support legislation invalidating all non-competes (Rosen 2011). A bill to this effect was introduced in April 2014, but it died when the legislative session ended in August. To test whether this proposed legislation contaminates our estimates of the effects of IDD rejection, we add additional dummy variables and interaction terms to our baseline specifications. These additional variables control for a reversal of the relative treatment effects after 2014, the year the legislation died. We fail to find significant reversals in most specifications, and our main results continue to hold.

Because our dependent variables are bounded at zero, are highly skewed, and have a large number of zero values, OLS regressions are almost surely misspecified. Nevertheless, as a robustness test, we run the stacked, triple difference analysis using linear two-way fixed effects OLS regressions due to the ubiquity of this specification in the literature. We use the same innovation dependent variables as in our ZIP analysis, except we scale them: by payroll if they are measured in dollars and employee count otherwise. We continue to find significant negative relative treatment effects of the IDD on total R&D, as well as R&D wages and R&D employee count. Additionally, inferences drawn from our main results might be contaminated if states in our control sample are impacted by IDD rulings in neighboring states. We thus add variables to our main specifications controlling for the spillover effects to neighboring states. Our inferences remain unchanged. Further, labor mobility rulings plausibly induce some firms to move states, which, in turn, could bias our tests. We thus test whether the IDD and non-compete rulings have a relative treatment effect on the odds of trade secret firms moving out of state, and we do not

detect one. Finally, in our main specifications, we assign firms to treated or control states based on the location they report in the BRDIS. Alternatively, we deem a firm to be in a treated or control state if a its establishments in a treated or control state account for 50% or 100% of its total payroll. Using these payroll-based firm location definitions, our results remain qualitatively unchanged.

6. Conclusion

There is a vast scholarly literature in finance and other business disciplines on the effects of court-imposed labor mobility restrictions and their impact on non-innovation outcomes. However, there is little direct evidence on whether these restrictions achieve their policy rationale of encouraging investment in innovation through trade secret protection. We find some of the first causal evidence that the inevitable disclosure doctrine, and in the case of startups, rulings on non-competes, stimulate innovation by firms where trade secrets are important relative to other firms. We also find evidence that firms substitute away from the use of trade secrets when faced with court rulings weakening trade secret protections. Further, we find evidence that significant "spillovers" occur between trade secret dependent firms and their non-trade secret dependent peers as a result of labor mobility restrictions, resolving contradictory evidence in the existing literature. Our evidence is useful to policy makers when determining whether labor mobility restrictions such as the IDD and non-competes should be enforceable.

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Figure 1: Kernel Density Plots of Firm Characteristics for Firms Stopping vs. Continuing R&D Post-IDD Ruling

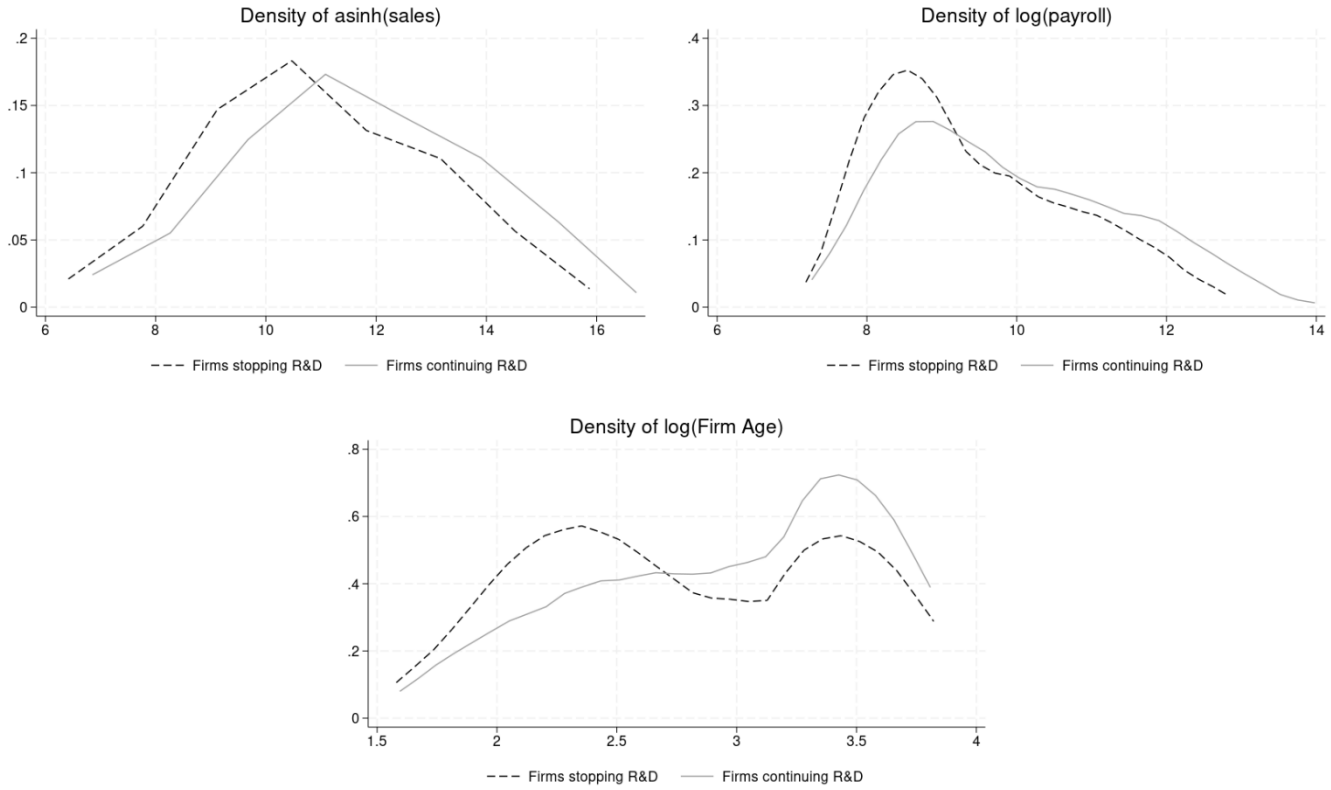


Figure 2: Dynamic Treatment Effects of IDD Adoption

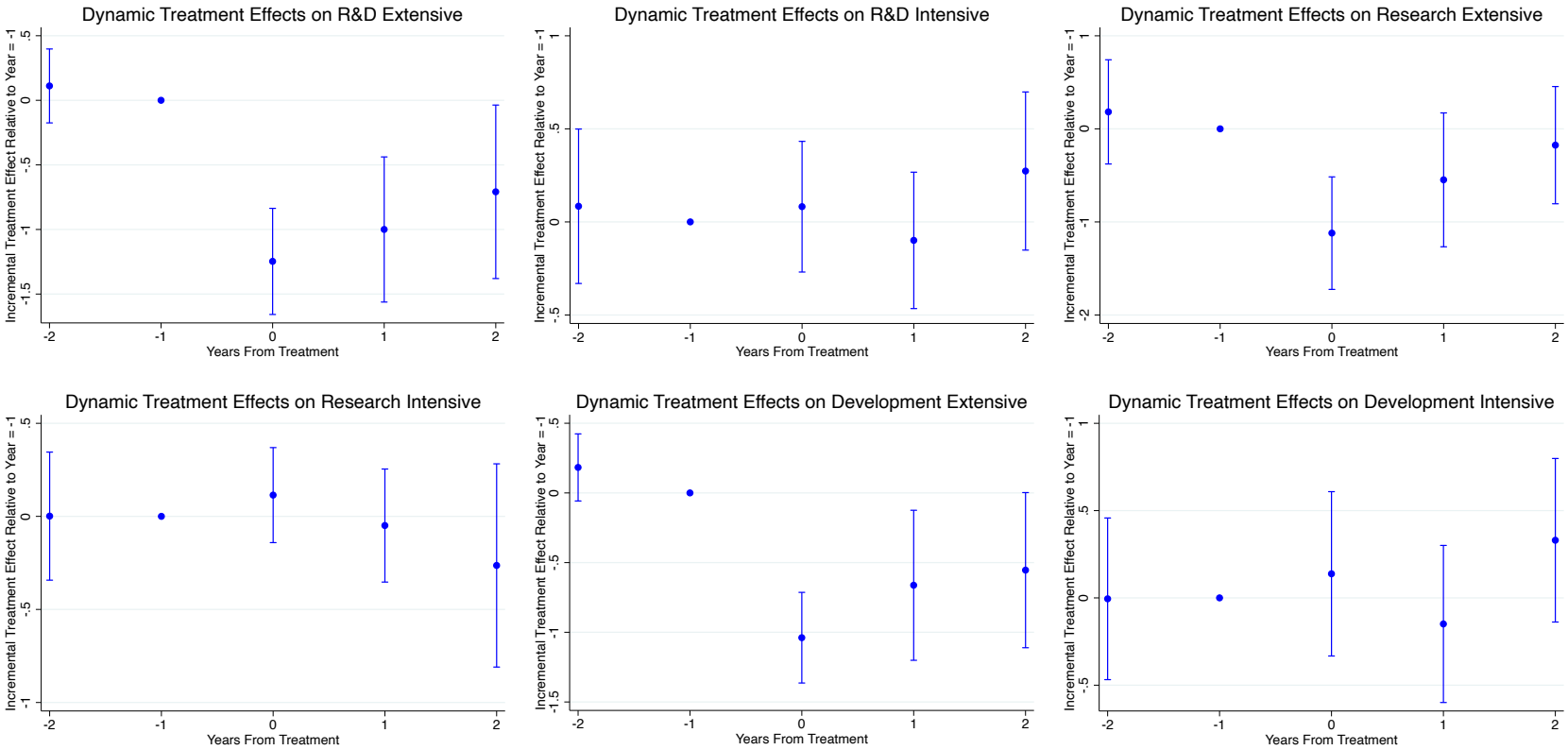


Figure 2 (continued): Dynamic Treatment Effects of IDD Adoption

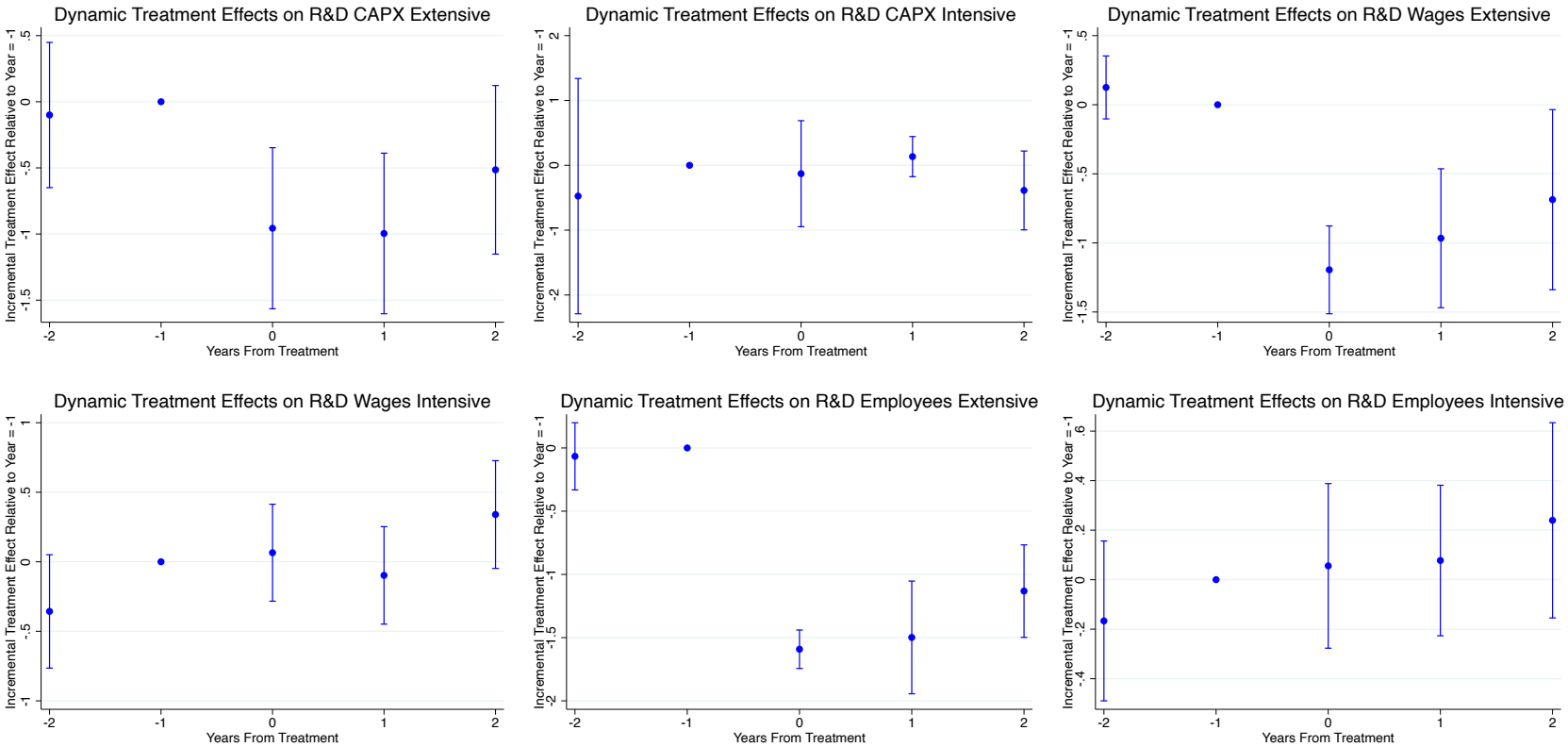


Figure 2 (continued): Dynamic Treatment Effects of IDD Adoption

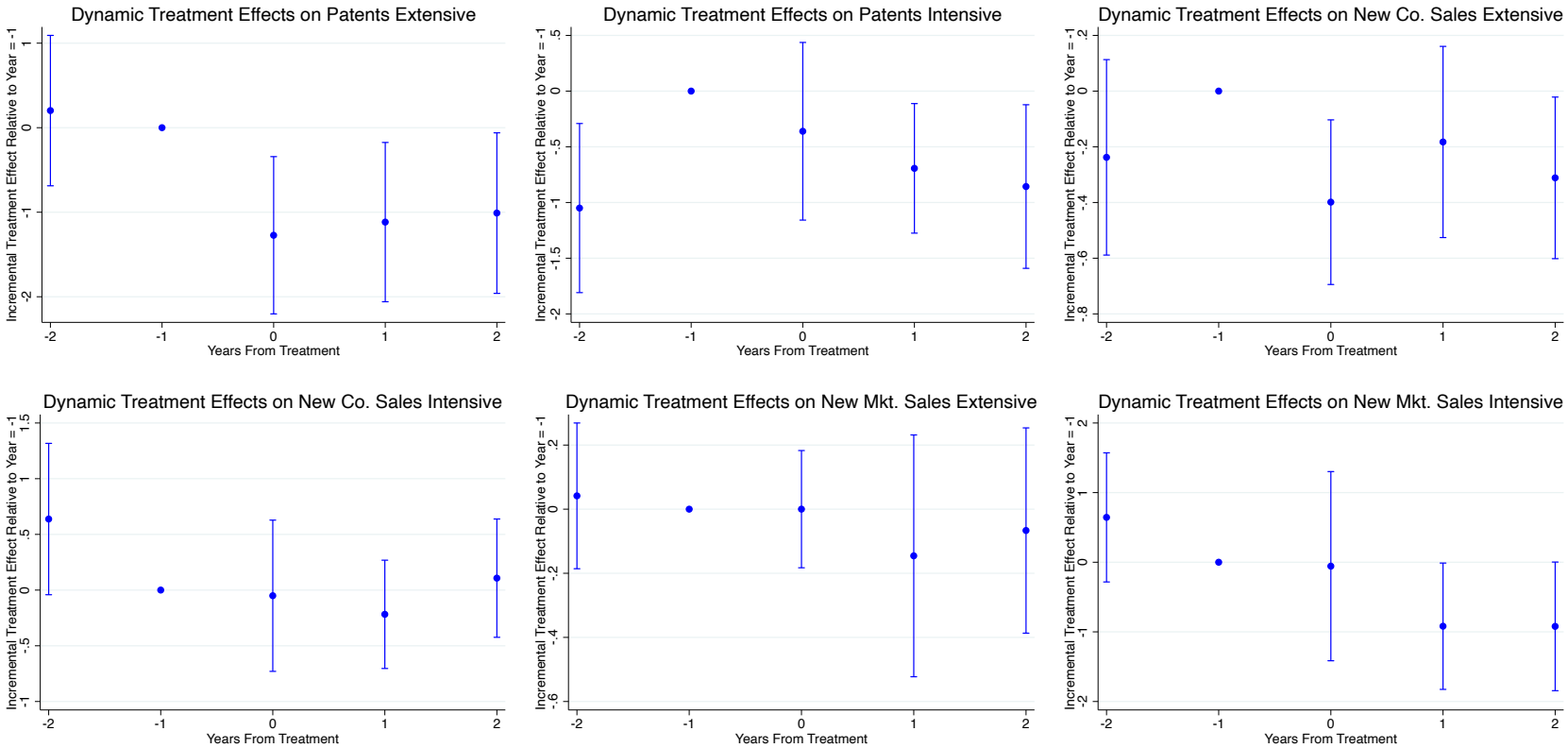


Table 1: Court Rulings

IDD denotes rulings on the inevitable disclosure doctrine, while NCL and NCF denote pro-labor and pro-firm rulings on non-compete agreements, respectively. Details on non-competes are from Jeffers (2023) commentary at the *Restrictive Covenant Report*, published by Jackson-Lewis, and court opinions.

