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INNOVATION, INVENTOR MOBILITY, AND THE ENFORCEABILITY OF NONCOMPETE AGREEMENTS

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ABSTRACT

Firms often restrict workers' mobility with Noncompete Agreements (NCAs). Using state-level law changes, we find that making NCAs easier to enforce ("stricter" enforceability) leads to fewer patents, an effect that we show reflects a loss in innovation. While stricter enforceability encourages firms' R&D investment, consistent with alleviating hold-up concerns, it also limits inventors' job mobility and new business formation; supplementary evidence indicates the decline in mobility stifles knowledge diffusion. Analyses of technology-specific nationwide exposure, as well as cross-state spillovers via firms' corporate networks, reveal that our state-level estimates, if anything, understate the economy-wide effects of NCA enforceability on innovation.

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1 Introduction

Over the last several decades, the U.S. labor market has experienced a pronounced decline in dynamism, as measured by declining job and worker reallocation (Decker et al., 2020), declining internal migration (Molloy et al., 2016), and a range of other characteristics (Akcigit and Ates, 2021). A key concern with these trends is how they affect the pace of innovation. A fluid and dynamic labor market facilitates interactions between skilled inventors and spreads knowledge, both of which are key to developing ideas that lead to innovation (Akcigit et al., 2018). However, such movements can be costly to firms because they allow valuable ideas to spread to competitors (Dasaratha, 2023); thus, a dynamic labor market could potentially discourage firms from investing in R&D and their workers' human capital, causing lower rates of innovation. While the relationship between business dynamism and innovation has been explored theoretically (e.g, Akcigit and Ates (2023)), it has proven difficult to examine empirically given the range of related macroeconomic trends over the same period.

A common way that employers prevent the movements of inventors and other innovative workers is with noncompete agreements (NCAs): contractual restrictions that prohibit workers from joining or starting a competing firm.[1] How the legal *enforceability* of NCAs—the key policy lever governing their use—affects innovation has been the subject of contentious debate. By construction, NCAs limit job mobility, which limits associated inventor interactions and knowledge spread. These limitations potentially hamper innovation: Gilson (1999) hypothesized that Silicon Valley overtook Massachusetts' Route 128 as a major technological hub due to NCAs being unenforceable in California. On the other hand, others argue that enforceable NCAs *facilitate* innovation (Barnett and Sichelman, 2020) by alleviating investment hold-up problems (Rubin and Shedd, 1981; Grossman and Hart, 1986). This conceptual ambiguity has spilled over into policy discussions, most notably manifested in the Federal Trade Commission's (FTC's) 2024 ban on NCAs[2] and differing opinions on how such a ban would affect innovation.[3]

This paper provides comprehensive evidence that stricter enforceability of NCAs (that

[1]As examples of their prevalence in innovative workplaces, 35% of surveyed workers in "Computer, Mathematical" occupations had signed NCAs in 2014 (Starr et al., 2020), and 54.2% of surveyed firms in "Information" industries used NCAs for at least some workers in 2019 (Colvin and Shierholz, 2019).

[2]Federal Trade Commission, "FTC Announces Rule Banning Noncompetes," April 23, 2024. https://www.ftc.gov/news-events/news/press-releases/2024/04/ftc-announces-rule-banning-noncompetes, accessed December 2024. The rule was set aside by the United States District Court for the Northern District of Texas in August 2024, a ruling that has been appealed by the FTC.

[3]MIT Sloan Management Review, "Will a Noncompete Ban Impact Innovation Beyond Tech Hubs?," June 28, 2023. https://sloanreview.mit.edu/strategy-forum/will-a-noncompete-ban-impact-innovation-beyond-tech-hubs/, accessed February 2024.

is, a legal environment that makes it easier for firms to enforce NCAs) reduces innovation, as measured by multiple measures of the quantity and quality of patenting. We: 1) use a dataset that measures NCA enforceability in a comprehensive and multi-dimensional way according to legal scholars and contains the universe of relevant legal changes; 2) distinguish between NCA enforceability's effect on truly innovative versus purely strategic patents; 3) provide evidence to reconcile the contrasting theoretical predictions of how NCA enforceability affects the innovative process; and 4) estimate the effect of enforceability on *economy-wide* innovation accounting for potential cross-state spillovers.

To estimate how changes in NCA enforceability affect state-level patenting, we use a dataset from Johnson et al. (2023) that quantifies multiple dimensions of NCA enforceability for all 50 states and the District of Columbia for each year from 1991 to 2014. The dataset draws from the work of leading legal scholars to generate a numerical measure of states' enforceability of NCAs each year, containing the universe of NCA law changes over this period. Changes to NCA enforceability during this time were evenly spread out across geographic regions and typically arose from precedent-setting judicial decisions. We combine this enforceability dataset with rich data on patenting from the US Patent and Trademark Office (PTO) and other sources that enable us to track rates of patenting, and the quality thereof, across states, technology classes, inventors, and firms over time.

Our primary measure of innovation is the number of (eventually granted) patent applications in a given year, weighted by the number of forward citations each patent receives. To avoid the bias that can arise from estimating the effect of a treatment that not only changes across states in a staggered fashion (Goodman-Bacon, 2021), but also is continuous and can increase or decrease in value, we conduct a stacked event study design around a state's first law change (Cengiz et al., 2019; De Chaisemartin and D'Haultfoeuille, 2022a).

We find that when a state makes NCAs easier for firms to enforce (that is, when enforceability becomes "stricter"), that state experiences a statistically and economically significant decrease in patenting. The average enforceability increase during our sample period led to a 16-18% reduction in the number of (citation-weighted) patents granted in a state. Event study estimates reveal that this effect grows over time and is persistent for at least 10 years. An average-sized NCA enforceability increase reduces patenting by roughly as much as: a 10% increase in the tax price of R&D (Bloom et al., 2019), moving a computer scientist from a technology cluster at the 75$^{\text{th}}$ percentile size to one at the median size (Moretti, 2021), and a one standard deviation increase in exposure to Chinese import penetration (Autor et al., 2020).

We must be careful to interpret a change in patenting as a change in innovation. One reason is that patents noisily measure true innovation: they vary enormously in their value and importance (Schankerman and Pakes, 1986; Trajtenberg, 1990). If changes to NCA enforceability only affect low-value patents, it would be difficult to conclude that NCA enforceability matters for underlying innovation. While our baseline measure—forward citation-weighted patent counts—accounts for this issue to some extent (Hall et al., 2005), we consider additional measures of quality including whether a patent's forward-citation count is in the top 1%, 5%, or 10% of its technology class, and whether it is a "breakthrough" patent based on textual similarity to previous and subsequent work (Kelly et al., 2021). Based on each of these measures, higher enforceability reduces high-quality patents by just as much as—if not more than—lower-quality patents.

A second reason that changes in patents may not reflect changes in innovation is that changes to NCA enforceability could affect firms' strategic incentive to patent new ideas, rather than the underlying creation of new ideas. For example, some patents do not reflect new innovations, but rather reflect incumbent firms' strategic efforts to protect market share (Argente et al., 2023). If strict NCA enforceability makes it harder for startups to form, incumbent firms may feel less need to file such strategic patents. However, this margin is unlikely to drive our results: we find that NCA enforceability has an especially large effect on patenting from startup firms unlikely to use such strategic patents.

Relatedly, NCAs also reduce the risk that a firm's ideas leak to its competitors; as a result, stricter NCA enforceability might lead firms to substitute patents for other ways of protecting new ideas (like trade secrecy). We provide two pieces of evidence indicating that this strategic margin cannot fully explain the effects we find on patenting. First, we find that strict NCA enforceability reduces patenting in the pharmaceutical and medical device industries, where the risk of reverse engineering leads firms to patent almost all new ideas (Cohen et al., 2000). Second, using measures from Glaeser (2018), we find no detectable effect of NCA enforceability on firms' reported use of valuable trade secrets, implying firms are not merely substituting one form of idea protection for another.

If strict NCA enforceability does in fact reduce state-level innovation, how do we interpret this result in light of contrasting theoretical arguments? In the second part of the paper, we revisit these arguments. Enforceable NCAs should, after all, solve holdup problems (e.g., Grossman and Hart (1986)), raising firms' willingness to invest in R&D, training, and other inputs to innovation. Is this effect non-existent, or just dominated by other forces? To investigate, we examine investment and innovative activity in publicly-traded firms using Compustat and the Duke Innovation & Scientific Enterprises Research Network (DISCERN) database (Arora et al., 2021). Consistent with alleviating holdup, we find that stricter NCA enforceability leads firms to increase intangible investment but leaves physical investment unchanged.[4] However, stricter NCA enforceability still leads to a large decline in (overall, citation-weighted, and value-weighted) patenting within firms. That is, any potential gain from enhanced investment is more than offset by countervailing ways that enforceable NCAs dampen innovation.

We next consider these countervailing forces directly. We start with Gilson (1999)'s hypothesis that strict NCA enforceability, by limiting the mobility of inventors across firms, slows innovation by stifling interfirm knowledge transfer and start-up vitality.[5] Consistent with Gilson's argument, we find that strict NCA enforceability leads to less inventor mobility, less overall job mobility in innovative industries, and lower new business formation. To the extent that worker mobility across firms spreads tacit technological knowledge (Saxenian, 1994), increases inventor interaction (Akcigit et al., 2018), and that startups function as "engines of innovation" (Chatterji et al., 2014), these effects could partially explain why stricter NCA enforceability lowers overall innovation.

In the third part of the paper, we provide two pieces of evidence suggesting that NCA enforceability's effect on inventor mobility is a direct driver of its effect on innovation. First, we estimate separate effects of enforceability on state-level patenting and inventor mobility for each Cooperative Patent Classification (CPC), which capture technology classes: we find that technology classes in which NCA enforceability has the largest effect on mobility are also the classes where it has the largest effect on patenting: the relationship is linear and the correlation coefficient is roughly one.

Second, we test a direct implication of the hypothesis that slower inventor mobility would hamper innovation—namely, that slower mobility decreases the rate of knowledge diffusion across firms. We categorize patents based on whether they cite at least one other U.S. firm, as opposed to only self-citing or citing universities, governments, or foreign entities, to capture whether firms draw on knowledge developed by other firms. Consistent with stymied knowledge diffusion, we find that NCA enforceability has twice as large an effect on patents that draw on knowledge from other firms, relative to patents that do not.

In the final part of the paper, we consider how NCA enforceability affects *economy-wide* innovation. Our state-level estimates may misrepresent this economy-wide effect if NCA enforceability changes in one state have spillover effects across state lines. On the one hand, these spillover effects might be positive if stricter NCA enforceability in one state simply reallocates innovative activity to other states. Anecdotes abound of technology workers

[4] Jeffers (2023) also investigates this relationship using a slightly different empirical strategy. Unlike us, Jeffers (2023) finds that strict enforceability increases physical investment with no effect on intangible investment. Our findings mirror Shi (2023), who considers the effect of NCA *use* on investment.

[5] Lobel (2013) argues that another way enforcing NCAs can dampen innovation is by reducing *worker* incentives to invest in discovering new ideas.

leaving Route 128 in Massachusetts (a state that broadly enforces NCAs) to found new firms in California's Silicon Valley (where NCAs are unenforceable) (Saxenian, 1994). If ideas that would have been discovered in the Route 128 corridor instead were eventually discovered in Silicon Valley, then NCA enforceability in one state might not matter for *overall* innovation. On the other hand, these spillover effects might be negative, either because multi-state firms reallocate resources to high-enforcing states or because the discovery of ideas is a cumulative process that crosses state lines.

We introduce a novel approach to examine the overall effects of enforceability on innovation that accounts for such spillovers. We change the unit of observation from *state* to *technology class* (3-digit CPC code). Intuitively, we make use of variation in the baseline dispersion of CPCs' patenting rates across states. CPCs with initial patenting concentration in states that subsequently experience NCA enforceability increases had higher "exposure" to strict enforceability than CPCs with initial concentration in states without changes (or that went on to decrease enforceability). If state-level NCA enforceability changes simply reallocate innovation across state lines, then such CPC-level exposure would have zero effect on CPCs' overall patenting.

This is not what we find: CPCs more exposed to NCA enforceability increases had significantly lower rates of patenting than CPCs less exposed. Our estimates imply that if all states experienced an average-sized NCA enforceability increase, the average CPC's citation-weighted patenting would decrease by 23%. Comparing this to our state-level estimates (which imply that the same-sized enforceability increase in a single state would lead a typical CPC's in-state patenting to decrease by 18.4%) suggests that the state-level analysis slightly *under-estimates* the effect of enforceability on overall innovation, due to negative spillovers within technology classes across state lines. We corroborate this CPC-level analysis by examining cross-state spillovers within *multi-state firms*: when NCA enforceability increases in one state, other states connected to that state via multi-state firms' internal corporate networks also experience a decline in patenting.

This paper contributes to a wide literature that has considered various aspects of the relationship between NCA enforceability and innovative activity. Several studies have examined the effects of NCA enforceability on firm investment, entrepreneurship, and inventor migration: what might be considered inputs in the innovation process. Jeffers (2023) finds that stricter NCA enforceability leads to higher investment in publicly-traded firms, but also leads to a decrease in new firm entry. Starr et al. (2018), Baslandze (2022), and Marx (2021) also find lower rates of employee spinoffs and entrepreneurship in states that enforce NCAs. Specific to inventor mobility, Marx et al. (2015) finds that inventor outmigration increased when Michigan made NCAs more enforceable, and Mueller (2022) finds that strict NCA enforceability makes inventors more likely to switch industries.[6]

Other studies have considered the role of firms' strategic decisions in the relationship between enforceability and innovation. Conti (2014), using two NCA law changes (in Texas and Florida), estimates that increased NCA enforceability leads firms to undertake riskier R&D projects. In contrast, using a broad set of NCA law changes, Xiao (2022) finds that stricter enforceability promotes "exploitative" invention (that builds on prior knowledge) but stifles "exploratory" invention (that departs from existing knowledge) in the medical devices industry. Kang and Lee (2022) find that a decrease in NCA enforceability in California led firms to make a strategic substitution between patents and secrecy. One challenge to comparing the findings from these papers is they all use differing subsets of NCA law changes and different subsets of industries.

Two contemporaneously written papers —He (2023) and Rockall and Reinmuth (2023)— also examine the effects of NCA enforceability on innovation. He (2023), using data on NCA laws from Ewens and Marx (2018), finds that stricter NCA enforceability reduces rates of patenting and the value of patents. Like us, Rockall and Reinmuth (2023) start with Bishara (2010) to measure NCA enforceability in the cross-section, but they use a different source than we do to identify and measure state-level changes in enforceability over time. They also find negative effects of state-level enforceability increases on patenting, with magnitudes quite similar to ours. We complement these papers by, among other things, examining the effects on firm investment and finding evidence of meaningful cross-state spillovers via technology classes and internal corporate networks. [7]

Our paper contributes to these literatures by providing a comprehensive analysis of how NCA enforceability affects innovation. We use an exhaustive and carefully-measured database of NCA enforceability, use multiple methods to distinguish between "true" innovation and strategic patenting, and show that state-level NCA law changes do not simply reallocate innovative activity across state lines. As such, our paper addresses the several issues that Barnett and Sichelman (2020) highlight have made it challenging to derive substantive conclusions from extant literature on this subject.Additionally, we provide secondary analysis to reconcile the competing predictions regarding the effects of NCA enforceability on knowledge transfer (increasing innovation) and firm investment in R&D (decreasing innovation). Finally, our test for the reallocation effect of NCA enforceability across state lines

[6]Other papers provide indirect evidence on the relationship between NCAs and innovation: Samila and Sorenson (2011) show that an expansion in the supply of venture capital financing leads to a larger increase in patenting in states that (in the cross section) have lower NCA enforceability, and Belenzon and Schankerman (2013) find that knowledge spillovers from university patents have wider geographic scope in states that have lower NCA enforceability.

