Labor Mobility, Trade Secrets, and Innovation

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Abstract

We examine whether restrictions on worker mobility stimulate firms to innovate by protecting trade secrets. With a broad sample of private and public U.S. firms, we find that when state courts increase labor mobility by weakening the inevitable disclosure doctrine, affected firms reliant on trade secrets reduce innovation relative to non-trade-secret reliant state peers. This difference in treatment response between trade-secret-reliant and non-reliant firms in affected states is larger than those in unaffected states. The effect is strongest for startups. We cannot detect innovation effects for rulings on non-compete agreements. Both types of rulings impact reliance on trade secrets.

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1. Introduction

An important policy rationale for court-enforced labor mobility restrictions imposed on workers by employers is to enhance the protection of trade secrets, which, in turn, is meant to incentivize firms to invest in innovation (Thomas 2014). There is an extensive literature on the impact of state judiciaries' adoption (or rejection) of labor mobility restrictions on various firm outcomes. These include the Inevitable Disclosure Doctrine (henceforth, "IDD") and non-compete agreements (henceforth, "non-competes"). The IDD allows firms to prevent their employees from accepting jobs at competitors or starting competing firms if a court determines there is significant risk that trade secrets would be disclosed as a result (Weisner 2012). Non-competes are contracts firms may require workers to sign that prohibit the worker from taking jobs at competitors or starting competing firms, and state judiciaries vary in their willingness to enforce such contracts. Many studies in these literatures, reviewed in Section 2, find that such labor mobility restrictions impact capital structure, capital expenditures, executive compensation, and other important outcomes at public firms. Other studies examine effects on aggregate measures of innovation activity at the state or county level, as well as the productivity of individual inventors.

As far as we are aware, however, we are the first to examine whether these mobility restrictions achieve their implicit policy goal of encouraging firms to invest in innovation by improving protection of trade secrets. Moreover, unlike prior studies on how mobility restrictions impact firm level outcomes, ours is not limited to public firms. We conduct our analyses on a large, nationally representative sample of innovative public and private U.S. firms of all sizes, including startups.

We address our research question using data from the Business, Research, Development and Innovation Survey (BRDIS), conducted annually by the U.S. Census Bureau and the National Science Foundation. In addition to asking firms to report detailed information on innovation inputs and outputs, the survey asks whether trade secrets are important to their business. We exploit answers to this question, along with credibly exogenous court rulings that change or significantly clarify legal precedent on labor mobility restrictions in the state, to identify whether these restrictions causally impact the innovation policy of trade secret dependent firms relative to non-trade secret dependent firms.

Using a stacked triple difference analysis of the effects of several court rulings staggered in time, we find that court rejections of the IDD cause trade secret dependent firms (henceforeth "trade secret firms"), to reduce investment in research and development by approximately 27% of pre-treatment total payroll, on average, relative to firms for whom trade secrets are unimportant (henceforth, "non-trade secret firms"). The effect is stronger for smaller firms and startups, but it is still significant for large firms, defined as those having at least \$200 million in real sales (2018 dollars) in at least one year of the three years before treatment. We also find significant negative effects on the fraction of employees devoted to R&D, labor-related R&D expenses, non-labor-related R&D expenses, and sales of products newly introduced to the market. We cannot detect any effects on patenting activity, however. Conversely, we do not find broad evidence that court rulings on the enforcement of non-competes affect innovation outcomes in either the full sample or for large firms.

However, we do find that rulings rendering non-competes easier to enforce have an intriguing effect on startups: relative to other startups, trade secret firms' real employment costs per R&D employee drops by approximately \$90,000 in 2018 dollars. This finding suggests that, for the purposes of protecting startups' trade secrets, the ability to enforce non-competes serves as an effective substitute for high pay. We cannot detect a similar effect in other kinds of firms, however.

We require relatively weak assumptions for our causal inferences to be valid. By comparing the changes in innovation policy of trade secret firms to that of non-trade secret firms within treated states, we rule out that our inferences are confounded by state-specific shocks. Specifically, the empirical framework reduces the likelihood that shocks coinciding with the court rulings, that differentially impact firms in treated and untreated states, are driving our results. Similarly, we can also rule out that our inferences are confounded by national shocks coincidental to the court rulings that differentially impact trade secret and non-trade secret firms. Specifically, by comparing the difference in innovation policy changes of trade secret and non-trade secret firms in treated states to that of the control group (firms in untreated states), we reduce the likelihood that our results are driven by nationwide shocks. Given our empirical strategy, we therefore must only make the reasonably weak assumption that there are no confounding shocks, coinciding with the court rulings, that differentially impact the innovation policy of trade secret and non-trade secret firms, *only in the treated states.*

As with any study attempting causal inference by utilizing a quasi-natural experiment, we cannot prove with certainty that our identifying assumption is satisfied. We do, however, have a strong argument for it. With a placebo test, we show that the trend in the difference between trade secret and non-trade secret firm innovation policy in treated states only begins to diverge from that of the control group precisely in the treatment year, not before. Hence, any hypothetical shocks invalidating our identifying assumption would have to come in precisely the same years as the court rulings we utilize as our exogenous shocks. Furthermore, courts base their rulings on technical legal reasoning, *not* economic conditions. Therefore, it is reasonably unlikely that the high court rulings we utilize are themselves caused by any state-specific economic shocks, unrelated to trade secret protection, that could explain the sudden divergence in innovation policy trends upon which we base our causal inferences. It is thus implausible that any confounding shocks that could generate our results can be attributable to anything but chance. The probability that our results are attributable to chance, however, can be estimated through standard methods of statistical inference.

We also conduct a simple differences-in-differences analysis to examine effects of labor mobility rulings on firms' intellectual property protection strategies. We find that firms located in states with rulings rejecting the IDD or making non-competes harder to enforce become less likely to report that trade secrets are important to their business after the rulings, relative to the control group. This effect, however, is not present in startups, and it is stronger in large firms. These results suggest that larger firms can find substitutes for trade secrets after their protection is weakened by pro-labor-mobility court rulings, whereas startups cannot. These findings potentially explain why we find that IDD rulings have particularly strong effects on startups.

Additional results provide suggestive evidence of the effects of two pieces of Federal legislation passed during our sample period related to intellectual property on innovation: the Defend Trade Secrets Act of 2016 (DTSA) and the America Invents Act of 2012 (AIA). We discuss each in turn.

The DTSA of 2016 allows firms to seek injunctions and damages for trade secret misappropriation under federal law; prior to the act's passage, trade secret protection was exclusively governed by state law (Flowers 2019). We run a differences-in-differences specification to test whether the DTSA had the effect of encouraging innovation for firms for whom trade secrets are important, relative to firms for whom trade secrets are not important. We find evidence in the affirmative for large firms, but we cannot detect any effects for the full sample or startups.

The America Invents Act (AIA) of 2012 had the stated purpose of lowering the risk of costly patent litigation that was allegedly discouraging innovation by firms and individuals with fewer resources (Driver 2023). Prior to 2012, patent holders could face legal challenges to their intellectual property rights by parties claiming to be the first to invent the patented innovation. Establishing who was the first to invent is not straightforward, potentially making the defense against even meritless challenges lengthy and costly. After the act's passage, a patent holder's property rights are secure so long as she was first to apply for the patent, a fact relatively easy to prove. In addition, the new law allegedly lowered litigation costs by having patent disputes adjudicated through arbitration rather than in federal court. By reducing risk of patents being challenged and by lowering litigation costs, it was thought that the new law would encourage innovation by people or entities, such as startups, that can less afford large legal bills. On the contrary, Driver (2023) finds that the act causes small firms dependent on patents to reduce investment in innovation relative to patent-dependent large firms relative to the control group.

Consistent with Driver (2023), we find that startups for whom trade-secrets are important decrease investment in R&D and other innovation inputs upon passage of the act in 2012, relative to startups for whom trade secrets are not important. We infer that the AIA had a particularly deleterious effect on innovation at startups that depend on trade secrets. Because the act did not change trade secrets law, these findings suggest that patents and trade secrets are complements at startups. We cannot detect any effects for large firms or in the full sample.

We also contribute by expanding the general literature on trade secrets, in addition to the literature focused on the IDD and non-competes. There is extensive research on patents but substantially less on trade secrets, which are the means most used by for-profit firms to protect their investments in innovation (Hall *et al.* 2014). The question of how trade secret protection impacts innovation at the firm level is therefore poorly understudied. Our results provide some of the first quantitative estimates of the impact on firm-level innovation of three specific trade secret protection policies: the IDD, non-competes, and the DTSA. Our secondary findings on the America Invents Act also provide insights on the complementary of trade secrets and patents, a topic on which there is little research.

This analysis also has 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 that trade secret protection encourages firms to innovate, and innovation is socially beneficial, restricting labor mobility also adversely impacts worker welfare (Rowe 2005). Therefore, the empirical evidence we provide on whether labor mobility restrictions encourage innovation through trade secret protection is helpful for informed policy making.

2. Literature review

Many scholarly studies in economics, finance, accounting, and management exploit court decisions on the IDD as exogenous shocks to test hypotheses on how labor mobility restrictions cause changes to various firm policies, often through an innovation channel. For example, Klassa *et al.* (2018) find effects on capital structure and labor retention; 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), 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; and, finally, Canil *et al.* (2023) and John *et al.* (2023) on payout policy.

Other studies find the IDD impacts firm-level stock market outcomes. Qiu and Wang (2018) find effects on stock price levels; Kim *et al.* (2021) on stock price synchronicity; Hu *et al.* (2023) and Li and Jian (2023) on crash risk; and, finally, Dai *et al.* (2023) on analyst forecast accuracy.

Nguyen *et al.* (2022) find that IDD rejections tend to decrease sales growth and the rate of new product introductions at smaller public firms. Overall, their evidence is consistent with ours in that they find negative effects of IDD rejection on innovation at smaller firms. However, unlike us, they do not distinguish between firms for whom trade secrets are important or not. Thus, the decreased innovation they find is not necessarily attributable to the loss of trade secret protects that come from IDD rejection. Labor mobility restrictions, such as the IDD, can impact innovation for reasons unrelated to trade secrets. 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 when courts are willing to invoke the IDD. Such loss of such incentives for within-firm cooperation, unrelated to trade secrets, that come from IDD rejection could plausibly the negative effect of such rejections on innovation that Nguyen *et al.* (2022) find. Moreover Nguyen *et al.* (2022) limit their attention to public firms. In contrast, we study a nationally representative sample that includes private firms, including startups.