State	Ruling Date	Treatment Year	Ruling Type	Case	Details
Massachusetts	6/16/2012	2012	IDD	U.S. Elec. Servs. v. Schmidt	IDD rejected
Georgia	5/6/2013	2013	IDD	Holton vs. Physician Oncology Services	IDD rejected
Wisconsin	7/14/2009	2010	NCF	Star Direct, Inc. v. Dal Pra.	Blue penciling allowed
South Carolina	5/24/2010	2010	NCL	Invs, Inc. v. Century Builders of Piedmont, Inc.	Blue penciling rejected
Colorado	5/31/2011	2011	NCF	Lucht's Concrete Pumping v. Horner	Continuing employment declared as sufficient compensation for requiring workers to sign non-competes
Montana	11/22/2011	2012	NCL	Wrigg v. Junkermier, Clark,Campanella, Stevens, P.C	Non-competes unenforceable on terminated workers
Illinois	12/2/2011	2012	NCF	Reliable Fire Equipment Co. v. Arredondo	Scope of allowed interests in non-competes broadened
Texas	12/16/2011	2012	NCF	Marsh USA Inc. v. Cook	Employee stock option ownership is sufficient interest in firm for enforceability
Illinois	6/24/2013	2013	NCL	Fifield v. Premier Dealer Services, Inc.	Only enforceable on employees on job for more than 2 years, unless other consideration is offered
Virginia	9/12/2013	2014	NCF	Assurance Data Inc. v. Malyevac	Reasons for automatic dismissal of non-compete suits curtailed
Kentucky	9/2/2014	2015	NCL	Charles T. Creech, Inc. v. Brown	Employers must offer additional compensation in exchange for requiring non-compete
Pennsylvania	11/15/2015	2016	NCL	Socko v. Mid-Atlantic Systems of CPA, Inc.	Employers must offer additional compensation in exchange for requiring non-compete

Table 2: Descriptive Statistics

Panel A: Variables

Means, standard deviations, and pseudo-percentiles for variables used in main specifications. All monetary measures of innovation are expressed in thousands of 2018 USD. Variables *NewProdCo* and *NewProdMkt* are sales from products new to a company and new to a company's market, respectively. The number of observations is reported according to Census requirements and is 170,000 for *NewProdCo* and *NewProdMkt* and 199,000 for all other variables.

	Mean	SD	p10	p25	p50	p75	p90
R&D	35,400	341,100	0	0	650.2	4,507	23,810
Research	6,400	85,910	0	0	0	420.3	3,405
Development	25,420	268,800	0	0	256.8	3,058	15,600
R&D CAPX	2,696	32,370	0	0	0	129.4	1,474
R&D Wages	18,010	172,900	0	0	385.4	2,790	12,590
Mean R&D Wage	73.84	359.1	0	0	26.82	111.9	178.4
R&D Employees	128.6	944.5	0	0	8	35	133
Patents Applied	15.13	165.5	0	0	0	1	10
Sales	1,474,000	10,040,000	0	4,105	36,080	218,300	1,457,000
Payroll	190,900	1,082,000	1,860	4,984	14,440	54,870	260,500
Employees	2,428	16,720	21	58	183	686	3,110
NewProdMkt	86,850	1,611,000	0	0	0	0	11,860
NewProdCo	71,610	1,560,000	0	0	0	0	13,820

Panel B: Indicator Variable Frequency

Approximate frequencies of indicator variables taking the value of one, based on sums and sample sizes according to Census requirements. Variables *IDD, NCL,* and *NCF*, indicate the firm experiences one of the following treatments: Inevitable Disclosure Doctrine rejection (i.e., *IDD*) and non-compete rulings favorable to labor (i.e., *NCL*) and to firms (i.e., *NCF*), respectively. Variable *Trade* indicates a firm reported trade secrets are important to its business the year before treatment. Variable *Post* equals one if the observation comes in or after the treatment year for a cohort.

	IDD	NCL	NCF	Trade	Post
Frequency	0.014	0.018	0.024	0.513	0.578

Table 3: Effects of Labor Mobility Restrictions for Full Sample

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effects of court rulings on innovation for all firms. A firm's monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.096***	0.037	-0.776***	-0.047	-0.896***	0.093
	(0.235)	(0.137)	(0.245)	(0.148)	(0.240)	(0.161)
Trade × NC × Post	0.030	0.183	0.195	0.093	0.075	0.214
	(0.267)	(0.140)	(0.203)	(0.182)	(0.223)	(0.156)
IDD × Post	0.422**	0.156	0.479**	0.183	0.442**	0.083
	(0.182)	(0.325)	(0.200)	(0.149)	(0.182)	(0.313)
NC × Post	-0.084	-0.319	-0.222	-0.277	-0.097	-0.320
	(0.144)	(0.232)	(0.163)	(0.238)	(0.138)	(0.254)
Trade × Post	-1.569***	0.016	-0.888***	0.041	-1.407***	0.010
	(0.094)	(0.073)	(0.048)	(0.066)	(0.111)	(0.075)
Trade × IDD	0.544***	-1.046***	0.380*	-0.223	0.346**	-0.990**
	(0.177)	(0.384)	(0.202)	(0.344)	(0.168)	(0.437)
Trade × NC	-0.088	-0.010	-0.213	-0.346*	-0.101	-0.020
	(0.238)	(0.233)	(0.144)	(0.190)	(0.179)	(0.261)
IDD	-0.235	0.412	-0.112	0.025	-0.260*	0.363
	(0.154)	(0.354)	(0.167)	(0.354)	(0.156)	(0.345)
NC	0.059	0.202	0.174	0.459**	0.059	0.206
	(0.129)	(0.246)	(0.126)	(0.218)	(0.122)	(0.275)
Trade	2.851***	0.119	1.832***	-0.109	2.627***	-0.012
	(0.134)	(0.218)	(0.087)	(0.139)	(0.169)	(0.230)
Post	1.023***	0.041	0.770***	0.074	0.951***	0.021
	(0.074)	(0.080)	(0.047)	(0.064)	(0.080)	(0.085)
ln(Age)	0.157	1.359	0.192	0.965	0.398	1.272
	(0.580)	(1.298)	(0.359)	(0.687)	(0.594)	(1.672)
$\ln(Age)^2$	-0.581**	-0.035	-0.447***	-0.189	-0.553**	0.110
	(0.259)	(0.301)	(0.146)	(0.213)	(0.251)	(0.361)
$\ln(\overline{RealPay})$	0.094***	0.996***	0.129***	0.898***	0.104***	1.035***
	(0.027)	(0.056)	(0.017)	(0.065)	(0.035)	(0.055)
Pseudo-R^2	0.745		0.623		0.766	
Obs.	199,000		199,000		199,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.842***	0.143	-1.062***	0.251**	-1.417***	0.170
	(0.199)	(0.521)	(0.222)	(0.116)	(0.126)	(0.115)
Trade × NC × Post	0.079	-0.241	0.001	0.021	-0.098	0.052
	(0.210)	(0.204)	(0.245)	(0.075)	(0.275)	(0.247)
IDD × Post	0.431**	-0.075	0.467**	-0.087	0.906***	0.001
	(0.178)	(0.357)	(0.186)	(0.326)	(0.277)	(0.329)
NC × Post	-0.123	0.011	-0.091	-0.217	-0.264	-0.179
	(0.152)	(0.184)	(0.149)	(0.220)	(0.185)	(0.223)
Trade × Post	-0.960***	-0.126	-1.518***	-0.005	-1.836***	0.063
	(0.059)	(0.117)	(0.086)	(0.062)	(0.099)	(0.064)
Trade × IDD	0.542***	-0.995*	0.509***	-1.045***	0.816***	-0.924***
	(0.192)	(0.587)	(0.161)	(0.390)	(0.126)	(0.339)
Trade × NC	-0.178	-0.217	-0.081	0.069	0.060	-0.050
	(0.171)	(0.323)	(0.217)	(0.198)	(0.251)	(0.223)
IDD	-0.172	0.297	-0.246	0.547	-0.567**	0.346
	(0.148)	(0.474)	(0.157)	(0.374)	(0.257)	(0.358)
NC	0.170	0.479	0.056	0.137	0.199	0.108
	(0.113)	(0.322)	(0.132)	(0.236)	(0.155)	(0.173)
Trade	2.008***	-0.097	2.794***	0.052	3.214***	0.005
	(0.070)	(0.223)	(0.125)	(0.207)	(0.084)	(0.134)
Post	0.894***	0.076	1.008***	0.045	1.294***	0.017
	(0.057)	(0.131)	(0.073)	(0.076)	(0.077)	(0.073)
ln(Age)	-0.235	-2.855**	0.223	0.589	0.294	0.451
	(0.451)	(1.447)	(0.582)	(1.431)	(0.828)	(0.882)
$\ln(\text{Age})^2$	-0.463**	0.352	-0.613**	0.174	-0.768**	0.177
	(0.208)	(0.482)	(0.260)	(0.334)	(0.335)	(0.225)
$\ln(\overline{RealPay})$	0.169***	0.914***	0.092***	0.979***		
	(0.019)	(0.041)	(0.027)	(0.053)		
$\ln(\overline{DomEmp})$					0.013	0.805***
					(0.018)	(0.045)
Pseudo-R^2	0.624		0.758		0.646	
Obs.	199,000		199,000		199,000	

Panel C, Innovation Outputs

	Patents Applied		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.233***	-0.497**	-0.318*	-0.216	-0.223	-0.920**
	(0.373)	(0.251)	(0.166)	(0.285)	(0.171)	(0.429)
Trade × NC × Post	-0.002	0.271	-0.322**	0.669	-0.155	0.338
	(0.263)	(0.260)	(0.161)	(0.905)	(0.191)	(0.400)
IDD × Post	0.943***	0.955**	0.132	0.006	-0.049	1.329***
	(0.339)	(0.380)	(0.158)	(0.349)	(0.198)	(0.494)
NC × Post	-0.132	-0.506	0.179	-0.859	0.091	-0.192
	(0.196)	(0.379)	(0.234)	(0.918)	(0.211)	(0.152)
Trade × Post	-1.445***	0.390***	-0.819***	-0.025	-0.898***	-0.120
	(0.067)	(0.111)	(0.055)	(0.159)	(0.057)	(0.122)
Trade × IDD	0.775**	0.190	-0.134	-0.414	-0.011	-0.105
	(0.369)	(0.414)	(0.168)	(0.388)	(0.133)	(0.400)
Trade × NC	-0.066	-0.081	-0.069	-0.039	-0.119	-0.837***
	(0.239)	(0.334)	(0.154)	(0.207)	(0.183)	(0.262)
IDD	-0.498	-0.795**	-0.102	-0.286	-0.005	-0.820*
	(0.344)	(0.333)	(0.127)	(0.330)	(0.146)	(0.459)
NC	0.136	0.289	-0.029	0.313**	0.023	1.080***
	(0.185)	(0.364)	(0.215)	(0.132)	(0.196)	(0.167)
Trade	2.745***	-0.153	1.677***	0.236	1.834***	0.312
	(0.086)	(0.192)	(0.080)	(0.238)	(0.084)	(0.218)
Post	1.356***	-0.158	0.944***	-0.069	0.967***	0.046
	(0.071)	(0.128)	(0.089)	(0.174)	(0.085)	(0.128)
ln(Age)	-0.899*	-0.358	1.075*	2.891	0.806	5.246
	(0.513)	(0.883)	(0.589)	(2.204)	(0.504)	(3.433)
$\ln(\text{Age})^2$	-0.216	0.143	-0.533**	-0.190	-0.549***	-0.834
	(0.209)	(0.267)	(0.242)	(0.558)	(0.209)	(0.668)
$\ln(\overline{RealPay})$			-0.006	1.048***	-0.007	1.108***
			(0.033)	(0.080)	(0.026)	(0.053)
$\ln(\overline{DomEmp})$	0.222***	0.770***				
	(0.048)	(0.069)				
Pseudo-R^2	0.538		0.760		0.766	
Obs.	199,000		170,000		170,000	

Table 4: Frequency Tabulations of Firms Reporting Positive R&D by Treatment Status and Susceptibility to Treatment

Reported estimates of the frequencies of observations (i.e., firms) with positive R&D pre- and post-IDD. Firms are first sorted into Panel A or Panel B by whether a firm was in a state that experienced an IDD ruling, or not, respectively. Then, within Panel A and Panel B, firms are separately further sorted by whether a firm stated trade secrets were important or not.

Panel A: Fraction of Firm-Years Treated by IDD Ruling Reporting Positive R&D

	Pre-IDD	Post-IDD
Trade Secrets Important	0.90	0.70
Trade Secrets Unimportant	0.23	0.53

Panel B: Fraction of Untreated Firm-Years in Cohorts 2012 & 2013, Reporting Positive R&D

	Pre-IDD	Post-IDD
Trade Secrets Important	0.87	0.73
Trade Secrets Unimportant	0.27	0.41

Table 5: Effects of Labor Mobility Restrictions ("Large" Firms)

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effect of court rulings on innovation at "large" firms (real 2018 USD sales > $220 M). Monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include the controls and component terms reported in Table 3, a constant term, and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.683***	0.019	-0.651***	-0.105	-1.563***	0.065
	(0.629)	(0.144)	(0.221)	(0.130)	(0.479)	(0.192)
Trade × NC × Post	-0.202	0.220	0.256	0.078	-0.145	0.254
	(0.299)	(0.135)	(0.226)	(0.177)	(0.281)	(0.156)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.649		0.509		0.676	
Obs.	50,000		50,000		50,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.859***	-0.113	-1.636***	0.253*	-2.350***	0.197
	(0.263)	(0.531)	(0.533)	(0.147)	(0.565)	(0.149)
Trade × NC × Post	0.184	-0.213	-0.130	0.039	-0.375	0.067
	(0.279)	(0.229)	(0.284)	(0.067)	(0.330)	(0.306)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.569		0.656		0.491	
Obs.	50,000		50,000		50,000	

Panel C, Innovation Outputs

	Patents Applied		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.224	-0.921**	0.060	-0.053	-0.425	-1.037*
	(0.856)	(0.401)	(0.384)	(0.325)	(0.341)	(0.545)
Trade × NC × Post	-0.034	0.297	-0.091	0.753	-0.114	0.387
	(0.233)	(0.297)	(0.184)	(0.943)	(0.290)	(0.441)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.429		0.654		0.658	
Obs.	50,000		42,000		42,000	

Table 6: Effects of Labor Mobility Restrictions (Startups)

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effect of court rulings on innovation at startups (age < 6 years & real 2018 USD sales < $220 M). Monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include the controls and component terms reported in Table 3, a constant term, and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.952	-0.922	-2.021***	-0.594	-0.381	-0.125
	(0.819)	(0.819)	(0.587)	(0.897)	(0.899)	(0.684)
Trade × NC × Post	0.177	-0.809***	0.831	-1.412***	0.066	-0.363
	(0.797)	(0.279)	(0.647)	(0.477)	(0.363)	(0.388)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.385		0.326		0.472	
Obs.	5,200		5,200		5,200	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.572***	1.209	-1.228	-0.737	-1.898	-0.571**
	(0.447)	(0.889)	(0.883)	(0.632)	(1.716)	(0.225)
Trade × NC × Post	0.031	0.735	0.007	-0.367	0.315	0.296
	(0.449)	(1.059)	(0.814)	(0.373)	(0.745)	(0.379)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.237		0.515		0.409	
Obs.	5,200		5,200		5,200	

Panel C, Innovation Outputs

	New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.229	-0.457	-0.721	0.171
	(1.480)	(0.360)	(1.105)	(0.770)
Trade × NC × Post	-0.356	-2.488***	0.444	0.035
	(1.011)	(0.488)	(0.718)	(0.774)
Table 3 Controls	Yes		Yes	
seudo-R^2	0.477		0.684	
Obs.	4,800		4,800	

Table 7: Outcomes Examined in Previous IDD Studies

This table summarizes the outcome variables examined in prior studies that utilize IDD court decisions in a quasi-natural experimental setting whose list of IDD decisions overlaps with ours. The vast majority of prior IDD studies use a different set of IDD cases and are not listed.