[7]Rockall and Reinmuth (2023) examines cross-state spillover effects related to initial patterns of inventor migration, finding no evidence of spillovers through this channel.

offers both a substantive and methodological contribution to understanding how state-level policies affect innovation, when inventors and other innovative input are mobile across state lines.[8]

We also contribute to prior work that has more generally considered the relationship between the dynamism of the labor market and innovation. Akcigit et al. (2018) show theoretically and empirically that inventor interactions, which are facilitated (among other ways) through job mobility between firms, are crucial for the discovery of new ideas. Dasaratha (2023) shows theoretically that firms over-invest in "secrecy" (discouraging worker mobility to protect investment) at the expense of "openness" (encouraging mobility to learn about ideas), resulting in inefficiently low innovation in the status quo equilibrium. Our findings corroborate this theoretical result: making NCAs more difficult to enforce—which is akin to ensuring firms increase "openness"—increases overall innovation, even though it decreases firms' investment. Relatedly, our findings complement a literature that has explored the causes and consequences of declining business dynamism in the U.S. Decker et al. (2020) find that declining responsiveness of firms to idiosyncratic productivity shocks largely explains the macro-level decline in the pace of job reallocation: our findings imply that the steady increase in NCA enforceability since 1991 (Johnson et al., 2023; Rockall and Reinmuth, 2023) could partially explain these trends. Akcigit and Ates (2023) build an endogenous growth model with the theoretical finding that the decline in the intensity of knowledge diffusion over time is a major culprit behind the decline in business dynamism: our article suggests that the enforceability of NCA could be a key driver of this decline in the pace of knowledge diffusion.[9]

[8]Bryan and Williams (2021) discuss how the relocation responses of inventors and firms to tax policies makes it particularly challenging to estimate the effects of tax incentives on overall innovation. Akcigit et al. (2022) attempt to overcome this challenge by estimating effects of state-level tax incentive changes on incumbent inventors that did not relocate; they find that the state-level reductions in patenting due to corporate tax rates are predominately due to the (zero sum) relocation of firms to lower-tax states, whereas personal income tax rates induce an actual innovation output response.

[9]Relatedly, our paper intersects previous work on the relationship between innovation and other types of employment regulation. Griffith and Macartney (2014) find that employment protection laws can increase overall innovation (but decrease technologically advanced innovation). Acharya et al. (2014) find that wrongful discharge laws that protect employees from unjust dismissal *increase* innovation by limiting employers' ability to hold up innovative workers after a successful discovery. That is, while previous work finds that laws making it harder to fire workers (in bad faith) can raise innovation, we find that laws making it harder for workers to quite unambiguously lowers innovation.

2 Data and Empirical Methods

2.1 Main Datasets

We link panel data on state-level NCA enforceability with several patent, job mobility and business dynamics datasets. We briefly discuss these datasets below and provide further details in Appendix A.

2.1.1 Measuring NCA Enforceability

The extent to which an NCA is legally enforceable is governed by employment law, which is set at the state level. As described by Bishara (2010), the relative strength of NCA enforceability varies widely across states, and over time within states, in sometimes subtle but often meaningful ways. For example, there is substantial variation across states in what is considered a "reasonable" NCA, or what is considered a legitimate business interest that justifies an NCA. Moreover, precedent–setting court cases—and, more rarely, statutory changes—have led to changes *within* states in NCA enforceability.

We use a state-level panel dataset—constructed by Johnson et al. (2023), extending a dataset created by Hausman and Lavetti (2021)— with annual measures of states' NCA enforceability for each of the 50 US states and the District of Columbia from 1991 to 2014.[10] This database draws from Bishara (2010) (an authoritative legal expert on NCAs)[11] that identifies seven quantifiable dimensions governing the extent to which an NCA is enforceable.[12] Bishara (2010) develops a theoretically-grounded approach to quantify states' treatment of each dimension on an integer scale from 0 (unenforceable) to 10 (easily enforceable), and he proposes a weighted sum of these seven dimensions to create an overall enforceability index, with weights based on legal reasoning regarding the likely importance of the dimension in a court's ruling over an NCA's enforceability.[13] Using these rules, Bishara (2010) quantified each dimension and an overall index for each state for the years 1991 and 2009.

[10] The NCA enforceabiity dataset and documentation is available at: https://doi.org/10.7910/DVN/37A0L2.

[11] Bishara (2010) draws from a series of legal treatises titled "Covenants Not to Compete: A State by State Survey," updated annually by Brian Malsberger.

[12] The seven dimensions are, labeled according to their question numbers in the original Malsberger texts, are: whether a state statute exists governing enforceability (Q1); the how strictly the courts define a business's protectable interest (Q2); the plaintiff's burden of proof (Q3); whether the advent of employment (Q3a) or continued employment (Q3bc) constitute adequate consideration for signing an NCA; whether the court can modify otherwise unenforceable agreements to make them enforceable (Q4); and whether NCAs of terminated workers are enforceable (Q8). See "Covenants Not to Compete: A State by State Survey," updated annually by Brian Malsberger, for more detail.

[13] Subsequent research uses confirmatory factor analysis as an alternative approach to determine these weights, and settles on an essentially identical weighting scheme as Bishara (Starr, 2019)

Hausman and Lavetti (2021) and Johnson et al. (2023) carefully replicate the approach in Bishara (2010) and extend the dataset for every year from 1991–2014.[14] For example, one court case that changed a state's enforceability index was *Mona Electric Group, Inc., v. Truland Service Corp.* in Virginia in 2002. The judge in that case held that continued employment does not by itself constitute adequate consideration (or compensation) for an NCA signed after the beginning of employment. This decision led to the score in Virginia for Question 3, parts (b) and (c), to decrease from 8 to 3.[15] In total, there were 73 changes in NCA enforceability over our 1991–2014 sample period.

Ninety percent of the NCA law changes in our sample period arose through precedent-setting court decisions rather than statutory changes, which is useful for our empirical strategy. A judicial decision that changes legal precedent, such as the *Mona Electric Group, Inc., v. Truland Service Corp.* decision described above, is initiated by an individual lawsuit that is idiosyncratic to a particular employment relationship; however, the consequences of the decision affect the legal environment for NCAs for the entire state going forward.[16] Relative to legislators, the doctrine of *stare decisis* ensures that stakeholder pressure has less influence on judges' decision-making (Knight and Epstein, 1996). Furthermore, judges do not base their decisions purely on policy preferences, but rather on a wide range of motivations (Epstein and Knight, 2013), implying that judges' decisions to break from precedent in an NCA case are unlikely to be related to underlying economic trends.[17]

Johnson et al. (2023) provide extensive institutional background, further details of the construction of this database, and justification for the cardinality of the index. That paper also provides empirical evidence that within-state changes to NCA enforceability were orthogonal to underlying trends in economic, social, and political forces and were not driven by latent changes to workplace litigiousness. This exogeneity of law changes with respect to such underlying trends corroborates the institutional factors of the legal environment described above and bolsters the credibility of our research design that uses these law changes for identifying variation.

[14]Law students at Ohio State University and Duke University used Bishara's internal notes and the annual Malsberger treatises to construct the enforceability database. They examined the Malsberger treatises in each year, identifying any changes from the prior year, and coding those changes according to the ten-point scale for the corresponding dimension.

[15]Mona Electric Group, Inc., v. Truland Service Corp., 193 F. Supp. 2d 874, 976.

[16]Using data from Courthouse News Service, we find that from 2002–2014 (2002 is the earliest year of the dataset), there were roughly 700 court filings per year about noncompetes. Compare this to the (0.9* 82 / 23=) 3.2 NCA law changes per year arising through precedent-setting court decisions. That is, the probability that a single NCA lawsuit would overturn precedent was roughly (3.2/700 =) 0.46% over this period. This proportion is nearly identical to the proportion (0.5%) of Supreme Court decisions in which the Court has reversed its own Constitutional precedent Schultz (2022).

[17]Epstein and Knight (2013) write that judicial decisions arise from "so many possible motivations that the entire enterprise begins to border on the idiosyncratic" (pg 16).

2.1.2 Data on Patents and Other Measures of Innovative Activity

To measure innovative activity, we begin with public-use administrative data (PatentsView) on the universe of granted utility patent applications submitted to the United States Patent and Trademark Office (USPTO) between 1991 and 2014.[18] For each patent, we obtain the name and address of the inventors and the assignees,[19] and we assign each patent to a state based on the inventor's state of residence (assigning fractional patents in the case of multiple inventors: e.g., for a patent with three inventors residing in three different states, we assign $\frac{1}{3}$ of a patent to each state). Each patent also has a unique patent number, application date, and a grant date. We focus on the year of application (Akcigit et al., 2022) for our empirical analysis, because it may take multiple years for a patent to be granted after the initial application.[20]

Many patents generate little to no value (Hall et al., 2005; Allison et al., 2003). Our primary measure of innovation therefore weights each patent by the number of citations that the patent receives after its publication, otherwise known as "forward citations" (Trajtenberg, 1990; Lanjouw and Schankerman, 2004; Hall et al., 2005). Because citation counts are not necessarily comparable across time (more recent patents mechanically have less time to accumulate citations, and typical citation practices may change over time) or across technology class (some classes might rely on prior knowledge more than others), we take each focal patent's citations received within the first five years after it was granted and normalize that number by the average forward citation count in the focal patent's three-digit CPC code (Hall et al., 2005; Arora et al., 2023)[21] and grant year cohort. We consider the (raw) count of patents in robustness checks.

We use additional datasets in secondary analyses to examine other dimensions of innovative activity and we use the Crunchbase dataset[22] to measure startup innovation performance. We discuss these datasets in Section 3.3. We use the Census Bureau's Job-to-Job

[18] According to the USPTO, utility patents are "issued for the invention of a new and useful process, machine, manufacture, or composition of matter, or a new and useful improvement... Approximately 90% of the patent documents issued by the USPTO in recent years have been utility patents, also referred to as 'patents for invention;'" see USPTO for more details. It is common practice to only consider utility patents as measures of innovation (see, e.g., Hall et al. (2001)).

[19] The entity that owns the property right to the patent is known as the assignee. In our sample, around 89% of the patents are assigned to a U.S. company or corporation. The remaining 11% of assignees are distributed among US individuals, various categories of governmental entities, and other categories.

[20] According to the USPTO, it takes an average of 25.6 months after a patent application is submitted for the patent to be granted. See: https://www.uspto.gov/dashboard/patents/pendency.html.

[21] Each patent has a technological classification following the Cooperative Patent Classification (CPC) scheme. Patents can be separated into 9 sections (1-digit CPC) or 125 subsections (3-digit CPC). See https://www.uspto.gov/web/patents/classification/cpc/html/cpc.html for details.

[22] Crunchbase is a startup directory that includes a set of high growth-oriented private firms and startups backed by Venture Capital and Private Equity funding.

(J2J) Flows dataset to measure the mobility of workers across firms, the Census Bureau's Business Dynamics Statistics (BDS) to measure new business formation, and Compustat and the Duke Innovation & Scientific Enterprises Research Network (DISCERN) database (Arora et al., 2021), which links the USPTO and Compustat data, to examine *firm-level* innovative activity in publicly-traded firms. We discuss the details of these datasets in Section 4.

2.2 Empirical Strategy: Stacked Difference-in-Differences

Our empirical setting includes continuous (non-discrete) changes in NCA enforceability which occur at different times in different states. Furthermore, states may have multiple law changes over the sample period. To avoid the potential biases that can arise from using the traditional two-way fixed effects approach in such a setting (Goodman-Bacon, 2021; De Chaisemartin and D'Haultfoeuille, 2022b), we conduct a "stacked" event-study analysis around a state's first law change during our sample period. The stacked design has been used in other recent applied settings (Cengiz et al., 2019; Deshpande and Li, 2019), and De Chaisemartin and D'Haultfoeuille (2022a) show that the treatment effect of a unit's *first* change can be estimated without bias. We create an analysis sample that fits this design, and that has pre- and post-periods sufficiently long to detect dynamic effects, by identifying the subset of NCA law changes that satisfy the following criteria: 1) are a state's first law change during the sample period; 2) occur at least four years after the start of our sample period (1991); 3) occur at least 10 years before the end of our sample period (2014); and 4) are not followed by subsequent countervailing law changes. We use the 11 states that never experienced a law change during our sample period as the set of control states. For each treatment state, we create a "subexperiment" (hereafter, a "block"): a panel dataset for that treatment and the control states comprising the four years prior and ten years following the treatment state's law change.

We take one additional step to refine our analysis sample. The distribution of patent counts 1) is prone to outliers, and 2) varies widely across states in the cross section (in both level and trend). While these features should not in theory bias our estimates if NCA law changes are orthogonal to prior patenting activity, in practice they can make our estimates sensitive to pre-existing trends in a small number of outlier states. In particular, California's trend (and level) of patenting vastly outpaced all other states, especially during the dot-com technology boom of the 1990s. Since California experienced a (relatively small) change in NCA enforceability in 1998,[23] the rapid pace of innovation in California generates a pre-

[23]Though noncompetes have been essentially unenforceable in California since the 1800s, a 1998 case confirmed that judicial modifications to contracts–in order to make otherwise unenforceable contracts

trend for this law change. A similar situation applies to the state of Washington, which also experienced a rapid acceleration in innovation during the dot-com boom of the 1990s and experienced an NCA law change in 2004. For these two states, there is no reasonable control group: their trend in innovation is "out-of-support" with respect to the trends in control states over the four years prior to treatment. We thus omit those two treated states from our primary analysis. That is, we omit blocks for which the treated state has the *most extreme* linear trend in patenting in the pre-period (in either the positive or negative direction) compared to control states; these omitted states end up being California and Washington, as well as the state of Vermont.[24]

Figure B1 provides a visual representation of the specific states that meet the criteria to be included in our estimation sample. Figure B2 shows that the subset of law changes that we use is broadly representative of the full variation in NCA enforceability over our sample period: the distribution of both the *level* of NCA enforceability (Panel a) and the *size* of enforceability changes (Panel b) is similar for the full set of states and the subset of states in our estimation sample. This comparison suggests that the subset of states we examine broadly captures the variation in NCA enforceability across the entire country.

Formally, we estimate the following model:

$$Y_{s,t,b} = \beta_1 * Enforceability_{s,t} + \rho_{s,b} + \gamma_{t,b} + \varepsilon_{s,t,b}, \tag{1}$$

where s indexes states, t indexes year, and b indexes block. Our two primary outcomes of interest, $Y_{s,t,b}$, are 1) annual patent counts weighted by the number of forward citations (described in Section 2.1.2), and 2) raw annual patent counts. The coefficient of interest, β_1, estimates the effect of a change in NCA enforceability on the outcome variable, relative to the "clean control" states. $\rho_{s,b}$ is a state by block fixed effect, and $\gamma_{t,b}$ is a year by block fixed effect. Finally, $\varepsilon_{s,t,b}$ is the error term. We weight each observation by the sum of normalized citation-weighted patent counts in the pre-period.We report robust standard errors clustered at the state by block level (Cengiz et al., 2019).

In some specifications, we amend our approach so that the unit of observation is a state-CPC-block-year, rather than state-block-year. That is, we estimate how changes in NCA enforceability affect state-level patenting rates *within* technology classes. In these specifications, blocks are defined at the state-by-CPC level, meaning that the estimating equation does not change, except for the definition of b.

enforceable–were not allowed, leading to a small decrease in our measure of enforceability.

[24]In robustness checks, we add these omitted treated states back into our analysis and obtain estimates that are, if anything, larger in magnitude.

3 The Effect of NCA Enforceability on State-Level Innovation

In this section, we show that making NCAs easier to enforce ("stricter" enforceability) leads to a substantial reduction in state-level patenting and that this effect reflects a loss in innovation.

3.1 Baseline Estimates: Effect on State-level Patents

Figure 1 presents coefficients from an event study regression analogous to Equation 1 that estimates the effect of NCA enforceability on state-level patenting in each year relative to a state's first law change. In Panels (a) and (b), the outcome variable is normalized citation-weighted patent counts, respectively estimated at the state-CPC level and the state level. For both levels of analysis, the event study graphs reassuringly do not demonstrate differential trends prior to the year of the treatment state's first law change. In the post period, the coefficients in each panel become negative just after the year of the law change and gradually become more negative over the following ten years, indicating that an increase in NCA enforceability leads to a decline in patenting that increases in magnitude over time. The overall difference-in-difference estimate (reported in the upper right corner of each figure, as well as in Column 1 of Table B1) reveals that these effects are statistically significant and economically meaningful. Among the treatment states in our estimation sample, the average magnitude (in absolute value) of initial enforceability changes was equal to 0.080 (on a 0 to 1 scale). Thus, an increase in enforceability of average size induced a decrease in normalized citation-weighted patenting of 18.4% within CPC, and 15.7% at the state level.

