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**ABSTRACT**

Worker mobility across firms can enhance innovation by spreading knowledge, but such mobility may also hinder innovation by making firms reluctant to invest in R&D. A common way that firms limit workers' mobility is with noncompete agreements (NCAs). We examine how the legal enforceability of NCAs affects innovation, as measured by patenting, using data on every state-level NCA enforceability change between 1991–2014. We find that making NCAs easier to enforce ("stricter" enforceability) substantially reduces the rate of patenting: an average-sized increase in NCA enforceability leads a state to have 16-19% fewer citation-weighted patents over the following 10 years. This effect reflects a true loss in innovation rather than a reduction in useless or strategic patents. We then reconcile these findings with contrasting theoretical predictions. Stricter NCA enforceability reduces job mobility and new business formation in innovative industries, suggesting slower knowledge spread. Within publicly-traded firms, stricter NCA enforceability increases investment, but still leads to less innovation, suggesting that any gains from enhanced incentives to invest are more than offset by other ways that NCAs slow down innovation. Finally, using variation in technology classes' exposure to NCA enforceability changes, we show that the economy-wide losses to innovation from strict enforceability are even larger than what our state-level estimates imply.

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# 1 Introduction

Innovation is essential for sustained economic growth (Jones, 2002). An open question is how the fluidity of the labor market affects the pace of innovation. The movement of skilled workers between firms facilitate interactions between inventors and knowledge spread, both of which are key to the development of 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; frequent inventor mobility could thus discourage firms from investing in R&D, potentially translating into lower rates of innovation.

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. NCAs by construction limit job mobility—and thereby limit associated inventor interactions and knowledge spread— potentially hampering 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 solving an investment hold-up problem that discourages firms from investing in R&D and workers' human capital (Rubin and Shedd, 1981; Grossman and Hart, 1986). While a large literature has examined aspects of this relationship, to date there is no definitive evidence that resolves this debate.

This paper provides comprehensive evidence that more stringent legal enforceability of NCAs reduces innovation, as measured by multiple measures of the quantity and quality of patenting. We: 1) use a dataset that measures NCA enforceability according to legal scholars and contains the universe of relevant legal changes; 2) distinguish between NCA

[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).

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 using a method that accounts for potential cross-state spillover effects.

The paper proceeds in three parts. First, we estimate how changes in NCA enforceability affect state-level patenting. We use a new dataset from Johnson et al. (2021) that quantifies the 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 quantify a summary measure of states' enforceability of NCAs. Changes to NCA enforceability over this period 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 enables 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 16-19% 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 percent 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. First, 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 changes in state-level patenting might not reflect changes in state-level innovation is that firms' decisions to patent a new idea is also a strategic choice. Because NCAs reduce the risk that a firm's ideas leak to its competitors, stricter NCA enforceability might make firms feel less compelled to patent new ideas without affecting the number of ideas they discover. To assess whether this strategic margin is behind our results, we focus on pharmaceutical and medical device industries, where the risk of reverse engineering leads firms to patent almost all new ideas (Cohen et al., 2000). Stricter NCA enforceability reduces patenting for these sectors by essentially the same as the overall reduction we estimate, suggesting a slowed pace of innovation above and beyond firms' strategic patenting choices.

How do our results square with the contrasting arguments that have characterized the debates regarding the relationship between NCA enforceability and innovation? In the second part of the paper, we revisit these arguments. We start with Gilson (1999)'s hypothesis (applied to California's Silicon Valley) that strict NCA enforceability slows innovation by stifling interfirm knowledge transfer and start-up vitality.[2] Consistent with Gilson's argument, we find that innovative industries experience less job mobility, lower new business formation, and an especially large drop in patenting among start-ups when states make NCAs more easily enforceable. To the extent that worker mobility across firms spreads tacit technological knowledge (Saxenian, 1994) and increases inventor interaction (Akcigit et al., 2018), and startups function as "engines of innovation" (Chatterji et al., 2014), these effects could partially explain why stricter NCA enforceability lowers overall innovation.

What about the counterargument that NCAs solve holdup problems (e.g., Grossman and Hart (1986)), raising firms' willingness to invest in R&D, training, and other inputs into innovation? Is this effect non-existent, or just dominated by countervailing 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 the holdup story, we find that stricter NCA enforceability leads firms to increase intangible investment but leaves physical investment unchanged.[3] 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 the countervailing effects of reduced knowledge transfer and inventor interaction.

In the third and final part of the paper, we consider how NCA enforceability affects the *economy-wide* effects of 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 NCA enforceability increases in one state simply reallocate innovative activity to other states. Anecdotes abound of technology workers leaving Route 128 in Massachusetts (a state that broadly enforces

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

[3]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.

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 concentration in patenting 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 should 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%. Compare this to our state-level estimates, which implied that the same-sized enforceability increase in a single state would lead a typical CPC's in-state patenting to decrease by 18.7%. That is, the state-level analysis slightly *under-estimates* the effect of enforceability on overall innovation, due to negative spillovers within technology classes across state lines.

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. One paper contemporaneous to ours, He (2021), analyzes the impact of several changes in NCA enforceability on rates of patenting and the value of patents, finding that stricter enforceability decreases those outcomes.[4]

Other studies have considered the role of firms' strategic decisions in this 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. Using a broad set of NCA law changes, Xiao (2022) finds somewhat contrary results 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.