	Main outcomes studied	#Outcomes
Qui and Wang (2018)	IDD announcement returns; overhead expenses	2
Na (2020)	Sensitivity of CEO pay to relative performance	1
Flammer and Kacperczyk (2019)	3 corporate social responsibility scores	3
Kannan *et al.* (2022)	Capital Expenditures; employment, wages; R&D, sales growth; profitability; leverage	7
Chen *et al.* (2022)	Vega of Executive Holdings in Firm; Stock Return Volatility	2
Cai *et al.* (2025)	Total cash payouts to shareholders	1

Table 8: Conditional Logit Analysis of Trade Secret Dependence

This table presents results from a conditional logistic regression where the odds of a firm declaring trade secrets are important is modeled as a function of year dummies, I(year), the natural log of firm age and its square, and interactions between indicator variables for the firm being located in a state that experienced an IDD (variable *IDD*) or non-compete ruling (variable *NC*), and an indicator variable for the observation occurring in or after the year of an IDD or NC ruling (variable *Post*). Odds modeled are conditional on firm-by-cohort fixed effects. Standard errors are clustered by firm-by-cohort, industry, and state-by-year groups. Asterisks ***, ** and *, indicate significance at 1%, 5%, and 10%, respectively. Census disclosure #11605.

	Trade Secret	Trade Secret	Trade Secret
	All Firms	Large Firms	Startups
IDD × Post	-0.233*	-0.376*	0.416
	(0.139)	(0.228)	(0.460)
NC × Post	-0.015	-0.107	0.189
	(0.093)	(0.169)	(0.582)
Controls	Yes	Yes	Yes
Pseudo-R^2	0.022	0.026	0.054
Obs.	66,000	16,000	2,300

Table 9: State-Industry Peer "Spillovers"

Estimates from stacked, triple difference zero-inflated Poisson regressions testing whether effects of inevitable disclosure doctrine (IDD) rejections on trade secret firms "spillover" to their non-trade-secret dependent industry peers in their state. Variable *Peer* is an indicator that takes the value of one if a firm has the following characteristics: it is headquartered in a state that experiences an IDD rejection, is non-trade-secret dependent, and shares a 4-digit NAICs code with at least one firm in the same state that is trade-secret dependent in the treatment year. All other variables are the same as in Table 3. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Asterisks ***, **, and *, indicate 1%, 5%, and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer× IDD × Post	-0.161	0.267**	-0.029	0.700**	-0.281	0.143
	(0.352)	(0.118)	(0.415)	(0.309)	(0.364)	(0.144)
Trade × IDD× Post	-1.218***	0.239	-0.781	0.579**	-1.115***	0.194
	(0.389)	(0.167)	(0.528)	(0.277)	(0.361)	(0.221)
Trade × NC × Post	0.030	0.183	0.195	0.093	0.075	0.214
	(0.267)	(0.14)	(0.203)	(0.181)	(0.223)	(0.156)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.7452		0.6228		0.7663	
Obs.	199,000		199,000		199,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer × IDD × Post	0.070	-0.385	-0.212	-0.041	0.027	0.100
	(0.392)	(0.44)	(0.367)	(0.222)	(0.417)	(0.128)
Trade × IDD × Post	-0.775*	-0.07	-1.224***	0.186*	-1.386***	0.226*
	(0.435)	(0.321)	(0.403)	(0.095)	(0.408)	(0.119)
Trade × NC × Post	0.079	-0.241	0.001	0.021	-0.098	0.053
	(0.211)	(0.206)	(0.245)	(0.077)	(0.275)	(0.247)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.6237		0.7577		0.6463	
Obs.	199,000		199,000		199,000	

Panel C, Innovation Outputs

	Patents		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer × IDD × Post	-0.441	2.095***	0.584	-1.681***	0.113	1.71***
	(0.72)	(0.585)	(0.441)	(0.472)	(0.394)	(0.639)
Trade × IDD × Post	-1.557	1.215***	0.244	-1.996***	-0.234	1.164*
	(1.021)	(0.369)	(0.335)	(0.374)	(0.321)	(0.645)
Trade × NC × Post	-0.002	0.271	-0.155	0.338	-0.322**	0.669
	(0.262)	(0.26)	(0.192)	(0.406)	(0.161)	(0.905)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.5381		0.7663		0.7598	
Obs.	199,000		170,000		170,000	

Table 10: COW Adoption

Estimates from stacked, triple difference zero-inflated Poisson regressions examining Corporate Opportunity Waiver (COW) Law passages (Fich *et al.* 2023) on the innovation for all firms in addition to labor mobility rulings studies in Table 3. Corresponding with Table 8 in the main text. Variable *COW* is an indicator that takes the value of one if a firm is headquartered in a state that experiences the passage of a COW law and zero otherwise. All other variables are the same as in Table 3. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Asterisks ***, **, and *, indicate 1%, 5%, and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.478	-1.387***	0.552**	-0.266	0.358	-1.309***
	(0.298)	(0.369)	(0.257)	(0.408)	(0.367)	(0.478)
Trade × IDD× Post	-1.073***	0.010	-0.760***	-0.027	-0.882***	0.048
	(0.248)	(0.128)	(0.247)	(0.146)	(0.25)	(0.155)
Trade × NC × Post	0.010	0.202	0.181	0.088	0.058	0.232
	(0.271)	(0.172)	(0.207)	(0.194)	(0.225)	(0.189)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.7523		0.611		0.7689	
Obs.	182,000		182,000		182,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.201	0.169	0.450	-1.339***	-1.489**	-1.691***
	(0.276)	(0.424)	(0.321)	(0.247)	(0.744)	(0.499)
Trade × IDD× Post	-0.840***	0.177	-1.041***	0.204*	-1.419***	0.116
	(0.201)	(0.537)	(0.235)	(0.118)	(0.128)	(0.117)
Trade × NC × Post	0.067	-0.155	-0.017	0.035	-0.112	0.087
	(0.211)	(0.205)	(0.248)	(0.1)	(0.277)	(0.274)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.6288		0.759		0.6414	
Obs.	182,000		182,000		182,000	

Panel C, Innovation Outputs

	Patents		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.225	0.186	0.555**	-2.752***	0.259	-1.860***
	(0.217)	(0.269)	(0.249)	(0.910)	(0.169)	(0.344)
Trade × IDD× Post	-1.245***	-0.473*	-0.319*	-0.251	-0.235	-0.986**
	(0.37)	(0.27)	(0.164)	(0.297)	(0.173)	(0.435)
Trade × NC × Post	-0.003	0.276	-0.318**	-0.291	-0.145	0.265
	(0.265)	(0.261)	(0.160)	(0.259)	(0.189)	(0.399)
Table 3 Controls	Yes		Yes		Yes	
Pseudo-R^2	0.5255		0.7718		0.7677	
Obs.	182,000		156,000		156,000	

Internet Appendix for

Trade Secrets, Labor Mobility, and Innovation

November 19, 2025

Any views expressed herein are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2316 (CBDRB-FY24-P2316-R11087, CBDRB-FY24-P2316-R11141, CBDRB-FY24-P2316-R11179, CBDRB-FY24-0449, and CBDRB-FY25-0377).

IA1. Court rulings after 2008 that we use as exogenous rejections of the IDD.

IA1.1. Massachusetts

U.S. Elec. Servs. v. Schmidt, Civil Action No. 12-10845-DJC (U.S. Dist. CT. for the Dist. of Mass. 2012), 6/19/2012. Classified as a precedent-changing IDD rejection by both Qiu and Wang (2018) and Flammer and Kacperczyk (2019)

U.S. Electric asked the Federal district court to apply Massachusetts trade secret common law to enjoin former employee Schmidt from taking a job at a competitor through application of the IDD. The court rejected the proposition that "inevitable future misuse of trade secrets is by itself sufficient to establish a violation of either common law or statutory obligations regarding trade secrets," thereby clearly rejecting the validity of the IDD in Massachusetts as the doctrine is commonly understood (Malloy, 2012). We initially concluded this ruling changed IDD precedent because Klasa et al. (2018) and Qiu and Wang (2018) both claim Massachusetts adopted the IDD in the mid-1990s.

Upon further investigation, however, we discovered there is some uncertainty about how this ruling changed precedent. While it clearly rejects the IDD as it is usually applied, it is not clear that Massachusetts had ever adopted the IDD, contrary to the claims of Klasa et al. (2018) and Qiu and Wang (2018). The IDD, as it is usually applied (i.e., in its "pure form"), requires only a significant threat of trade secret misappropriation for a court to enjoin a former employee from taking a job with a competitor (e.g., *Pepsico v. Redmond*). A non-compete binding the employee or evidence of bad faith on the part of the latter are not necessary. In contrast, in all prior cases where Massachusetts courts had applied the IDD, in addition to there being a threat of misappropriation, the former employee was either bound by a non-compete or there was evidence of bad faith. Neither of these factors were present in the U.S. Electric case, and their absence formed much of the basis for the judge's refusal to enjoin the former employee.[1] For this reason, some scholars and practitioners argue that the decision did not change IDD precedent, but

[1] See the first full paragraph on p. 14 of the judge's opinion.

merely confirmed prior precedent that either a non-compete or evidence of bad faith are necessary to enjoin a former employee (e.g., see Dogan and Slater (2022) and (Bialas, 2012)).

Nevertheless, prior to the U.S. Electric decision, some legal scholars asserted that Massachusetts courts applied the IDD, albeit inconsistently. Weisner (2012) argued, "Massachusetts applies the [inevitable disclosure] doctrine, but is yet to officially adopt it…Ultimately, courts have provided mixed views…applying it in one case to enforce a non-compete in the absence of bad faith and declaring that it is yet to be adopted in another." Therefore, it is likely that, prior to the U.S. Electric decision, there was some significant positive perceived probability (likely less than one) that Massachusetts courts would apply the IDD in its "pure" form. That U.S. Electrical Services would bother to bring the case, which almost surely required significant expenditure of resources, further suggests that economic agents in Massachusetts perceived there was a reasonable chance courts would enjoin former employees based on threatened trade secret misappropriation alone, without a non-compete or evidence of bad faith. The court's unambiguous rejection in U.S. Electric of the IDD in its "pure" form almost surely changed this perception. For this reason, Malsberger (2022) cites the case as setting a precedent definitively rejecting "the theory that a claim for trade secret misappropriation alone could be based on the inevitable disclosure doctrine" (p. 55 in the Massachusetts chapter). Therefore, it is reasonable to hypothesize the decision had significant effects, notwithstanding disagreement about the degree to which it changed legal precedent.

IA1.2. Georgia

Holton v. Physician Oncology Servs., LP, No. S13A0012, 2013 WL 1859294, 5/16/2013. Classified by as a precedent-changing IDD rejection by Flammer and Kacperczyk (2019) but *not* Qui and Wang (2018). An expert legal commentator described the effect of the ruling as follows:

> The inevitable disclosure doctrine is a common law doctrine that has been used by some courts to prevent a former employee from working for a competitor…This doctrine…remains the subject of considerable debate. Recently, the Georgia Supreme Court joined the debate in Holton v. Physician Oncology Services, LP, 2013 Ga. LEXIS 414 and rejected the doctrine.(Van Dyke, 2013)

In the decision itself, the court makes abundantly clear that it is rejecting the inevitable disclosure doctrine: "In this case, we hold that the inevitable disclosure doctrine is not a independent claim under which a trial court may enjoin an employee from working for an employer or disclosing trade secrets."

Malsberger (2022), however, disagrees with Van Dyke (2013) and asserts that the court in Holton, rather than rejecting the IDD, merely made its status ambiguous. Nevertheless, even if Malsberger (2022) is correct and Van Dyke (2013) is wrong (a matter upon which we take no position), the ruling still constitutes an exogenous shock to IDD status because. According to Weisner (2012), there is little doubt Georgia had accepted the IDD before this ruling in Essex v. Southwire. Therefore, we can safely infer that the ruling, at the very least, increased economic agents' uncertainty about whether courts would apply the IDD in the state, making it reasonable to hypothesize it had significant effects.

IA2. Robustness Tests

1A2.1. Attempt to abolish non-competes in Massachusetts

In September of 2011, then Governor Patrick of Massachusetts stated that he was considering supporting legislation that would make all non-compete agreements unenforceable in the state (Rosen 2011). In April 2014, Governor Patrick introduced a bill to this effect (Rosen 2014a). A similar bill made it out of committee in May (Rosen 2014b), but the legislation died when the legislative session ended in August. As far as we are aware, there were no subsequent attempts to abolish non-competes, though a less radical reform bill passed in 2018 (Winton & Thakkar 2018). Governor Patrick's first raising of the possibility that all non-competes might soon become unenforceable in Massachusetts roughly coincides with the 2012 court case clarifying that the inevitable disclosure doctrine does not apply to the state. Hence, it is possible that some of the effects we are attributing to IDD court rulings could be driven by Massachusetts firms anticipating a near complete future abolition of non-competes.

To test whether the prospect of non-compete abolition in Massachusetts contaminates our estimates of the effects of IDD rejection, we modify our stacked sample that we use in the full sample ZIP regressions by extending the Massachusetts treatment 2012 subsample, as well as the control subsample

for that cohort, to include all observations up through 2015, the year after the non-compete legislation died. We then add the following two indicator variables to the specification: variable *NCLF,* which takes the value of one for firms in our stack that are located in Massachusetts as of 2015 and is zero otherwise, and well as *PostMA,* which takes the value of one if the observations after 2014 and is zero otherwise. We also include the triple interaction *Trade* × *NCLF* × *PostMA* as well as all component interactions and report results in Table IA-7. If the prospect of near complete abolition of non-competes in Massachusetts is driving our IDD results in Table 3, we expect the failure of the non-compete abolition in 2014 to cause an increase in innovative activity.

As reported in Table IA, the coefficient on *Trade* × *IDD* × *Post* remains unchanged and the coefficient on the new triple interaction is statistically indistinguishable from zero in nearly all specifications. We conclude that the prospect of non-compete abolition in Massachusetts over the 2012–2014 period mostly did not impact innovation and was not driving our results, suggesting effects from this attempted non-compete abolition are unlikely to be contaminating our baseline results.

IA2.2. Linear OLS fixed effects regressions

Because our dependent variables are bounded at zero, are highly skewed, and have a large number of zero values, OLS regressions are almost surely mis-specified. Nevertheless, as a robustness test, we run the triple difference analysis using linear two-way fixed effects OLS regressions due to the ubiquity of this specification in the literature, which are given by:

$$y_{(i,k,t)} = a_{(i,k)} + b_1 Trade_{(i,k)} IDD_{(i,k)} Post_{(k,t)} + b_2 Trade_{(i,k)} NC_{(i,k)} Post_{(k,t)} +$$

$$+ b_3 Trade_{(i,k)} Post_{(k,t)} + \Gamma'_1 SY_{(i,k,t)} + \Gamma'_2 IY_{(i,t)} + d_1 ln\left(age_{i,k,t}\right) + d_2 ln\left(age_{i,k,t}\right)^2 + e_{(i,k,t)} \qquad (4)$$

Where $y_{(i,k,t)}$ is a scaled innovation input or output variable. Dependent variables measured in real USD are scaled by the average real payroll over the prior three years, whereas those measured in physical units are scaled by average employment over the prior three years. All other variables are defined as in sections 4.2 and 5.2.1. We include firm-by-cohort fixed effects, $a_{(i,k)}$, and also include vectors of indicator variables for all possible state-by-year-by-cohort and four-digit NAICS industry (as of the treatment

year)-by-year combinations, $SY_{(i,k,t)}$ and $IY_{(i,t)}$. We cluster standard errors by firm, state-year, and four-digit NAICS industry for the current year and perform our analysis on the full sample of firms. The firm-by-cohort fixed effects ensure that omitted firm characteristics, time-invariant within five years around the cohort treatment year, do not confound our inferences. The other indicators ensure that our inferences are not confounded by any time-varying omitted variables that impact the innovation policy of all firms in the same industry, or any time-varying omitted variables that impact all firms in the same state.[2]

We report the results of these regressions in Table IA-9 in the Internet Appendix. We continue to find a significant negative relation (although somewhat weakened) between IDD rejection and the innovation policy trade secret dependent firms. We find that treated firms significantly reduce R&D investment, R&D wages, and the number of R&D employees in response to IDD rejections. While we do not find significant empirical responses for disaggregated research or development, R&D capital expenditures, or innovation outputs, we note that the signs and magnitudes of these estimated coefficients are negative and economically large despite being unable to observe the separated extensive and intensive margin effects using this methodology. Overall, our OLS results are consistent with our ZIP results.