Kannan *et al.* (2022) use a differences-in-differences analysis to study the effects of the IDD on capital expenditures and research and development (R&D) expenses at public firms. While they find an effect on capital expenditures, they fail to find an effect on R&D. Unlike us, however, they do not distinguish between trade secret dependent and non-trade secret dependent firms. Thus, their results cannot speak to the question of whether labor mobility restrictions encourage firm-level innovation by improving protection of trade secrets. They also limit their attention to public firms.

The literature on enforceability of non-competes is less vast. Jeffers (2023) finds that rulings making it *easier* to enforce non-competes *increase* net capital expenditures at the firm level, but she fails to find an effect for R&D. Like Kannan *et al.* (2022) and Nguyen *et al.* (2022), however, she cannot distinguish between trade secret-dependent and non-trade secret dependent firms. Therefore, her results do speak to our research question on whether labor mobility restrictions impact firm innovation policy through trade secrets. Her analysis is also limited to public firms. Further, our findings build off on the work of Jeffers (2023). Specifically, Jeffers (2023) argues that labor mobility ruling treatment effects on R&D expenses could be misleading because such rulings are likely to impact wages as well as employment. We are able to explicitly test the impact of these rulings on wages in our setting. Other studies include Kini *et al.* (2021), Lin *et al.* (2022), and Starr *et al.* (2018) who, respectively, find that non-compete enforceability impacts executive compensation, CEO turnover, and the propensity for employees to leave and form startups.

Several studies examine the effects of labor mobility restrictions rulings on innovation, but not at the firm level. Patel and Devaraj (2022) cannot detect evidence that IDD adoption significantly effects the rate of new business formation at the county level. Jeffers (2023) finds that 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. Because venture capital tends to fund innovation, their results are consistent with our inference that the IDD encourages innovation. However, because they do not distinguish between trade secret and non-trade secret firms, it is unclear whether their results are attributable to the greater trade secret protection provided by the IDD or other consequences of reduced labor mobility. Contigiani *et al.* (2018) find that IDD adoption 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. While these studies provide valuable insights into the effects of labor mobility restrictions on aggregate innovation, none speaks to our research question of whether the trade secret protection consequences of such restrictions that impact innovation at the firm level.

A nascent literature examines how policies that protect trade secrets directly affect to innovation and other firm outcomes. There is evidence that passage of the Uniform Trade Secrets Act by U.S. states reduces leverage and increases intangible investment (Guernsey *et al.* 2022); reduces knowledge spillovers (Wang 2023) and corporate transparency (Glaeser 2018); and finally, that it reduces patenting while it increases predatory, anti-competitive behavior (Bradley *et al.* 2023). Guernsey (2019) finds that prohibitions on reverse engineering of unpatented innovations increases investment and decrease patenting. We bolster this literature with new evidence on three additional measures intended to protect trade secrets: the IDD, non-competes, and the DTSA.

3. Changes to state law on labor mobility restrictions

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

3.1 The initiable 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 of disclosure of its trade secrets. The status of the IDD in a state is not determined by legislation, but by judges' interpretation of their state's trade secret law, which is either so version of the uniform trade secret act (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 law.[1]

Because the IDD reduces the odds that a former employee will disclose trade secrets to competitors, it plausibly reduces the odds that competition will erode a firm's returns on investment in innovations protected by trade secrets. Therefore, it is reasonable to hypothesize that the IDD encourages firm investment in innovation best protected by trade secrets. Because judges base their interpretations of

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

state law on technical 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 likely to impact firm innovation policy. These rulings, therefore, constitute quasi-random shocks for the purpose of testing our hypothesis that trade secret protections derived from labor mobility restrictions causally impact innovation by firms for whom trade secrets are important to their business.

We gather information on IDD court rulings during our sample period (2008-2018) from various sources. From the second half of 2008 through 2011,[2] we use the comprehensive list in Klassa *et al.* (2018), which is based on law review articles that include Weisner (2012), as well as legal treatises. According Klassa *et al.* (2018) and Weisner (2012), there are no rulings that change IDD precedent from the second half of 2008 through 2011.

To update the list of precedent-changing IDD rulings through 2014, we turn to Qiu and Wang (2018), who claim 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, where the two latter lists overlap chronologically, they are inconsistent: there are court rulings on the Flammer and Kacperczyk (2019) list that allegedly reject the IDD not on the Qiu and Wang (2018) list, and vice versa. There are also many rulings on the Flammer and Kacperczyk (2019) list that chronologically overlap with the Klassa *et al.* (2018) list but are not on the latter. We therefore scrutinize all rulings on the Qiu and Wang and Flammer and Kacperczyk lists that fall between the second half of 2008 and 2014. After reading commentary in the legal literature about these cases, as well as the court opinions themselves, we determine that a ruling rejecting the IDD included on both lists, from Massachusetts in 2012, is appropriate to use as an exogenous shock to IDD precedent. While it is unclear that the case reversed an IDD adoption, it brought legal clarity about the status of the IDD in the commonwealth, whose courts had previously applied it inconsistently (Weisner 2012). Qiu and Wang (2018) omit Georgia's IDD rejection in 2013, but it is on the Flammer and Kacperczyk (2019) list. We

[2] 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.

independently verify that it changes precedent for the state. We determine the remaining cases on the two lists after mid 2008 do not constitute true IDD rejections or adoptions, so we discard them. In Appendix A, we discuss our reasoning on which cases to retain and discard and the sources upon which it is based.

Kannan *et al.* (2022) include in their list some of the cases listed in Flammer and Kacperczyk (2019) that we conclude are not true IDD rejections. John *et al.* (2023) use the Flammer and Kacperczyk (2019) list through 2013, and they add the alleged 2014 North Carolina IDD rejection on the Qiu and Wang (2018) list that we determine does not change IDD status in the state. 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. Studies with sample periods ending in 2011 or earlier generally rely on Klassa *et al.* (2018).

To obtain all relevant IDD rulings after 2014, as well as ensure the two more recent lists did not omit any from the second half of 2008 through 2014, we examine all state supreme and appellate court rulings returned from a Lexis-Nexis full text search 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.[3] 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, leaving two IDD treatments: the rejections of Massachusetts in 2012 and Georgia in 2013.

3.2 Non-competes

In some states, courts will readily enforce almost any non-compete. In other states, such as in California, courts will not enforce any. Most states fall somewhere in between, allowing enforcement of

[3] The blogs are restrictivecovenantreport.com by Jackson Lewis; employmentlawspotlight.com by Baker Hostetler; tradesecretlaw.com by Seyfarth; and non-competes.com

non-competes only if they are written sufficiently narrowly 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 with the IDD, however, states also sometimes change non-compete enforceability through legislation, which is potentially endogenous to economic shocks that might impact innovation.[4]

We independently verify and use the list of court rulings and legislation that change non-compete enforcement over the latter half of 2008 through 2013 from Jeffers (2023). To obtain cases and legislation from 2014-2018, we follow Jeffers (2023) and consult professional legal blogs, discussed in the prior section. We also consult an up-to-date state-by-state summary on non-compete enforcement practices published by a prominent law firm.[5]

We find three precedent-changing cases after 2013: Kentucky in 2014, Pennsylvania in 2015, and Nevada in 2016. We also find four instances of legislation: Arkansas and Alabama in 2015; Nevada in 2017; and Utah and Idaho in 2018. We follow Jeffers in *not* utilizing legislative changes in our research design because of their endogenous nature. We classify changes effected by court rulings that make non-competes easier and harder to enforce as, respectively, "pro-firm" and "pro-labor." 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 do rejections of the IDD. We thus expect non-competes to have a weaker impact on innovation. Four of the ten rulings we consider merely change the requirement that employers must offer additional compensation for the non-compete to be enforceable. While these rulings plausibly affect firms' 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, "Gives courts the authority to either (1) strike unreasonable clauses from a noncompete agreement, leaving the rest to be enforced, or (2) actually

[4] See Jeffers (2023) for details on how non-competes work and how court rulings change their enforceability.
[5] https://beckreedriden.com/50-state-noncompete-chart-2/, accessed 12/31/2023.

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, so likely =increases implementation costs, albeit modestly. The remaining rulings are unique in how they change non-compete enforcement. In each case, however, it is hard to see how they would dramatically alter the ability or cost of utilizing non-competes to protect trade secrets. We therefore hypothesize modest effects.

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 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) over 2008-2018. The BRDIS is conducted annually by the National Science Foundation and the Census Bureau.[6] We delete observations for firms that report they are majority owned by another firm. We also delete observations based on survey forms BRD-FED, BRD-1S, or BRDI-1S because the question on the importance of trade secrets to the firm's business was not included in those forms. We also delete observations where the field indicating which form was being used is missing. Within this set of observations, wherever the value a continuous variable is missing in the BRDIS data, we assume it is zero.

We keep the following annual BRDIS variables measuring aspects of firm-funded (as opposed to government funded) domestic innovation inputs (which are also used in Driver *et al.* (2023)): research and development expenses combined (R&D), research only, development only, labor costs associated with R&D, non-labor-related R&D, expenditure on capital equipment used in R&D, and the count of

[6] For a sample BRDIS questionnaire, see https://www.nsf.gov/statistics/srvyindustry/about/brdis/surveys/srvybrdis-2014-BRDI-1.pdf

employees devoted to R&D. We focus on only domestic inputs because the state court rulings we study can impact domestic operations.

We keep several additional annual BRDIS variables. We keep the following R&D outputs: sales attributable to products or services new to the market in the year; sales attributable to products or services new to the firm but not the market; and the count of patents applied. 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 it is zero otherwise. We keep firm sales as our proxy for firm size. Finally, we keep the state the firm reports in its primary address and designate it as the firm's location for the year.