In Panels (c) and (d) of Figure 1, the dependent variable is raw (unweighted) patent counts. The coefficients are somewhat smaller but qualitatively similar.

One useful way to interpret our estimates is by comparing their magnitude to how other economic and policy factors affect innovation. We estimate that an average-sized NCA enforceability increase leads to a 16-18% decline in citation-weighted patenting. A 16% reduction in patenting is comparable to the effect of: a 10% increase in the tax price of R&D (Bloom et al., 2019), moving a computer scientist from a technology cluster at the 75$^{\text{th}}$ percentile size to one at the median size (Moretti, 2021), and a one standard deviation increase in exposure to Chinese import penetration (Autor et al., 2020).[25] Another constructive com-

[25]Specifically, Moretti (2021) finds that the elasticity of inventor productivity (measured by the number of annual patents filed) with respect to cluster size is 0.0676. To put this in context, a computer scientist moving from a cluster at the median size in computer science to one at the 75th percentile of size would experience a 12.0 percent increase in the number of patents filed per year. Bloom et al. (2019) report that a

parison is to Akcigit et al. (2022), who analyze the impact of personal income and corporate tax rates on innovation. They estimate that both higher personal and higher corporate tax rates decrease innovation, with the elasticity of state-level patents in response to personal income (corporate) net-of-tax rates ranging from 0.8 to 1.8 (1.3 to 2.8).[26]

3.2 Robustness Checks on Baseline Estimates

Table B1 shows that the negative estimated effect of NCA enforceability on state-level patenting is robust to a range of potential confounds and specification concerns. We consider: the full sample (including the "out of support" treatment states, California, Washington and Vermont); weights based on 1991 normalized citation weighted patent counts; a binary (rather than continuous) NCA score change variable; positive and negative changes only; using ordinary least squares instead of Poisson pseudo-maximum likelihood; using a Census region by year by block fixed effect; and using two-way fixed effects (rather than the stacked estimator) on the baseline sample and the full sample. By and large, our main estimate is quite robust, and indeed conservative compared with many other possible estimates. We describe these results further in Appendix C.

Our difference-in-difference estimate of NCA enforceability's effect on patenting represents a weighted average over 15 focal states. A natural concern would be that one or two states drive this overall estimate. Fortunately, our stacked model enables us to estimate separate effects of NCA enforceability on patenting for each of the focal states in our sample. We estimate separate regressions corresponding to Equation 1 for each focal state in our sample with state-level patent counts as the dependent variable; we report the estimates in Figure B3. The coefficients are negative for 10 of the 15 states in our sample. Of the five states with positive coefficients, three are essentially zero; one of the two states with positive and statistically significant coefficients (Alaska) has a very small number of baseline patents relative to the mean and could plausibly reflect sampling variation. Thus, while there may be interesting variation in effect sizes across states, our estimated negative effect of NCA enforceability on patenting reflects a general effect across a broad range of states.

A final concern is that NCAs are not the only way that firms can protect intellectual property from spreading to competitors, and changes to NCA enforceability could in theory coincide with legal changes to such other forms of protection. Although Johnson et al. (2023)

10 percent fall in the tax price of R&D generates at least a 10 percent increase in R&D in the long run, based on a reasonable summary of the estimated elasticities found in this literature. Autor et al. (2020) found that a one standard deviation increase in import penetration from China is estimated to reduce firm-level patent counts by 10–15 percent.

[26]Our estimate is not directly comparable to Akcigit et al. (2022) since we do not report estimates as elasticities.

show that changes to NCA enforceability are exogenous to underlying economic or legal conditions, we directly test whether changes to two other forms of trade secret protection confound our results. First, we consider the Inevitable Disclosure Doctrine (IDD), a legal doctrine which allows a court to enjoin a worker from working for a competitor under the assumption that the worker's new position would inevitably rely on the initial firm's trade secrets. Second, we consider states' adoption of the Uniform Trade Secrets Act (UTSA), which standardized and strengthened how firms can protect trade secrets. We encode the changes in states' use of IDD reported in Castellaneta et al. (2016) and states' adoption of UTSA using a six-measure index from Png (2017a) and Png (2017b) (IDXE6). Table B2 shows that our baseline results are essentially unchanged when we control for these additional measures of trade secret protection.[27]

3.3 Does a Change in Patenting Reflect a Change in the Pace of Innovation?

Changes in state-level patent counts might not necessarily reflect changes in the state-level pace of innovation, particularly in our context.

One issue is that patents are not always a direct measure of innovation. Many patents generate little to no private value to firms (Hall et al., 2001; Kline et al., 2019), let alone social value. If NCA enforceability only affects the creation of relatively low-value patents, its impact on underlying innovation may be minimal. Our finding that NCA enforceability similarly affects raw and citation-weighted patent counts indicates that this scenario is unlikely; however, the number of citations is a noisy measure of a patent's "value" (Jaffe et al., 2000; Hall et al., 2001). We thus follow prior studies to consider several alternative approaches to capture a patent's contribution to innovation: (a) patents in the top 1, 5, and 10% of the normalized citation distribution of their "cohort"[28] (Gambardella et al., 2008; Abrams et al., 2013), and (b) "breakthrough" patents based on a given patent's textual similarity to previous and subsequent work.[29]

Moreover, some patents may not be protecting new genuine innovations, and instead reflect firms' strategic efforts to maintain market share and deter competitors from engaging in creative destruction (Argente et al., 2023). By directly making it harder for startups to form and grow (Starr et al., 2018; Jeffers, 2023), strict NCA enforceability may attenuate

[27]Note that reduced sample sizes in Columns 5 and 6 result from the IDXE6 measure being available only through 2008.

[28]We define a "cohort" as patents granted in the same year.

[29]Breakthrough patents qualitatively differ from previous patents but are strongly associated with successive innovation; see Kelly et al. (2021).

incumbent firms' motivation to file such strategic patents. To ensure this channel does not drive our results, we draw inspiration from (Argente et al., 2023), who find that such strategic patents are more common in incumbent firms with large market shares. We therefore measure the count of state-level patents in which the assignee is a startup. To identify whether an assignee is a startup, we match the USPTO data to CrunchBase, an online database with business information on over 200,000 companies and 600,000 entrepreneurs, with extensive information on each company's name, address, products, acquisitions, age, and other features. To link CrunchBase with USPTO, we implement string fuzzy match using company names and addresses; see Appendix A for further details.

A second issue is that patents are but one way that firms can protect new ideas, and changes in NCA enforceability could lead firms to substitute between these forms of protection. Firms do not patent every new discovery: to apply for and maintain a patent can be costly,[30] and firms have other means to protect newly-developed trade secrets and other discoveries (Cohen et al., 2000).[31] By making it harder for workers to move to competitors (and bring newly-discovered ideas with them), stricter NCA enforceability might make firms feel less compelled to patent new discoveries. That is, NCA enforceability might be a substitute for patents as a source of knowledge protection. If so, the relationship observed in Figure 1 might simply reflect fewer new ideas getting patented, rather than fewer new ideas being generated.

To examine this concern, we take two approaches. As our first approach, we use the number of state-level (forward-citation–weighted) patents in the medical devices and pharmaceutical sectors[32] as an outcome variable. Cohen et al. (2000) show that patents are the most effective way to protect product innovation in these industries due to the ease of reverse engineering. As a result, nearly all new product discoveries in these sectors are patented. Thus, any change in patenting in these sectors is likely to reflect changes in the discovery of new ideas, rather than changes in firms' strategic protection of ideas.

As our second approach, we directly test whether NCA enforceability changes influence firms' use of valuable trade secrets. The use of trade secrets complements the use of NCAs (since NCAs make it less likely that workers will have the opportunity to share trade secrets with competitors) and substitute for the use of patents as a way to protect innovation. This strategic margin could alter the interpretation of our results thus far, if a decrease in patenting merely reflects a change in how firms protect new ideas. If substitution between

[30] According to Leavitt & Eldredge, a firm's costs associated with filing a utility patent can range from $7,000 to $20,000.

[31] See Ganglmair and Reimers (2019) for a discussion of the relationship between trade secrecy and innovation.

[32] We define these two sectors based on CPC codes, following Belenzon and Schankerman (2013).

these forms of protections does occur, we would expect that firms would *increase* their use of trade secrets when NCA enforceability increases.

We use data on trade secret usage by publicly-traded firms. Public firms are legally required to discuss valuable trade secrets in their 10-K filings. Glaeser (2018) scraped 10-K filings to determine which firms report using trade secrets in which years. To use this dataset, we must measure the NCA enforceability that a given *firm* faces, which is complicated by the fact that most publicly-traded firms operate in multiple states. Since NCA enforceability is determined by state employment law, the relevant law is the law in the state in which a worker works, not the state in which a firm is headquartered. Thus, simply using the NCA enforceability score of a firm's headquarter's state would result in severe measurement error and attenuation bias. We construct a firm-specific NCA score in each year that is a weighted average based on a firm's employee-inventors' locations. That is, first, we link patents in the USPTO data to firms' unique identifiers using the DISCERN database (Arora et al., 2021). Then, for every patent filed between 1991–2014 in which firm i is the assignee, we note the state in which the patent was filed based on inventors' locations. We then calculate the share of firm i's patents over this period that were filed in each state s: $\omega_{is} = \frac{\#Patents_{is}}{\sum_{s'=1}^{51} \#Patents_{is'}}$. Firm i's NCA score in year t is a weighted average of the NCA score across all states in that year, with weights equal to ω_{is}. The score therefore varies over time (as states change their laws), though the weights do not (to avoid endogenous movement of firms across states).

Since we measure firms' exposure to NCA enforceability as a weighted average across states, we cannot use the stacked design used thus far for this analysis. Instead, we estimate the effect of NCA enforceability on firm-level use of trade secrets:

$$Y_{it} = \beta * \text{NCA Score}_{it} + \rho_{r(i)t} + \iota_i + \epsilon_{it},$$

where Y_{it} is an indicator for use of trade secrets by firm i in year t (from Glaeser (2018)), and ρ and ι are region-year and firm fixed effects, respectively. We omit from the sample firms that never report using valuable trade secrets in their 10-K filings at any point during the sample period, which serves to effectively isolate the impact of NCA enforceability among firms for which trade secrets are presumably valuable.

Figure 2 displays results that examine the possibility that NCA enforceability only impacts low-value or strategic patents.[33] Row (1) reports our baseline estimate on citation-weighted patents at the state level for comparison. Rows (2) – (6) test whether NCA enforceability affects the rate of patenting for patents that are most likely to be valuable or innovative. Rows (2), (3) and (4) show that stricter NCA enforceability leads to a reduction

[33] Appendix Table B3 reports the regression output underlying this figure.

in patents with citation counts in the top 1, 5, and 10%, though only the estimate for the top 10% is statistically significant at conventional levels (possible due to the scarcity—by construction—of patents in the top 1% or 5% leading those estimates to be underpowered). Rows (5) and (6) show that stricter NCA enforceability reduces both breakthrough and non-breakthrough patenting, though the magnitude is substantially larger for breakthrough patents.[34]

Row (7) shows that stricter NCA enforceability reduces the number of state-level (citation-weighted) patents for which the assignee is a startup: the coefficient is negative, large in magnitude, and highly statistically significant ($p < .01$). Row (8) provides a basis for comparison: at the state level, the impact of NCA enforceability on citation-weighted patenting for all other (non-startup) companies is approximately half that for startups, though the coefficient is more noisily estimated.

Row (9) of Figure 2 considers normalized citation-weighted patent counts in the medical device and pharmaceutical sectors.[35] We find a large and negative effect on medical device and pharmaceutical patents, though the estimate is only statistically significant at the 10% level ($p = 0.08$).

Finally, Row (10) of Figure 2 considers the use of trade secrets by publicly traded firms. The coefficient is positive, but it is not statistically significant and the magnitude is very close to zero; the point estimate implies that an average-sized NCA enforceability increase leads to 1.2% higher probability that a firm uses trade secrets (relative to a mean of 87.3% usage in the data). The coefficient is precisely estimated: the 95% confidence interval rules out an effect size of more than 2.9% from an average-sized law change. In other words, strict NCA enforceability may indeed lead to some strategic substitution between the use of NCAs and the use of patents, but the effect is nowhere near large enough to explain the negative effect on patenting.[36]

These results collectively bolster the interpretation that the reduction in state-level patenting caused by strict NCA enforceability reflects a reduction in underlying state-level innovation.

[34]The p-value on the difference between the breakthrough and non-breakthrough coefficients is 0.079

[35]We include separate observations for medical device and for pharmaceutical patents, analogous to the model at the CPC level, which accounts for the additional observations in Column 6 of Table B3.

[36]Using a different set of NCA law changes, Greenwood et al. (2024) find that banning NCAs has no effect on firms' use of trade secrets in the short run, aligning with our results here.

4 Reconciling our Estimates with Contrasting Theoretical Arguments: Investment Holdup versus Knowledge Diffusion

How should one interpret our empirical results in Section 3, given that theory gives contrasting predictions for how NCA enforceability should affect innovation? Enforceable NCAs should, after all, increase firms' incentives to invest in R&D and other knowledge inputs by alleviating holdup concerns. Is this effect non-existent? Or is it just dominated by other ways that enforceable NCAs can hinder innovation, such as by inhibiting inventor mobility and startup formation?

4.1 NCA Enforceability, Investment, and Patenting Within Publicly-Traded Firms

The idea that enforceable NCAs may increase firms' investment in R&D has been considered in prior work, through both a theoretical (Rubin and Shedd, 1981; Shi, 2023) and empirical lens (Garmaise, 2011; Jeffers, 2023). However, despite the intuitive argument that NCAs alleviate holdup concerns, extant results on the relationship between NCA enforceability and investment are not unanimous.[37] We extend this prior work by using our comprehensive database on NCA enforceability, implementing an empirical strategy that closely captures multi-state firms' effective NCA enforceability, and examining effects on various measures of incumbent firms' patenting.

We estimate the effect of NCA enforceability on investment and patenting within publicly-traded firms. We use the Compustat database and, following Jeffers (2023) and Shi (2023), measure both intangible[38] and physical investments.[39] To measure firm-level patenting, we use the DISCERN database (Arora et al., 2021), which links patents from the USPTO to

[37]For example, the theoretical model in Garmaise (2011) implies that strict NCA enforceability on one hand raises firms' incentives to invest in general training and R&D, but on the other hand diminishes managers' incentives to invest in their own human capital. Analyzing a handful of state law changes, he finds that stricter enforceability *reduces* capital investment per employee. Amir and Lobel (2012) make a similar argument and find supporting evidence in a lab experiment. Analyzing several NCA law changes since 2009, Jeffers (2023) finds that stricter enforceability *increases* capital investment, with no effect on intangible investment. Shi (2023), analyzing NCA *use* (rather than enforceability), finds that firms with CEOs bound by an NCA invest more in intangibles but have no difference in capital investment.

[38]Research and development expenses (xrd) scaled by one year-lagged total assets (at). Following Jeffers (2023), we do not replace missing values of R&D with zeros. We topcode this variable at the 99^{th} percentile in each year to prevent undue influence from extreme outliers.

[39]Capital investment less the sales of property (capxv-sppe) and scaled by one-year lagged total assets (at). As with intangible investment, we topcode at the 99^{th} percentile and do not replace missing values with zeros.

Compustat. Since firms operate across multiple states, we use the weighted firm-level NCA scores discussed in Section 3.3, as well as the estimating equation reported in Equation 3.3.

We report results in Table 1. Columns 1 and 2 consider effects on firm investment. We estimate a positive and statistically significant effect of NCA enforceability on intangible investment (Column 1): the point estimate suggests that an average-sized increase in NCA enforceability leads to a 10.4% ($p = 0.005$) increase in intangible investment. We estimate essentially no effect of NCA enforceability on capital investment.[40]

While the results in Columns 1 and 2 suggest that enforceable NCAs may indeed alleviate an investment hold-up problem, investment is but one of many inputs into innovation. Despite this increase in investment, the remaining columns show that stricter NCA enforceability still leads to a large decline in innovation within publicly-traded firms. Columns 3 and 4 report a statistically significant negative effect on raw and normalized citation-weighted patent counts, respectively. An average-sized increase in NCA enforceability leads to a 26.8% percent decrease in patent counts and 31.8% percent decrease in citation-weighted patent counts.[41] In Column 5, we consider an additional measure of patent quality, other than forward citations, that has been developed for publicly-traded firms: the excess stock returns on the date a patent is granted, which proxies for a patent's private financial return (Kogan et al., 2017). Stricter NCA enforceability leads to a 22.6% decline in financial-value-weighted patents ($p = 0.043$).