IA2.3. Spillover effects on neighboring states

A potential concern with our results is that the control sample contains firms located in states neighboring those where court rulings changed labor mobility restrictions by rejecting the IDD. Trade secret firms located in neighboring states may also be impacted by the rulings through geographic spillovers through several channels. First, trade secret firms located in neighboring states may change their innovation policy in anticipation of a similar ruling occurring in their own state. Second, the employees from the neighboring state may relocate to nearby states where labor mobility restrictions have been removed. These effects may be driving our baseline result by impacting firms in the control sample, rather than the treated firms directly. To ensure this between-state spillover mechanism is not driving our results, we conduct the following robustness test. We modify equations (1) and (2) by adding

[2] Due to the number of indicator variables and fixed effect categories, we use the STATA *reghdfe* procedure to estimate parameters and standard errors.

$IDD\ Neighbor_{(i,k)}$ as well as its interactions with $Trade_{(i,k)}$ and $Post_{(k,t)}$. $IDD\ Neighbor_{(i,k)}$ is an indicator variable equal to one if firm *i* is located in a state sharing a geographic border with a state that experiences an inevitable disclosure doctrine treatment in cohort treatment year *k*. We do not add a similar variable for neighbors of states where non-competes were changed since we did not observe significant effects in the general sample. We remove all firms where $IDD\ Neighbor_{(i,k)}$ is equal to one from the control samples following treatment. We report these results in Table IA-10 of the Internet Appendix. As seen in the table, our results are nearly unchanged after explicitly controlling for the impact of IDD rejection on neighboring states. Interestingly, we find that neighboring states also generally experience significant declines in innovation inputs and outputs. This may be consistent with firms anticipating changes in their own state or with firms with locations in both states. However, we explicitly study the latter potential alternative channel next, and do not find evidence that it is driving our results. Taken together, our baseline results are robust to these alterative mechanisms.

IA2.4. Firm propensity to move states

We report evidence that firms significantly reduce innovation inputs and outputs in response to court rulings rejecting labor mobility restrictions. We now also investigate whether firms choose to move to states with better trade secret protection in response to these rejections. This effect would likely bias us against finding that IDD and non-compete rejections have significant treatment effects. However, for completeness, we now explicitly test whether IDD and non-compete rejections significantly increase the likelihood of a treated firms moving locations to another state.

We employ logistic regressions modeling the conditional probability that a firm leaves its state within three years of an IDD or non-compete ruling. We run two separate regressions: the first is on the sample of firms in the IDD treatment samples, as well as all firms in the control samples corresponding to the IDD treatment years (2012 and 2013); the second on a sample that includes all firms located in non-compete-treated states, as well as all firms in the control samples corresponding to the non-compete treatment years. Independent variables include the trade secret dummy, its interaction with an indicator

that the firm is in a treated state as of the treatment year (IDD or NC), and control variables measured over the pre-treatment period. We include state and cohort-year fixed effects. We double cluster standard errors by firm and state-by-cohort-year. In Table IA-11 of the Internet Appendix we find that the treatment effect of both IDD and non-compete rulings on the probability of moving is statistically indistinguishable from zero for trade secret firms relative to other firms. Taken together with our previous results, we firm that, while changes in labor mobility restrictions cause firms to significantly change innovation policy, they do not appear to impact a firm's likelihood to move states.

IA2.5. Firms with establishments in multiple states

In our baseline analysis, we rely on the location listed in the BRDIS to determine whether the firm is in a treated state. This assumption is reasonable because the BRDIS asks firms to list the location where their innovation activity primarily takes place. However, some firms operate establishments in multiple states. It is possible that some firms report to BRIDS a location in a treated state, while having most of their operations at establishments in other states, making their treated status unclear. We perform untabulated robustness tests to ensure firms with such ambiguous treatment status are not driving our results. While we observe innovation measures at the firm-level, we can use establishment-level data to compute the proportion of each firm's payroll expenses in a state for a given year. Therefore, we define two alternate versions of treatment variables *IDD* and *NC*. Specifically, we require 100% of a firm's payroll to be located in a treated state for the firm to be considered treated; or, alternatively, for more than 50% of a firm's payroll to be located in a treated state. In each case, we also control for firms that list the treated state on the BRDIS but have less than these percentages of payroll in that state (and we do not include them in the control samples). In untabulated tests, using these alternative, and stricter measures of treatment, our results remain economically and statistically consistent with our findings in Table 3.[3] Specifically, we continue to find that treated firms (trade secret firms with significant operations, 100% or

[3] We were able to obtain a "sign and significance" disclosure for these tests from the U.S. Census. In this disclosure we are able to report the statistical significance and sign (if significant) of our triple interacted variables of interest along with the number of observations for each model in analysis similar to Table 3. However, we are unable to report the exact coefficients themselves.

>50% of payroll) in states where labor mobility restrictions were weakened through court rulings rejecting IDD, significantly reduced innovation inputs and outputs. Therefore, we rule out the hypothesis that our main results are driven by biases arising from the ambiguous treatment status of some firms with establishments in multiple states.

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Table IA-1 Miscategorized IDD court decisions from Qiu and Wang (2018) and Flammer and Kacperczyk (2019).

Qiu and Wang (2018)				
State	Year	Case	Reasoning	Source(s)
North Carolina	2014	RCR Enters., LLC v. McCall, 14 CVS 3342 (N.C. Sup. Ct. 2014)	A state trial court refused to grant an IDD injunction, but it could not have changed IDD precedent, as trial courts do not have precedent-setting authority. Malsberger (2022) does not mention this case in his discussion of North Carolina IDD case law, but he does cite a 1996 case as applying the IDD (Merck v. Lyon). Contemporaneous legal commentary reports that, in the same year, two state **appellate** courts, who **do** have precedent-setting authority, issued rulings *affirming* the IDD. One of these IDD-affirming rulings came after this one.	Malsberger (2022), Weisner (2012), and https://www.jdsupra.com/legalnews/has-the-inevitable-disclosure-doctrine-26009/, accessed 1/2/2024, as well as the court opinion available on LexisNexis.
Flammer and Kacperczyk (2019)				
State	Year	Case	Reasoning	Source(s)
Ohio	2008	Hydrofarm, Inc. v. Orendorff , Ohio App. LEXIS 5717 (Ohio App. Ct. 2008)	The appellate court ruled that the plaintiff did not provide sufficient evidence of a threat of trade secret disclosure to warrant a preliminary injunction. It *did not* reject the IDD. The court noted there was no non-compete, and in dicta it noted that no Ohio court had yet applied the IDD without a non-compete. However, the court explicitly denies taking a position on whether a non-compete is always necessary for an IDD injunction and speculated it might not be in some cases. We conclude no change in precedent.	https://www.littler.com/publication-press/publication/inevitable-disclosure-absence-non-compete-maybe-not-says-ohio-court, accessed 1/24/2024. Also omitted from Klasa et al. (2018), whose list is current through 2011.
Arkansas	2009	Cellco Partnership v. Langston	Weisner (2012) reports that the IDD still applied in Arkansas as of 2012. Malsberger (2022) reports the state's courts continue to apply the IDD, and he omits this case in his discussion of the state's case law. Court documents indicate this case is unrelated to the IDD.	Weisner (2012), Malsberger (2022), and court opinion: https://casetext.com/case/cellco-partnership-v-langston, accessed 1/29/2024. Also omitted from Klasa et al. (2018), whose list is current through 2011.
New York	2009	American Airlines, Inc. v. Imhof U.S. Dist. LEXIS 46750	Weisner (2012) reports different New York courts have different opinions on the IDD as of 2012 and he does not mention this case. Malsberger (2022) reports, "Courts applying New York law generally disfavor the	Weisner (2012) and Malsberger (2022). Also omitted from Klasa et al. (2018), whose list is current through 2011. From the court's opinion available on Lexis-Nexis:

			doctrine of inevitable disclosure" (p.62 in the NY chapter). The first case he cites as disfavoring the IDD is from 2003, and he does not mention this case. At least two legal scholars thus do not think this ruling affected IDD status in New York. In addition, the court explicitly denies it is rejecting the IDD but is merely denying a preliminary injunction due to insufficient evidence.	"American has not demonstrated a likelihood of success on its inevitable disclosure theory…because it has failed to show that Mr. Imhof carries in his head specific confidential information that would be of any material benefit to Delta…**Nevertheless, American's…inevitable disclosure theory cannot be rejected out of hand**…it presents a substantial question that is a fair ground for litigation."
Wisconsin	2009	Clorox Co. v. SC Johnson & Son Inc., 2:09-cv-00408-JPS	The court merely ruled that California and not Wisconsin law applied to this case. Hence the case is irrelevant to IDD applicability under Wisconsin law. The case is not mentioned in the Malsberger (2022) discussion of the case law, which shows continual application of the IDD by Wisconsin courts since 1996.	http://www.non-competes.com/2009/06/choice-of-law-ends-inevitable.html accessed 1/24/2024. Malsberger (2022). Text of ruling taken from LexisNexis. Also omitted from Klasa et al. (2018), whose list is current through 2011.
New Hampshire	2010	Allot Communications v. Cullen, 10-E-0016 (N.H. Merrimack Superior Ct. 2010)	The court ruled California law, not New Hampshire law, applied to this case. Hence, the case is irrelevant to IDD applicability under New Hampshire law. Malsberger (2022) and Weisner (2012) are silent on whether New Hampshire applies the IDD and do not mention this case.	Weisner (2012) and Malsberger (2022). Also omitted from Klasa et al. (2018), whose list is current through 2011. Text of court ruling taken from LexisNexis.
New Jersey	2012	SCS Healthcare Marketing, LLC v. Allergan USA, Inc., N.J. Super.	The court only that clarified New Jersey's recently passed version of the UTSA does not preempt the state's common law on trade secrets. Thus, this case is of no relevance to the IDD. The case is not mentioned in the Malsberger (2022) discussion of the case law, which shows continual application of the IDD by New Jersey courts since 2005.	new-jersey-trade-secret-act-does-not-preempt-new-jersey-common-law.pdf (wilentz.com): Accessed 8/30/2024. Malsberger (2022).
Washington	2012	Amazon.com, Inc. v. Powers , Case No. C12-1911RAJ	The court dismissed the case on grounds the plaintiff did not have sufficient evidence for an IDD injunction. The court *explicitly denies* it is ruling on whether the IDD can be applied in Washington. Malsberger (2022) reports no changes to IDD status in the state since the publication of Weisner (2012), who reported that the state accepts the IDD. Clearly this case did not cause Washington to reject the IDD.	Weisner (2012) and Malsberger (2022). Also https://casetext.com/case/amazoncom-1, accessed 1/24/2024. Paragraph 12 states: "Amazon has not proffered evidence from which the court can conclude that it is likely that Mr. Powers will 'inevitably disclose' Amazon's confidential information. The parties debate whether Washington has ever

				recognized inevitable disclosure... On this record, that debate is **largely beside the point.**"

Table IA-2: Effects of Labor Mobility Restrictions (Large Firms)

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effect of court rulings on innovation at large firms (real 2018 USD sales > $220 M), corresponding with Table 4 in the main text. Monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.683***	0.019	-0.651***	-0.105	-1.563***	0.065
	(0.629)	(0.144)	(0.221)	(0.130)	(0.479)	(0.192)
Trade × NC × Post	-0.202	0.220	0.256	0.078	-0.145	0.254
	(0.299)	(0.135)	(0.226)	(0.177)	(0.281)	(0.156)
IDD × Post	0.821**	0.175	0.590***	0.166*	0.874***	0.107
	(0.324)	(0.317)	(0.186)	(0.087)	(0.334)	(0.301)
NC × Post	-0.450**	-0.354	-0.489**	-0.241	-0.413***	-0.361
	(0.178)	(0.237)	(0.207)	(0.250)	(0.159)	(0.255)
Trade × Post	-1.385***	0.008	-0.675***	-0.013	-1.214***	-0.002
	(0.098)	(0.072)	(0.071)	(0.078)	(0.087)	(0.080)
Trade × IDD	0.844	-1.167***	0.154	-0.442	0.649	-1.020**
	(0.664)	(0.440)	(0.264)	(0.468)	(0.528)	(0.485)
Trade × NC	0.136	0.000	-0.136	-0.374*	0.137	-0.016
	(0.252)	(0.253)	(0.195)	(0.209)	(0.238)	(0.282)
IDD	-0.363	0.400	-0.138	0.005	-0.271	0.352
	(0.240)	(0.370)	(0.162)	(0.395)	(0.252)	(0.361)
NC	0.372***	0.177	0.403**	0.463*	0.314***	0.188
	(0.124)	(0.269)	(0.173)	(0.242)	(0.119)	(0.298)
Trade	2.776***	0.076	1.729***	-0.148	2.578***	-0.053
	(0.118)	(0.235)	(0.102)	(0.162)	(0.119)	(0.250)
Post	0.089	-0.028	0.183**	0.087	0.115	-0.060
	(0.101)	(0.079)	(0.075)	(0.064)	(0.094)	(0.093)
ln(Age)	5.306***	7.837**	1.494	6.846***	4.692***	6.939**
	(1.858)	(3.218)	(1.046)	(2.508)	(1.791)	(3.371)
$\ln(\text{Age})^2$	-2.333***	-1.197*	-1.075***	-1.221**	-2.189***	-0.969
	(0.546)	(0.621)	(0.306)	(0.536)	(0.517)	(0.641)
$\ln(\overline{RealPay})$	0.075	1.014***	0.151***	0.920***	0.061	1.051***
	(0.049)	(0.069)	(0.038)	(0.076)	(0.046)	(0.069)
Pseudo-R^2	0.649		0.509		0.676	

Obs.	50,000	50,000	50,000

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.859***	-0.113	-1.636***	0.253*	-2.350***	0.197
	(0.263)	(0.531)	(0.533)	(0.147)	(0.565)	(0.149)
Trade × NC × Post	0.184	-0.213	-0.130	0.039	-0.375	0.067
	(0.279)	(0.229)	(0.284)	(0.067)	(0.330)	(0.306)
IDD × Post	0.457*	-0.030	0.836**	-0.078	0.566*	-0.062
	(0.255)	(0.355)	(0.325)	(0.326)	(0.321)	(0.344)
NC × Post	-0.332	-0.026	-0.476***	-0.247	-0.244	-0.154
	(0.202)	(0.213)	(0.170)	(0.226)	(0.166)	(0.254)
Trade × Post	-0.671***	-0.047	-1.290***	-0.020	-1.534***	0.027
	(0.069)	(0.100)	(0.095)	(0.068)	(0.109)	(0.079)
Trade × IDD	0.398	-0.924	0.756	-1.123***	1.369***	-1.061***
	(0.257)	(0.622)	(0.573)	(0.435)	(0.504)	(0.380)
Trade × NC	-0.319	-0.236	0.086	0.093	0.271	-0.063
	(0.237)	(0.315)	(0.240)	(0.216)	(0.296)	(0.262)
IDD	-0.093	0.287	-0.293	0.551	-0.134	0.431
	(0.182)	(0.493)	(0.235)	(0.388)	(0.198)	(0.382)
NC	0.369***	0.519*	0.367***	0.109	0.217*	0.049
	(0.130)	(0.316)	(0.120)	(0.257)	(0.121)	(0.197)
Trade	1.865***	-0.234	2.697***	0.017	2.932***	0.013
	(0.086)	(0.231)	(0.121)	(0.226)	(0.121)	(0.162)
Post	0.086	-0.011	0.106	-0.029	0.372***	-0.097
	(0.082)	(0.142)	(0.099)	(0.084)	(0.107)	(0.079)
ln(Age)	4.077**	-3.570	5.362***	6.619**	3.649**	6.800***
	(1.671)	(4.519)	(1.822)	(3.147)	(1.776)	(2.242)
$\ln(\text{Age})^2$	-1.689***	0.653	-2.390***	-0.931	-1.809***	-1.040**
	(0.413)	(0.827)	(0.538)	(0.607)	(0.520)	(0.409)
$\ln(\overline{RealPay})$	0.144***	0.941***	0.078	0.995***		
	(0.045)	(0.048)	(0.048)	(0.067)		
$\ln(\overline{DomEmp})$					-0.001	0.783***
					(0.039)	(0.054)
Pseudo-R^2	0.569		0.656		0.491	
Obs.	50,000		50,000		50,000	