We then merge our BRDIS-based sample with the Longitudinal Business Database with Revenue (LBDREV) using internal Census firm identifiers specific to each year. From the LBDREV firm file we keep the total domestic employee count and total domestic payroll, which we use to scale some of our innovation input and output variables, as discussed below. We drop firm-year observations with less than three employees because we are not interested in cottage industry firms. We use the sales from the BRDIS rather than revenue from LBDREV to construct our size measure because the BRDIS variable is better populated. We also keep the age of the firm's oldest establishment as our measure of firm 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 our own longitudinally consistent firm identifiers in our merged 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. Treatments, listed in Table 1, are state appellate or supreme court decisions rejecting the Inevitable Disclosure Doctrine or changing the enforceability of non-competes. As it takes time for firms to adjust their R&D budgets to changes in the legal environment, we assume that court rulings coming down in the second half of the year only start to impact firm innovation the year after the ruling takes place. Thus, for rulings that occur after June 30, we designate the year after the ruling as the "treatment year." 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 high court ruling related to non-compete agreements in the second half of 2016, making 2017 the Nevada treatment, we exclude all Nevada firms from our sample because our measure of susceptibility to treatment is not available in 2017. As discussed further below, we define firms as susceptible to treatment if they report, in the year before the treatment year (2016 in this case), that trade secrets are very or somewhat important to their business. The BRDIS did not ask the trade secret question in 2016, so we cannot assess susceptibility to treatment in 2017.

In addition, we exclude observations from firms in the following states in the years indicated, and all years following, because the states adopted endogenous legislation on non-compete enforcement: 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 in 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, namely, each year from 2010 to 2016 inclusive. 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 (discussed in detail below), we also include in the treatment sample corresponding to a given treatment year all available BRDIS observations for these firms in the three years before and the three years after treatment. However, we drop observations for treated firms in the years before treatment for which the firm was in a state different from its treatment year state. For example, the 2014 treatment sample includes all observations in 2014 for firms located in Virginia in 2014. It also includes all available observations for these firms from 2011-2013 for which they report Virginia as their state; it does not include observations from this period where the firm is located in some other state. The treatment sample also includes all observations for these firms from 2015-2016, regardless of their state in those years, to allow for the possibility that treatment induces some firms to move.

Treated firms located in Illinois in 2012 or 2013 have different inclusion criteria for the 2012 and 2013 treatment samples 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 2009-2011 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 2012-2016, to ensure the 2012 treatment does not confound our inferences about the effect of the 2013 treatment.

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 three years prior to three years after the treatment year, for all firms in BRDIS that are never located in a treated state over our entire sample period (2008-2018).

Within each treatment or 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. If 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 three years. If the trade secret field is missing for all three pre-treatment years, we drop the firm.

If a firm is in a treated state as of the year of treatment, but it states that trade secrets are not important to its business in the year before treatment, we label it as treated but not susceptible to treatment. If the firm reports that trade secrets as somewhat or very important in the year before treatment, but the firm is not located in a treated state as of the year of treatment, we designate it as susceptible to treatment but not treated. Finally, if the firm is in a treated state as of the year of treatment and it says in the prior year that trade secrets are important to its business, we designate it as both treated and susceptible to treatment.

The master sample consists of the union of treatment and control samples for treatment years 2010 through 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 before the treatment year and at least one either in the treatment year or within the three years afterward. Otherwise, the firm-by-cohort fixed and state-by-year fixed effects we employ in regressions discussed below are perfectly co-linear with the interactions between our indicator variables for treated firm, susceptibility to treatment, and post-treatment indicator variables. Second, because our regressions include state-by-year fixed effects, there must be variation in susceptibility to treatment within the observations for each state/year combination to avoid perfect collinearity between various interactions of indicator variables employed in our regressions.

4.2 Variable Adjustments and Descriptive Statistics

Table 1 present all descriptive statistics. Panel A presents all continuous variables. which include our innovation input and output dependent variables, as well as real *Average R&D Wage,* firm age, real firm sales, real firm payroll, and total employee count. In Panel B we present statistics on the frequency with which our categorical variables take the value of 1.

The continuous innovation input and output variables described in Panel A are scaled to make them comparable across firms of different sizes. All innovation input variables measured in nominal dollars (*R&D, research, development, R&D CAPEX, R&D Wages, and Non-Wage R&D*), are scaled by the firm's average total domestic payroll in the three years prior to treatment. We also use the three-year average pre-treatment domestic payroll to scale sales from products new to the company (*NewProdCo*) and market the (*NewProdMkt*). R&D employee and patent application counts are scaled by the average count of domestic employees at the firm over the three years prior to treatment. Sales, total payroll, and per-worker R&D labor costs (*R&D Wages* divided by *R&D employee count*), are expressed in real 2018 dollars. We use the average annual consumer price index for all urban consumers, as published by the Bureau of Labor Statistics, to convert nominal dollars to real 2018 dollars.

Panel A reveals that the scaled continuous innovation variables take the value of zero for significant number of observations. However, R&D, development, labor-related R&D, R&D employee count have non-zero values for most observations, suggesting we can get reasonable power in linear regressions with these dependent variables. Other dependent variables, however, take the value of zero for most observations, suggesting that linear regressions utilizing them might have low power.[7]

Panel A also reveals a great deal of variation in firm size across our sample. The median observation has sales of approximately \$32 million in real 2018 dollars, whereas the 25th percentile is at approximately \$3.1 million. The largest 10% of firms have sales of over \$1.4 billion. Similar patterns are apparent in real payroll and employee count.

Panel B describes indicators for whether a firm is ever subjected to a given type of treatment (IDD rejection, pro-labor non-compete ruling, and pro-firm non-compete ruling), an indicator for whether the firm is susceptible to treatment *(Trade)*, and finally, an indicator for whether the observation in a given cohort comes after treatment year corresponding to the cohort (*Post*). Close to half our observations

[7] In future drafts, we plan to report results from zero-inflated Poisson models, which are better specified when the dependent variable is highly skewed and takes the value of zero for many observations.

correspond to firms that are susceptible to treatment, namely, they say trade secrets are important in the year before treatment. 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 treatment during our sample.

5. Research Design, sample construction, and results

In this section, first we discuss our research design for testing hypotheses related to how trade secret protections afforded by labor mobility restrictions impact firm innovation policy (Section 5.1). We then discuss the results of this analysis (Section 5.2). In Section 5.3, we conduct falsification tests of the parallel trends assumption that must hold for our causal inferences to be valid. In Section 5.4, we discuss and present results on our difference-in-differences analysis of how labor mobility restrictions change firm reliance on trade secrets. In Section 5.5, we report results of some robustness tests.

5.1 Research Design for Effects on Innovation Policy

We employ a stacked triple differences analysis. We define firms located in states that experience a non-compete or IDD court ruling as treated. We define trade secret dependent firms (as indicated on the BRDIS survey in the year prior to treatment) as susceptible to treatment. We use firms' response to the trade secret importance question as of the year before treatment because firms sometimes change their reliance on trade secrets in response to the court ruling. We first compare how innovation policy changes from before to after treatment for trade secret (susceptible) firms that are treated, to how it changes for non-trade secret (*not* susceptible) firms that are treated. Then we compare how this difference-in-differences in innovation policy, between trade secret and non-trade secret firms, differs in treated states from the analogous difference-in-differences in non-treated states. If we find that innovation changes significantly differently for trade secret firms relative to non-trade secret firms in treated states, compared to the analogous change in non-treated states, we infer that the court decisions have causal treatment effects through the channel of trade secret protection.

We do not simply examine how innovation policy changes for trade secret firms when they are treated. The high court decisions are almost surely not causally related to changes in business conditions in the state that could plausibly influence firm innovation policy. Court decisions are based on technical legal reasoning and *not* economic conditions. These decisions are therefore credibly exogenous, or “as-good-as-random,” for our purposes. Despite this, if we relied on the simple effect of treatment on trade secret firms, we could not rule out that these decisions are coincidental to state or national confounding economic shocks that make investment in innovation more or less favorable for trade secret firms. If we were to just examine pre-to-post treatment changes in innovation policy of trade secret firms that are treated, we would run the risk of erroneously attributing to state court decisions innovation policy changes that are really caused by economic shocks that only coincide with the court decisions by chance.

By comparing the changes in innovation policy of trade secret to non-trade secret firms within treated states, we can rule out the possibility that our inferences are being confounded by confounding economic shocks coincidental to court rulings that similarly impact the innovation policy of trade secret and non-trade secret firms within the treated states. Use of such a double difference research design, however, would not allow us to rule out the hypothesis that nation-wide shocks coincidental to court decisions, that differently impact innovation policy of trade secret and non-trade secret firms in all states, confound our inferences. The third difference in our triple difference analysis makes inferences robust to such national confounding shocks. That is, we compare the differences in the change in innovation policy response to the court decisions, between trade secret and non-trade secret treated firms, to the analogous differences between trade secret and non-trade secret firms in the control group. We thus eliminate from our treatment effect estimates the effects of 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 from our results, we must make the reasonable assumption that there are no economic shocks, purely coincidental to the quasi-random state court decisions, that differently impact the innovation policy of trade secret and non-trade secret firms *only in the treated states*. Moreover, because we control for industry-by-year fixed effects in all regressions, the degree to which these hypothetical confounding shocks differently impact trade secret and non-trade secret treated firms must be independent of time-varying industry effects. As with all studies utilizing a quasi-natural experiment for its research design, we cannot prove that our identifying assumption is satisfied. However, based on mainstream economic theory, it is difficult to imagine a sensible economic story under which it would be violated. In Section 5. 3 we provide some suggestive evidence that this assumption holds by showing that the differences in trends for trade secret and non-trade secret firms in treated states are similar to that of untreated states in the years leading up to treatment. We can thus rule out that our results are driven by confounding shocks that occurs close to, but not exactly in the same year as, the exogenous court decisions.