Stricter NCA enforceability indeed increases firm-level investment, consistent with alleviating holdup concerns. However, this increase in investment is clearly dominated by other ways in which NCA enforceability hinders innovation. We turn to these countervailing channels next.

4.2 NCA Enforceability, Job Mobility, and Startup Activity

The counterargument that NCAs stifle innovation, first popularized by Gilson (1999), centers on the idea that strict NCA enforceability limits the movement of workers between employers and to start-ups. NCAs limit worker mobility by construction—to both incumbent and to

[40] These results are consistent with Shi (2023), who finds that intangible investment is higher in firms with a higher proportion of executives under NCAs. They contrast somewhat with Jeffers (2023), who estimates that strict NCA enforceability has a positive effect on physical investment but no effect on intangible investment. However, our magnitudes are not directly comparable to those in Jeffers (2023) since we measure firms' exposure to NCA enforceability differently, examine a different set of legal changes, and use a different estimation sample.

[41] These magnitudes are larger than what our state-level estimates (reported in Figure 1), but the estimates are not necessarily comparable: these estimates are of *within-firm* (not within-state) effects, they do not include the in-support restriction of our state-level estimates, and they are not from a stacked design. When we estimate the state-level effect of enforceability using two-way fixed effects and without the in-support restriction, our state-level estimate is closer to these within-firm estimates, as shown in Table B1.

startup firms—and stricter NCA enforceability can reduce job mobility more broadly by slowing labor market churn and making it more costly for firms to hire (Johnson et al., 2023).

We follow several studies that have considered the effect of NCA enforceability on job mobility (Marx et al., 2009; Balasubramanian et al., 2016; Lipsitz and Starr, 2022; Jeffers, 2023; Johnson et al., 2023) and startup activity (Balasubramanian et al., 2016; Jeffers, 2023). We extend this work by implementing a novel approach to estimate effects on inventor-specific mobility and by using several new datasets to measure related outcomes.

Job Mobility (Primary Measure): The USPTO data offer a direct way to observe inventor job mobility: if an inventor's successive patents have different assignees, we can infer the inventor has changed jobs during the interim period. However, because this measure is only observed conditional on patenting, it poses particular challenges to use as a dependent variable in our setting. For one, as Marx et al. (2009) explain, it exhibits classical measurement error. If we only observe an inventor patent once, we do not know whether she changed jobs in subsequent years. If an inventor patents with firm X in one year and firm Z in a later year, we do not know the exact year she moved from firm X to Z; moreover, if in the interim period she worked for (but did not patent with) firm Y, we do not observe that job change. More concerning for our setting is that this measure suffers from a form of selection bias: because strict NCA enforceability leads to fewer patents, it leads to fewer opportunities for us to observe inventors' job mobility. Without accounting for this concern, we might find that strict NCA enforceability leads to lower *observed* job mobility, even if *true* job mobility was unchanged. We develop an approach to overcome this issue.

We create a panel of inventors who have patented at least twice during the period 1970–2021. In our baseline model, we further restrict the sample to inventors who have patented at least five times during this period; we consider alternative cutoffs in robustness checks. We assign inventors to states using the state of residence in their first patent, and we assign them to technology classes based on the CPC of their first patent[42]. If the assignee differs in an inventor's next patent, we infer that the inventor has changed jobs in the intervening period. If there is a gap of more than one year between these patents, we assign the midpoint year as the year of the job change. We use these rules to create an indicator variable ($\overline{M}$) equal to one in the year that we observe an inventor switch jobs, and zero otherwise. We then collapse $\overline{M}$ to get the number of observed inventor moves at the state–year level, $\overline{M_{st}}$. We then use $\overline{M_{st}}$ as the dependent variable in the stacked regression model in Equation 1. The coefficient on *Enforceability* ($\hat{\overline{\beta}}$) is a biased estimate that captures the effect of enforceability

[42]We avoid assigning state of residence and CPC based on subsequent patents to avoid bias associated with endogenous mobility or technology class.

on mobility but also its effect on patenting.

To net out the effect on patenting, we simulate a placebo constant mobility rate, which (by construction) is unaffected by NCA enforceability but is also only *observed* conditional on patenting. Specifically, we assign a placebo job change $\dot{M}$ each year with $P(\dot{M}=1) = 0.08$.[43] We then construct "observed placebo" mobility $\tilde{\dot{M}}$ as an indicator equal to one in the midpoint year of the range $[t, t']$ if the inventor patents in year t and t' and $\dot{M} = 1$ in any year between t and t'. We collapse $\tilde{\dot{M}}$ to get the number of observed (placebo) inventor moves at the state–year level: $\tilde{\dot{M}}_{st}$. We use this measure as the dependent variable in the stacked regression model and obtain the coefficient $\hat{\tilde{\beta}}$. Because $\dot{M}$ is constant, $\hat{\tilde{\beta}}$ captures the effect of *Enforceability* on the likelihood of *observing* mobility due a change in the rate of patenting.

The difference between these two coefficients, $\hat{\beta} \equiv \hat{\overline{\beta}} - \hat{\tilde{\beta}}$, captures the effect of enforceability on inventor mobility, netting out the impact on the probability of observing it. Though it nets out this impact, the estimate will be noisy due to sampling variation in the assignment of placebo mobility $\dot{M}$. Thus, we repeat this process for 250 draws of $\dot{M}$: we use the median $\hat{\beta}$ across these draws as our point estimate of the effect of NCA enforceability on mobility. We bootstrap the entire process 1,000 times (each time resampling *inventors* who patent during the 1991–2014 time period with replacement) to obtain standard errors.

To summarize, our process is as follows:

1. Construct inventor panel between first and last patent 1970–2021[44]

2. Estimate (biased) effect on mobility using observed patents

   - If inventor patents in year t and t' with two different firm assignees, assign $\overline{M} = 1$ in midpoint year of $[t, t']$
   - Collapse $\overline{M}$ to state–year level: $\overline{M_{st}}$
   - Estimate effect of $Enforce$ on $\overline{M_{st}}$ in stacked regression, save $\hat{\overline{\beta}}$

3. Estimate effect on "placebo" (constant) mobility using observed patents

   - Assign placebo mobility $\dot{M}$ each year with $P(\dot{M} = 1) = 0.08$

[43] This 8% chance of switching jobs each year reflects the overall average rate of mobility that we observe among inventors in our sample period. As we show later, our results are similar using different placebo mobility rates.

[44] Note that we use the time period 1970–2021 to calculate mobility (since, e.g., an inventor may patent in 1990 and 1996, and would then be assigned a mobility date of 1993), but we perform estimation over the time period 1991–2014: the period over which we have data on NCA enforceability scores.

- If inventor patents in year t and t' and $\dot{M} = 1$ in any year between t and t', assign "observed placebo" mobility $\tilde{\dot{M}} = 1$ in the midpoint of $[t, t']$
- Collapse $\tilde{\dot{M}}$ to state–year level: $\tilde{\dot{M}}_{st}$
- Estimate effect of $Enforce$ on $\tilde{\dot{M}}_{st}$ in stacked regression, save $\hat{\dot{\beta}}$

4. Calculate $\hat{\beta} = \hat{\bar{\beta}} - \hat{\dot{\beta}}$
5. Repeat steps 2–4 250 times
6. Point estimate of effect of $Enforceability$ on inventor mobility = median$(\hat{\beta})$
7. Repeat steps 2–6 1,000 times, resampling inventors with replacement. Use the distribution of point estimates to obtain the standard error

Figure B4 illustrates how we obtain our point estimate on the effect of enforceability on job mobility. The vertical line indicates $\hat{\bar{\beta}}$: our estimate (-1.41) of *Enforceability* on observed mobility in the patent data. The density plot represents the distribution of $\hat{\dot{\beta}}$ obtained from our 250 draws of $\dot{M}$. The distribution is centered around the median (represented by the blue dotted line) of roughly -0.94 and does not cross zero. That is, even though we simulate a constant placebo mobility rate, the uniformly negative estimates of $\hat{\dot{\beta}}$ reflect the selection bias in observing it: the effect of enforceability on patenting. Our point estimate of the effect of enforceability on mobility is the difference between the gray dashed line and the blue dotted one.

We report the end result in Column 1 of Table 2. The point estimate (-0.45, $p = 0.055$)) is negative and implies that an average-sized enforceability increase leads to a 3.5% decrease in inventor mobility. Table B4 shows that this estimate is robust to different reasonable specification choices. We adjust the restriction on the number of patents an inventor must have to enter our sample (either two or eight patents, relative to our baseline of five), and we adjust the placebo inventor mobility rate we use (6% and 10%, relative to our baseline of 8%). In each case, the point estimate moves a bit but is qualitatively unchanged.[45]

Job Mobility (Secondary Measure): We use the Census Bureau's Job-to-Job Flows[46] (J2J) dataset to create a secondary measure of worker mobility in innovative industries. J2J contains information on the number of job-to-job changes,[47] the outcome of interest, by state,

[45] The point estimates increase in magnitude substantially when we restrict to inventors with at least 8 patents. We hesitate to over-interpret this difference, as this restriction drops nearly 70% of inventors.

[46] U.S. Census Bureau. (2023). Job-to-Job Flows Data (2000-2019). Washington, DC: U.S. Census Bureau, Longitudinal-Employer Household Dynamics Program, accessed on April 7, 2020 at https://lehd.ces.census.gov/data. Version R2019Q1.

[47] We measure job to job changes as new hires with no nonemployment spell or a short nonemployment spell.

year, quarter, sex, age group, and industry-of-origin, where industries are measured at the 2-digit NAICS level. We obtain data on total employment at the same level of aggregation using the Census Bureau's Quarterly Workforce Indicators[48] (QWI).

We define innovative industries based on the National Science Foundation's (NSF's) classification of high-technology industries (National Science Foundation, 2014). Since NSF classifies industries at the level of 4-digit 2002/2007 NAICS codes, we include all 2-digit NAICS industries that contain any 4-digit industries classified as innovative.

We estimate a regression comparable to Equation 1, with some minor changes. First, we use a 2-digit NAICS-state-block fixed effect (instead of state-block fixed effect), a year-by-quarter-by-block fixed effect (replacing the year-by-block fixed effect, since measures are reported at the quarterly level), and controls for sex and age group which define the bins in the J2J and QWI data. Second, we weight each observation by a state–industry's total employment (from QWI) in the baseline year (the first year in the block). Finally, whereas we estimate effects on patenting using a 10-year post-period, we estimate effects of enforceability on job mobility using a four-year post period window. We do this for statistical power: since the J2J data begins in the year 2000, using a 10-year window would leave us with only one block (since many of our treatment states' first law change occurred prior to 2000). Using a four-year window enables us to include two additional treatment states with law changes occurring after 2004.

We report our estimate of the effect of enforceability on job mobility in innovative industries in Column 2 of Table 2. We estimate a negative effect of NCA enforceability on the count of job to job changes that is highly significant ($p < .01$) and essentially identical in magnitude to our estimated effect on the mobility of inventors in Column 1.

New Business Formation and Startup Patenting: A longstanding literature posits that entrepreneurship spurs innovation (Chatterji et al., 2014), and NCA enforceability might affect the ability of new startup firms to form and be successful. Prior studies have indeed found that stricter NCA enforceability reduces rates of entrepreneurship (Jeffers, 2023; Marx, 2021; Starr et al., 2018). Additionally, stricter NCA enforceability could attenuate the "creative destruction" capacity of new firm entrants that do form (Schumpeter, 1942), for example by making it harder for startups with a successful idea to scale up and poach workers from incumbent firms.[49]

To measure the rate of new business formation, we use the Business Dynamics Statistics

[48] U.S. Census Bureau. (2023). Quarterly Workforce Indicators (1990-2022). Washington, DC: U.S. Census Bureau, Longitudinal-Employer Household Dynamics Program, accessed on April 7, 2020 at https://lehd.ces.census.gov/data. Version R2019Q1.

[49] On the other hand, NCA enforceability might "screen out" only the startups least likely to survive (as found by Starr et al. (2018)), which may be the startups least likely to innovate.

(BDS) dataset from the U.S. Census Bureau, which contains annual measures of establishment births and job creation from new establishment births. We use the BDS aggregated at the state by 2-digit NAICS level and restrict attention to innovative industries (as used in the job mobility analysis above). We estimate our baseline stacked difference-in-difference model given by Equation 1, except that we interact sector fixed effects with both the state–block and block–year fixed effects.

We report results in the remaining columns of Table 2. Columns 3 and 4 report estimates of the effects on new business formation and job creation by new businesses. We estimate that stricter NCA enforceability leads to a substantial decline in both the counts of new establishment openings (Column 3) and job creation from new establishment openings (Column 4). An average-sized increase in NCA enforceability leads to a 3.1% decline in new business formation, and a 7.0% decline in new job creation at new businesses. The larger magnitude for job creation implies that strict enforceability inhibits not just the formation, but also the growth of startups; this finding corroborates the model in Fallick et al. (2006) that NCAs make it harder for new firms that discover a good idea to quickly scale up by hiring workers from other companies.

5 (Why) Does Lower Inventor Mobility Cause Fewer Patents?

The movement of workers between employers and to startups facilitates the spread of knowledge between firms (Stoyanov and Zubanov, 2012; Poole, 2013; Kaiser et al., 2015), the enhancement of inventors' human capital through interactions with other inventors (Akcigit et al., 2018), the ability of startups with good ideas to quickly scale up production (Fallick et al., 2006), and the innovation-enhancing effects of spinoffs (Baslandze, 2022). Our results in Section 4.2 showed that strict NCA enforceability reduces inventor mobility across firms, potentially hindering these channels for new innovation. In this section, we conduct two tests to assess whether this decline in inventor mobility is, in fact, an important mediating factor explaining NCA enforceability's effect on patenting.

5.1 Is the Effect of Enforceability on Patenting Correlated with its Effect on Mobility?

We first examine whether NCA enforceability affects patenting the most where it also affects inventor mobility the most. We refine our unit of analysis to be the state–CPC–year level, and we separately estimate the effects of enforceability on patenting and inventor mobility for each CPC (technology class). In practice, estimating treatment effects for all CPCs would be noisy and unreliable due to sampling variability and technology classes with few inventors and patents. To guard against this issue, we 1) restrict attention to the 19 CPCs with at least 20 inventors in each state during the baseline period, on average, and 2) we remove CPCs with estimated effects on mobility or patenting that are more than two standard deviations away from the mean. If the correlation between these CPC-specific effects is positive, it is suggestive evidence that the effects of NCA enforceability on mobility partially explain the effects on patenting.

We show this correlation in Figure 3. The correlation is positive and the relationship is roughly linear; that is, the CPCs for which stricter NCA enforceability leads to the biggest reduction in inventor mobility are also the CPCs for which it leads to the biggest reduction in patenting. The slope of the regression line is 0.92 ($p = 0.007$), implying an essentially one-to-one relationship between NCA enforceability's effect on patenting and its effect on inventor mobility.

This analysis is suggestive and does not provide a definitive causal link: NCA enforceability could be affecting a third variable that causes both a decline in inventor mobility and patenting. Next, we directly test why lower inventor mobility leads to fewer new ideas.

5.2 Does NCA enforceability especially reduce innovation that draws on knowledge from other firms?

To develop new ideas, a firm can draw on the stock of knowledge accumulated through its own prior discoveries or on the stock of knowledge developed by other firms. Worker mobility facilitates the diffusion and transfer of knowledge across firms. If strict NCA enforceability attenuates this knowledge diffusion, then we should expect that it has a particularly negative effect on patents that draw on knowledge from *other firms*. In particular, we focus attention on other U.S.-based, private, non-university firms, which are those most likely to have workers whose knowledge would diffuse in the absence of NCAs.

To test this hypothesis, we measure each patent's share of backward citations that are to patents from other U.S.-based firms, as opposed to self-citations, citations to government/university sources, or citations to foreign entities. We aggregate the data to the state level and create a discrete variable partitioning the total patent count into the number of patents that cite at least one other U.S.-based firm and the number of patents that do not. We estimate the effect of NCA enforceability on these measures with the stacked difference-in-difference model in Equation 1.