Panel C, Innovation Outputs

	Patents Applied		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.224	-0.921**	0.060	-0.053	-0.425	-1.037*
	(0.856)	(0.401)	(0.384)	(0.325)	(0.341)	(0.545)
Trade × NC × Post	-0.034	0.297	-0.091	0.753	-0.114	0.387
	(0.233)	(0.297)	(0.184)	(0.943)	(0.290)	(0.441)
IDD × Post	0.812	1.405***	-0.011	0.048	0.486	1.659***
	(0.658)	(0.384)	(0.411)	(0.462)	(0.317)	(0.627)
NC × Post	-0.084	-0.444	-0.162	-1.033	-0.144	-0.279
	(0.186)	(0.415)	(0.186)	(0.935)	(0.272)	(0.219)
Trade × Post	-1.487***	0.340***	-0.748***	-0.071	-0.800***	-0.125
	(0.077)	(0.117)	(0.071)	(0.143)	(0.086)	(0.137)
Trade × IDD	0.524	0.609	-0.630**	-0.499	-0.279	-0.054
	(0.856)	(0.579)	(0.265)	(0.423)	(0.284)	(0.489)
Trade × NC	-0.073	-0.064	-0.279	-0.087	-0.121	-0.905***
	(0.184)	(0.380)	(0.185)	(0.220)	(0.223)	(0.276)
IDD	-0.401	-1.269***	-0.202	-0.395	-0.214	-1.108*
	(0.613)	(0.257)	(0.261)	(0.433)	(0.239)	(0.572)
NC	0.139	0.151	0.195	0.400***	0.178	1.173***
	(0.115)	(0.409)	(0.144)	(0.144)	(0.203)	(0.192)
Trade	3.092***	-0.145	1.693***	0.245	1.861***	0.335
	(0.118)	(0.242)	(0.096)	(0.252)	(0.122)	(0.235)
Post	0.910***	-0.319**	0.600***	0.041	0.674***	0.112
	(0.089)	(0.125)	(0.061)	(0.149)	(0.076)	(0.119)
ln(Age)	-0.217	4.835***	5.496***	4.578*	2.641**	6.321
	(1.294)	(1.876)	(1.443)	(2.540)	(1.251)	(6.227)
$\ln(\text{Age})^2$	-0.328	-0.996**	-1.850***	-0.598	-1.341***	-0.959
	(0.313)	(0.409)	(0.314)	(0.510)	(0.313)	(1.104)
$\ln(\overline{RealPay})$			-0.088***	1.025***	0.010	1.093***
			(0.034)	(0.097)	(0.039)	(0.067)
$\ln(1+\overline{DomEmp})$	0.222***	0.773***				
	(0.040)	(0.087)				
Pseudo-R^2	0.429		0.654		0.658	
Obs.	50,000		42,000		42,000	

Table IA-3: Effects of Labor Mobility Restrictions (Startups)

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effect of court rulings on innovation at startups (age < 6 years & real sales < $220 M). Corresponding with Table 5 in the main text. Monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.952	-0.922	-2.021***	-0.594	-0.381	-0.125
	(0.819)	(0.819)	(0.587)	(0.897)	(0.899)	(0.684)
Trade × NC × Post	0.177	-0.809***	0.831	-1.412***	0.066	-0.363
	(0.797)	(0.279)	(0.647)	(0.477)	(0.363)	(0.388)
IDD × Post	1.046*	1.038	2.177***	0.221	1.053	0.559
	(0.630)	(0.744)	(0.490)	(0.698)	(0.737)	(0.708)
NC × Post	-0.275	-0.050	-1.022***	1.122**	-0.284	-0.193
	(0.418)	(0.384)	(0.395)	(0.552)	(0.425)	(0.359)
Trade × Post	-1.705***	-0.043	-0.669***	0.137	-1.251***	-0.151
	(0.268)	(0.267)	(0.198)	(0.225)	(0.366)	(0.271)
Trade × IDD	0.879**	-0.300	1.862***	0.059	0.580	-0.504
	(0.390)	(0.480)	(0.302)	(0.543)	(0.472)	(0.307)
Trade × NC	-1.063	0.541	-1.657***	0.824	-0.422	0.397
	(0.711)	(0.436)	(0.262)	(0.516)	(0.396)	(0.453)
IDD	-0.778**	-0.254	-1.525***	0.101	-0.966*	-0.450
	(0.349)	(0.382)	(0.371)	(0.546)	(0.543)	(0.295)
NC	0.556**	-0.054	1.014***	-0.990*	0.552**	0.055
	(0.260)	(0.360)	(0.195)	(0.520)	(0.278)	(0.353)
Trade	2.539***	0.005	1.226***	0.002	1.958***	-0.002
	(0.268)	(0.179)	(0.230)	(0.240)	(0.393)	(0.117)
Post	0.319**	-0.215	0.039	-0.578**	0.362***	0.175
	(0.133)	(0.339)	(0.154)	(0.274)	(0.113)	(0.393)
ln(Age)	0.385	0.506	0.353	2.292***	-0.147	-0.576
	(1.051)	(0.522)	(1.055)	(0.810)	(0.911)	(0.546)
$\ln(\text{Age})^2$	-1.745***	-0.642**	-1.351***	-1.542***	-1.237***	-0.151
	(0.409)	(0.295)	(0.407)	(0.418)	(0.405)	(0.310)
$\ln(\overline{RealPay})$	0.072	0.629***	0.071	0.539***	0.136**	0.735***
	(0.056)	(0.084)	(0.061)	(0.054)	(0.065)	(0.120)
Pseudo-R^2	0.385		0.326		0.472	

Obs.	5,200		5,200		5,200	
Panel B, Innovation Spending on Capital and Labor						
	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.572***	1.209	-1.228	-0.737	-1.898	-0.571**
	(0.447)	(0.889)	(0.883)	(0.632)	(1.716)	(0.225)
Trade × NC × Post	0.031	0.735	0.007	-0.367	0.315	0.296
	(0.449)	(1.059)	(0.814)	(0.373)	(0.745)	(0.379)
IDD × Post	1.256***	-0.865	1.275*	0.805	1.085	0.619***
	(0.327)	(1.217)	(0.763)	(0.534)	(0.703)	(0.223)
NC × Post	-0.666*	-0.626	-0.281	-0.107	-0.477	0.013
	(0.403)	(0.976)	(0.419)	(0.351)	(0.434)	(0.359)
Trade × Post	-1.095***	-0.921	-1.637***	-0.021	-2.171***	-0.037
	(0.187)	(0.642)	(0.251)	(0.264)	(0.144)	(0.143)
Trade × IDD	1.099***	-0.093	1.173***	0.227	1.604	0.337
	(0.403)	(0.831)	(0.396)	(0.334)	(1.501)	(0.297)
Trade × NC	-0.880**	-1.119	-0.923	0.441	-0.862	-0.005
	(0.430)	(1.013)	(0.712)	(0.459)	(0.572)	(0.214)
IDD	-0.776***	-0.094	-0.982**	-0.549**	-1.202***	-0.777***
	(0.235)	(0.828)	(0.475)	(0.228)	(0.314)	(0.256)
NC	0.768**	0.826	0.563**	0.034	0.687**	-0.053
	(0.315)	(0.942)	(0.274)	(0.334)	(0.323)	(0.283)
Trade	1.874***	0.513	2.458***	-0.051	3.049***	0.073
	(0.242)	(0.544)	(0.254)	(0.117)	(0.200)	(0.078)
Post	0.190	0.952*	0.269**	-0.024	0.684***	-0.016
	(0.132)	(0.493)	(0.131)	(0.322)	(0.151)	(0.140)
ln(Age)	1.471	3.441	0.224	0.070	0.687	0.641*
	(0.942)	(2.120)	(1.071)	(0.436)	(0.985)	(0.379)
$\ln(\text{Age})^2$	-2.047***	-2.119**	-1.730***	-0.417*	-2.277***	-0.594***
	(0.398)	(0.990)	(0.409)	(0.237)	(0.405)	(0.154)
$\ln(\overline{RealPay})$	0.080	0.703***	0.075	0.715***		
	(0.053)	(0.248)	(0.054)	(0.075)		
$\ln(\overline{DomEmp})$					-0.081	0.588***
					(0.076)	(0.053)
Pseudo-R^2	0.237		0.515		0.409	
Obs.	5,200		5,200		5,200	

Panel C, Innovation Outputs

	New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.229	-0.457	-0.721	0.171
	(1.480)	(0.360)	(1.105)	(0.770)
Trade × NC × Post	-0.356	-2.488***	0.444	0.035
	(1.011)	(0.488)	(0.718)	(0.774)
IDD × Post	1.086	0.632**	0.409	-0.741
	(1.340)	(0.283)	(1.152)	(0.587)
NC × Post	0.441	0.678***	-0.103	-1.428***
	(0.756)	(0.167)	(0.762)	(0.393)
Trade × Post	-0.793***	0.409*	-0.622**	-0.801**
	(0.276)	(0.219)	(0.243)	(0.408)
Trade × IDD	1.032	-0.079	0.873	-1.537**
	(1.419)	(0.380)	(0.923)	(0.773)
Trade × NC	-0.316	0.540	-0.776	0.052
	(1.024)	(0.683)	(0.655)	(0.676)
IDD	-0.861	-0.329	-0.505	1.063**
	(1.193)	(0.316)	(1.044)	(0.521)
NC	-0.024	-0.318	0.324	0.648**
	(0.807)	(0.441)	(0.672)	(0.307)
Trade	1.153***	0.128	1.155***	-0.172
	(0.234)	(0.295)	(0.250)	(0.309)
Post	0.026	-0.317	0.108	0.792
	(0.303)	(0.336)	(0.298)	(0.546)
ln(Age)	-0.923	0.060	-0.733	2.274
	(1.137)	(1.119)	(1.582)	(1.462)
$\ln(\text{Age})^2$	0.279	-0.155	0.155	-1.549***
	(0.560)	(0.453)	(0.739)	(0.494)
$\ln(\overline{RealPay})$	0.298***	0.758***	0.162	1.192***
	(0.101)	(0.102)	(0.107)	(0.132)
Pseudo-R^2	0.477		0.684	
Obs.	4,800		4,800	

Table IA-4: Conditional Logit Analysis of Trade Secret Dependence

This table presents results from a conditional logistic regression where the odds of a firm declaring trade secrets are important is modeled as a function of year dummies, I(year), and interactions between indicator variables for the firm being located in a state that experienced an IDD (variable *IDD*) or non-compete ruling (variable *NC*), and an indicator variable for the observation occurring in or after the year of an IDD or NC ruling (variable *Post*). Corresponding with Table 6 in the main text. Odds modeled are conditional on firm-by-cohort fixed effects. Standard errors are clustered by firm-by-cohort, industry, and state-by-year groups. Asterisks ***, ** and *, indicate significance at 1%, 5%, and 10%, respectively. Census disclosure #11605.

	Trade Secret	Trade Secret	Trade Secret
	All Firms	Large Firms	Startups
IDD × Post	-0.233*	-0.376*	0.416
	(0.139)	(0.228)	(0.460)
NC × Post	-0.015	-0.107	0.189
	(0.093)	(0.169)	(0.582)
ln(Age)	0.812***	-1.661***	1.497
	(0.007)	(0.009)	(1.325)
$\ln(Age)^2$	-0.628***	0.318***	-2.306***
	(0.031)	(0.066)	(0.520)
I(2009)	-0.007	-0.297***	0.750
	(0.049)	(0.072)	(0.620)
I(2010)	-0.176***	-0.653***	1.465***
	(0.046)	(0.085)	(0.339)
I(2011)	0.005	-0.586***	2.496***
	(0.042)	(0.083)	(0.400)
I(2012)	-0.397***	-1.127***	2.749***
	(0.050)	(0.087)	(0.329)
I(2013)	-0.129**	-1.170***	4.033***
	(0.051)	(0.082)	(0.308)
I(2014)	-0.061	-1.388***	5.308***
	(0.038)	(0.082)	(0.198)
I(2015)	-0.134***	-1.682***	6.088***
	(0.031)	(0.073)	(0.143)
I(2017)	-0.261***	-2.030***	8.447***
	(0.049)	(0.059)	(0.246)
I(2018)	-0.110**	-1.961***	10.350***
	(0.046)	(0.095)	(0.336)
Pseudo-R^2	0.022	0.026	0.054
Obs.	66,000	16,000	2,300

Table IA-5: State-Industry Peer Spillovers

Estimates from stacked, triple difference zero-inflated Poisson regressions testing whether effects of inevitable disclosure doctrine (IDD) rejections on trade secret firms spill over to their non-trade-secret dependent industry peers in their state. Corresponding with Table 7 in the main text. *Peer* is an indicator variable that takes the value of one if a firm has the following characteristics: it is headquartered in a state that experiences an IDD rejection, is non-trade-secret dependent, and shares a 4-digit NAICs code with at least one firm in the same state that is trade-secret dependent in the treatment year. All other variables are the same as in Table 3. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Asterisks ***, **, and *, indicate 1%, 5%, and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer × IDD × Post	-0.161	0.267**	-0.029	0.700**	-0.281	0.143
	(0.352)	(0.118)	(0.415)	(0.309)	(0.364)	(0.144)
Trade × IDD × Post	-1.218***	0.239	-0.781	0.579**	-1.115***	0.194
	(0.389)	(0.167)	(0.528)	(0.277)	(0.361)	(0.221)
Trade × NC × Post	0.030	0.183	0.195	0.093	0.075	0.214
	(0.267)	(0.14)	(0.203)	(0.181)	(0.223)	(0.156)
IDD × Post	0.544	-0.045	0.484	-0.443	0.661	-0.018
	(0.388)	(0.272)	(0.486)	(0.371)	(0.419)	(0.262)
NC × Post	-0.084	-0.319	-0.222	-0.277	-0.097	-0.32
	(0.144)	(0.232)	(0.162)	(0.239)	(0.138)	(0.254)
Trade × Post	-1.569***	0.016	-0.888***	0.041	-1.407***	0.01
	(0.094)	(0.076)	(0.048)	(0.067)	(0.111)	(0.078)
Peer × IDD	0.211	0.799***	0.772**	0.013	0.256	0.621***
	(0.311)	(0.272)	(0.373)	(0.446)	(0.324)	(0.215)
Trade × IDD	0.707**	-0.413	1.001**	-0.211	0.545	-0.509
	(0.348)	(0.41)	(0.479)	(0.343)	(0.332)	(0.463)
Trade × NC	-0.088	-0.01	-0.213	-0.346*	-0.101	-0.02
	(0.238)	(0.233)	(0.144)	(0.191)	(0.179)	(0.262)
IDD	-0.399	-0.221	-0.734	0.013	-0.459	-0.118
	(0.339)	(0.349)	(0.446)	(0.347)	(0.362)	(0.335)
NC	0.059	0.201	0.174	0.459**	0.059	0.205
	(0.129)	(0.247)	(0.126)	(0.218)	(0.122)	(0.276)
Trade	2.851***	0.119	1.832***	-0.109	2.627***	-0.011
	(0.134)	(0.22)	(0.088)	(0.14)	(0.169)	(0.23)
Post	1.023***	0.041	0.770***	0.074	0.951***	0.021
	(0.074)	(0.081)	(0.047)	(0.064)	(0.080)	(0.086)
ln(Age)	0.157	1.364	0.190	0.97	0.399	1.276
	(0.58)	(1.297)	(0.360)	(0.691)	(0.595)	(1.67)
$\ln(Age)^2$	-0.581**	-0.037	-0.447***	-0.191	-0.553**	0.108