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

[4]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.

issues that Barnett and Sichelman (2020) highlight have made it challenging to derive substantive conclusions from extant literature on this subject.Additionally, our test for the reallocation effect of NCA enforceability across state lines provides an interesting parallel to an adjacent literature on how migration decisions mediate the effects of taxes on innovation.[5] This approach also offers a methodological contribution: a similar approach could be used to examine the extent to which state-level taxation and other policies geographically reallocate economic activity.[6]

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 across 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), which results in inefficiently low innovation in 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 leads to a decrease in firms' investment.

# 2 Data and Empirical Methods

## 2.1 Main Datasets

To conduct our empirical analysis, we link panel data on state-level NCA enforceability with several patent, job mobility and business dynamics datasets. We briefly discuss these

[5]For example, Akcigit et al. (2022) 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.

[6]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.

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. (2021), 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. This database draws from Bishara (2010) (an authoritative legal expert on NCAs)[7] that identifies seven quantifiable dimensions governing the extent to which an NCA is enforceable.[8] 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.[9] Using these rules, Bishara (2010) quantified each dimension and an overall index for each state for the years 1991 and 2009. Hausman and Lavetti (2021) and Johnson et al. (2021) carefully replicate the approach in Bishara

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

[8] For example, one dimension (Q3a) indicates the extent to which employers are legally required to compensate workers that sign NCAs at the beginning of a job spell. Another dimension (Q8) reflects whether the NCA is enforceable when the employer terminates the employee who signed the NCA (as opposed to a voluntary separation).

[9] 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)

(2010) and extend the dataset for every year from 1991–2014.[10] Overall, there were 82 changes in NCA enforceability over this period, 90% of which arose through case law rather than statutory changes. Johnson et al. (2021) provide further details of the construction of this database, justifications of the cardinality of the index, as well as extensive institutional background and empirical evidence that within-state changes to NCA enforceability were orthogonal to underlying trends in economic, social, and political forces.

### 2.1.2 Data on Patents and Other Measures of 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.[11] For each patent, we obtain the name and address of the inventors and the assignees,[12] 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). 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.[13]

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 forward citations that the patent receives (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) or across technology

[10]Law students at Ohio State University and Duke University used Bishara's internal notes and the annual Malsberger treatises to construct the enforceability database.

[11]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 (e.g., Hall et al. (2001)).

[12]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.

[13]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.

class (some technologies 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 it by the average forward citation count in the focal patent's three-digit CPC code (Hall et al., 2005; Arora et al., 2023)[14] 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. We use the Census Bureau's Job-to-Job (J2J) Flows dataset to measure the mobility of workers across firms. We use the Census Bureau's Business Dynamics Statistics (BDS) to measure new business formation and use the Crunchbase dataset[15] to measure startup innovation performance. We use 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 (nonbinary) 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 first identify the subset of NCA law changes that

[14] 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.

[15] 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.

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,[16] the rapid pace of innovation in California generates a pre-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.[17]

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

[17]In robustness checks, we add these omitted treated states back into our analysis and, if anything, obtain even stronger results.

Figure A1 provides a visual representation of the specific states that meet the criteria to be included in our estimation sample. Figure A2 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, $\beta_1$, 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 (see, e.g., Cengiz et al. (2019)).

In some specifications, we amend Equation 1 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.

# 3 The Effect of NCA Enforceability on State-Level Innovation

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 A3) 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 0.081 (on a 0 to 1 scale). Thus, an increase in enforceability of average size induced a decrease in normalized citation-weighted patenting by 18.7% within CPC, and 16.0% 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 16-19% decline in citation-weighted patenting. A 16% reduction in patenting is comparable to the effect of: a 10 percent 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).[18] Another constructive comparison is Akcigit et al. (2022), who analyze the impact of personal income and corporate tax rates on innovation. They estimate that higher personal and corporate tax rates both 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).[19]

Table A3 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 and Washington); 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.

## 3.1 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 many patents generate little to no private value to firms (Hall et al.,

[18]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 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.

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

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"[20] (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.[21]

A second issue is that changes in NCA enforceability could affect firms' strategic decisions to protect new ideas, rather than affecting the creation of those ideas. Firms do not patent every new discovery: to apply for and maintain a patent can be costly,[22] and firms have other means to protect newly-developed trade secrets and other discoveries (Cohen et al., 2000).[23] If stricter NCA enforceability makes it harder for workers to move to competitors (and bring newly-discovered ideas with them), it 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 use the number of state-level (forward-citation–weighted) patents in the medical devices and pharmaceutical sectors[24] 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

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

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

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

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

[24]We define these two sectors based on CPC codes, using methods from Belenzon and Schankerman (2013).

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.

Figure 2 displays results that examine these two issues.[25] Rows (1) – (5) test whether NCA enforceability affects the rate of patenting for patents that are most likely to be valuable or innovative. Rows 1, 2 and 3 show that stricter NCA enforceability leads to a reduction 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 4 and 5 show that stricter NCA enforceability reduces both breakthrough and non-breakthrough patenting, though the magnitude is substantially (and statistically significantly) larger for breakthrough patents.[26]

Row 6 of Figure 2 considers normalized citation-weighted patent counts in the medical device and pharmaceutical sectors.[27] We find a large and negative effect on medical device and pharmaceutical patents, though the estimate is only statistically significant at the 10% level ($p = 0.08$).

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

# 4 Interpreting our Estimates in Light of Contrasting Theoretical Arguments

The paper's introduction described two arguments for how NCA enforceability could affect innovation. One side argues that NCAs stifle innovation by reducing the flow of ideas across

[25]Appendix Table A1 reports the regression output underlying this figure.