We report the results in Table 3. In Column 1 the dependent variable is the number of patents that cite at least one other U.S. firm: the point estimate (-1.65) is negative and statistically significant. In Column 2 the dependent variable is the number of patents that cite no other U.S. firms: the point estimate (-0.56) is negative, but smaller in magnitude and statistically insignificant ($p = 0.36$).[50] We note that the difference between these coefficients is not statistically significant ($p = 0.29$). However, in Columns 3 and 4, we repeat these regressions restricting the sample period to the first five years after the focal state's NCA law change (instead of the full ten years). Under that restriction, the difference between the two coefficients becomes larger in magnitude (driven by an attenuated coefficient on patents citing no other U.S. firms) and statistically significant at the 10% level ($p = 0.072$). A larger difference in effect sizes in the first few years after a law change, relative to later years, could reflect that over time, enforceable NCAs slow down the development of ideas within firms and thus through cumulative effects also hinder the development of ideas drawing solely from "in–house expertise." However, other possibilities could also explain this pattern.

6 The Impact of NCA Enforceability on Economy-Wide Innovation

We have shown that increases in state-level NCA enforceability lead to less innovation in that state and that declining inventor mobility and associated knowledge diffusion is an important underlying mechanism. However, enforceability changes in one state could have spillover effects on innovation across state lines. If such spillover effects are present and economically meaningful, then our state-level estimates might misrepresent the effect of NCA enforceability on overall innovation.

The direction of these spillovers is *ex ante* ambiguous. On the one hand, these spillover effects might be *positive* if changes in NCA enforceability in one state reallocate innovation to other states. Inventors might move across state lines to escape NCAs (Marx et al., 2015) and subsequently patent ideas elsewhere that they otherwise would have discovered in their initial state. Such migration would lead our state-level analysis to *over-estimate* the impact of NCA enforceability on economy-wide innovation.[51] On the other hand, these spillover

[50]Though we see citations to other firms as primarily a measure of knowledge diffusion, Akcigit and Ates (2023) also argue that patents that only cite a firm's own prior patents may disproportionately reflect firms' strategic efforts to build a protective "thicket" around their existing technologies. With this distinction in mind, the findings in this table further bolster the interpretation that our effects of NCA enforceability on overall patenting reflect losses in innovation and not simply changes in firms' strategic decisions to protect ideas.

[51]In a similar vein, Akcigit et al. (2022) find that increases in state corporate tax rates lead to a large outflow of inventors to other states, causing a big reduction in state-level patenting but little change in overall patenting.

effects might be negative. Innovation is a cumulative process that results from the reuse, recombination, and accumulation of prior ideas (Murray and O'Mahony, 2007; Acemoglu et al., 2016; Liu and Ma, 2024). A slowdown in the discovery of ideas in one state could therefore have ripple effects that reduce subsequent innovation in other states. This scenario would lead our state-level analysis to *under-estimate* the effects of NCA enforceability on economy-wide innovation.

We conduct two analyses to shed light on the nationwide versus state-level effects of NCA enforceability. First, we examine whether technology classes whose geographic footprint exposed them to stricter NCA enforceability had differential rates of (nationwide) patenting over our sample period. Second, we examine whether state-level NCA law changes affect patenting in *other* states connected via the internal corporate networks of multi-state firms.

6.1 Effects of State-level Law Changes on Nationwide Patenting Within Technology Classes

For our first analysis of spillovers, we change the level of observations to the *technology class* (CPC) of patents. Inventors specializing in different technology classes (measured by CPC codes) are often clustered in different states for idiosyncratic reasons (Bell et al., 2019). As a result, CPCs with initial clusters in states that happened to experience subsequent increases in NCA enforceability had higher "exposure" to NCA increases than CPCs with initial clusters in states without changes (or states that decreased enforceability). This *CPC-level* exposure measure enables us to estimate the economy-wide effect of NCA enforceability on innovation that accounts for potential spillovers across state lines.

Formally, we measure the change in NCA exposure for CPC c at time t as:

$$\Delta Exposure_{c,t} = \sum_{s} \omega_{c,s,t} \Delta NCA_{s,t}, \tag{2}$$

where

$$\omega_{c,s,t} = \frac{\#Patents_{c,s,t-1}}{\#Patents_{c,t-1}}.$$

We partition our sample period into four sub-periods t: 1991–1996, 1997–2002, 2003–2008, and 2009–2014. Here, $\Delta NCA_{s,t}$ is the change in NCA Enforceability score for state s over sub-period t (i.e., the difference in NCA Enforceability from the beginning of the period to the end of the period). The weight $\omega_{c,s,t}$ captures, for a particular CPC c in sub-period t, the share of that CPC's patents over the prior sub-period that were applied for in state s.[52] We

[52] An example is illustrative. Consider CPC XYZ for the period 1991–1996. We calculate the number of XYZ's patents applied for from 1985 to 1990 in each of the 51 states. We divide by the total number of

use these shares to create CPC-specific average exposure to NCA enforceability changes over the sub-period. Thus, a CPC's change in NCA enforceability exposure, $\Delta Exposure_{c,t}$, is a weighted average of the change in NCA enforceability across all 51 states over the sub-period, where the weights correspond to the CPC's baseline state-specific patenting shares. We use the baseline (prior sub-period's) allocation of patenting across states since contemporaneous state-specific patenting is endogenous to NCA law changes.

We use this measure to estimate the effect of a change in a CPC's exposure to NCA enforceability on the change in the number of (citation-weighted or unweighted) patents applied for in that CPC:

$$\Delta Patents_{c,t} = \alpha + \beta \Delta Exposure_{c,t} + \gamma_{s(c),t} + \epsilon_{c,t}, \tag{3}$$

where $\Delta Patents_{c,t}$ is the annualized percent change in patents for CPC c between period $t-1$ and t, and $\gamma_{s(c),t}$ is a CPC section $\times$ sub-period fixed effect. (A CPC section is a broad classification of major technological fields.)

Figure 4 shows binned scatterplots of the relationship described in Equation 3, for citation-weighted (Panel (a)) and raw (Panel (b)) patent counts. There is a clear negative relationship in both plots, indicating that CPCs exposed to increases in NCA enforceability went on to have lower rates of patenting. Columns 1 and 2 of Table 4 report the regression estimates of $\hat{\beta}$ from Equation 3 for citation-weighted patents and patent counts, respectively; the estimated effects are economically meaningful and highly statistically significant ($p < .01$) in both cases.

The identifying variation in these regressions comes from differences in CPCs' patent shares across states in a baseline period (five preceding years). Interpreting $\hat{\beta}$ in Equation 3 as a causal effect requires an assumption that these shares are exogenous to other omitted variables that might affect CPCs' contemporaneous changes in patenting, similar to a canonical Bartik instrument (Goldsmith-Pinkham et al., 2020). We gauge the plausibility of this assumption in two ways. First, we account for CPCs' concentration of patenting activity across states. This could be important if variation in $\omega_{c,s,t}$ comes from variation in consolidation across CPCs and if, for example, more consolidated sectors happen to cluster in high-enforcing states. We control for each CPC's Herfindahl–Hirschman Index (HHI) of patent shares (sum of squares of patent shares across states); this specification thus holds constant CPCs' dispersion across states. As shown in Columns 3 and 4, including this control leaves our estimates essentially unchanged.

Second, we instrument $\omega_{c,s,t}$ with $\omega_{c,s,85-90}$: CPC c's patent shares in state s over the

XYZ's patents from 1985 to 1990 to create state-specific weights for XYZ.

period 1985–1990. That is, we keep the shares fixed in a baseline period before our sample window begins. As shown in Columns 5 and 6, our estimates are, if anything, slightly larger in magnitude.

Estimating the relationship between CPCs' patenting and exposure to NCA enforceability in first differences (rather than with fixed effects as in prior analyses) allows a more interpretable graphical exposition in the binned scatterplots in Figure 4. In Columns 7 and 8 of Table 4, however, we report estimates from fixed effects difference-in-difference regressions to more closely mirror the specifications in previous analyses. We modify Equation 3 to model the effect of CPCs' initial *level* of effective NCA exposure on subsequent counts of patents over the sub-period, and we additionally include a CPC fixed effect.[53] Using this approach yields very similar estimates to the first differences approach.

We can compare the results from this CPC-level analysis to our state-level results to estimate the size and direction of spillovers across state lines. Consider what each result implies would be the reduction in patenting within a typical CPC if every state experienced an average-sized enforceability increase (equal to 0.080 on the 0-to-1 scale). As reported in Section 3, $\hat{\beta}_1$ from Equation 1 implies that an enforceability increase of this size reduces a CPC's within-state (citation-weighted) patenting by 18.4% ($=exp(-2.56 * 0.080) - 1$). The estimate from the CPC-level analysis ($\hat{\beta}$ from Equation 3) implies that a nationwide enforceability increase of this size would reduce a CPC's *overall* citation-weighted patenting by 23% ($-2.88 * 0.080$)—an effect size that is (23/18.4 =) 25% *larger* in magnitude than the state-level CPC effect.That is, NCA enforceability increases in one state have *negative* spillover effects on innovation across state lines within the same technology class.[54]

These results suggest that changes in NCA enforceability may have an even larger effect on overall innovation than what our state-level estimates imply.

[53]The regression model is:

$$\#Patents_{ct} = \alpha + \beta Exposure_{ct} + \delta_c + \gamma_{s(c)t} + \epsilon_{ct}.$$

where $Exposure_{ct}$ is the CPC's effective NCA exposure score in the first year of the sub-period, $\#Patents_{ct}$ is the number of patents for CPC c over sub-period t, δ is a CPC fixed effect, and $\gamma_{s(c),t}$ is a CPC section by sub-period fixed effect. (A CPC section is a broad classification of major technological fields.) We estimate this model with a Poisson regression.

[54]This result provides further (indirect) evidence that our estimated negative effect of enforceability on patenting reflects a loss of innovation and not just firms' strategic decisions to substitute patents for other forms of protection. Strict NCA enforceability in one state may in theory make firms in that state less compelled to patent new ideas, but it would not affect this margin for firms in *other* states. Rather, firms in other states would only be affected by the negative externality of lower knowledge diffusion.

6.2 Cross-state Spillovers Within Firms

NCA enforceability increases in one state may create spillovers not only within technology classes, but also within *firms*. Firms' internal corporate networks are an important conduit for the diffusion of knowledge, even across large geographic distances (Argote and Ingram, 2000; Giroud et al., 2024), and innovations learned in an establishment often spread to other units in the same firm (Bloom et al., 2020). If higher NCA enforceability in one state leads to fewer new ideas in that state, this could hinder the innovation process not only for local firms, but also for establishments in other states connected to that state via their corporate network.[55] Moreover, whereas an NCA enforceability increase in a focal state might increase investment in R&D among a firm's locations in that state by alleviating hold-up concerns, this countervailing positive effect on innovation is not present for a firm's locations in other states.

To identify cross-state spillovers arising from within-firm linkages, we leverage the interstate networks of multi-unit firms. We generate a "weighted other-state enforceability" (WOSE) score for each focal state corresponding to a six-year window. For a focal state, we identify the set of firms that were the assignee to at least one patent in that state in the first year of the six-year window. The WOSE is a weighted average of the NCA score in the other 50 states (plus DC) in each year, where the weights represent the share of total patents across the other states assigned to firms in this set in the baseline year that occurred in each state.

More formally, for the six-year windows 1991-1996, 1997-2002, 2003-2008, and 2009-2014, we calculate:

$$WOSE_{s,t,\hat{t}} = \sum_{\sigma \neq s} w_{\sigma,s,\hat{t}} \cdot Enforceability_{\sigma,t},$$

where s indexes the focal state (i.e., the state in which changes in outcomes are observed), t indexes the year of observation, $\hat{t}$ indexes the baseline year (the first year in the six-year window), and $w_{\sigma,s,\hat{t}}$ is given by:

$$w_{\sigma,s,\hat{t}} = \frac{\displaystyle\sum_{f \in F(s,\hat{t})} PC_{f,\sigma,\hat{t}}}{\displaystyle\sum_{s' \neq s} \sum_{f \in F(s',\hat{t})} PC_{f,s',\hat{t}}}$$

[55] Another reason that firms' establishments in other states may innovate less is if firms strategically choose to reallocate their innovative resources to high-enforcing states. Firms might make this choice because high enforceability ensures they can most confidently recoup their investments in R&D, or alternatively because it allows them to pay lower wages (Johnson et al., 2023).

Here, the set $F(s,\hat{t})$ is the set of firms that patented in state s in year $\hat{t}$, and $PC_{f,s,\hat{t}}$ is the patent count of firm f in state s in year $\hat{t}$ (evenly dividing patents between inventors, as described in Section 2.1.2).[56] Similar to our analysis of spillovers within CPCs in Section 6, we estimate a difference-in-difference model, with the six-year windows entering as blocks, that also includes $WOSE_{s,t,\hat{t}}$ as an explanatory variable in addition to the focal state's NCA score. Formally, the model we estimate is:

$$Y_{s,t,\hat{t}} = \alpha_1 Enforceability_{s,t} + \alpha_2 WOSE_{s,t,\hat{t}} + \gamma_t + \phi_{s,\hat{t}} + \varepsilon_{s,t,\hat{t}}.$$

Table 5 reports regression estimates. The coefficients on the *NCA Score* (i.e., the NCA score in the focal state) are negative, highly significant, and similar to the effect sizes from our baseline state-level stacked design. The coefficients on *WOSE* are negative and statistically significant; when NCA enforceability increases (on average) in other states that are connected to a focal state via internal corporate networks, patenting in that focal state *decreases*. Note that although the magnitude of the coefficients on *WOSE* are larger in magnitude than the coefficients on (focal) *NCA score*, these coefficients are not directly comparable due to the smaller variation in *WOSE* compared to (focal) *NCA score*. An average-sized increase in *WOSE* leads to a 2.6% (2.1%) decline in citation-weighted (overall) patenting in the focal state.

This analysis corroborates the implications from our CPC-level analysis in Section 6 and provides a potential explanation for why the economy-wide effects of NCA enforceability we estimate in Section 6.1 are larger than what our state-level estimates imply. Given that internal corporate networks are an important conduit for knowledge transmission, the connections across states via these corporate networks could explain why fewer ideas developed in one state could lead to fewer ideas developed in other states, generating larger economy-wide effects. Furthermore, this evidence of spillovers within firms could explain why the estimated effect of NCA enforceability on patenting of publicly-traded firms (who have more sprawling corporate networks) in Section 4.1 was even larger than the state-level estimates in Section 3.

Overall, these results indicate that increases in NCA enforceability lead to lower economy-wide rates of patenting that are not limited to state boundaries, driven (at least in part) by spillovers within firms.

[56] In the CPC-level analysis presented in Section 6.1, we create the weights $\omega_{c,s,t}$ using several years of data prior to the beginning of the window. In contrast, here we construct the weights $w_{\sigma,s,\hat{t}}$ using the first year of the six-year window. We make this change because firms enter and exit the sample at different times (due to firm births, deaths, and changes in ownership), and we therefore do not have multiple years of data prior to $\hat{t}$ for many firms.

7 Conclusion

Prior literature has highlighted a tension between positive and negative ways that worker mobility could affect innovation: while mobility may encourage the spread and sharing of ideas, thus facilitating innovation, mobility may also discourage firms from making innovation-enhancing investments. Given this ambiguity, it is no surprise that academics and policy makers have fiercely contested whether NCAs—a common way that employers directly limit workers' mobility—enhance or stifle innovation.

We find that patenting diminishes by an economically meaningful amount when states make NCAs more easily enforceable. Using multiple quantitative and qualitative metrics, we show that this relationship reflects a true loss of innovation, rather than simply substitutions in the methods firms use to protect new ideas. We conduct secondary analyses to reconcile the motivating theoretical tension. Stricter NCA enforceability decreases mobility rates among workers in innovative industries, drives down rates of entrepreneurship, and causes an especially large decline in patenting by startups. Finally, we show that the state-level reductions in innovation do not simply reflect zero-sum effects via reallocation to other states; on the contrary, the economy-wide reductions in innovation extend beyond state lines, driven in part by negative spillovers of NCA enforceability on innovation within cross-state internal corporate networks.