	(0.26)	(0.302)	(0.146)	(0.214)	(0.25)	(0.362)
$\ln(\overline{RealPay})$	0.094***	0.996***	0.129***	0.898***	0.104***	1.035***
	(0.027)	(0.064)	(0.018)	(0.066)	(0.035)	(0.068)
Pseudo-R^2	0.745		0.623		0.766	
Obs.	199,000		199,000		199,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer × IDD × Post	0.070	-0.385	-0.212	-0.041	0.027	0.100
	(0.392)	(0.44)	(0.367)	(0.222)	(0.417)	(0.128)
Trade × IDD × Post	-0.775*	-0.07	-1.224***	0.186*	-1.386***	0.226*
	(0.435)	(0.321)	(0.403)	(0.095)	(0.408)	(0.119)
Trade × NC × Post	0.079	-0.241	0.001	0.021	-0.098	0.053
	(0.211)	(0.206)	(0.245)	(0.077)	(0.275)	(0.247)
IDD × Post	0.363	0.138	0.628	-0.023	0.875*	-0.055
	(0.412)	(0.189)	(0.413)	(0.412)	(0.491)	(0.323)
NC × Post	-0.123	0.011	-0.091	-0.217	-0.265	-0.179
	(0.152)	(0.186)	(0.149)	(0.22)	(0.185)	(0.224)
Trade × Post	-0.960***	-0.126	-1.518***	-0.005	-1.836***	0.063
	(0.059)	(0.119)	(0.087)	(0.065)	(0.099)	(0.065)
Peer × IDD	0.224	-0.388	0.336	1.085**	0.236	1.382***
	(0.317)	(0.513)	(0.319)	(0.52)	(0.396)	(0.471)
Trade × IDD	0.717*	-1.259***	0.772**	-0.163	1.002***	0.15
	(0.407)	(0.448)	(0.345)	(0.554)	(0.388)	(0.482)
Trade × NC	-0.178	-0.217	-0.081	0.069	0.060	-0.05
	(0.171)	(0.323)	(0.217)	(0.199)	(0.251)	(0.223)
IDD	-0.346	0.561	-0.509	-0.335	-0.752	-0.727
	(0.379)	(0.378)	(0.354)	(0.648)	(0.472)	(0.558)
NC	0.170	0.48	0.056	0.137	0.199	0.108
	(0.113)	(0.322)	(0.132)	(0.236)	(0.155)	(0.173)
Trade	2.008***	-0.097	2.794***	0.052	3.214***	0.005
	(0.071)	(0.227)	(0.126)	(0.208)	(0.084)	(0.134)
Post	0.894***	0.075	1.008***	0.045	1.294***	0.017
	(0.057)	(0.131)	(0.074)	(0.077)	(0.077)	(0.072)
ln(Age)	-0.236	-2.861**	0.223	0.593	0.294	0.455
	(0.451)	(1.452)	(0.582)	(1.429)	(0.828)	(0.883)
$\ln(\text{Age})^2$	-0.463**	0.354	-0.612**	0.173	-0.768**	0.175
	(0.208)	(0.482)	(0.26)	(0.335)	(0.335)	(0.225)
$\ln(\overline{RealPay})$	0.169***	0.914***	0.093***	0.979***		
	(0.018)	(0.042)	(0.026)	(0.057)		
$\ln(\overline{DomEmp})$					0.013	0.805***

					(0.019)	(0.045)
Pseudo-R^2	0.624		0.758		0.646	
Obs.	199,000		199,000		199,000	

Panel C, Innovation Outputs

	Patents		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Peer × IDD × Post	-0.441	2.095***	0.584	-1.681***	0.113	1.710***
	(0.72)	(0.585)	(0.441)	(0.472)	(0.394)	(0.639)
Trade × IDD × Post	-1.557	1.215***	0.244	-1.996***	-0.234	1.164*
	(1.021)	(0.369)	(0.335)	(0.374)	(0.321)	(0.645)
Trade × NC × Post	-0.002	0.271	-0.155	0.338	-0.322**	0.669
	(0.262)	(0.260)	(0.192)	(0.406)	(0.161)	(0.905)
IDD × Post	1.267	-0.756***	-0.515	2.405***	0.048	-1.374***
	(1.011)	(0.253)	(0.366)	(0.348)	(0.327)	(0.485)
NC × Post	-0.132	-0.506	0.091	-0.192	0.179	-0.860
	(0.196)	(0.378)	(0.212)	(0.162)	(0.234)	(0.918)
Trade × Post	-1.445***	0.390***	-0.898***	-0.120	-0.819***	-0.025
	(0.073)	(0.115)	(0.057)	(0.126)	(0.054)	(0.159)
Peer × IDD	1.328*	-1.672***	-0.205	1.379***	-0.156	-0.337
	(0.701)	(0.58)	(0.382)	(0.400)	(0.394)	(0.482)
Trade × IDD	1.845*	-1.181**	-0.167	0.700***	-0.252	-0.709
	(1.000)	(0.504)	(0.310)	(0.267)	(0.371)	(0.635)
Trade × NC	-0.066	-0.081	-0.119	-0.837***	-0.069	-0.039
	(0.239)	(0.337)	(0.184)	(0.262)	(0.154)	(0.209)
IDD	-1.568	0.577***	0.151	-1.625***	0.017	0.009
	(0.990)	(0.222)	(0.337)	(0.214)	(0.355)	(0.384)
NC	0.137	0.289	0.023	1.080***	-0.029	0.313**
	(0.186)	(0.365)	(0.197)	(0.171)	(0.215)	(0.132)
Trade	2.745***	-0.153	1.834***	0.312	1.677***	0.236
	(0.086)	(0.186)	(0.083)	(0.218)	(0.080)	(0.237)
Post	1.356***	-0.158	0.967***	0.046	0.944***	-0.069
	(0.072)	(0.128)	(0.086)	(0.129)	(0.089)	(0.173)
ln(Age)	-0.901*	-0.357	0.806	5.230	1.075*	2.892
	(0.512)	(0.883)	(0.506)	(3.434)	(0.589)	(2.204)
$\ln(\text{Age})^2$	-0.215	0.142	-0.550***	-0.829	-0.533**	-0.191
	(0.211)	(0.268)	(0.209)	(0.668)	(0.242)	(0.558)
$\ln(\overline{RealPay})$			-0.007	1.108***	-0.006	1.048***
			(0.026)	(0.053)	(0.033)	(0.079)
$\ln(\overline{DomEmp})$	0.223***	0.770***				
	(0.046)	(0.081)				
Pseudo-R^2	0.538		0.766		0.760	

Obs.	199,000	170,000	170,000

Table IA-6: COW Adoption

Estimates from stacked, triple difference zero-inflated Poisson regressions examining Corporate Opportunity Waiver (COW) Law passages (Fich et al., 2023) on the innovation for all firms in addition to labor mobility rulings studies in Table 3. Corresponding with Table 8 in the main text. *COW* is an indicator variable that takes the value of one if a firm is headquartered in a state that experiences the passage of a COW law and zero otherwise. All other variables are the same as in Table 3. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Asterisks ***, **, and *, indicate 1%, 5%, and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.478	-1.387***	0.552**	-0.266	0.358	-1.309***
	(0.298)	(0.369)	(0.257)	(0.408)	(0.367)	(0.478)
Trade × IDD × Post	-1.073***	0.010	-0.760***	-0.027	-0.882***	0.048
	(0.248)	(0.128)	(0.247)	(0.146)	(0.25)	(0.155)
Trade × NC × Post	0.010	0.202	0.181	0.088	0.058	0.232
	(0.271)	(0.172)	(0.207)	(0.194)	(0.225)	(0.189)
COW × Post	-0.109	1.525***	-0.315	0.314	-0.099	1.41***
	(0.216)	(0.316)	(0.195)	(0.303)	(0.26)	(0.433)
IDD × Post	0.416**	0.194	0.470**	0.163	0.438**	0.13
	(0.183)	(0.317)	(0.205)	(0.141)	(0.180)	(0.307)
NC × Post	-0.080	-0.328	-0.212	-0.261	-0.094	-0.335
	(0.145)	(0.256)	(0.165)	(0.256)	(0.139)	(0.277)
Trade × Post	-1.591***	-0.042	-0.911***	0.017	-1.421***	0.051
	(0.092)	(0.050)	(0.050)	(0.066)	(0.110)	(0.053)
Trade × COW	-0.413	0.804	-0.648**	0.222	-0.220	0.636
	(0.378)	(0.534)	(0.294)	(0.513)	(0.366)	(0.554)
Trade × IDD	0.507***	-0.882**	0.357*	-0.151	0.316*	-0.824*
	(0.189)	(0.384)	(0.204)	(0.345)	(0.179)	(0.426)
Trade × NC	-0.070	-0.018	-0.205	-0.342*	-0.087	-0.034
	(0.242)	(0.255)	(0.148)	(0.196)	(0.182)	(0.280)
IDD	-0.230	0.286	-0.103	-0.034	-0.258*	0.236
	(0.154)	(0.351)	(0.170)	(0.338)	(0.156)	(0.339)
NC	0.052	0.215	0.170	0.473**	0.053	0.212
	(0.129)	(0.265)	(0.127)	(0.225)	(0.122)	(0.292)
COW	0.148	-1.550***	0.423***	-1.094***	0.092	-1.388***
	(0.165)	(0.357)	(0.156)	(0.303)	(0.212)	(0.4)
Trade	2.884***	-0.057	1.850***	-0.18	2.653***	-0.181
	(0.131)	(0.207)	(0.089)	(0.138)	(0.168)	(0.218)
Post	1.048***	-0.002	0.790***	0.073	0.973***	-0.019
	(0.076)	(0.060)	(0.049)	(0.058)	(0.082)	(0.067)
ln(Age)	0.345	1.391	0.294	0.745	0.531	1.555

	(0.609)	(1.445)	(0.365)	(0.519)	(0.593)	(2.058)
ln(Age)^2	-0.652**	-0.030	-0.481***	-0.071	-0.609**	0.02
	(0.278)	(0.281)	(0.153)	(0.151)	(0.271)	(0.398)
$\ln(\overline{RealPay})$	0.095***	1.016***	0.131***	0.902***	0.106***	1.047***
	(0.029)	(0.067)	(0.017)	(0.075)	(0.034)	(0.069)
Pseudo-R^2	0.752		0.611		0.769	
Obs.	182,000		182,000		182,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.201	0.169	0.450	-1.339***	-1.489**	-1.691***
	(0.276)	(0.424)	(0.321)	(0.247)	(0.744)	(0.499)
Trade × IDD × Post	-0.840***	0.177	-1.041***	0.204*	-1.419***	0.116
	(0.201)	(0.537)	(0.235)	(0.118)	(0.128)	(0.117)
Trade × NC × Post	0.067	-0.155	-0.017	0.035	-0.112	0.087
	(0.211)	(0.205)	(0.248)	(0.1)	(0.277)	(0.274)
COW × Post	-0.102	0.451**	-0.168	1.418***	-0.343	1.82***
	(0.159)	(0.188)	(0.212)	(0.227)	(0.303)	(0.548)
IDD × Post	0.430**	-0.103	0.461**	-0.041	0.903***	0.057
	(0.178)	(0.354)	(0.188)	(0.324)	(0.278)	(0.326)
NC × Post	-0.115	-0.071	-0.087	-0.235	-0.254	-0.206
	(0.153)	(0.198)	(0.15)	(0.245)	(0.188)	(0.237)
Trade × Post	-0.955***	-0.163	-1.539***	0.04	-1.837***	0.124**
	(0.058)	(0.131)	(0.086)	(0.049)	(0.107)	(0.051)
Trade × COW	-0.207	0.838**	-0.394	0.615	1.586**	0.953*
	(0.264)	(0.4)	(0.393)	(0.553)	(0.727)	(0.545)
Trade × IDD	0.533***	-0.883	0.475***	-0.901**	0.798***	-0.811**
	(0.193)	(0.596)	(0.174)	(0.384)	(0.142)	(0.329)
Trade × NC	-0.174	-0.178	-0.065	0.057	0.070	-0.074
	(0.170)	(0.266)	(0.221)	(0.215)	(0.254)	(0.242)
IDD	-0.172	0.182	-0.241	0.435	-0.547**	0.265
	(0.151)	(0.467)	(0.158)	(0.374)	(0.26)	(0.359)
NC	0.163	0.427	0.049	0.143	0.190	0.123
	(0.112)	(0.266)	(0.132)	(0.255)	(0.157)	(0.181)
COW	0.008	-1.563***	0.186	-1.358***	0.560**	-1.431**
	(0.16)	(0.293)	(0.175)	(0.328)	(0.258)	(0.574)
Trade	2.013***	-0.218	2.823***	-0.098	3.232***	-0.116
	(0.072)	(0.224)	(0.123)	(0.204)	(0.091)	(0.146)
Post	0.909***	0.145	1.033***	0.009	1.313***	-0.044
	(0.06)	(0.131)	(0.077)	(0.064)	(0.084)	(0.063)
ln(Age)	-0.123	-2.881*	0.414	0.823	0.499	0.303

	(0.468)	(1.522)	(0.607)	(1.566)	(0.868)	(0.926)
ln(Age)^2	-0.516**	0.244	-0.685**	0.084	-0.844**	0.193
	(0.222)	(0.463)	(0.28)	(0.326)	(0.356)	(0.226)
$\ln(\overline{RealPay})$	0.168***	0.948***	0.094***	0.992***		
	(0.02)	(0.048)	(0.03)	(0.059)		
$\ln(\overline{DomEmp})$					0.016	0.813***
					(0.019)	(0.063)
Pseudo-R^2	0.629		0.759		0.641	
Obs.	182,000		182,000		182,000	

Panel C, Innovation Outputs

	Patents		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × COW × Post	0.225	0.186	0.259	-1.86***	0.555**	-2.752***
	(0.217)	(0.269)	(0.169)	(0.344)	(0.249)	(0.91)
Trade × IDD × Post	-1.245***	-0.473*	-0.235	-0.986**	-0.319*	-0.251
	(0.370)	(0.27)	(0.173)	(0.435)	(0.164)	(0.297)
Trade × NC × Post	-0.003	0.276	-0.145	0.265	-0.318**	-0.291
	(0.265)	(0.261)	(0.189)	(0.399)	(0.16)	(0.259)
COW x Post	-0.484**	-0.319	-0.241	0.923***	-0.460	1.994**
	(0.223)	(0.329)	(0.254)	(0.323)	(0.328)	(0.803)
IDD × Post	0.956***	0.935**	-0.031	1.368***	0.142	0.02
	(0.337)	(0.396)	(0.203)	(0.492)	(0.160)	(0.361)
NC × Post	-0.130	-0.517	0.080	-0.157	0.175	0.052
	(0.198)	(0.381)	(0.204)	(0.15)	(0.229)	(0.163)
Trade × Post	-1.434***	0.369***	-0.883***	-0.05	-0.810***	0.09
	(0.068)	(0.134)	(0.058)	(0.126)	(0.059)	(0.173)
Trade x COW	0.128	-1.498***	-0.052	2.233***	-0.069	0.916*
	(0.285)	(0.319)	(0.291)	(0.466)	(0.366)	(0.497)
Trade × IDD	0.761**	0.176	-0.008	-0.138	-0.139	-0.496
	(0.371)	(0.435)	(0.133)	(0.411)	(0.171)	(0.408)
Trade × NC	-0.062	-0.077	-0.123	-0.789***	-0.070	0.012
	(0.241)	(0.331)	(0.181)	(0.267)	(0.153)	(0.235)
IDD	-0.486	-0.765**	-0.026	-0.769*	-0.117	-0.194
	(0.343)	(0.353)	(0.145)	(0.458)	(0.130)	(0.34)
NC	0.132	0.278	0.020	1.017***	-0.034	0.246
	(0.186)	(0.363)	(0.19)	(0.166)	(0.212)	(0.168)
COW	0.444**	0.288	-0.178	-1.901***	-0.135	-0.502*
	(0.207)	(0.34)	(0.301)	(0.404)	(0.350)	(0.263)
Trade	2.749***	-0.134	1.825***	0.332	1.676***	0.259
	(0.092)	(0.22)	(0.085)	(0.238)	(0.081)	(0.262)
Post	1.345***	-0.143	0.989***	0.036	0.973***	-0.109
	(0.073)	(0.151)	(0.087)	(0.133)	(0.089)	(0.17)

ln(Age)	-0.734	-0.088	0.987*	5.785*	1.132*	4.259**
	(0.499)	(0.96)	(0.549)	(3.388)	(0.613)	(2.058)
$\ln(\text{Age})^2$	-0.258	0.062	-0.618***	-1.083*	-0.571**	-0.663
	(0.211)	(0.275)	(0.215)	(0.631)	(0.248)	(0.505)
$\ln(\overline{RealPay})$			-0.006	1.117***	-0.008	1.07***
			(0.026)	(0.056)	(0.033)	(0.086)
$\ln(\overline{DomEmp})$	0.222***	0.767***				
	(0.048)	(0.076)				
Pseudo-R^2	0.526		0.772		0.768	
Obs.	182,000		156,000		156,000	