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

[27]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.

firms and the frequency and vitality of entrepreneurship (Gilson, 1999). Supporting this view is evidence that interactions between inventors—which NCAs limit—are critical for the discovery of new ideas Akcigit et al. (2018). On the other side is the argument that NCAs can enhance innovation by alleviating an investment holdup problem, increasing firms' incentives to invest in R&D and other knowledge inputs (Rubin and Shedd, 1981).

Our results thus far suggest that the former effect dominates the latter. In this section, we examine intermediate outcomes and conduct heterogeneity analysis to understand the extent to which these two contrasting arguments contribute to the aggregate effect of NCA enforceability on innovation.

## 4.1 NCA Enforceability, Job Mobility, and Startup Activity

Gilson (1999)'s argument that NCAs stifle innovation centers on the idea that strict NCA enforceability limits the movement of workers between employers and to start-ups, thereby limiting the spread of knowledge between firms. We directly test if NCA enforceability affects these intermediate outcomes.

*Job Mobility*: NCAs limit worker mobility by construction, and stricter NCA enforceability can reduce job mobility more broadly by slowing labor market churn and making it more costly for firms to post vacancies (Johnson et al., 2021). Prior work has shown that stricter NCA enforceability reduces job mobility (Johnson et al., 2021; Lipsitz and Starr, 2022; Balasubramanian et al., 2016; Jeffers, 2023). We build on this work by testing whether NCA enforceability affects job mobility in innovative industries, where the dynamic movements of workers are most likely to spur the discovery of new ideas.

We measure rates of worker mobility using data from the Job-to-Job Flows[28] (J2J) and Quarterly Workforce Indicators[29] (QWI) datasets compiled by the US Census Bureau. The

[28]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.

[29]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

datasets contain information on total employment, the number of employee separations, and the number of job-to-job changes,[30] by state, year, quarter, sex, and age group, as well as industry (for employment and separations) or industry-of-origin (for job to job changes). We combine and aggregate the datasets to the state-industry-year-quarter-sex-age group level, where industries are measured at the 2-digit NAICS level, and represent the industry-of-origin (rather than destination) for J2J data.

The outcomes of interest are the count and rate of both job-to-job changes and overall job separations, where rates are calculated as proportions of start-of-quarter employment. An advantage of focusing on job-to-job changes (from J2J) is that these are the types of job transitions most closely linked to NCAs, as such changes are likely due to on-the-job-search or firm poaching. An advantage of focusing on the overall separation rate (from QWI) is that it is a more comprehensive measure of worker mobility and labor market dynamism. 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 which contain any 4-digit industries classified as innovative.

We estimate a regression comparable to Equation 1, with some minor changes. First, we include an additional 2-digit NAICS-by-block fixed effect, a year-by-quarter-by-block fixed effect (replacing the year-by-block fixed effect), 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 in the baseline year. Finally, whereas we estimate effects on innovation 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

https://lehd.ces.census.gov/data. Version R2019Q1.

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

window enables us to include 2 additional treatment states with law changes occurring after 2004.

We report results in Panel A of Table 1. Column 1 shows that we estimate a negative effect of NCA enforceability on the rate of job to job changes, significant at the 10% level. The coefficient implies that an average-sized increase in NCA enforceability (0.081) leads to a 2.8% reduction in job-to-job changes (relative to the sample mean). In Column 2 we estimate the same specification, but with a Poisson regression on the *count* (as opposed to *rate*) of job changes: the estimated effect size ($-0.36$) is similar in magnitude (2.9% reduction for an average change), but is much more precise ($p < .01$). Column 3 hints at why using counts improves precision so much: stricter NCA enforceability has a negative (albeit noisy) effect on employment.[31] A change in both the numerator and denominator introduces noise into the impact on the rate variable, making it clearer to interpret the effect in Column 2. Finally, Column 4 shows that stricter NCA enforceability also negatively affects the overall separation rate by a similar magnitude as the more restricted job-to-job changes.

*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 giving incumbent firms superior access to human capital.

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

[31] This negative effect on employment could arise if, for example, stricter NCA enforceability expands firms' monopsony power.

To investigate whether NCA enforceability affects innovation performance among startups that do form, we measure the count of state-level patents in which the assignee is a startup. To identify whether an assignee is a startup, we conduct fuzzy linking between USPTO and 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.

We report results in Panel B of Table 1. Columns 5 and 6 reports estimates of the effects on new business formation. We estimate that stricter NCA enforceability leads to a substantial decline in both the counts of new establishment openings (Column 5) and job creation from new establishment openings (Column 6). An average-sized increase in NCA enforceability leads to a 3.0% decline in new business formation, and a 7.6% decline in new job creation at new businesses. Column 7 reports our estimate that stricter NCA enforceability reduces the number of state-level patents for which the assignee is a startup: the coefficient is negative, large in magnitude, and highly statistically significant ($p < .01$). Column 8 provides a basis for comparison: at the state level, the impact of NCA enforceability on citation-weighted patenting for all other companies is approximately half that for startups, though the coefficient is more noisily estimated.

The movement of workers between employers and to startups facilitates the spread of knowledge across firms and the interactions between inventors. By limiting such movements, Gilson (1999) argued that strict NCA enforceability slows down innovation. Our results in this section provide evidence that supports this argument.

## 4.2 NCA Enforceability, Investment, and Patenting Within Publicly-Traded Firms

Even if stricter NCA enforceability reduces overall innovation by slowing down job mobility and entrepreneurship, it could in theory increase innovation within incumbent firms by alleviating investment hold-up problems (Shi, 2023; Jeffers, 2023).