We also find evidence that stricter NCA enforceability has a positive effect on publicly-traded firms' investment in R&D and other intangible assets. Even though investment is an important input in the innovation production function, we find that the net impact of NCA enforceability on innovation at those firms is still substantially negative. In theory, higher intangible investment could lead to other material benefits. However, given prior evidence that stricter NCA enforceability reduces workers' earnings (Johnson et al., 2023), leads to higher industrial concentration and prices for consumers (Hausman and Lavetti, 2021; Lipsitz and Tremblay, 2021), and is not demonstrably valued by firms (Hiraiwa et al., 2023), it is not clear what such a benefit could be.

At the same time, it is interesting that, in light of the evidence in this paper, many commentators still argue that firms need enforceable NCAs to stay competitive.[57] One possible way to rationalize these arguments is a tension between private and social optimality. It could very well be that it is privately optimal for a firm to use an (enforceable) NCA—for example, to ensure a greater return on intangible investments— regardless of whether their competitors are also using them. But, it could be that the slowed rates of interactions,

[57]For an outline of such arguments, see, e.g., the Chamber of Commerce's comment on the Federal Trade Commission's Notice of Proposed Rulemaking on the Non-Compete Clause Rule, available at https://www.uschamber.com/assets/documents/FTC-Noncompete-Comment-Letter_FINAL_04.17.23.pdf.

difficulties hiring, and other externalities from enforceable NCAs are so large that all firms would be more innovative if NCAs were unenforceable. Such externalities might be less salient or difficult to quantify for those who continue to argue for NCAs. This distinction between the private and social benefits of NCA enforceability is a fruitful area for future research.

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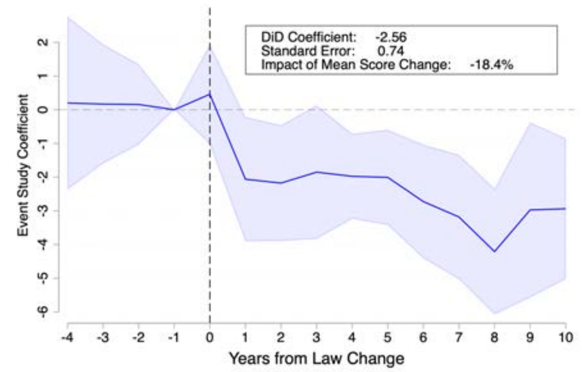
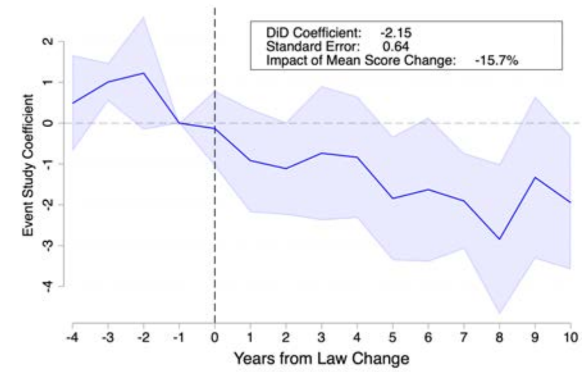
8 Exhibits

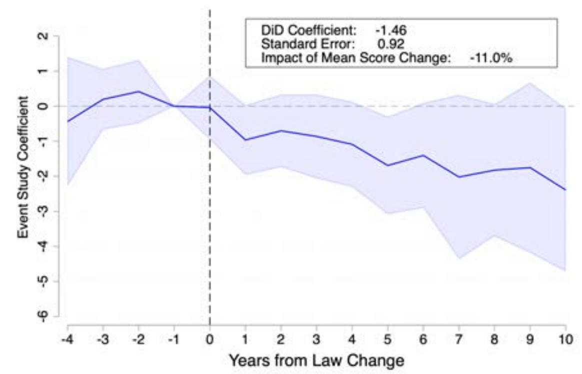
Figure 1: Event Study Estimates of the Effect of NCA Enforceability on State-level Patenting



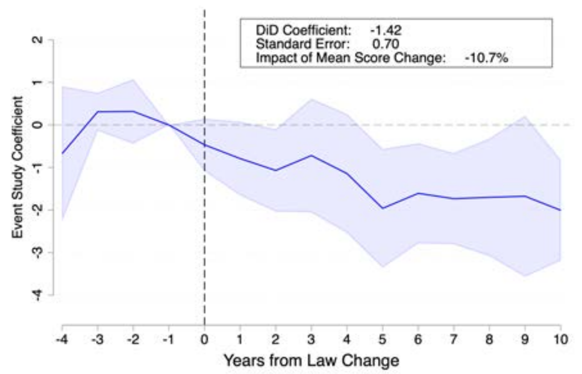
(a) Normalized Forward-Citation-Weighted Patent Counts - State CPC Year



(b) Normalized Forward-Citation-Weighted Patent Counts - State Year



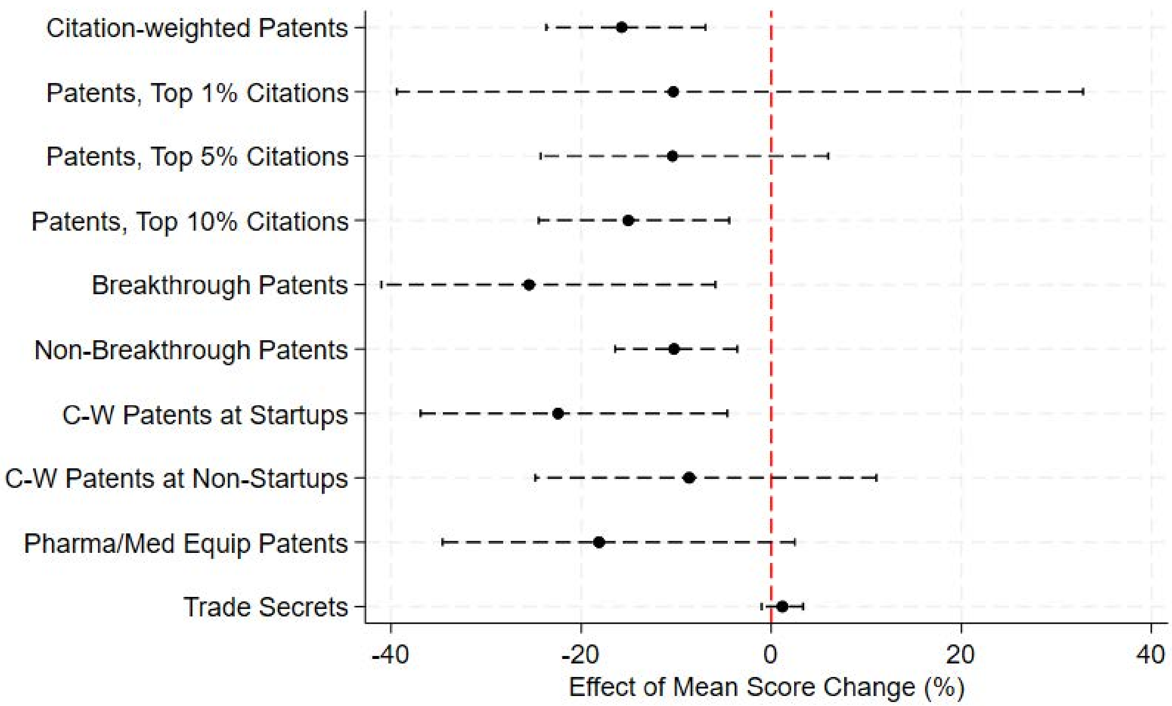
(c) Unweighted Patent Count - State CPC Year



(d) Unweighted Patent Count - State Year

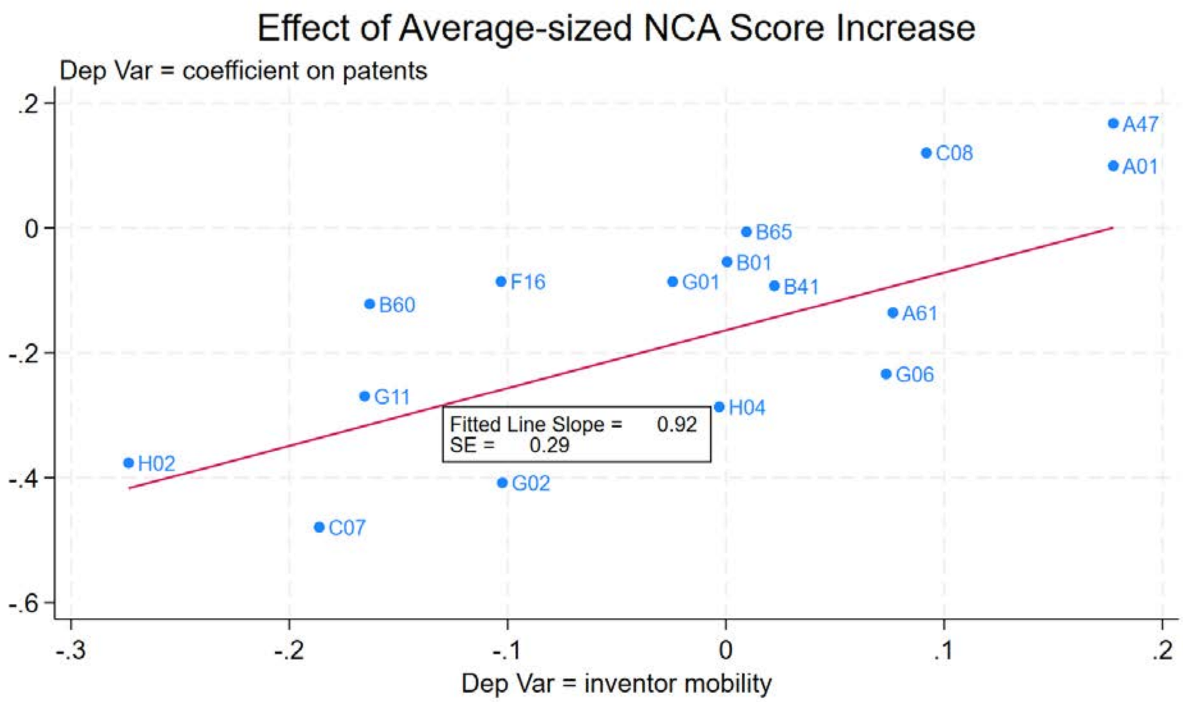
Notes. Each panel displays the coefficients and 95% confidence intervals from event-study Poisson psuedo-likelihood stacked difference-in-difference regression models, weighted by the count of normalized citation-weighted patents before the treatment year in each state in each subexperiment. See Equation 1 for an analogous regression equation. The dependent variables are forward-citation-weighted patent counts and unweighted patent counts in the top and bottom rows respectively; the level of analysis is the state by CPC by year level and the state by year level in the left and right columns, respectively. The stacked difference-in-difference coefficient and standard error, as well as the estimated impact of a mean score change on the relevant dependent variable, are reported on each plot.

Figure 2: The Effect of NCA Enforceability on Various Measures of "True" Innovation



Notes. The first nine rows display the estimated impact of a mean score change (and 95% confidence interval) from separate Poisson psuedo-likelihood regression models, weighted by the counts of normalized citation-weighted patents before the treatment year in each state in each subexperiment. See Equation 1 for details. The tenth row (Trade Secrets) displays the estimated impact of a mean score change (and 95% confidence interval) from a linear probability model. See 3.3 for details. The dependent variable for each regression is listed on the vertical axis. The dependent variables are: forward citation-weighted patents, the number of patents with forward citations in the top 1, 5, and 10% of their state-year distributions, respectively; the number of patents that are and are not considered "breakthrough" (from Kelly et al. (2021)); the number of citation-weighted patents generated by startups and non-startups, respectively; the number of citation-weighted patents, with the sample restricted to the pharmaceutical and drug/medical device sectors; and the use of valuable trade secrets by public firms (from Glaeser (2018)).

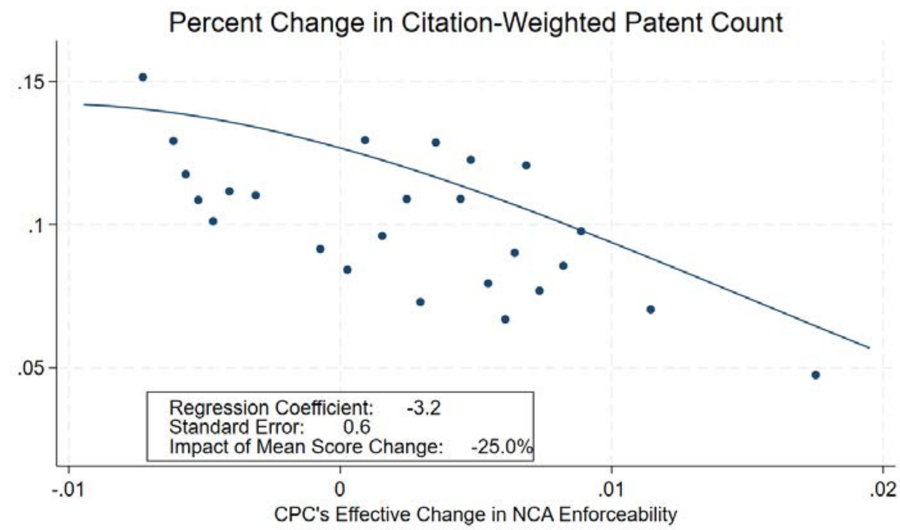
Figure 3: State-level NCA Enforceability Changes have the Largest Effect on Patenting in CPCs where it Also has the Largest Effect on Inventor Mobility



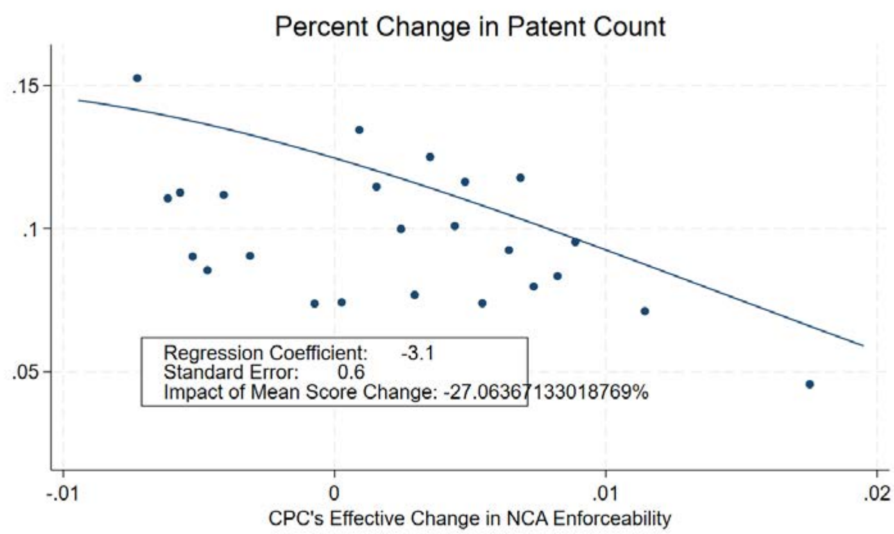
Notes: The figure displays a scatterplot in which a unit of observation is a CPC. The variable on the y-axis is the CPC-specific effect of NCA enforceability on patenting (at the state level). The variable on the x-axis is the CPC-specific effect of enforceability on inventor mobility (at the state level). The coefficients are estimated in the stacked regression model as described in Section 5. Each dot is weighted by the CPC's average number of inventors across all states in the baseline period. Next to each dot is the CPC code it represents.

Figure 4: CPCs More Exposed to NCA Enforceability Increases Experience Lower Rates of Patenting Nationwide

(a)



(b)



Notes: Each panel displays a binned scatterplot in which the unit of observation is a CPC–6-year-period. On the horizontal axis is $\Delta Exposure_{ct}$, a CPC's change in NCA exposure over the 6-year period, as defined in Equation 2. On the vertical axis is the annualized percent change in the number of (citation-weighted or raw) granted patents for that CPC over the sub-period, relative to the number of patents for that CPC over the prior sub-period. The values are residualized on CPC section–period fixed effects, where CPC sections are broad technology sectors. Scatterplots created with the **binsreg** package of Cattaneo et al. (2024).