To implement the stacked triple differences analysis, we define treatment and control samples for each year there is a court decision (i.e., for each "treatment year") and "stack" them into a single master sample, as discussed in Section 4. We then run linear regressions of the form:

$$\begin{aligned} Y_{(i,k,t)} = a_{(i,k)} + b_{(1,1)}Trade_{(i,k)} * IDD_{(i,k)} * Post_{(k,t)} + b_{(1,2)}Trade_{(i,k)} * NCL_{(i,k)} * Post_{(k,t)} + \\ b_{(1,3)}Trade_{(i,k)} * NCF_{\mathrm{i,k}} * Post_{(k,t)} + \textstyle\sum_{k=2010}^{2016}\left(b_{(2,k)}Trade_{(i,k)} * Post_{(k,t)}\right) + \\ \Gamma'_1 SY_{(i,k,t)} + \Gamma'_2 IY_{(i,t)} + d_1 ln\left(age_{i,k,t}\right) + d_2 ln\left(age_{i,k,t}\right)^2 + e_{(i,k,t)} \end{aligned} \quad (1)$$

Where *k* identifies the year of the treatment (or "cohort") corresponding to the sample to which the observation belongs; *i* identifies the firm, while *t* identifies the year of the observation. Notice that, for the control group, the same firm-year case from the BRDIS data can appear multiple times in our master sample. $\mathrm{Y}_{(\mathrm{i,k,t})}$ is one of several innovation input or output variables, discussed in more detail below. $\mathrm{Trade}_{(\mathrm{i,k})}$ is an indicator variable that takes the value of one if firm *i* reported that trade secrets were important to its business in the year before treatment year *k,* and is zero otherwise. $Trade_{(i,k)}$ identifies whether firm *i* is susceptible to treatment in year *k.* $\mathrm{Post}_{(\mathrm{k,t})}$ is in an indicator variable that takes the value of one for all years t that come after treatment year k, and is zero otherwise. $IDD_{(i,k)}$ is an indicator variable identifying whether firm i of cohort k was located in a state where a high court rejected of the inevitable disclosure doctrine in year k. $NCL_{(i,k)}$ and $NCF_{(i,k)}$ are indicator variables identifying whether firm i of cohort k was located in a state in year k that, respectively, experienced pro-labor and pro-firm high court rulings on the enforceability of non-competes. Pro-labor rulings make non-competes harder to enforce, whereas pro-firm rulings make them easier to enforce.

We include in each regression firm-by-cohort fixed effects, $a_{(i,k)}$. We also include $SY_{(i,k,t)}$and $IY_{(i,t)}$, vectors of indicator variables for all possible state-by-year-by-cohort and industry-by-year combinations. Industries are defined by the firm's primary four-digit NAICS code, as listed on its BRDIS form, as of the year of treatment within the cohort. We do not report coefficients on these indicator vectors or the values of the firm-by-cohort fixed effects. The firm-by-cohort fixed effects ensure that omitted firm characteristics, time-invariant invariant within seven years around the cohort treatment year, do not confound our inferences. We include the vectors of indicators to ensure that our inferences are not confounded by any time-varying omitted variables that impact innovation policy of all firms in the same industry, or any time-varying omitted variables that impact all firms in the same state.[8] Control variables include the natural logarithm of the firm's age, as well as the square of this natural logarithm.

To ensure correlations in the residual terms do not bias our standard errors and confound inferences, we cluster standard errors in three dimensions: by firm, which makes our inferences robust residual correlation across observations for the same firm; by state-year combination, which ensures cross-sectional residual correlation across observations within the same state in a given year are not confounding our inferences; and finally, by four digit NAICS codes, which makes our standard errors robust to arbitrary residual correlation across observations within the same industry, both in the cross-section and in the time series. Note that for the purposes of standard error clustering we use the primary

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

four-digit NAICS industry indicated by the firm on the BRDIS survey in the current year, whereas for the purposes of creating industry-by-year indicator variables, we use the four digit NAICS code in the year before the cohort treatment year.

In the above regression, the coefficients of interest are $b_{(1,1)}$, $b_{(1,2)}$, and $b_{(1,3)}$, which, respectively measure the average relative treatment effects for IDD rejections, as well as pro-labor and pro-firm rulings on non-competition agreement enforcement.

5.2 Results for Effects on Innovation Policy

We first estimate equation (1) on the full sample where Y takes the value of the following innovation inputs: research and development (R&D), research, development, non-labor-related R&D expenses, and capital expenditures for R&D activities. In each case, the dollar value of the R&D input is scaled by the mean payroll of the firm in the three years prior to treatment. We scale by pre-treatment payroll, rather than by sales because pre-treatment sales are zero for some of the startups in our sample and we do not wish to arbitrarily exclude them. We do not scale by 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 US 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). The results are reported in Panel A of Table 3.

As reported column 1, rejection of the inevitable disclosure doctrine has large average treatment effect on R&D investment for trade-secret dependent firms: a reduction equal to 26% of pre-treatment payroll. Moreover, the effect is statistically significant at the 5% level. We also see negative and significant effects for both research and development when considered separately (columns 2 and 3), as well as on the non-labor-related component of R&D (column 4), which is equal to a reduction of 16% of pre-treatment payroll. Curiously, the effect on labor-related R&D expenses (column 5) is smaller in magnitude than the effect on non-labor related R&D (approximately 10.5% versus 16%). On the other hand, we do not find a significant average treatment effect on capital expenditures related to R&D activities (column 6). We also fail to find significant effects for rulings on non-competition agreement enforcement.

Jeffers (2023) argues that labor mobility ruling treatment effects on R&D expenses could be misleading because such rulings are likely to impact wages as well as employment. It is plausible that rejection of the IDD 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 without any real increase in R&D input. The richness of the BRDIS data allows us to overcome this problem because it allows us to observe the number of employees devoted to R&D activities. We thus estimate another version of equation (1) where Y is the number of employees devoted to R&D activities, scaled by the average employee count for the firm over the three years before treatment. The results in column 7 imply that IDD rejection has a real impact on R&D labor inputs, as the average treatment effect on trade secret dependent firms is to reduce R&D employees by approximately 10% of the firm's total employee count. Finally, notice that the effect on R&D labor related expenses per R&D employee (column 8) is statistically indistinguishable from zero. We infer that IDD rejection reduces real R&D labor inputs for trade secret dependent firms, but not does not significantly alter R&D employee wages for the full sample. Again, rulings on enforceability of non-competition agreements do not have statistically significant relative average treatment effects on innovation inputs for trade secret dependent firms.

We also consider relative average treatment effects on trade secret dependent firms for the following innovation outputs: the number of patents, scaled by average total employees in the three years before treatment, as well as sales from products new to the company and sales from products new to the market, both scaled by average pre-treatment payroll. As can be seen, we cannot detect an effect of IDD rejection on patents or on products new to the company but not the market. However, we find a negative average treatment effect of IDD rejection on sales new to the market for trade-secret-dependent firms equal to approximately 27% of pre-treatment payroll. This effect is economically large, but it is only marginally significant at the 10% level.

We also find a marginally significant positive effect of pro-firm non-competition agreement rules on sales from products that are new to the firm but not the market. However, given that we find no other significant effects of these types or rulings, we caution the reader from drawing strong inferences from one marginally significant parameter estimate given that these rulings do not have any other significant effects for our full sample.

We next consider how the average treatment effects might differ for large firms and startups who depend on trade secrets. We define large firms as those that have at least one year of pre-treatment sales equal to $200 million, in real 2018 dollars. We use $200 million because it is approximately equal to the 75 percentile in sales for the full sample in Table 2. We define startups as all firms that are five years old or younger at treatment and do not fit our definition of "large."

The results for large trade-secret dependent firms are in Table 3, Panel B. The direction of the average treatment effects of IDD rejections for large firms are in the same direction as that of the full sample, but they are smaller in magnitude. IDD rejections reduce total R&D by only approximately 10% of pre-treatment payroll for large firms, compared to 26% for all firms. Still, the effect is statistically significant, now at the 1% level (as opposed to the 5% level for all firms). Effects on other outcome variables are also statistically significant but of a muted magnitude. One qualitative difference is that IDD rejections do not have a statistically significant effect on new product sales for large firms. We infer that IDD rejections have negative effect on innovation inputs for large trade secret dependent firms, but we cannot detect an effect on outputs.

We have only one statistically significant effect for non-compete-agreement court rulings a negative effect of approximately $39,000 on the average labor costs for R&D employees after a pro-labor ruling. This finding could be explained by the best paid employees of trade-secret-dependent large firms departing after a pro-labor non-compete agreement ruling, but we cannot rule out other plausible interpretations.

In Table 3, Panel C, we have results for startups. The effects of IDD rulings on innovation inputs and outputs for trade-secret dependent startups are much stronger than for the full sample and for large firms. The R&D reduction is over 2.5 times pre-treatment payroll. Combined with columns 4 and 5, these results suggest that the vast majority of cuts in R&D spending for trade-secret-dependent startups caused by IDD rejection is in the form of non-labor expenses. Consistent with our results for the full sample, we fail to find effects on patents. Note that we are unable to report the sign, estimate, or significance (including standard errors) for some of the coefficient estimates that did not pass Census disclosure review (denoted using a "D" in the Table 3, Panel C) due to concerns that these could be used to identify specific restricted data. Thus, we are unable to report the coefficient estimate on new product sales in this subsample.

In general, non-compete agreement enforceability rulings do not appear to have detectable treatment effects on trade-secret dependent startups, except one: pro-firm rulings appear to reduce total costs of employing the average R&D worker by around $95,000 (in real 2018 dollars). This finding is consistent with the proposition that, when non-compete agreements are harder to enforce, startups must give highly productive R&D employees higher pay to retain them. However, there are other potential explanations for this finding, so we do not emphasize it.

5.3 Testing for parallel trends before treatment

The identifying assumption behind our triple difference research design is that, without the state court rulings in question, the trend in the difference between trade secret and non-trade secret firm innovation policy in treated states would have remained parallel to the trend in the difference between trade secret and non-trade secret firms in untreated states. Our finding of a negative 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 it was than in non-treated states.