We examine this idea by testing 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[32] and physical investments.[33] To measure firm-level patenting, we use the DISCERN database (Arora et al., 2021), which links patents from the USPTO to Compustat.

To measure the NCA enforceability that a given firm faces, we must address the fact that most publicly-traded firms operate in multiple states. Since NCA enforceability is determined by state employment law, the most 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, 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 $\omega_{is}$. The score therefore varies over time (as states change their laws), though the weights do not (to avoid endogenous selection of firms into states).

Since we measure firms' exposure to NCA enforceability as a weighted average across

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

[33] Capital investment less the sales of property (capxv-sppe) and scale by one-year lagged total assets (at). As with intangible investment, we topcode at the 99$^{\text{th}}$ percentile.

states, we cannot use the stacked design used thus far. Instead, we estimate the effect of NCA enforceability on firm-level investment and patenting using the following regression:

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

where $\rho$ and $\iota$ are region-year and firm fixed effects, respectively.

We report results in Table 2. 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 that an average-sized increase in NCA enforceability leads intangible investment to increase by 8.1% ($p = 0.035$). However, we estimate essentially no effect of NCA enforceability on capital investment.[34] These estimates suggest that stricter NCA enforceability may increase firms' incentives to invest in R&D.

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 28.4% percent decrease in patent counts and 32.7% percent decrease in citation-weighted patent counts.[35] 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

[34]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.

[35]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. We note that 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 A3.

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 28.5% decline in patent counts weighted by this measure of financial value ($p = 0.046$).

The negative effects of NCA enforceability on innovation are far-reaching, affecting both startups and publicly-traded firms, and even occurring *within firms*. Additionally, this section helps reconcile the conflicting theoretical predictions of this relationship: stricter NCA enforceability might indeed increase firm-level investment, but this increase in investment is swamped by the other ways that NCA enforceability hinders innovation.

# 5 The Economy-Wide Impact of NCA Enforceability on Innovation

The results in Section 3 indicate that increases in state-level NCA enforceability lead to less innovation in that state, and Section 4 offers evidence of mechanisms underlying this effect. 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.

On the one hand, these spillover effects might be *positive* if changes in NCA enforceability in one state reallocate innovation to other states. In a similar context, 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 (Akcigit et al., 2022). In our context, 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 effects would lead our state-level analysis to *over-estimate* the impact of NCA enforceability on economy-wide innovation.

On the other hand, these spillover effects might be negative. If firms value high NCA enforceability,[36] then multi-state firms might reallocate their internal resources *toward* high-enforcing states. Moreover, innovation is a cumulative process and is the result of the reuse, recombination, and accumulation of prior ideas (Murray and O'Mahony, 2007). A slowdown in the discovery of ideas in one state could thus have ripple effects that reduce subsequent innovation in other states within the same technology class. This scenario would lead our state-level analysis to *under-estimate* the effects of NCA enforceability on economy-wide innovation.

To better understand the economy-wide effects of NCA enforceability, we examine whether *technology classes* whose geographic footprint exposed them to stricter NCA enforceability had differential rates of patenting over our sample period. For idiosyncratic reasons, inventors specializing in different technology classes (measured by CPC codes) might be clustered in different states (Bell et al., 2019). As a result, CPCs with initial clusters in states that experienced 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 broader effect of NCA enforceability on innovation that accounts for potential spillovers across state lines.

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

$$\Delta Exposure_{ct} = \sum_{s} \omega_{cst} \Delta NCA_{st}, \tag{2}$$

where

$$\omega_{cst} = \frac{\#Patents_{cst-1}}{\#Patents_{ct-1}}.$$

We partition our sample period into four sub-periods $t$: 1991–1996, 1997–2002, 2003–2008, and 2009–2014. Here, $\Delta NCA_{st}$ is the change in NCA Enforceability score for state $s$ over sub-period $t$. $\omega_{cst}$ captures, for a particular CPC $c$ in sub-period $t$, the share of that CPC's

[36]This might occur because, for example, high enforceability enables firms to pay lower wages (Johnson et al., 2021). Marx (2021) finds that higher NCA enforceability leads to an increase in firm valuation.

patents over the prior sub-period that were applied for in state $s$.[37] We use these shares to create CPC-specific changes in NCA enforceability exposure over the sub-period. Thus, a CPC's change in NCA exposure, $\Delta Exposure_{ct}$, 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:

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

where $\Delta Patents_{ct}$ is the annualized percent change in patents for CPC $c$ between period $t-1$ and $t$, and $\gamma_{s(c)t}$ is a technology class $\times$ sub-period fixed effect, where technology classes are broad categories of CPCs.

Figure 3 provides 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. Table A2 reports the regression estimates of $\hat{\beta}$ from Equation 3; the estimated effect is economically meaningful and highly statistically significant ($p < .01$) in both cases.

Estimating this 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 3. In Columns 3 and 4 of Table A2, we report estimates from fixed effects difference-in-difference regressions to

[37] An example is illustrative. Consider CPC XYZ for the period 1991–1996. We calculate the number of XYZ's patents applied for between 1985–1990 in each of the 51 states. We divide by the total number of XYZ's patents 1985–1990 to create state-specific shares for XYZ.

more closely mirror the specifications in our state-level analysis. 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.[38] Using this approach yields essentially identical estimates as 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 0.081 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 18.7% ($=exp(-2.56 * .081) - 1$). 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% ($-2.84 * .081$)—an effect size that is 23% *larger* than the state-level effect.That is, NCA enforceability increases in one state have *negative* spillover effects on innovation across state lines within the same technology class.