Table 1: The Effects of NCA Enforceability on Firm-level Investment and Patenting

	(1) Intangible Investment	(2) Capital Investment	(3) Patent Counts	(4) Citation Weighted Patents	(5) Patents' KPSS Value
NCA score	.248***	-.0102	-3.93***	-4.82**	-3.22**
	(.0844)	(.0552)	(1.05)	(2.19)	(1.59)
Mean DV	0.190	0.061	20.8	18.9	322.6
Effect of Mean Change	10.4%	-1.3%	-26.8%	-31.8%	-22.6%
N	44,468	40,176	52,606	51,467	48,380

Notes. This table shows the impact of NCA enforceability on firm-level outcomes. Samples are comprised of publicly traded firms with at least one patent between 1991 and 2014. Results in Columns (1) and (2) are from an OLS model and results in Column (3) - (5) are from a Poisson pseudo-likelihood regression model. All regressions include firm and year × Census region fixed effects. Standard errors clustered at state level in parentheses. *** p <.01, ** p <.05, * p <.1.

Table 2: The Effect of NCA Enforceability on Job Mobility and Entrepreneurship

Dep Var =	(1) Number of Inventor Moves	(2) Number of Job-to-Job Flows	(3) Rate of Establishment Births	(4) Job Creation from Establishment Births
NCA Score	-.45*	-.47***	-.044*	-.051**
	(.22)	(.151)	(.023)	(.020)
N	2,700	167,928	29,700	29,700
Effect of Mean Change	-3.5%	-3.7%	-3.0%	-7.1%

Notes. Columns (1) and (2)—those with outcomes that are count variables—report estimates from Poisson pseudo-likelihood regression models. Those in Columns (3) and (4), with dependent variables as rates, report OLS models. The outcome variable in Column 1 is the number of instances of an inventor moving across employers in a state–year as observed in the PTO data; the procedure we use to obtain the estimate and standard error is described in Section 4.2. Outcomes in Columns (2)–(4) are measured for the subset of industries that the NSF classifies as "high tech." The outcome variable in Column (2), from the J2J dataset, is the number of job=to-job changes at the state-year level. The regression in Column (2) includes state $\times$ subexperiment, year $\times$ quarter $\times$ subexperiment, industry $\times$ subexperiment, sex, and age-group fixed effects. The outcome variables in Columns (3) and (4) are taken from BDS. The *establishment entry rate* is the number of new establishments formed in year t divided by the number of existing establishments averaged over years t and $t-1$. The *job creation rate* from new establishment formation is the count of employment gains from establishments that open in year t divided by the overall employment count averaged over years t and $t-1$. Regressions in Columns (3) and (4) include year $\times$ subexperiment and state $\times$ subexperiment fixed effects. Standard error clustered at state $\times$ subexperiment level in parentheses. *** p <.01, ** p <.05, * p <.1.

Table 3: NCA Enforceability Has an Especially Large Effect on Patents that Draw on Knowledge from Other Firms

	(1)	(2)	(3)	(4)
Dep Var =	Patents that Cite…			
	…**At Least One** Other U.S. firm	…**No Other** U.S. firms	…**At Least One** Other U.S. firm	…**No Other** U.S. firms
Sample period =	Full post-period		First five-years of post-period	
	(1)	(2)	(3)	(4)
NCA Score	-1.56**	-.753	-1.55**	-.315
	(.702)	(.63)	(.696)	(.599)
Mean Dep Var	677.909	385.598	610.470	384.401
Effect of Mean Change	-0.117	-0.058	-0.116	-0.025
N	2,700	2,700	1,800	1,800
p-value on difference =	.29		.072	

Notes. This table reports estimates from the state-level Poisson stacked regression model described in Equation 1. The dependent variable for each column is described in the table header. Standard errors clustered at state × subexperiment level in parentheses. *** p <.01, ** p <.05, * p <.1.

Table 4: The Effect of Exposure to NCA Enforceability on CPCs' Nationwide Patenting

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	Annualized Percent Change in:						Total Count of:	
	Citation-weighted patents	Unweighted patents	Citation-weighted patents	Unweighted patents	Citation-weighted patents	Unweighted patents	Citation-weighted patents	Unweighted patents
Δ NCA Exposure	-3.150***	-3.085***	-3.201***	-3.103***	-4.072***	-3.404***		
	(0.614)	(0.637)	(0.618)	(0.660)	(1.105)	(1.060)		
Initial NCA Exposure							-2.86***	-4.86***
							(0.98)	(1.69)
Mean of Dep Var	0.029	0.036	0.029	0.036	0.029	0.036	10734.011	11610.329
Effect of Mean Change	-25.0	-24.5	-24.5	-24.5	-32.4	-27.1	-20.3	-32.0
N	492	492	492	492	492	492	492	492
Section-year FE	Y	Y	Y	Y	Y	Y	Y	Y
Subsection FE	N	N	N	N	N	N	Y	Y
Specification	OLS	OLS	OLS	OLS	IV	IV	Poisson	Poisson

Notes: Columns 1–6 display estimates of β from Equation 3. The unit of observation is a CPC–6-year-period. Δ *NCA Exposure* is a CPC's change in NCA exposure over the 6-year period, as defined in Equation 2, and the dependent variable is the percent change in the number of citation-weighted (Column 1) or raw (Column 2) granted patents for that CPC over the 6-year period, relative to the number of patents in the prior 6-year period. Columns 3 and 4 additionally control for HHI of patent shares. In Columns 5 and 6, the shares used to construct the state-specific weights underlying Δ *NCA Exposure* are instrumented with 1985–1990 shares; see text for details. Columns 7 and 8 display estimates from a Poisson regression that is a modification to Equation 3, in which the dependent variable is the *count* of patents over the 6-year period, and *Initial NCA Exposure* is the CPC's effective NCA exposure in the first year of the 6-year period. Standard errors clustered at the 3-digit CPC (subsection) level in parentheses. *** p <.01, ** p <.05, * p <.1.

Table 5: NCA Enforceability Changes in One State Spill Over to Other States Via Firms' Internal Corporate Networks

	(1) Citation-Weighted Patent Count	(2) Patent Count
NCA Score	-3.28***	-3.07***
	(1.21)	(.974)
WOSE	-12.3**	-9.56**
	(5.86)	(4.4)
State × Block	Y	Y
Year × Block	Y	Y
Impact of Mean Score Change	-22.9%	-21.7%
Impact of Mean WOSE Change	-2.6%	-2.1%
N	1,224	1,224

Notes. This table reports coefficient estimates from Poisson pseudo-maximum likelihood regressions. The sample is constructed using six-year windows: 1991-1996, 1997-2002, 2003-2008, and 2009-2014. Each of the two columns reports results from a regression of the outcome on two variables: *NCA Score* and *Weighted other-state enforceability (WOSE)*, including window-state and year fixed effects. Standard errors clustered at state level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Online Appendix

A Data Construction

A.1 Patent Data Construction

Starting with the patent-assignee data from Patentsview, we first drop patents with multiple assignees, which comprises 3.2% of patents. We then match the patent to its inventor(s) and inventors' geographic location. We end up with 2,391,805 unique patents with applications between years 1991 to 2014. These patents are invented by 1,249,369 unique inventors, and assigned to 133,500 unique assignees.

Some patents have inventors living in different states. For our analysis that aggregates the patent-inventor-year level data to the state-year level, we assign each inventor on a patent an equal fraction of the patent (and the patent's weighted citations).

A.2 Linkage of USPTO Data to Other Data Sources

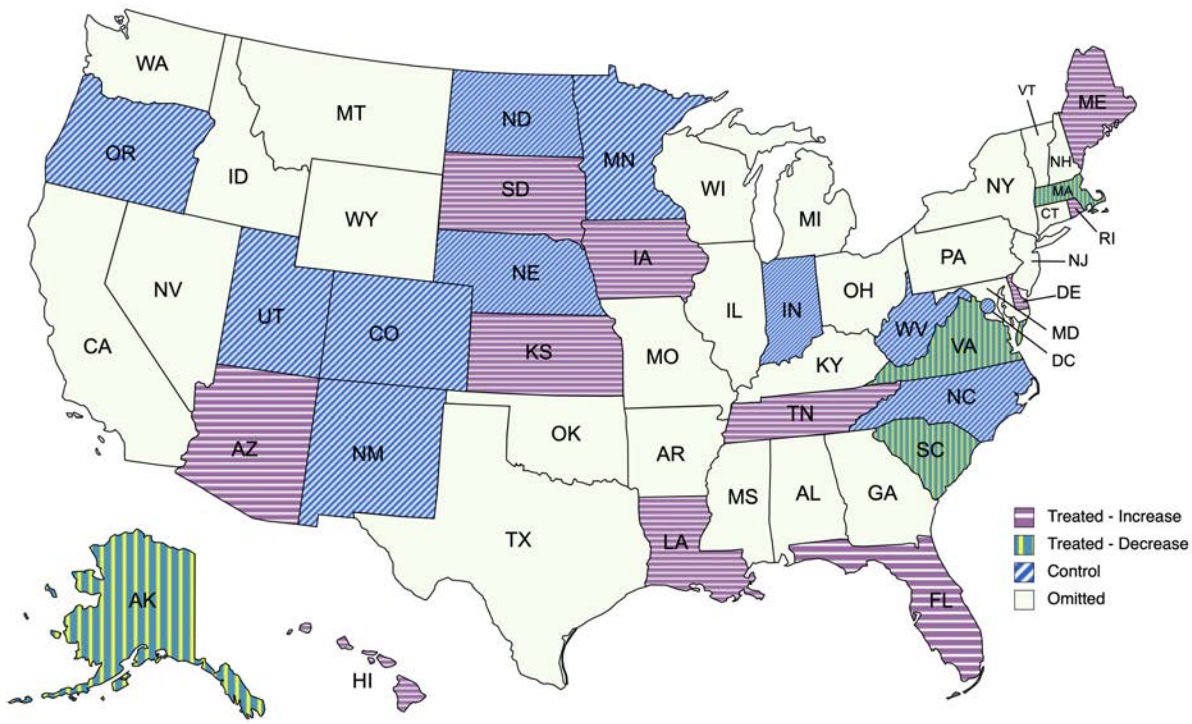
DISCERN and Compustat: To identify patents assigned to publicly-traded firms, we use the Duke Innovation & Scientific Enterprises Research Network (DISCERN) database created by Arora et al. (2021). DISCERN enables us to match patent assignees to publicly-traded firms and their subsidiaries from Compustat, while accommodating changes in corporate names and ownership structures. DISCERN extends the NBER 2006 patent dataset (Hall et al., 2001) from 1980 to 2015. By matching on patent IDs directly, we match 985,402 patents (41.2%) in our sample to GVKEYs provided by DISCERN, which allows us to further match to Compustat to obtain firm-level information.

Crunchbase: To identify patents assigned to startups, we utilize Crunchbase, an online database with business information on over 200,000 companies and 600,000 entrepreneurs. We first exclude the patents with assignees already matched to Compustat. Among the remaining patents, we conduct a fuzzy match between a patent's assignee in the USPTO data and firm names in Crunchbase, requiring that matched records have the same state and city. For the cases when a patent assginee is matched to multiple Crunchbase records, we further conduct a Levinstein string distance on their names again to keep the one with the smallest string distance. Crunchbase includes each firm's founding year, enabling us to calculate the age of a firm, as well as firms' IPO and M&A status. We define a patent as being assigned to a startup if the assignee company is 1) matched to Crunchbase 2) not acquired or IPOed; 3) is less than 10 years old relative to the patent application year. Using this approach, we identify 289,729 patents (12.1%) in our sample as startup patents.

Breakthrough patents: We take the measure of breakthrough patents from (Kelly et al., 2021), which can be directly linked to the UPSTO dataset using patent IDs. We define breakthrough patents as those that fall in the top 10 percent of the unconditional distribution of the "importance measure," where importance is defined as the ratio of the 5-year forward to the 5-year backward textual similarity to other patents, net of year fixed effects. (Kelly et al., 2021) calculate this textual similarity for patents granted 1840-–2010, which makes the above five-year measure valid for patents granted before 2005. Because we use patent *application* year in our analysis—which is years earlier than the grant year—we only include patents with an application year in or before 2000 to ensure our breakthrough measure is not truncated.

B Appendix Tables and Figures

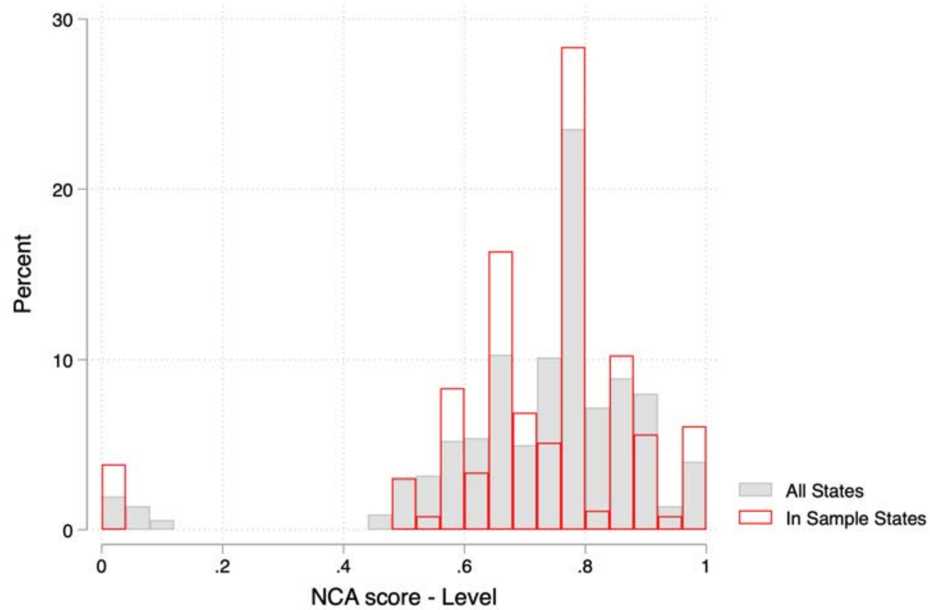
Figure B1: States Included in the "Stacked" Difference-in-difference Model



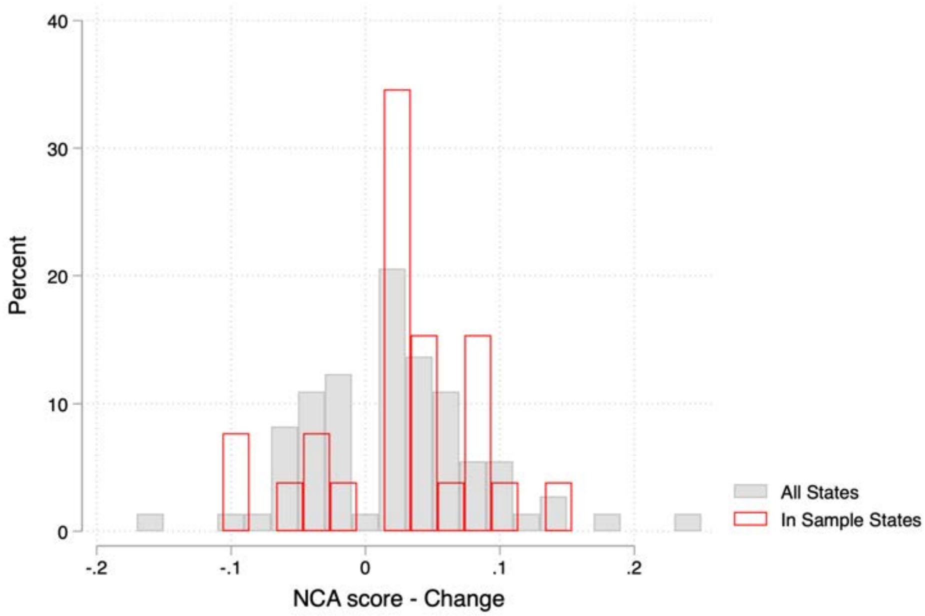
Notes. The control group consists of 11 control states, namely Colorado, the District of Columbia, Indiana, Minnesota, Nebraska, New Mexico, North Carolina, North Dakota, Oregon, Utah, and West Virginia. The treatment group includes 15 states, which are Alaska, Arizona, Delaware, Florida, Hawaii, Iowa, Kansas, Louisiana, Maine, Massachusetts, Rhode Island, South Carolina, South Dakota, Tennessee, and Virginia.