To test for a more negative pre-treatment trend in treated states relative to untreated states, we estimate specifications similar to that of equation (1), except we add additional triple interaction terms for the IDD treatment and susceptibility to treatment dummies:

$$\begin{gathered} Y_{(i,k,t)} = a_{(i,k)} + \\ \sum_{j=-2}^{2}\left(b_{(1,j)}Trade_{(i,k)} * IDD_{(i,k)} * Post_{(k,t+j)}\right) + \\ +b_{(1,3)}Trade_{(i,k)} * NCL_{(i,k)} * Post_{(k,t)} + b_{(1,4)}Trade_{(i,k)} * NCF_{\mathrm{i,k}} * Post_{(k,t)} \\ \sum_{k=2010}^{2016}\left(b_{(2,k)}Trade_{(i,k)} * Post_{(k,t)}\right) + \Gamma'_1 SY_{(i,k,t)} + \\ \Gamma'_2 IY_{(i,t)} + d_1 ln\left(age_{i,k,t}\right) + d_2 ln\left(age_{i,k,t}\right)^2 + e_{(i,k,t)} \end{gathered} \qquad (2)$$

Where $Post_{(k,t+j)}$ is an indicator variable that takes the value of 1 for all observations beginning *j* years relative to the treatment year in cohort *k,* for values of $j \in \{-2,-1,0,1,2\}$. Intuitively, our new specification now has five triple interaction terms of IDD rejection: one with the post-treatment dummy that takes the value of one for all years starting two years before IDD rejection, and is zero otherwise; another with the post-treatment dummy taking the value of one starting one year before IDD rejection, and so on, with the last triple interaction term being with the post-treatment dummy taking the value of one starting two years after IDD rejection. We only run these tests for IDD rejection because we generally find the effects of non-compete rulings to be statistically indistinguishable from zero.

The coefficient on each of these new triple interaction terms parameterizes the degree to which the trend in the difference between trade secret and non-trade secret firm innovation policy shifts in states that reject the IDD, j years from the IDD ruling, relative to how the analogous trend shifts in untreated states in that year. If the difference in innovation policy between firms in treated states begins trending more negatively than it does in untreated states before treatment, we expect some of the coefficients on triple interaction terms for $j < 0$ to be significantly less than zero. On the other hand, if the pre-treatment trends are parallel, we expect these coefficients to be statistically indistinguishable from zero.

We run the specification in equation (2) for all dependent variables for which we estimated equation (1) above. We then plot point estimates and 95% confidence intervals for the coefficients on the seven triple interaction terms with different values j in Figures 1. As can be seen, for all $j < 0$, the coefficients are statistically indistinguishable from zero at the 5% level for the following dependent variables: domestic R&D, research, development, non-labor-related R&D, sales from product new to the firm but not the market, R&D capital expenditures, and patent applications. Thus for these variables, we cannot detect any differences in pre-treatment trends for treated and untreated states, consistent with our identifying assumption.

On the other hand, the triple interaction coefficient is positive and significant for $j = -1$ for the count of R&D employees, total R&D labor costs, real average R&D labor costs per R&D employee, and sales of products new to the market. These findings suggest the differences in these outcomes variables, between susceptible and unsusceptible firms, were trending more positively in treated states than in non-treated states in the year before treatment, not strictly inconsistent with our identifying assumption. Recall, however, that wherever we find significant average treatment effects in Table 2 for IDD rejections, they are negative. The differences in pre-trends we uncover, however, are all positive, and they of small absolute magnitude compared to that of the average treatment effects. The small positive differences in pre-trends between treated and untreated states that we find in Figure 1 for some variables, therefore, cannot be causing us to spuriously infer there is a negative average treatment effect for that same variable. If anything, these differences suggest our estimates of negative treatment effects are biased toward zero.

5.4 Treatment effects on trade secret reliance

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 can) will change their business strategy to rely on trade secrets less. We therefore hypothesize that firms become less likely to report trade secrets are important to their business after the inevitable disclosure doctrine is rejected, or after a pro-labor ruling on the enforcement of non-competition agreements. For similar reasons, we would expect them to become more reliant on trade secrets after pro-firm rulings on non-competition agreements.

We test these hypotheses using a stacked differences-in-differences (double difference) analysis, using the stacked sample of treated and untreated firms that we use for our triple difference analysis discussed in Section 5.1. To that end, we estimate conditional logit regressions of the form:

$$E_{i,k,t}\left(Trade_{i,k,t}\right) = \Lambda\left(\alpha_{i,k} + \delta_t + \beta_1 IDD_{i,k} * Post_{k,t} + \beta_2 NCF_{i,k} * Post_{k,t} + \beta_3 NCL_{,ki} * Post_{k,t} + \beta_4 ln\left(age_{i,k,t}\right) + \beta_5 ln\left(age_{i,k,t}\right)^2\right) \quad (3)$$

Where Λ is the logistic cumulative distribution function and all covariates are defined as before. If firms change their dependence on trade secrets when courts change their ability to stop employees from moving to competitors, we expect $\beta_1 < 0$, $\beta_3 < 0$ and $\beta_2 > 0$. 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, the subsample of large firms, and on the subsample of startups. In Table 4, we present the numerical results for the full sample and large firm subsample. Firms become significantly less likely to report that trade secrets are important to their business after IDD rejection and after pro-labor non-competes rulings. This result implies that firms adapt to IDD and pro-labor non-compete rulings by reducing their reliance on trade secrets. The effects are also of greater magnitude for later firms. On the other hand, the effects of pro-firm non-compete rulings are not statistically significant for zero.

In untabulated results, we also find the effects for startups are statistically indistinguishable from zero for all types of rulings.[9] We thus also infer that startups are less able to adapt to pro-labor rulings than large firms.

The results for the full sample and large firm sample are also economically meaningful. For the full sample, the exponentiated coefficient on *IDD*Post* is 0.71, which implies that IDD reduction reduced the odds that a firm depends on trade secrets by approximately 29%. From Table 1, Panel B, trade secrets are declared to be important for a firm's business for around half of the observations. This implies the unconditional probability of trade secrets being important is approximately 50%, which, is equivalent to odds of one-too-one. Hence IDD rejection reduces the odds of trade secret reliance for the average firm from one-to-one to 0.71-to-one, or from a probability of 0.50 to approximately 41.6% [(0.416 = 0.71/(1+0.71)]. The effect for large firms is even more dramatic.

5.5. Robustness Tests

Two states adopted the uniform trade secrets act (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 run a new set of regressions. These are identical to the ones discussed in Section 5.3, and which produced the results in Tables 2-4, except we add additional indicator variables as covariates: *Trade*TX*Post2013*, which takes the value of one if the firm was in Texas as of 2013, said trade secrets were important to its business as of 2012, and the observation is for the year 2012 or later; and *Trade*NJ*Post2012,* which takes the value of one if the firm was in New Jersey as of 2012, said trade secrets were important to its business as of 2011, and the observation is for the year 2012 or later. Estimates of the coefficients of interest (*Trade*IDD*Post*, *Trade*NCL*Post*, *Trade*NCF*Post*, *Trade*Post2012,* and *Trade*Post2016*) from these new specifications have the same sign and are not

[9] The Census Bureau has only authorized qualitative disclosure of the startup results.

statistically different from the analogous non-suppressed estimates in Table 3 (Census disclosure #11087).[10]

To ensure our inferences from the conditional logit regressions of trade secret dependence are also robust to controlling for the New Jersey and Texas adoptions of the UTSA, we run additional regressions that are identical to those discussed in section 5.4, except we add two additional indicator variables as covariates: *NJ*Post2012,* which takes the value of one if the firm was in New Jersey as of 2012 and the observation comes in 2012 or later; and *TX*Post2013,* which takes the value of 1 if the firm was in Texas as of 2013 and the observation is for 2013 or later; and both are zero otherwise.

The numerical results from the expanded conditional logit specification for the full sample and large firm sample are in Table 5. The effects of pro-labor non-compete agreements and the IDD on firm trade secret reliance are robust to controlling for the Texas and New Jersey adoptions of the UTSA. For the startup subsample, untabulated results indicate the effects of all types of court rulings continue to be statistically indistinguishable from zero.

However, for the full sample, when we control for state UTSA adoption, pro-firm non-compete rulings on trade secret reliance become negative and significant, albeit of smaller magnitude than the effects of the IDD and pro-labor non-compete rulings. This finding constitutes suggestive evidence that the effects of pro-firm and pro-labor non-compete rulings are asymmetric. However, we hesitate to draw strong inferences from this result, as that we cannot detect a similar effect in the two subsamples, and asymmetric effects are hard to rationalize with mainstream economic theory.

6. Conclusion

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[10] The Census Bureau has only authorized qualitative disclosure of the sign and significance of the results of this robustness test (CBDRB-FY24-P2316-R11141).

There is a vast scholarly literature in economics, finance, and other business disciplines on the effects of court-imposed mobility restrictions on workers. In addition, there is significant research studying the impacts of these restrictions 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 has the effect of stimulating innovation by firms for whom trade secrets are important. We also find evidence that firms substitute away from use of trade secrets when faced with court rulings weakening trade secret protection. While we cannot infer form our results whether labor mobility restrictions are a social good, on net, we do provide evidence that they provide a social benefit. Our evidence may thus prove to be useful to policy makers in making decisions about whether such mobility restrictions ought to be enforceable.

Appendix A

1: Court rulings after 2008 that we use as exogenous rejections of the IDD

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 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 Klassa *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 Klassa et al and Qiu and Wang. 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.[11] For

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

this reason, some scholars and practitioners argue that the decision did not change IDD precedent, but 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 US 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.

<u>Georgia</u>: 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)

Taking Van Dyke at face value would imply the case changed precedent because the Georgia Supreme Court had previously applied the IDD in substance, if not in name, starting with the *Essex v. Southwire* decision in 1998 (Weisner 2012).

Malsberger (2022), however, disagrees with Van Dyke (2013) and asserts that the Holton decision, rather than definitively rejecting the IDD, merely made its status ambiguous. Nevertheless, even if Malsberger is correct and Van Dyke is wrong (a matter upon which we are agnostic), 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. 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 is reasonable to hypothesize it had significant effects.