Table IA-7: Massachusetts Robustness

Estimates from stacked, triple difference zero-inflated Poisson regressions examining whether IDD results in Table 3 are robust to controlling for Massachusetts' failed attempt to abolish non-competes over the 2011–2014 period. Corresponding with Table 9 in the main text. Variable *NCLF*, is an acronym for non-compete legislation failure, and equals one if a firm is located in Massachusetts as of 2014, the year the proposed non-compete abolishment law failed, and zero otherwise. Variable *PostMA* is an indicator for observations occurring in or after 2014. All other variables are the same as in Table 3. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Asterisks ***, **, and *, indicate 1%, 5%, and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.110***	-0.009	-0.791***	-0.050	-0.898***	0.051
	(0.242)	(0.089)	(0.244)	(0.165)	(0.247)	(0.116)
Trade × NCLF × PostMA	0.134	-0.062	0.312***	-0.699**	0.142	-0.120
	(0.106)	(0.150)	(0.092)	(0.295)	(0.099)	(0.159)
Trade × NC × Post	0.035	0.180	0.197	0.086	0.077	0.214
	(0.267)	(0.146)	(0.204)	(0.191)	(0.223)	(0.156)
IDD × Post	0.411**	0.224	0.482**	0.206	0.433**	0.144
	(0.178)	(0.168)	(0.210)	(0.163)	(0.171)	(0.213)
NCLF × PostMA	-0.121	-0.029	-0.218**	0.487**	-0.126	-0.096
	(0.094)	(0.160)	(0.094)	(0.234)	(0.094)	(0.215)
NC × Post	-0.080	-0.314	-0.219	-0.267	-0.089	-0.319
	(0.148)	(0.233)	(0.165)	(0.243)	(0.141)	(0.253)
Trade × Post	-1.570***	0.019	-0.884***	0.044	-1.400***	0.013
	(0.095)	(0.080)	(0.048)	(0.068)	(0.113)	(0.080)
Trade × PostMA	-0.548***	0.069	-0.426***	0.532**	-0.514***	-0.025
	(0.100)	(0.189)	(0.073)	(0.238)	(0.102)	(0.191)
Trade × IDD	0.447*	-1.330**	0.170	-0.115	0.318	-1.440**
	(0.257)	(0.556)	(0.323)	(0.351)	(0.250)	(0.572)
Trade × NCLF	0.128	0.211	0.263	-0.347	0.030	0.614
	(0.206)	(0.525)	(0.271)	(0.356)	(0.194)	(0.517)
Trade × NC	-0.090	-0.008	-0.217	-0.337*	-0.100	-0.021
	(0.238)	(0.235)	(0.147)	(0.200)	(0.178)	(0.262)
IDD	-0.142	0.088	-0.216	-0.578**	-0.110	0.228
	(0.210)	(0.338)	(0.282)	(0.290)	(0.220)	(0.351)
NCLF	-0.158	0.847**	0.111	1.070***	-0.229	0.492
	(0.217)	(0.379)	(0.224)	(0.175)	(0.211)	(0.348)
NC	0.050	0.195	0.170	0.446**	0.047	0.204
	(0.132)	(0.247)	(0.129)	(0.221)	(0.124)	(0.276)
Trade	2.850***	0.119	1.830***	-0.109	2.620***	-0.012

	(0.137)	(0.227)	(0.089)	(0.142)	(0.172)	(0.242)
Post	1.020***	0.053	0.769***	0.081	0.953***	0.034
	(0.074)	(0.087)	(0.048)	(0.065)	(0.081)	(0.091)
PostMA	0.942***	0.065	0.683***	-0.327	0.896***	0.117
	(0.103)	(0.237)	(0.068)	(0.254)	(0.109)	(0.264)
ln(Age)	0.552	1.930	0.476	1.250	0.790	1.840
	(0.587)	(1.470)	(0.344)	(0.762)	(0.579)	(1.870)
$\ln(Age)^2$	-0.726***	-0.178	-0.544***	-0.269	-0.695***	-0.021
	(0.257)	(0.322)	(0.141)	(0.218)	(0.249)	(0.382)
$\ln(\overline{RealPay})$	0.094***	0.997***	0.129***	0.898***	0.104***	1.040***
	(0.027)	(0.057)	(0.018)	(0.065)	(0.035)	(0.057)
Pseudo-R^2	0.745		0.623		0.766	
Obs.	203,000		203,000		203,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.852***	0.167	-1.080***	0.194***	-1.430***	0.112
	(0.202)	(0.425)	(0.227)	(0.070)	(0.141)	(0.083)
Trade × NCLF × PostMA	-0.139	-0.658*	0.152	-0.146	-0.025	-0.087
	(0.144)	(0.354)	(0.102)	(0.132)	(0.175)	(0.111)
Trade × NC × Post	0.081	-0.247	0.006	0.018	-0.097	0.050
	(0.212)	(0.211)	(0.246)	(0.080)	(0.273)	(0.248)
IDD × Post	0.443**	-0.108	0.457**	-0.016	0.911***	0.112
	(0.180)	(0.295)	(0.182)	(0.214)	(0.278)	(0.268)
NCLF × PostMA	0.081	0.743***	-0.114	-0.159	-0.146	-0.070
	(0.143)	(0.277)	(0.098)	(0.148)	(0.150)	(0.167)
NC × Post	-0.120	0.018	-0.086	-0.212	-0.257	-0.175
	(0.159)	(0.189)	(0.154)	(0.222)	(0.190)	(0.224)
Trade × Post	-0.956***	-0.129	-1.510***	-0.002	-1.830***	0.066
	(0.059)	(0.120)	(0.087)	(0.068)	(0.100)	(0.067)
Trade × PostMA	-0.465***	0.362	-0.554***	0.057	-0.580***	0.116
	(0.091)	(0.281)	(0.092)	(0.168)	(0.100)	(0.148)
Trade × IDD	0.419	-1.320**	0.405*	-1.420**	0.726***	-1.140*
	(0.282)	(0.668)	(0.243)	(0.584)	(0.257)	(0.613)
Trade × NCLF	0.138	0.728	0.135	0.517	0.096	0.373
	(0.215)	(0.500)	(0.203)	(0.542)	(0.235)	(0.634)
Trade × NC	-0.182	-0.209	-0.084	0.072	0.061	-0.047
	(0.173)	(0.324)	(0.218)	(0.199)	(0.249)	(0.225)
NCLF	0.152	-0.433	-0.144	0.450	-0.161	0.172
	(0.197)	(0.383)	(0.219)	(0.382)	(0.280)	(0.516)
IDD	-0.312	0.445	-0.163	0.407	-0.475	0.292

	(0.250)	(0.534)	(0.218)	(0.440)	(0.357)	(0.504)
NC	0.164	0.474	0.047	0.132	0.186	0.104
	(0.118)	(0.323)	(0.136)	(0.236)	(0.158)	(0.173)
Trade	2.010***	-0.096	2.790***	0.052	3.210***	0.006
	(0.071)	(0.262)	(0.128)	(0.229)	(0.085)	(0.145)
Post	0.897***	0.068	1.010***	0.058	1.290***	0.028
	(0.059)	(0.137)	(0.074)	(0.083)	(0.075)	(0.078)
PostMA	0.779***	-0.064	0.943***	0.069	1.240***	-0.002
	(0.084)	(0.270)	(0.101)	(0.190)	(0.106)	(0.184)
ln(Age)	0.108	-3.090**	0.615	1.070	0.763	0.829
	(0.450)	(1.410)	(0.588)	(1.590)	(0.831)	(0.903)
$\ln(\text{Age})^2$	-0.584***	0.423	-0.758***	0.050	-0.944***	0.067
	(0.203)	(0.482)	(0.259)	(0.359)	(0.331)	(0.222)
$\ln(\overline{RealPay})$	0.169***	0.915***	0.092***	0.980***		
	(0.020)	(0.042)	(0.027)	(0.054)		
$\ln(\overline{DomEmp})$					0.012	0.805***
					(0.021)	(0.046)
Pseudo-R^2	0.626		0.757		0.645	
Obs.	203,000		203,000		203,000	

Panel C, Innovation Outputs

	Patents		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.220***	-0.508*	-0.311*	-0.137	-0.220	-0.889**
	(0.325)	(0.300)	(0.168)	(0.352)	(0.173)	(0.444)
Trade × NCLF × PostMA	0.166	-0.259	-0.024	0.941**	0.001	0.229
	(0.181)	(0.258)	(0.265)	(0.421)	(0.256)	(0.379)
Trade × NC × Post	-0.006	0.275	-0.319**	0.670	-0.152	0.338
	(0.264)	(0.264)	(0.160)	(0.904)	(0.192)	(0.398)
IDD × Post	0.956***	1.060**	0.118	-0.080	-0.028	1.290***
	(0.347)	(0.443)	(0.151)	(0.313)	(0.195)	(0.476)
NCLF × PostMA	-0.278*	-0.005	0.175	0.193	0.041	1.020***
	(0.167)	(0.242)	(0.260)	(0.488)	(0.229)	(0.214)
NC × Post	-0.124	-0.508	0.176	-0.857	0.089	-0.193
	(0.200)	(0.378)	(0.236)	(0.918)	(0.213)	(0.155)
Trade × Post	-1.440***	0.393***	-0.812***	-0.023	-0.893***	-0.116
	(0.066)	(0.115)	(0.061)	(0.167)	(0.061)	(0.122)
Trade × PostMA	-0.379***	-0.064	-0.366***	-0.193	-0.387***	0.361
	(0.087)	(0.239)	(0.094)	(0.353)	(0.107)	(0.350)
Trade × IDD	0.250	-0.331	-0.008	-0.096	0.032	-0.115
	(0.469)	(0.550)	(0.228)	(0.430)	(0.176)	(0.464)
Trade × NCLF	0.609**	0.899*	-0.178	-0.344	-0.086	-0.025
	(0.287)	(0.521)	(0.252)	(0.575)	(0.225)	(0.386)

Trade × NC	-0.065	-0.084	-0.068	-0.045	-0.119	-0.844***
	(0.239)	(0.339)	(0.156)	(0.204)	(0.184)	(0.266)
NCLF	-0.029	-0.084	-0.253	-0.340	0.019	-0.083
	(0.276)	(0.383)	(0.236)	(0.285)	(0.253)	(0.181)
IDD	-0.492	-0.759**	0.060	-0.108	-0.051	-0.736*
	(0.449)	(0.353)	(0.167)	(0.317)	(0.159)	(0.438)
NC	0.126	0.287	-0.037	0.317**	0.015	1.080***
	(0.188)	(0.367)	(0.217)	(0.134)	(0.197)	(0.176)
Trade	2.740***	-0.154	1.670***	0.238	1.830***	0.314
	(0.086)	(0.189)	(0.085)	(0.241)	(0.084)	(0.220)
Post	1.360***	-0.144	0.958***	-0.067	0.983***	0.053
	(0.070)	(0.131)	(0.093)	(0.179)	(0.090)	(0.129)
PostMA	0.664***	0.247	0.448***	-0.023	0.463***	-0.338
	(0.104)	(0.261)	(0.095)	(0.266)	(0.091)	(0.236)
ln(Age)	-0.573	-0.033	1.530**	3.280	1.300***	5.780
	(0.505)	(0.902)	(0.605)	(2.560)	(0.480)	(3.760)
$\ln(\text{Age})^2$	-0.339*	0.020	-0.708***	-0.245	-0.731***	-0.962
	(0.205)	(0.272)	(0.246)	(0.640)	(0.197)	(0.735)
$\ln(\overline{RealPay})$			-0.007	1.050***	-0.008	1.110***
			(0.034)	(0.078)	(0.028)	(0.052)
$\ln(\overline{DomEmp})$	0.223***	0.772***				
	(0.048)	(0.070)				
Pseudo-R^2	0.539		0.760		0.766	
Obs.	203,000		174,000		174,000	

Table IA-8: Effects of Labor Mobility Restrictions for Full Sample Without Trade Secret Dependence
Estimates from stacked, difference-in-differences zero-inflated Poisson regressions examining the effects of court rulings on innovation for all firms. A firm's monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Unlike in Table 3, we do not include the variable *Trade* or any of its interactions. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11605.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
IDD × Post	0.060	0.272	0.084	0.173	0.068	0.235
	(0.179)	(0.352)	(0.146)	(0.112)	(0.161)	(0.371)
NC × Post	0.014	-0.182	-0.062	-0.193	0.008	-0.161
	(0.168)	(0.119)	(0.137)	(0.194)	(0.160)	(0.129)
IDD	-0.114	-0.355	0.050	-0.129	-0.146	-0.343
	(0.155)	(0.393)	(0.132)	(0.235)	(0.136)	(0.423)
NC	-0.092	0.196**	-0.019	0.170	-0.085	0.189*
	(0.155)	(0.097)	(0.127)	(0.126)	(0.147)	(0.111)
Post	0.471***	0.042	0.312***	0.111***	0.413***	0.029
	(0.089)	(0.050)	(0.055)	(0.038)	(0.090)	(0.049)
ln(Age)	1.101*	1.211	0.827**	0.982	1.123*	1.297
	(0.638)	(1.238)	(0.408)	(0.692)	(0.633)	(1.639)
$\ln(\text{Age})^2$	-0.954***	0.002	-0.730***	-0.196	-0.892***	0.102
	(0.296)	(0.275)	(0.192)	(0.207)	(0.292)	(0.342)
$\ln(\overline{RealPay})$	0.107***	0.998***	0.138***	0.895***	0.115***	1.034***
	(0.022)	(0.055)	(0.016)	(0.065)	(0.028)	(0.054)
Pseudo-R^2	0.744		0.623		0.766	
Obs.	199,000		199,000		199,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
IDD × Post	0.058	0.139	0.093	0.146	0.547**	0.188
	(0.173)	(0.133)	(0.181)	(0.357)	(0.257)	(0.307)
NC × Post	-0.030	-0.126	-0.007	-0.205	-0.184	-0.138*
	(0.117)	(0.100)	(0.168)	(0.158)	(0.216)	(0.074)
IDD	0.043	-0.358*	-0.120	-0.194	-0.387	-0.330
	(0.162)	(0.190)	(0.155)	(0.402)	(0.243)	(0.342)
NC	-0.009	0.308**	-0.090	0.193*	0.083	0.071
	(0.108)	(0.120)	(0.154)	(0.111)	(0.199)	(0.081)
Post	0.413***	-0.002	0.458***	0.037	0.657***	0.060
	(0.072)	(0.080)	(0.088)	(0.047)	(0.093)	(0.045)
ln(Age)	0.610	-2.497*	1.139*	0.566	1.577*	0.333
	(0.529)	(1.354)	(0.635)	(1.365)	(0.817)	(0.829)
$\ln(\text{Age})^2$	-0.765***	0.263	-0.970***	0.180	-1.202***	0.205
	(0.254)	(0.449)	(0.296)	(0.310)	(0.356)	(0.213)
$\ln(\overline{RealPay})$	0.172***	0.911***	0.106***	0.979***		
	(0.018)	(0.040)	(0.022)	(0.052)		
$\ln(\overline{DomEmp})$					0.034*	0.805***
					(0.020)	(0.044)
Pseudo-R^2	0.622		0.757		0.645	
Obs.	199,000		199,000		199,000	

Panel C, Innovation Outputs

	Patents Applied		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
IDD × Post	0.239	0.526*	-0.099	0.816***	0.001	-0.132
	(0.162)	(0.274)	(0.149)	(0.301)	(0.070)	(0.327)
NC × Post	-0.146	-0.283	-0.026	0.102	-0.054	-0.251**
	(0.119)	(0.218)	(0.179)	(0.307)	(0.212)	(0.117)
IDD	-0.048	-0.623	-0.069	-0.961***	-0.235**	-0.642*
	(0.168)	(0.427)	(0.131)	(0.313)	(0.105)	(0.366)
NC	0.066	0.221	-0.110	0.364*	-0.117	0.267**
	(0.117)	(0.160)	(0.166)	(0.207)	(0.189)	(0.122)
Post	0.422***	0.173**	0.469***	-0.072	0.505***	-0.099
	(0.064)	(0.072)	(0.086)	(0.112)	(0.090)	(0.138)
ln(Age)	0.532	-0.784	2.197***	5.053	2.399***	2.826
	(0.520)	(0.842)	(0.610)	(3.451)	(0.745)	(2.202)
$\ln(\text{Age})^2$	-0.681***	0.246	-1.171***	-0.794	-1.125***	-0.174
	(0.245)	(0.269)	(0.260)	(0.676)	(0.285)	(0.559)
$\ln(\overline{RealPay})$			0.016	1.116***	0.016	1.055***
			(0.023)	(0.054)	(0.030)	(0.082)
$\ln(\overline{DomEmp})$	0.205***	0.771***				
	(0.037)	(0.068)				
Pseudo-R^2	0.536		0.765		0.759	
Obs.	199,000		170,000		170,000	

Table IA-9: OLS Robustness

Results from stacked, triple difference OLS regressions examining the effect of non-compete and inevitable disclosure doctrine rulings on various innovation inputs and outputs. All monetary outcome variables are measured in 2018 USD and scaled by the average real pre-treatment payroll. All outcome variables that are counts are scaled by average domestic pre-treatment employee counts. All regressions include firm-by-cohort, state-by-year-by-cohort, and industry-by-year fixed effects. Standard errors are clustered by firm, state-by-year, and industry. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Census disclosure #11605.