Figure B2: Distribution of NCA Scores Across States: All States vs. "In-Sample" subset

(a)



(b)



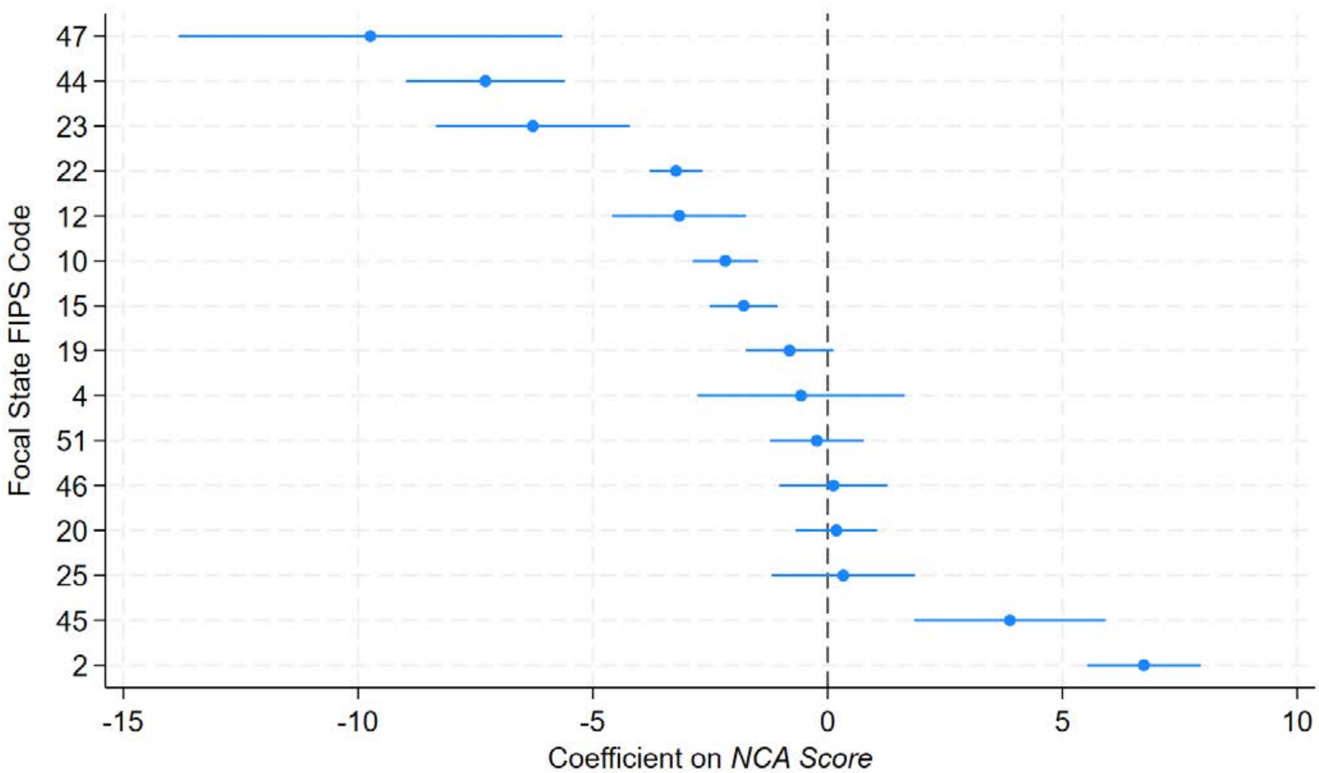
Notes. This figure shows a comparison of NCA score between all states and in sample states at state-year level from 1991 to 2014. Panel (a) is a histogram of score levels, with binwidth=0.04. Panel (b) is a histogram of score changes, with binwidth=0.02.

Table B1: The Estimated Effect of NCA Enforceability on State-Level Patenting is Robust to a Range of Potential Confounds and Specification Checks

	(1) Baseline	(2) Full Sample	(3) 1991 Weights	(4) Binary Changes	(5) Positive Changes Only
NCA Score	-2.56***	-5.14***	-2.89***		-4.25***
	(.738)	(1.01)	(.729)		(.678)
Binary Score				-.104**	
				(.0406)	
Mean of DV	10.14	11.37	14.13	10.14	10.02
N	246,798	297,497	172,373	246,798	240,949
	(6) Negative Changes Only	(7) OLS with log(CWP)	(8) Interact Region in FE	(9) TWFE Baseline	(10) TWFE Full Sample
NCA Score	-1.37	-1.43***	-3.18***	-1.98***	-3.08
	(.949)	(.326)	(.898)	(.274)	(2.07)
Mean of DV	10.41	1.19	10.62	13.41	24.43
N	231,910	248,925	227,887	19,787	78,401

Notes. In Column (4), the mean of binary changes in the sample is 0.05 and the standard deviation is 0.22. Standard errors in parentheses are clustered at the state-subexperiment level in Columns (1)–(8) and at the state level in Columns (9) and (10). *** p <.01, ** p <.05, * p <.1.

Figure B3: Effect of NCA Enforceability on State-level Patenting, by Focal State



Notes. The plot shows the point estimates and 95% confidence intervals on the coefficients on *Enforceability* from Equation 1, estimated separately for each of the 15 focal states in our estimation sample.

Table B2: Effect of NCA Enforceability on State-level Patenting, Controlling for the Inevitable Disclosure Doctrine and State Trade Secret Protection

	Baseline		IDD Control		IDXE6 Control	
	(1)	(2)	(3)	(4)	(5)	(6)
	Patent Count	C-W Count	Patent Count	C-W Count	Patent Count	C-W Count
NCA Score	-1.42**	-2.15***	-1.41**	-2.16***	-1.35*	-1.81***
	(.696)	(.637)	(.663)	(.631)	(.725)	(.682)
IDD			.00248	-.00514		
			(.0315)	(.0251)		
IDXE6					.0163	-.933***
					(.0982)	(.119)
N	2,700	2,700	2,700	2,700	2,532	2,532

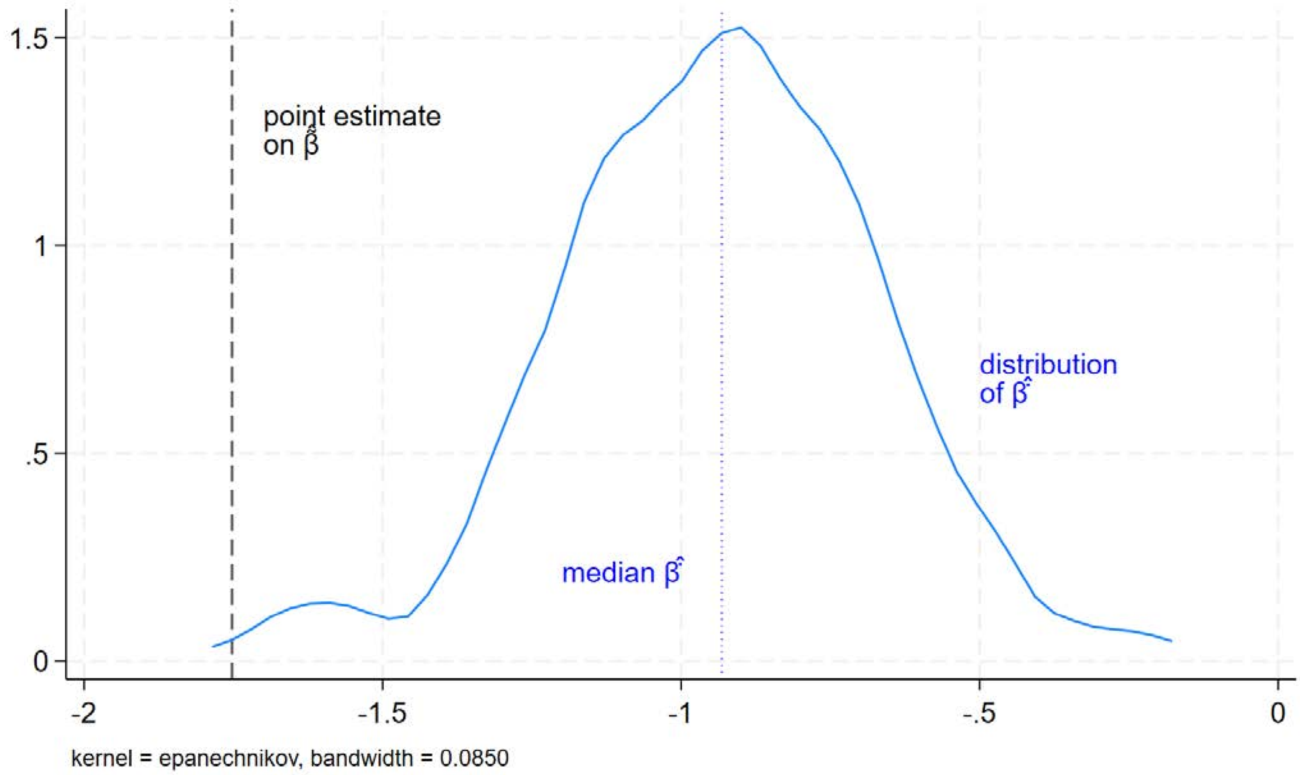
Notes. Columns 1 and 2 report the baseline estimates reported in Figure 1. Columns 3 and 4 report results from a model identical to the ones estimated in Columns 1 and 2, additionally controlling for whether the state follows the inevitable disclosure doctrine (IDD) in the relevant year, as reported by Castellaneta et al. (2016). Columns 5 and 6 report results from a model identical to the ones estimated in Columns 1 and 2, additionally controlling for an index (IDXE6) reporting the strength of trade secret protection in the state and year, as constructed in Png (2017a) and Png (2017b). Standard errors clustered at the state-subexperiment level in parentheses. *** p <.01, ** p <.05, * p <.1.

Table B3: The Effect of NCA Enforceability on Various Measures of State-level "True" Innovation

	(1) Baseline	(2) Top 1%	(3) Top 5%	(4) Top 10%
NCA Score	-2.15***	-1.37	-1.38	-2.05***
	(.637)	(2.52)	(1.08)	(.754)
N	2,700	2,700	2,700	2,700
Effect of Mean Change	-15.7%	-10.3%	-10.4%	-15.0%
	(5) Breakthrough	(6) Non-Breakthrough	(7) Startups' C-W Patents	(8) Non-Startups' C-W Patents
NCA Score	-3.7**	-1.36***	-3.19**	-1.14
	(1.5)	(.46)	(1.32)	(1.25)
N	1,332	1,332	2,700	2,700
Effect of Mean Change	-25.5%	-10.2%	-22.4%	-8.6%
	(9) Pharma/ Med Equip	(10) Trade Secrets		
NCA Score	-2.51*	0.127		
	(1.44)	(.12)		
N	5,250	30,438		
Effect of Mean Change	-18.1%	1.2%		

Notes. Columns 1–9 report estimates from Poisson pseudo-likelihood regression models, weighted by the count of normalized citation-weighted patents before the treatment year in each state in each subexperiment, and include year-subexperiment and state-subexperiment fixed effects. See Equation 1 for details. Column 10 reports the estimate from a linear probability model (since the dependent variable is binary), including firm and year-Census region fixed effects. See Equation 3.3 for details. In Column 1, the dependent variable is the baseline measure of forward citation-weighted patents. In Columns 2–4, the dependent variables are the numbers of patents with forward citations in the top 1, 5, and 10% of the state-year distributions. In Columns 5 and 6, the dependent variable is the number of state-year patents that are and are not considered "breakthrough" respectively. The measure of breakthrough patents is from Kelly et al. (2021); we restrict this analysis to patents with applications before 2000 to avoid truncation problems (see details in data appendix A.1). Therefore, the sample size in Columns 5 and 6 is smaller than in Columns 1–4. In Columns 7 and 8, the dependent variable is the citation-weighted patent count, restricting to startups and non-startups, respectively. In Column 9, the unit of observation is expanded to the state-sector-subexperiment-year, the dependent variable is the number of (citation-weighted) patents, and we restrict the sample to the pharmaceutical sector and the drug and medical device sector. In Column 10, the unit of observation is the firm-year (which accounts for the greater sample size), and the dependent variable is an indicator for whether the firm reported using trade secrets from Glaeser (2018). Standard errors clustered at the state-subexperiment level in parentheses, except for Column 10, which is clustered at the state level. *** p <.01, ** p <.05, * p <.1.

Figure B4: Estimating the Effect of NCA Enforceability on Inventor Mobility When Mobility is Only Observed Conditional on Patenting



Notes. The vertical line represents our estimate of the effect of NCA enforceability on the number of instances of observed inventor mobility, where "observed inventor mobility" is equal to one in the midpoint year between an inventor's two successive patents that contain different assignees. The sample is drawn from the set of inventors who patent at least five times over the period 1970–2021, and the estimate is from a stacked regression at the state–year level, as outlined in Equation 1. The density plot is the distribution of estimates on the effect of enforceability on 250 draws of "placebo" observed mobility, which is equal to one in the midpoint year between an inventor's successive patents if, in any year between those patents, a Bernoulli random variable representing placebo mobility $\dot{M}$ (with $Pr(\dot{M} = 1) = 0.08$) is equal to one. See Section 4.2 for details.

Table B4: Point Estimate of Effect of NCA Enforceability on Inventor Mobility: Sensitivity Analysis

Minimum Number of	**Placebo Mobility Rate**		
Patents Per Inventor	**6%**	**8%**	**10%**
2	-0.5	-0.44	-0.37
5	-0.49	-0.47	-0.41
8	-0.79	-0.78	-0.68

Notes. The table shows point estimates from various tweaks to our specification to estimate the effect of NCA enforceability on inventor mobility using the PTO data, as described in Section 4.2.

C Robustness Checks on the Effects of NCA Enforceability on State-level Patenting

Table B1 considers the sensitivity of our estimated effect of NCA enforceability on state-level patenting to a range of potential alternative specifications and other concerns. Column 1 represents our baseline estimate on state-level patenting (the unit observation is a state-block-year, and the regression is estimated based on Equation 1. In Column 2, we estimate the same model, except that we include the three treatment states with out-of-support baseline patenting trends (California, Washington and Vermont) that lacked a suitable control group. In Column 3, we estimate the baseline model except that we weight observations by a state's 1991 citation-weighted patent count, rather than the patent count in the block's four baseline years. In both cases, the coefficient is similar and, if anything, larger in magnitude.

Recent work has highlighted that using a continuous treatment variable in a difference-in-difference setting can yield magnitudes that are difficult to interpret (Callaway et al., 2021). In light of this issue, in Column 4 we replace our $Enforceability$ measure, a continuous variable (between 0 and 1), with a dichotomous variable. That is, for treated states experiencing an enforceability increase (decrease), we code this new variable to equal 1 (-1) in the years beginning with year 0. The variable is equal to 0 for treated states in the pre-period and for control states in all years. The coefficient is negative (-0.104) and statistically significant ($p = 0.010$). The implied effect of enforceability on patenting using this result is $exp(-0.104) - 1 = -9.9\%$, which is in the same direction as, but somewhat lower than, the magnitude using the continuous treatment variable.

An interesting question is whether enforceability increases and decreases have symmetric effects on patenting. In Columns 5 and 6, we estimate our baseline model but only consider blocks in which the treated state experiences a positive and negative enforceability change, respectively. In both cases, the estimates are negative and large in magnitude, indicating that increases in enforceability decrease patenting, and decreases in enforceability increase patenting. The estimate for negative changes is not quite statistically significant ($p = 0.148$), though this is not surprising since the sample size is smaller due to the fact that negative score changes only make up a third of law changes in our estimation sample.

The remaining columns consider other tweaks to our specification. In Column 7, we estimate our baseline model except that we use OLS and switch the dependent variable to be the log number of patents in a state-year. In Column 8, we again use Poisson but include region–block–year (rather than just block–year) fixed effects, so that we compare treated states only to control states in their same Census region. In Columns 9 and 10 we estimate the effect of enforceability using a two-way fixed effects regression instead of the stacked design, omitting California, Washington, and Vermont (Column 9) and not omitting them (Column 10). In all cases, the coefficient remains qualitatively similar to our baseline estimate and (with the exception of Column 10, in which $p = 0.136$) statistically significant.