2. Court decisions that Qiu and Wang (2018) or Flammer and Kacperczyk (2019) incorrectly claim caused states to reject the IDD

State	Year	Case	Reasoning	Source(s)
Qiu and Wang (2018)				
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 the court does not have precedent-setting authority. Malsberger does not mention this ruling in his discussion of North Carolina IDD case law. Contemporaneous legal commentary reports that, in the same year, two NC **appellate** courts, which **do** have precedent-setting authority, issued rulings *affirming* the IDD. One of those these IDD-affirming appellate court rulings came after this one.	Malsberger (2022) and https://www.jdsupra.com/legal disclosure-doctrine-26009/; accessed 1/2/2024, as well the court opinion available on Lexis Nexis.
Flammer and Kacperczyk (2019)				
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 Klassa *et al.* (2018), whose list is current through 2011.
Arkansas	2009	Cellco Partnership v. Langston	Weisner reports that the IDD still applied in Arkansas as of 2012. Malsberger 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 Klassa *et al.* (2018), whose list is current through 2011.
New York	2009	American Airlines, Inc. v. Imhof U.S. Dist. LEXIS 46750	Weisner reports that different New York courts have different opinions on the IDD as of 2012 and he does not mention this case. Malsberger reports, "Courts applying New York law generally disfavor the doctrine of inevitable disclosure" (p.62 in the NY chapter). The first case he cites as disfavoring the IDD	Weisner (2012) and Malsberger (2022). Also omitted from Klassa *et al.* (2018), whose list is current through 2011. From the court's opinion available on Lexis-Nexis: "American has not demonstrated a likelihood of success on its inevitable disclosure theory…because

			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.	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 Malsberger' 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). Court opinion available on Lexis-Nexis. Also omitted from Klassa *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 that California and not New Hampshire Law applied to this case. Hence the case is irrelevant to IDD applicability under New Hampshire law. The case is not mentioned in Malsberger discussion of the case law, which shows continual application of the IDD by New Hampshire courts since 2005.	Malsberger (2022). Court opinion available on Lexis Nexis. Also omitted from Klassa *et al.* (2018), whose list is current through 2011.
New Jersey	2012	SCS Healthcare Marketing, LLC v. Allergan USA, Inc., N.J. Super.	The court only clarified that New Jersey's recently passed version of the UTSA does not preempt the state's common law on trade secrets. Case is of no relevance to the IDD. The case is not mentioned in Malsberger's discussion of the case law, which shows continual application of the IDD by New Jersey courts since 2005.	https://www.wilentz.com/about/publications/2013-01-14-in-the-absence-of-conflict-the-new-jersey-trade-secrets-act-does-not-preempt-new-jersey-common-law/_res/id=Attachments/index=0/new-jersey-trade-secret-act-does-not-preempt-new-jersey-common-law.pdf. Accessed 1/2/2024. Malsberger (2022
Washington	2012	Amazon.com, Inc. v. Powers , Case No. C12-1911RAJ	The court dismissed the case on the grounds the plaintiff did not have sufficient evidence for an IDD injunction. The court *explicitly denies* that it is ruling on whether the IDD can be applied in Washington. Malsberger 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.	Malsberger (2022) and Weisner (2012). Also https://casetext.com/case/amazoncom-1 Accessed 1/24/2024. Paragraph 12: "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.**"

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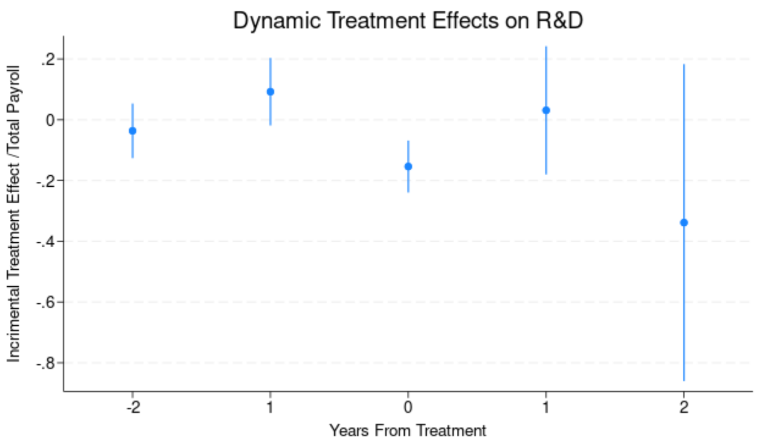
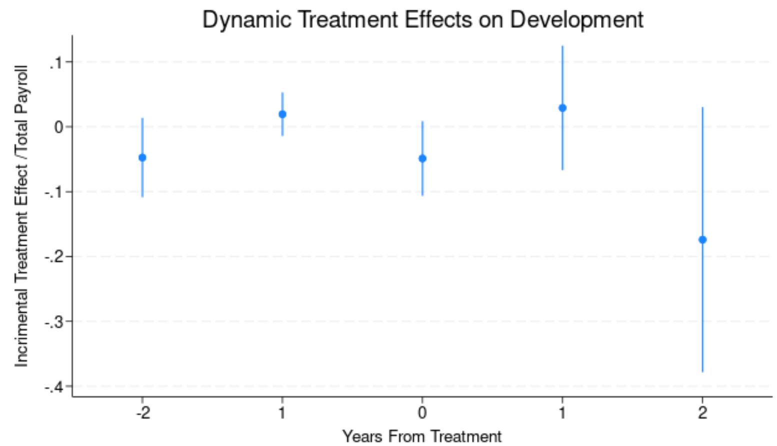
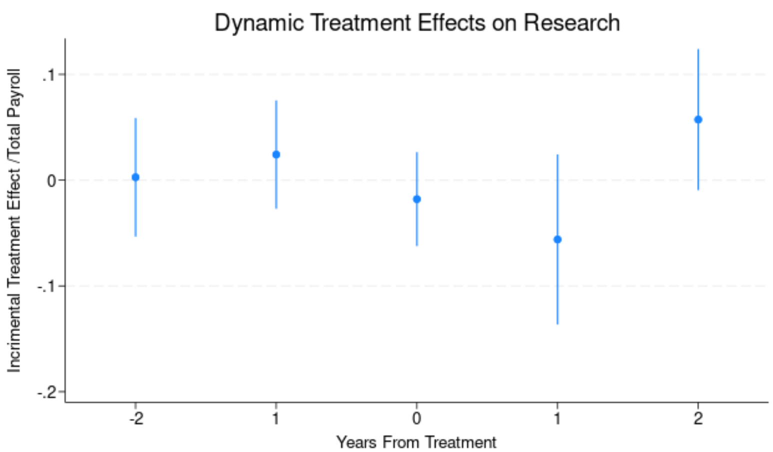
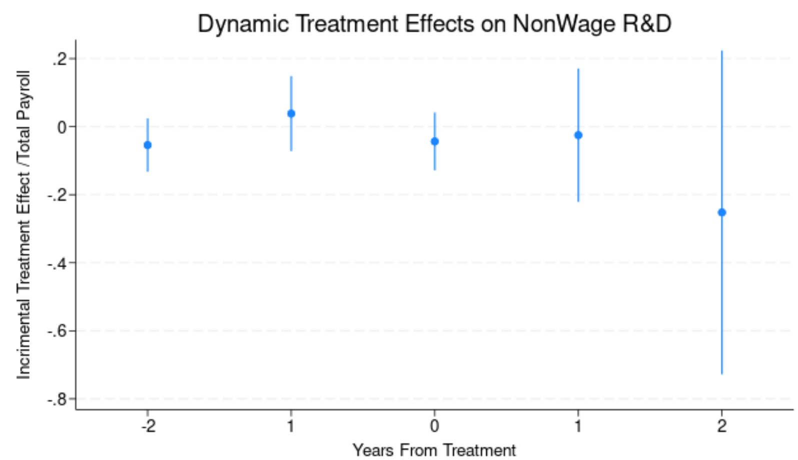
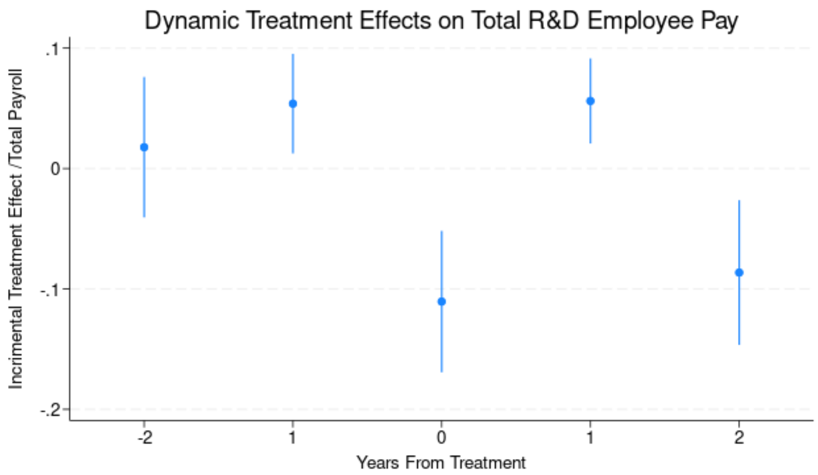
Figures: placebo tests of parallel trends before IDD adoption







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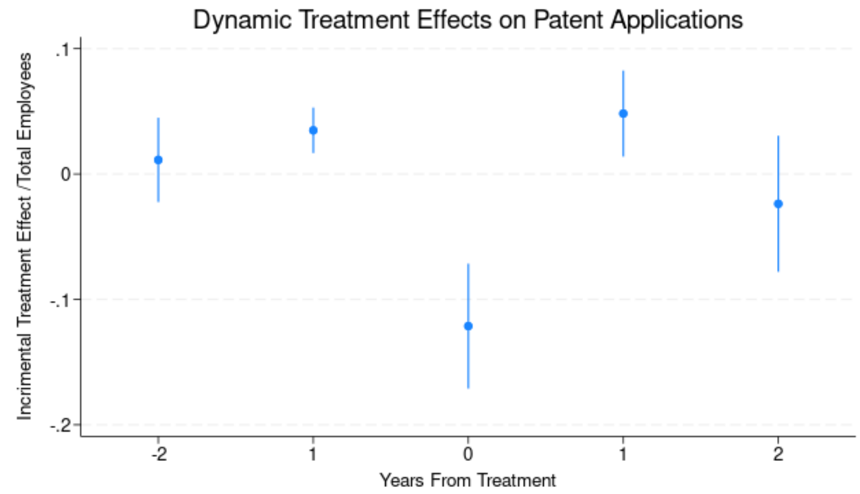
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Years From Treatment