These results indicate that increases in NCA enforceability lead to lower economy-wide rates of patenting that are not limited to state boundaries. Moreover, they suggest that changes in NCA enforceability may have an even larger effect on overall innovation than what our state-level estimates imply.

# 6 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,

[38]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 $t$, and $\delta$ is a CPC fixed effect. We estimate this model with a Poisson regression.

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 diminished by an economically meaningful amount when states made 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.

We find some evidence that stricter NCA enforceability has a positive effect on publicly-traded firms' investment in R&D and other intangible assets. However, investment is not a socially valuable outcome unto itself. 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., 2021), 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 hard to think of an economic actor that is evidently made better off when NCAs are more easily enforceable.

At the same time, it is interesting that, in light of the evidence in this paper, many still argue that firms need enforceable NCAs to stay competitive.[39] One possible way to

[39]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.

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, 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 has important implications for policy discussions.

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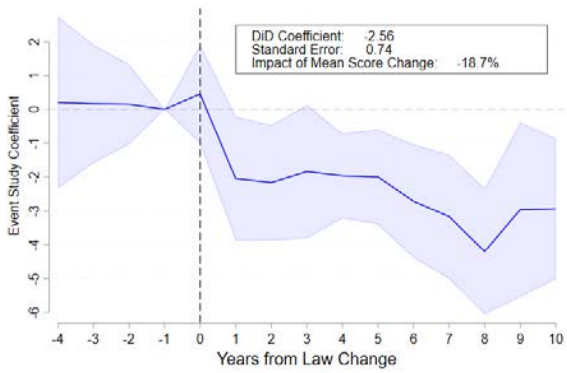
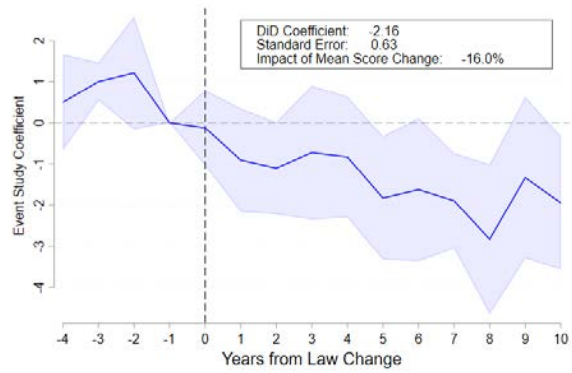
# 7 Exhibits

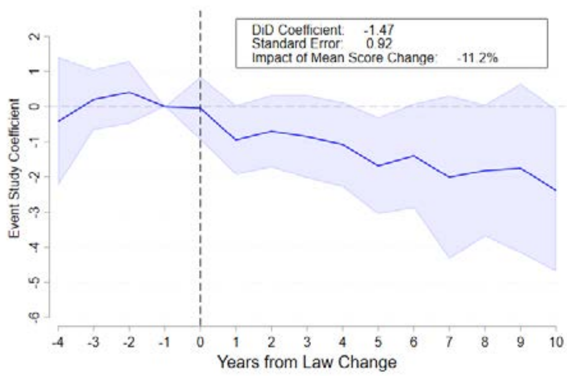
Figure 1: Event Study Estimates of the Effect of NCA Enforceability on State-level Patenting