Panel A, R&D Spending

	R&D	Research	Development
Trade × IDD × Post	-0.142**	-0.002	-0.046
	(0.064)	(0.009)	(0.030)
Trade × NC × Post	-0.043	-0.019	-0.020
	(0.056)	(0.028)	(0.029)
ln(Age)	-0.098	-0.107	-0.329
	(0.969)	(0.428)	(0.417)
$\ln(Age)^2$	0.235	0.119	0.231
	(0.450)	(0.215)	(0.206)
Firm-Cohort Fes	Y	Y	Y
Industry-by-Year Fes	Y	Y	Y
State-Year-Cohort Fes	Y	Y	Y
Adj. R^2	0.117	0.066	0.105
Obs.	199,000	199,000	199,000

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX	R&D Wage	R&D Employees
Trade × IDD × Post	0.028	-0.053**	-0.062***
	(0.018)	(0.021)	(0.019)
Trade × NC × Post	-0.007	0.002	0.012
	(0.020)	(0.007)	(0.014)
ln(Age)	0.293	0.228	0.298
	(0.263)	(0.217)	(0.221)
$\ln(Age)^2$	-0.148	-0.062	-0.066
	(0.109)	(0.074)	(0.065)
Firm-Cohort FEs	Y	Y	Y
Industry-Year FEs	Y	Y	Y
State-Year-Cohort FEs	Y	Y	Y
Adj. R^2	0.021	0.312	0.848
Obs.	199,000	199,000	199,000

Panel C, Innovation Outputs

	Patents Applied	New Co. Sales	New Mkt. Sales
Trade × IDD × Post	-0.011	-0.054	-0.103
	(0.010)	(0.033)	(0.165)
Trade × NC × Post	0.003	-0.209	0.074
	(0.006)	(0.168)	(0.118)
ln(Age)	-0.044	-2.726	-1.787*
	(0.032)	(2.683)	(0.943)
$\ln(Age)^2$	0.009	1.417	0.793*
	(0.009)	(1.427)	(0.416)
Firm-Cohort FEs	Y	Y	Y
Industry-Year FEs	Y	Y	Y
State-Year-Cohort FEs	Y	Y	Y
Adj. R^2	0.004	0.030	0.018
Obs.	199,000	170,000	170,000

Table IA-10: Effects of Labor Mobility Restrictions on Neighboring States for Full Sample

Estimates from stacked, triple difference zero-inflated Poisson regressions examining the effects of court rulings on innovation for all firms. A firm's monetary innovation measures are in thousands of 2018 USD. Variables *IDD* and *NC* indicate a firm is located in a state that rejected the inevitable disclosure doctrine or had a pro-labor or pro-firm non-compete ruling, respectively. Variable *Trade* indicates trade secrets are important to a firm's business in the year prior to its cohort's treatment. Variable *Post* indicates an observation is in or after a cohort's treatment year. All specifications include a constant term and control for firm and year fixed effects with the Mundlak approach. Standard errors (in parentheses) are clustered by firm-cohort, industry, and state-year. Asterisks ***, **, and *, indicate 1%, 5% and 10%, significance, respectively. Panel A presents results for R&D spending. Panel B presents results for innovation spending on capital and labor. Panel C presents results for innovation outputs. Census disclosure #11964.

Panel A, R&D Spending

	R&D		Research		Development	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.075***	0.042	-0.740***	-0.041	-0.863***	0.091
	(0.244)	(0.144)	(0.242)	(0.148)	(0.243)	(0.169)
Trade × NC × Post	0.025	0.158	0.182	0.068	0.067	0.190
	(0.268)	(0.122)	(0.211)	(0.180)	(0.225)	(0.130)
Trade × IDD	-0.613***	-0.048	-0.588***	-0.193	-0.611***	0.032
Neighbor × Post	(0.165)	(0.397)	(0.207)	(0.391)	(0.191)	(0.495)
IDD × Post	0.435**	0.145	0.470**	0.162	0.449**	0.078
	(0.191)	(0.336)	(0.203)	(0.153)	(0.190)	(0.326)
NC × Post	-0.094	-0.328	-0.227	-0.249	-0.107	-0.336
	(0.151)	(0.241)	(0.168)	(0.224)	(0.146)	(0.267)
Trade × Post	0.512***	-1.14***	0.331	-0.319	0.306*	-1.071**
	(0.189)	(0.41)	(0.205)	(0.362)	(0.177)	(0.470)
Trade × IDD	-0.080	0.000	-0.201	-0.284	-0.093	-0.014
	(0.24)	(0.231)	(0.154)	(0.187)	(0.182)	(0.261)
Trade × NC	0.272*	0.444	0.273	1.356**	0.285	0.306
	(0.159)	(0.451)	(0.212)	(0.621)	(0.187)	(0.555)
Trade × IDD	0.564***	0.000	0.517**	-0.059	0.594***	0.024
Neighbor	(0.187)	(0.331)	(0.214)	(0.315)	(0.188)	(0.413)
IDD Neighbor ×	-0.229	0.419	-0.087	0.102	-0.251	0.338
Post	(0.161)	(0.376)	(0.171)	(0.335)	(0.163)	(0.376)
IDD	0.043	0.179	0.158	0.391*	0.045	0.190
	(0.133)	(0.259)	(0.132)	(0.204)	(0.126)	(0.293)
NC	-0.374**	-0.91**	-0.279	-0.918***	-0.399**	-1.042**
	(0.174)	(0.366)	(0.210)	(0.283)	(0.180)	(0.454)
IDD Neighbor	2.869***	0.204	1.871***	-0.016	2.653***	0.054
	(0.124)	(0.251)	(0.097)	(0.183)	(0.163)	(0.258)
Trade	1.067***	0.100	0.810***	0.087	0.994***	0.082
	(0.08)	(0.089)	(0.054)	(0.094)	(0.086)	(0.092)
Post	-1.572***	0.013	-0.913***	0.054	-1.423***	0.013
	(0.092)	(0.088)	(0.053)	(0.086)	(0.109)	(0.089)

ln(Age)	0.832	1.289	0.634*	0.804	1.149*	1.072
	(0.649)	(1.364)	(0.333)	(0.776)	(0.598)	(1.721)
$\ln(\text{Age})^2$	-0.858***	-0.035	-0.621***	-0.160	-0.838***	0.143
	(0.287)	(0.331)	(0.150)	(0.249)	(0.275)	(0.378)
$\ln(\overline{RealPay})$	0.097***	1.003***	0.133***	0.883***	0.106***	1.043***
	(0.027)	(0.054)	(0.018)	(0.058)	(0.035)	(0.053)
Pseudo-R^2	0.758		0.634		0.780	
Obs.	181,000		181,000		181,000	

Panel B, Innovation Spending on Capital and Labor

	R&D CAPX		R&D Wage		R&D Employees	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-0.788***	0.173	-1.039***	0.252**	-1.436***	0.143
	(0.201)	(0.522)	(0.229)	(0.119)	(0.131)	(0.115)
Trade × NC × Post	0.064	-0.203	-0.006	0.007	-0.082	0.030
	(0.218)	(0.202)	(0.247)	(0.078)	(0.269)	(0.225)
Trade × IDD	-0.522**	0.771**	-0.594***	-0.194	-0.721***	-0.026
Neighbor × Post	(0.227)	(0.392)	(0.172)	(0.407)	(0.197)	(0.318)
IDD × Post	0.411**	-0.083	0.479**	-0.093	0.931***	0.028
	(0.186)	(0.359)	(0.195)	(0.337)	(0.294)	(0.340)
NC × Post	-0.130	-0.027	-0.101	-0.226	-0.271	-0.187
	(0.160)	(0.186)	(0.157)	(0.233)	(0.197)	(0.227)
Trade × Post	0.492**	-0.935	0.475***	-1.125***	0.839***	-0.957***
	(0.193)	(0.585)	(0.170)	(0.421)	(0.128)	(0.369)
Trade × IDD	-0.160	-0.217	-0.072	0.079	0.049	-0.012
	(0.179)	(0.312)	(0.219)	(0.207)	(0.244)	(0.208)
Trade × NC	0.246	0.408	0.258	0.445	0.418**	0.140
	(0.215)	(0.305)	(0.166)	(0.467)	(0.171)	(0.383)
Trade × IDD	0.448**	-0.529	0.550***	0.089	0.907***	0.068
Neighbor	(0.218)	(0.353)	(0.186)	(0.348)	(0.258)	(0.292)
IDD Neighbor ×	-0.158	0.252	-0.239	0.541	-0.556**	0.316
Post	(0.156)	(0.489)	(0.165)	(0.404)	(0.275)	(0.374)
IDD	0.151	0.477	0.040	0.117	0.175	0.066
	(0.120)	(0.312)	(0.137)	(0.25)	(0.163)	(0.17)
NC	-0.348	-0.474	-0.364**	-0.916**	-0.561**	-0.516
	(0.214)	(0.343)	(0.176)	(0.399)	(0.239)	(0.321)
IDD Neighbor	2.051***	-0.155	2.815***	0.121	3.194***	0.025
	(0.071)	(0.240)	(0.117)	(0.236)	(0.086)	(0.149)
Trade	0.968***	0.098	1.054***	0.100	1.325***	0.046
	(0.063)	(0.139)	(0.080)	(0.081)	(0.082)	(0.074)
Post	-1.003***	-0.152	-1.526***	-0.003	-1.816***	0.096
	(0.060)	(0.138)	(0.087)	(0.071)	(0.105)	(0.068)
ln(Age)	0.175	-2.918*	0.860	0.458	1.164	0.670
	(0.486)	(1.625)	(0.647)	(1.578)	(0.904)	(0.921)
$\ln(\text{Age})^2$	-0.656***	0.290	-0.878***	0.203	-1.134***	0.097
	(0.222)	(0.557)	(0.290)	(0.380)	(0.363)	(0.243)
$\ln(\overline{RealPay})$	0.172***	0.905***	0.096***	0.986***		
	(0.019)	(0.044)	(0.027)	(0.051)		
$\ln(\overline{DomEmp})$					0.014	0.817***
					(0.018)	(0.048)
Pseudo-R^2	0.622		0.767		0.654	

Obs.	181,000	181,000	181,000

Panel C, Innovation Outputs

	Patents Applied		New Co. Sales		New Mkt. Sales	
	Extensive	Intensive	Extensive	Intensive	Extensive	Intensive
Trade × IDD × Post	-1.194***	-0.364	-0.319*	-0.170	-0.237	-0.978**
	(0.371)	(0.251)	(0.168)	(0.294)	(0.175)	(0.455)
Trade × NC × Post	-0.008	0.270	-0.322**	0.611	-0.148	0.351
	(0.269)	(0.246)	(0.160)	(0.855)	(0.192)	(0.402)
Trade × IDD	-0.579	-0.030	-0.662***	1.085*	-0.712***	-0.135
Neighbor × Post	(0.390)	(0.688)	(0.191)	(0.584)	(0.183)	(0.548)
IDD × Post	0.939***	0.837**	0.138	-0.074	-0.031	1.337***
	(0.343)	(0.358)	(0.155)	(0.333)	(0.198)	(0.510)
NC × Post	-0.145	-0.506	0.176	-0.790	0.082	-0.213
	(0.201)	(0.358)	(0.237)	(0.863)	(0.213)	(0.166)
Trade × Post	0.732**	0.094	-0.161	-0.463	-0.041	-0.102
	(0.364)	(0.405)	(0.170)	(0.441)	(0.135)	(0.428)
Trade × IDD	-0.063	-0.114	-0.068	-0.004	-0.119	-0.858***
	(0.246)	(0.324)	(0.156)	(0.248)	(0.186)	(0.258)
Trade × NC	0.394	0.185	0.332**	-0.549	0.358**	0.150
	(0.388)	(0.565)	(0.153)	(0.522)	(0.142)	(0.543)
Trade × IDD	0.596	0.206	0.471***	-1.135**	0.479***	-0.080
Neighbor	(0.409)	(0.595)	(0.165)	(0.552)	(0.143)	(0.450)
IDD Neighbor ×	-0.499	-0.704**	-0.099	-0.251	-0.010	-0.814*
Post	(0.348)	(0.308)	(0.124)	(0.333)	(0.144)	(0.471)
IDD	0.123	0.306	-0.043	0.289*	0.010	1.100***
	(0.189)	(0.353)	(0.218)	(0.151)	(0.197)	(0.177)
NC	-0.574	-0.501	-0.168	0.078	-0.238*	-0.001
	(0.408)	(0.594)	(0.150)	(0.254)	(0.126)	(0.299)
IDD Neighbor	2.766***	-0.032	1.694***	0.291	1.854***	0.347
	(0.088)	(0.185)	(0.079)	(0.292)	(0.088)	(0.268)
Trade	1.408***	-0.025	0.996***	0.008	1.009***	0.016
	(0.076)	(0.132)	(0.100)	(0.177)	(0.096)	(0.174)
Post	-1.462***	0.289**	-0.806***	-0.108	-0.872***	-0.095
	(0.075)	(0.119)	(0.060)	(0.173)	(0.062)	(0.153)
ln(Age)	-0.597	-0.177	1.329**	2.888	1.121**	3.225
	(0.551)	(1.196)	(0.615)	(2.516)	(0.507)	(2.637)
$\ln(\text{Age})^2$	-0.361*	0.059	-0.631**	-0.003	-0.674***	-0.306
	(0.217)	(0.349)	(0.256)	(0.616)	(0.217)	(0.558)
$\ln(\overline{RealPay})$			-0.005	1.071***	-0.008	1.098***
			(0.033)	(0.095)	(0.026)	(0.063)
$\ln(\overline{DomEmp})$	0.224***	0.760***				
	(0.044)	(0.065)				
Pseudo-R^2	0.539		0.757		0.753	

Obs.	181,000	158,000	158,000

Table IA-11: Logit Analysis of Firm Propensity to Move States Following Treatment
This table presents results from two logistic regressions: the first studies the odds of moving locations following treatment on the sample of firms in the IDD treatment samples, as well as all firms in the control samples corresponding to the IDD treatment years (2012 and 2013); the second studies the odds of moving locations following treatment on a sample that includes all firms located in non-compete-treated states, as well as all firms in the control samples corresponding to the non-compete treatment years. Independent variables include the trade secret dummy, its interaction with an indicator that the firm is in a treated state as of the treatment year (IDD or NC), and control variables measured over the pre-treatment period. We include state and cohort-year fixed effects. We double cluster standard errors by firm and state-by-cohort-year. Asterisks ***, ** and *, indicate significance at 1%, 5%, and 10%, respectively. Census disclosure #11964.

	Move	
Trade × IDD	-0.111	
	(0.590)	
Trade × NC		0.150
		(0.272)
Trade	0.049	-0.102
	(0.149)	(0.092)
ln(Age)	0.304	0.316
	(0.962)	(0.475)
$\ln(Age)^2$	-0.139	-0.139
	(0.193)	(0.092)
$\ln(\overline{RealPay})$	0.279***	0.307***
	(0.042)	(0.034)
Pseudo-R^2	0.058	0.076
Obs.	8,500	40,000