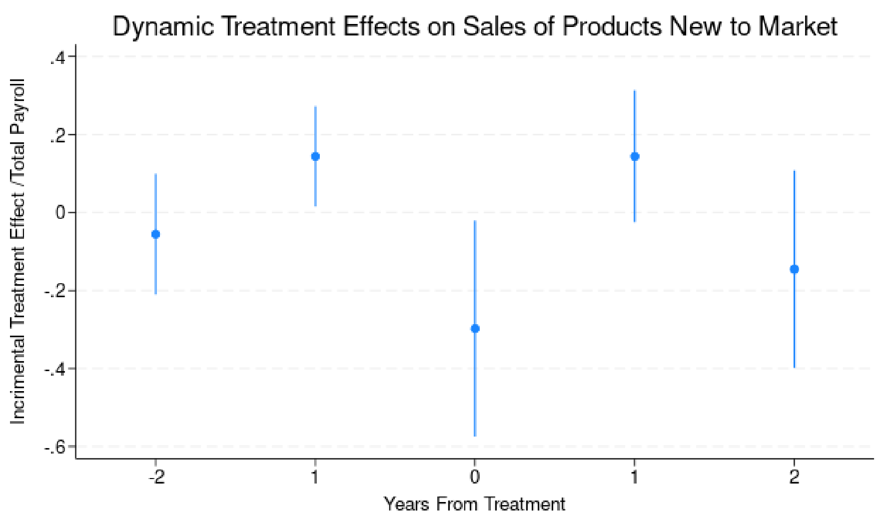
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Table 1: Court Rulings

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

Sate	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	NFL	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 (N=296,000; Census disclosure clearance #11087)

Panel A: Continuous Variables

Continuous variable mean, standard deviation, and Census-approved pseudo-percentiles. All monetary measures of innovation are scaled by average payroll three years prior to the treatment year for the cohort. Physical count measures of innovation are scaled by average employee count three years prior to the treatment year. Mean Real R&D Wage is in real 2018 dollars. Sales and Payroll are in thousands of real 2018 dollars. NewProdCo and NewProdMkt are sales from products new to the company and market, scaled by the firm's average payroll in the three years before the cohort treatment year.

	Mean	Stdev	p10	p25	p50	p75	p90
R&D	0.334	3.662	-	-	0.018	0.250	0.733
Research	0.075	1.306	-	-	-	0.016	0.118
Development	0.203	2.714	-	-	0.003	0.171	0.513
Non-wage R&D	0.165	2.668	-	-	-	0.050	0.269
R&D CAPX	0.036	1.088	-	-	-	0.006	0.039
NewProdMKt	0.280	7.936	-	-	-	-	0.481
NewProdCo	0.316	13.140	-	-	-	-	0.504
R&D Wages	0.169	1.829	-	-	0.001	0.170	0.475
Mean Real R&D Wage	65.65	344.90	-	-	0.44	102.80	169.40
R&D Employees	0.180	0.573	-	-	0.020	0.200	0.595
PatentsApplied	0.027	0.461	-	-	-	0.000	0.036
Firm Age (years)	25.54	11.59	9	16	27	36	39
Real Sales	1,438,000	10,150,000	0	3,171	32,870	201,700	1,402,000
Real Payroll	185,100	1,038,000	1,788	4,775	13,970	53,400	259,300
Employees	2,354	15,580	21	57	178	678	3,067

Panel B: Indicator Variable Frequency

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

	IDD	NCL	NCF	Trade	Post
Frequency	0.031	0.076	0.071	0.463	0.544

Table 3: Effects of Labor Mobility Restrictions for full sample

Results from stacked triple-difference OLS regressions examining the effect of labor mobility court rulings on innovation measures. Monetary innovation measures (except mean real R&D wage) are scaled by average payroll over the three years before the cohort treatment year. Count innovations measures are scaled by average employee count over the three years before the cohort treatment year. *IDD, NCL and NCF* indicate the firm is located in a state that rejected the inevitable disclosure doctrine and had a pro-labor or pro-firm non-compete ruling. *Trade* indicates trade secrets are important to the business. *Post* indicates the observation comes in or after the cohort treatment year; *PostN* indicates the observation comes on or after year N. Standard errors (in parenthesis) are clustered by firm-cohort, industry, and state- year groups. ***, ***, and * indicate 1%, 5% and 10% significance. Panel A presents results for the full sample. Panel B for the sample of large firms, defined those with at least one pre-treatment year with real sales over $200 million. Panel C is for startups: firms 5 years that are not large. Estimates that did not pass Census disclosure review are unable to be reported and are denoted with a "D". Census disclosure clearance #11087.

Panel A: Full Sample

	R&D	Research	Development	Non-Wage R&D	R&D Wages	R&D CAPX	R&D Employees	Mean Real R&D Wage	Patents	New to Co Sales	New to Mkt Sales
Trade*IDD*Post	-0.265**	-0.028***	-0.119*	-0.160*	-0.105***	0.031	-0.103***	-22.03	-0.014	0.122	-0.272*
	(0.113)	(0.009)	(0.063)	(0.086)	(0.037)	(0.025)	(0.024)	(16.67)	(0.011)	(0.157)	(0.149)
Trade*NCL*Post	-1.391	-0.033	-1.320	-0.553	-0.838	0.016	-0.035	-1.80	-0.036	0.157	0.052
	(1.259)	(0.032)	(1.243)	(0.484)	(0.776)	(0.030)	(0.037)	(12.13)	(0.022)	(0.223)	(0.075)
Trade*NCF*Post	0.015	-0.006	0.022	0.014	0.000	0.025	-0.006	0.96	0.060	0.159*	-0.091
	(0.056)	(0.022)	(0.046)	(0.043)	(0.025)	(0.021)	(0.020)	(7.86)	(0.060)	(0.092)	(0.132)
Trade*Post2010	-0.059***	-0.022**	-0.030**	-0.067***	0.008	-0.049*	-0.028***	6.52	-0.007*	0.095	0.017
	(0.021)	(0.010)	(0.013)	(0.020)	(0.020)	(0.027)	(0.008)	(11.86)	(0.004)	(0.128)	(0.095)
Trade*Post2011	-0.108	-0.050	-0.056	-0.089	-0.019	-0.025**	-0.017	-20.13	-0.003	-0.057	-0.236**
	(0.074)	(0.036)	(0.038)	(0.074)	(0.012)	(0.013)	(0.011)	(13.83)	(0.002)	(0.243)	(0.103)
Trade*Post2012	-0.098	-0.025	-0.057	-0.019	-0.079***	-0.009	-0.046*	-15.08**	-0.012**	-0.058	0.201
	(0.076)	(0.026)	(0.035)	(0.053)	(0.030)	(0.021)	(0.025)	(7.13)	(0.005)	(0.121)	(0.174)
Trade*Post2013	0.012	0.000	0.029	0.000	0.012	-0.007	-0.010	-9.99	0.001	-0.681	-0.250
	(0.051)	(0.021)	(0.024)	(0.025)	(0.031)	(0.010)	(0.013)	(6.57)	(0.003)	(0.633)	(0.181)
Trade*Post2014	0.011	-0.002	-0.017	0.019	-0.008	-0.042	-0.015	1.09	-0.022*	0.173	-0.052
	(0.050)	(0.011)	(0.030)	(0.034)	(0.022)	(0.031)	(0.013)	(7.14)	(0.012)	(0.243)	(0.061)
Trade*Post2015	-0.098*	-0.004	-0.078**	-0.044	-0.054**	-0.025	-0.042*	-7.34	-0.018	-0.071	-0.124***
	(0.051)	(0.011)	(0.038)	(0.027)	(0.026)	(0.018)	(0.022)	(8.37)	(0.015)	(0.099)	(0.039)
Trade*Post2016	0.113	0.022	0.066	0.080	0.033	0.000	0.030	-1.79	-0.005	-0.093	-0.093*
	(0.081)	(0.022)	(0.047)	(0.054)	(0.028)	(0.012)	(0.022)	(6.18)	(0.024)	(0.085)	(0.049)
ln(age)	0.363	0.017	0.024	0.150	0.212	0.279	0.333	-71.78**	-0.096	-3.176	-0.634
	(0.652)	(0.245)	(0.279)	(0.509)	(0.200)	(0.261)	(0.215)	(33.76)	(0.061)	(2.422)	(0.616)
$\ln(age)^2$	0.026	0.055	0.090	0.040	-0.014	-0.122	-0.083	17.75	0.036	1.643	0.213
	(0.247)	(0.107)	(0.107)	(0.215)	(0.068)	(0.099)	(0.065)	(16.39)	(0.025)	(1.256)	(0.284)
Adj R-squared	0.313	0.089	0.441	0.137	0.534	-0.027	0.758	0.155	-0.053	0.245	0.099
Obs	296,000	296,000	296,000	296,000	296,000	296,000	296,000	296,000	296,000	296,000	296,000

Panel B: Large Firms (Real Pre-Treatment Sales > $200 million, 2018 dollars)