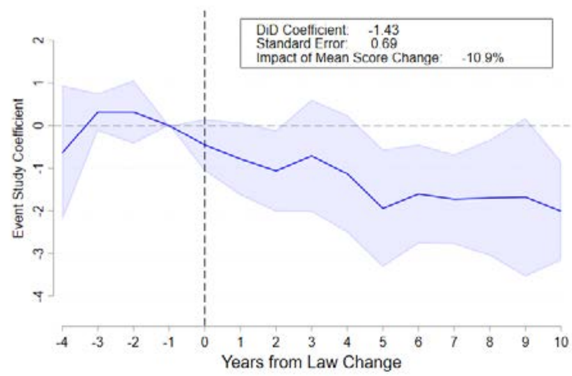

(a) Normalized Forward-Citation-Weighted Patent Counts - State CPC Year




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



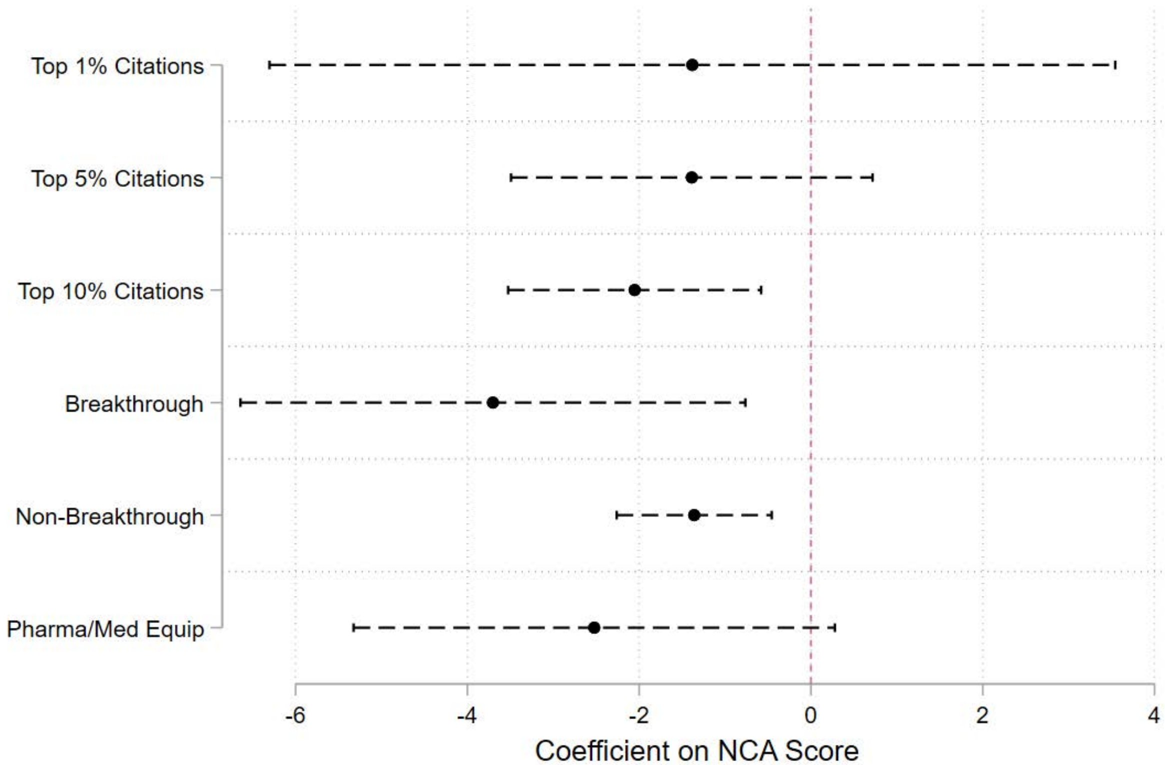

(c) Unweighted Patent Count - State CPC Year




(d) Unweighted Patent Count - State Year

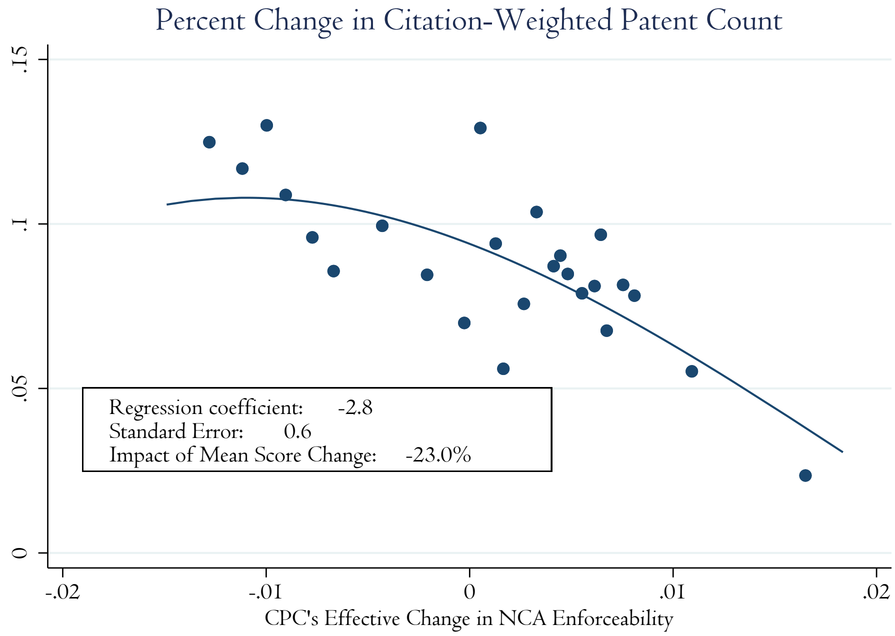
*Notes.* Each panel displays the coefficients and 95% confidence intervals from event-study Poisson psuedo-likelihood stacked difference-in-difference regression models, weighted by the count of normalized citation-weighted patents before the treatment year in each state in each subexperiment. See Equation 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