	R&D	Research	Development	Non-Wage R&D	R&D Wages	R&D CAPX	R&D Employees	Mean Real R&D Wage	Patents	New to Co Sales	New to Mkt Sales
Trade*IDD*Post	-0.101***	-0.028*	-0.050**	-0.049*	-0.052	-0.005	-0.066*	8.09	0.006	0.194	-0.276
	(0.033)	(0.017)	(0.021)	(0.028)	(0.052)	(0.011)	(0.036)	(23.47)	(0.018)	(0.344)	(0.323)
Trade*NCL*Post	0.122	0.073	0.075	0.130	-0.008	-0.008	0.001	3.17	0.194	-0.067	-0.010
	(0.230)	(0.111)	(0.116)	(0.219)	(0.022)	(0.006)	(0.020)	(12.68)	(0.176)	(1.034)	(0.379)
Trade*NCF*Post	-0.004	0.001	0.009	-0.001	-0.003	-0.005	-0.002	-39.25***	-0.079	0.999	0.106
	(0.060)	(0.019)	(0.035)	(0.048)	(0.022)	(0.005)	(0.023)	(12.01)	(0.089)	(0.798)	(0.161)
Trade*Post2010	-0.233	-0.094	-0.128	-0.214	-0.019	-0.002	-0.018	2.02	-0.001	-0.132	-0.186
	(0.164)	(0.082)	(0.081)	(0.166)	(0.034)	(0.005)	(0.012)	(28.87)	(0.005)	(0.590)	(0.264)
Trade*Post2011	-0.210	-0.079	-0.114	-0.194	-0.016	-0.012	-0.022	-46.82	-0.008	0.073	-0.541*
	(0.138)	(0.067)	(0.069)	(0.140)	(0.014)	(0.007)	(0.019)	(44.18)	(0.009)	(0.578)	(0.308)
Trade*Post2012	0.056	0.016	0.010	0.121	-0.065	0.001	0.009	1.89	-0.003	0.140	0.835
	(0.126)	(0.045)	(0.068)	(0.104)	(0.046)	(0.012)	(0.036)	(27.81)	(0.008)	(0.415)	(0.612)
Trade*Post2013	0.052	0.025	0.037	0.024	0.028	0.014	-0.008	-27.78**	0.009	-1.910	-0.537
	(0.093)	(0.034)	(0.054)	(0.022)	(0.077)	(0.011)	(0.017)	(13.00)	(0.010)	(1.821)	(0.530)
Trade*Post2014	0.017	0.016*	0.002	-0.009	0.026	-0.010*	-0.017	4.40	-0.020	0.727	-0.148
	(0.017)	(0.009)	(0.013)	(0.018)	(0.020)	(0.006)	(0.013)	(19.26)	(0.017)	(0.731)	(0.120)
Trade*Post2015	-0.052	-0.014	-0.050	-0.004	-0.048*	-0.010**	-0.023	3.82	-0.011	-0.063	-0.261**
	(0.046)	(0.017)	(0.031)	(0.029)	(0.026)	(0.005)	(0.018)	(13.60)	(0.007)	(0.265)	(0.103)
Trade*Post2016	0.086**	0.015	0.057**	0.048*	0.038**	0.003	0.020	4.67	-0.044	-0.182	-0.134
	(0.041)	(0.012)	(0.027)	(0.026)	(0.017)	(0.004)	(0.013)	(8.28)	(0.046)	(0.159)	(0.109)
ln(age)	-1.670	-0.796	-0.767	-1.293	-0.378	-0.120	-0.070	-143.00*	-0.321	-12.690	-2.706
	(1.265)	(0.598)	(0.639)	(1.228)	(0.256)	(0.120)	(0.187)	(80.85)	(0.302)	(8.488)	(2.040)
$\ln(\text{age})^2$	1.034	0.479	0.561	0.718	0.317**	0.033	0.081	62.27	0.161	6.941	1.028
	(0.720)	(0.344)	(0.361)	(0.725)	(0.123)	(0.044)	(0.101)	(51.41)	(0.145)	(4.936)	(1.063)
Adj R-squared	0.196	0.175	0.209	0.189	0.146	0.145	0.868	0.176	-0.022	0.300	0.136
Obs	96,000	96,000	96,000	96,000	96,000	96,000	96,000	96,000	96,000	96,000	96,000

Panel C (Startups)

	R&D	Research	Develop-ment	Non-Wage R&D	R&D Wages	R&D CAPX	R&D Employees	Mean R&D Wage	Patents	New to Co Sales	New to Mkt Sales
Trade*IDD*Post	-2.509*	-0.545*	-0.283	-1.894*	-0.616**	-0.075	-0.320	-100.60	-0.121	D	D
	(1.381)	(0.296)	(0.243)	(1.136)	(0.279)	(0.097)	(0.288)	(135.60)	(0.121)	(D)	(D)
Trade*NCL*Post	D	D	D	D	D	D	D	D	D	D	D
	(D)	(D)	(D)	(D)	(D)	(D)	(D)	(D)	(D)	(D)	(D)
Trade*NCF*Post	0.470	0.191	0.181	D	-0.292	-0.459*	0.025	-91.98**	0.033	0.370	-0.683
	(0.661)	(0.355)	(0.397)	(D)	(0.272)	(0.245)	(0.223)	(42.74)	(0.100)	(0.379)	(0.538)
Trade*Post2010	-0.160***	-0.103**	0.004	-0.112**	-0.048	0.048	-0.025	-40.53***	-0.030	-0.005	-0.243**
	(0.054)	(0.046)	(0.059)	(0.044)	(0.060)	(0.089)	(0.030)	(14.35)	(0.031)	(0.056)	(0.105)
Trade*Post2011	0.000	0.028	-0.029	-0.025	0.025	-0.018	-0.018	-2.46	-0.033	0.030	0.221**
	(0.098)	(0.027)	(0.105)	(0.051)	(0.064)	(0.033)	(0.047)	(11.22)	(0.037)	(0.046)	(0.086)
Trade*Post2012	-0.584**	-0.248***	-0.269*	-0.186	-0.398***	0.049	-0.295***	-45.04*	-0.195***	-0.012	-0.259***
	(0.228)	(0.044)	(0.162)	(0.173)	(0.086)	(0.070)	(0.097)	(26.01)	(0.058)	(0.115)	(0.093)
Trade*Post2013	0.045	-0.069	0.073	0.069	-0.025	-0.071	0.102	-16.15	-0.055	-0.095	0.080
	(0.287)	(0.058)	(0.116)	(0.224)	(0.081)	(0.050)	(0.068)	(12.08)	(0.053)	(0.069)	(0.107)
Trade*Post2014	0.093	-0.062	-0.046	0.096	-0.003	0.018	0.025	3.78	-0.049	-0.023	-0.211***
	(0.319)	(0.091)	(0.150)	(0.244)	(0.105)	(0.039)	(0.075)	(18.50)	(0.051)	(0.052)	(0.081)
Trade*Post2015	0.416*	0.305**	0.024	0.443**	-0.027	0.102***	-0.019	-41.56***	0.060	0.000	-0.003
	(0.212)	(0.127)	(0.135)	(0.190)	(0.097)	(0.038)	(0.077)	(9.55)	(0.046)	(0.154)	(0.068)
Trade*Post2016	0.121	0.125	0.277	0.139	-0.018	-0.152*	0.057	-32.48**	-0.137**	-0.148	-0.220**
	(0.285)	(0.105)	(0.243)	(0.161)	(0.107)	(0.090)	(0.134)	(13.86)	(0.054)	(0.127)	(0.104)
ln(age)	0.596	0.127	0.489	0.309	0.286***	0.140	0.371	-25.08	-0.109	0.005	0.879*
	(0.455)	(0.140)	(0.377)	(0.390)	(0.101)	(0.126)	(0.237)	(131.90)	(0.121)	(0.217)	(0.499)
ln(age)2	1.304	1.418**	-1.000	0.523	0.782	0.091	0.172	40.38	0.240	-0.675*	-0.524
	(1.139)	(0.573)	(0.665)	(1.046)	(0.549)	(0.221)	(0.280)	(141.10)	(0.178)	(0.365)	(0.483)
Adj R-squared	0.34	0.247	0.103	0.332	0.268	-0.133	0.366	0.218	0.223	0.005	0.280
Obs	6,600	6,600	6,600	6,600	6,600	6,600	6,600	6,600	6,600	6,600	6,600

Table 4: Conditional Logit Analysis of Trade Secret Dependence

This table presents results from a conditional logistic regression analysis were the odds of a firm declaring trade secrets are important is a function of firm-specific effects, year dummies (I_year)., and interactions between indicator variables for the firm being located in state the IDD, or had a pro-labor (NCL) or pro-firm (NCF) ruling on non-competes, and an indicator variable for the observation occurring after treatment (Post). Odds modeled are conditional of firm-by-cohort fixed effects. Standard errors clustered by firm-by-cohort, industry and state-by-year groups. ***, ** and ** indicate statistical significance at the 1%, 5% and 10% level. Census disclosure clearance #11179.

	Full Sample	Large Firms
IDD*Post	-0.341***	-0.602**
	(0.124)	(0.275)
NCL*Post	-0.416***	-0.541**
	(0.093)	(0.254)
NCF*Post	-0.111	-0.076
	(0.077)	(0.185)
ln(age)	0.901***	0.708***
	(0.007)	(0.008)
$\ln(\text{age})^2$	-0.622***	-0.005
	(0.028)	(0.053)
I_2009	0.015	-0.097**
	(0.032)	(0.042)
I_2010	-0.149***	-0.396***
	(0.036)	(0.047)
I_2011	0.023	-0.304***
	(0.032)	(0.057)
I_2012	-0.287***	-0.636***
	(0.039)	(0.066)
I_2013	0.039	-0.515***
	(0.042)	(0.064)
I_2014	0.108***	-0.512***
	(0.039)	(0.068)
I_2015	0.088***	-0.733***
	(0.029)	(0.063)
I_2017	0.412***	-0.616***
	(0.043)	(0.054)
I_2018	0.623***	-0.437***
	(0.040)	(0.076)
Pseudo R-squared	0.014	0.007
Obs	109,000	36,500

Table 5: Robustness of Conditional Logit Analysis of Trade Secret Dependence

Results from a conditional logistic regression analysis were the odds of a firm declaring trade secrets are important is a function of firm-specific effects, year dummies (I_year)., and interactions between indicator variables for the firm being located in state the IDD, or had a pro-labor (NCL) or pro-firm (NCF) ruling on non-competes, and an indicator variable for the observation occurring after treatment (Post). Odds modeled are conditional of firm-by-cohort fixed effects. Standard errors clustered by firm-by-cohort, industry and state-by-year groups. ***, ** and ** indicate statistical significance at the 1%, 5% and 10% level. Census disclosure clearance #11179.

	Full Sample	Large Firms
IDD*Post	-0.346***	-0.611**
	(0.125)	(0.276)
NCL*Post	-0.416***	-0.541**
	(0.099)	(0.253)
NCF*Post	-0.142**	-0.100
	(0.059)	(0.205)
NJ*Post2012	-0.122***	-0.151***
	(0.016)	(0.015)
TX*Post2013	0.132	0.059
	(0.265)	(0.286)
ln(age)	0.901***	0.708***
	(0.008)	(0.008)
ln(age)2	-0.624***	-0.007
	(0.028)	(0.052)
I_2009	0.015	-0.096**
	(0.034)	(0.041)
I_2010	-0.147***	-0.395***
	(0.038)	(0.046)
I_2011	0.025	-0.302***
	(0.032)	(0.058)
I_2012	-0.277***	-0.621***
	(0.042)	(0.066)
I_2013	0.048	-0.501***
	(0.045)	(0.065)
I_2014	0.117***	-0.497***
	(0.040)	(0.068)
I_2015	0.098***	-0.718***
	(0.033)	(0.064)
I_2017	0.423***	-0.599***
	(0.044)	(0.054)
I_2018	0.635***	-0.420***
	(0.042)	(0.076)
Pseudo R-squared	0.015	0.008
Obs	109,000	36,500