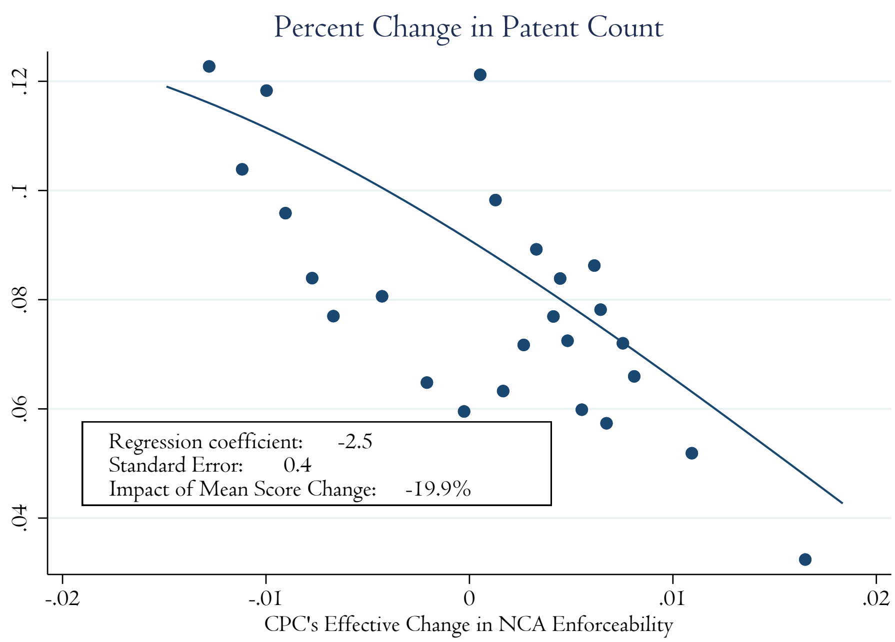



*Notes.* Each row displays the coefficient and 95% confidence interval from a separate Poisson psuedo-likelihood regression model, weighted by the count of normalized citation-weighted patents before the treatment year in each state in each subexperiment. See Equation 1 for details. The dependent variable for each regression is listed on the vertical axis. The dependent variables are: the number of state-year patents with forward citations in the top 1, 5, and 10% of the distribution, respectively; the number of state-year patents that are and are not considered “breakthrough” (from Kelly et al. (2021)); and the number of (citation-weighted) patents, with the sample restricted to the pharmaceutical and drug/medical device sectors.

Figure 3: CPCs More Exposed to NCA Enforceability Increases Experience Lower Rates of Patenting




(a) Forward-Citation-Weighted Patent Count




(b) Unweighted Patent Count

*Notes:* Each panel displays a binned scatterplot in which the unit of observation is a CPC–5-year-period. On the horizontal axis is $\Delta Exposure_{ct}$, a CPC's change in NCA exposure over the 5-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.

Table 1: The Effect of NCA Enforceability on Job Mobility and Entrepreneurship

| | (1) J2J Changes (Rate) | (2) J2J Changes (Count) | (3) Employment (Count) | (4) Separation (Rate) |
|---|---|---|---|---|
| NCA Score | -.0215* | -.36*** | -.236 | -.0715*** |
| | (.0124) | (.134) | (.184) | (.0168) |
| Mean DV | 0.062 | 234.0 | 4970.2 | 0.235 |
| Effect of Mean Change | -2.8% | -2.9% | -1.9% | -2.5% |
| N | 167,845 | 167,928 | 167,848 | 167,045 |
| | (5) Establishment Entry Rate | (6) Job Creation Rate | (7) Startups' C-W Patents | (8) Non-Startups' C-W Patents |
| NCA Score | -.49* | -.565** | -2.54*** | -1.25 |
| | (.256) | (.218) | (.923) | (1.11) |
| Mean DV | 1.3 | 0.6 | 65.0 | 328.7 |
| Effect of Mean Change | -3.2% | -7.2% | -18.6% | -9.6% |
| N | 2700 | 2700 | 2700 | 2700 |

Standard error clustered at state × subexperiment level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes.* This table reports the effect of NCA enforceability on job mobility (Panel A) and entrepreneurship (Panel B). Columns (1), (4), (5) and (6)—those with outcomes that are rate variables—report estimates from OLS models. Columns (2), (3), (7) and (8)—count variables—report estimates from Poisson pseudo-likelihood regression model. The outcome variables in column (5) and (6) 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 Panel A include state × subexperiment, year × quarter × subexperiment, industry × subexperiment, sex, and age-group fixed effects. Regressions in Panel B include year × subexperiment and state × subexperiment fixed effects.

Table 2: 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 | .190** | -.0227 | -4.13*** | -4.88** | -4.15** |
| | (.088) | (.052) | (1.03) | (2.22) | (2.08) |
| Mean DV | 0.190 | 0.060 | 20.3 | 18.4 | 314.6 |
| Effect of Mean Change | 8.1% | -3.1% | -28.4% | -32.6% | -28.6% |
| N | 45,747 | 41,337 | 53,987 | 52,798 | 49,637 |

Standard errors in parentheses

Standard error clustered at state level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes.* This table shows the impact of NCA enforceability on firm-level outcomes. Samples comprised of publicly traded firms with at least one patent during the period of 1991 to 2014. Results in column (1) and (2) are from OLS and results in column (3) - (5) are from a Poisson pseudo-likelihood regression model. All regressions include firm and year × Census region fixed effects.

# A Data Appendix

## 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 prior to 2001 to ensure our breakthrough measure is not truncated.

# B Appendix Tables and Figures

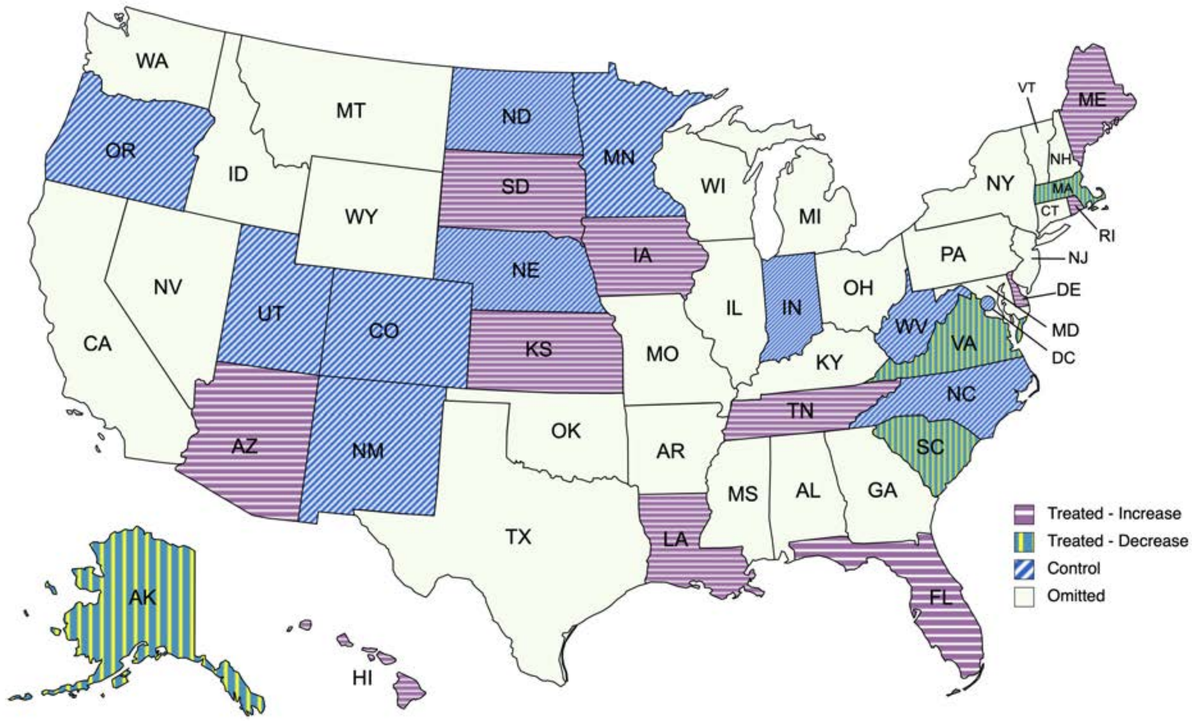
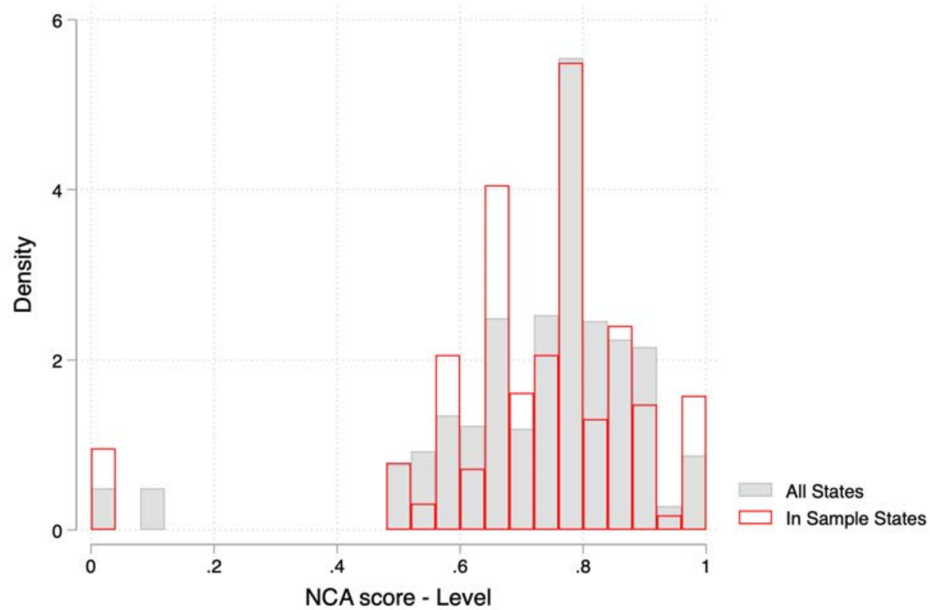



Figure A1: States Includes in the "Stacked" Difference-in-difference model

*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.

(a) NCA score levels




(b) NCA score changes

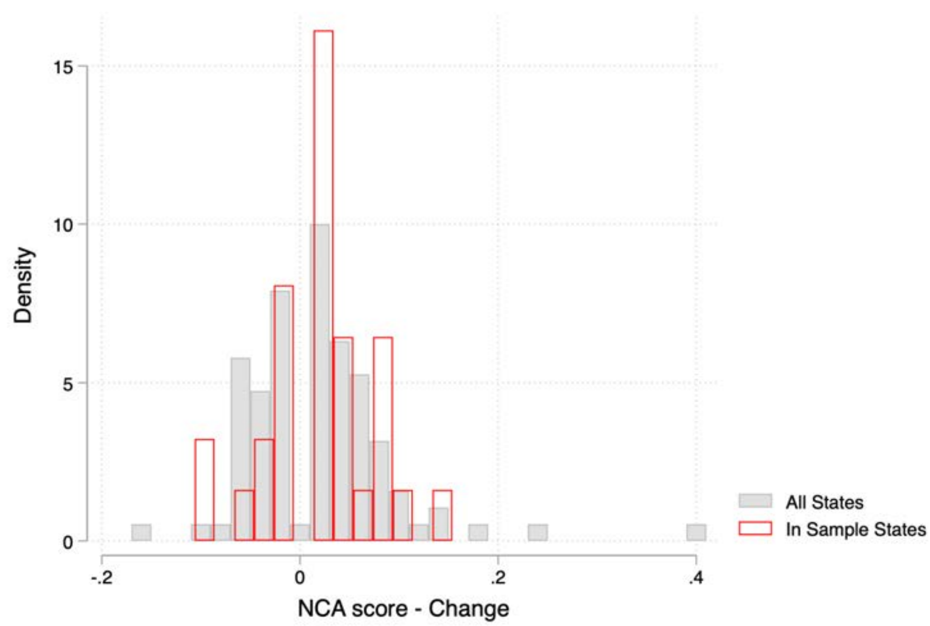



Figure A2: The Distribution in NCA Scores Across states, 1991–2014 (in Levels and Changes): all states and the "in-sample" subset

*Notes.* This figure shows a comparison of NCA score between all states and in sample states at state-year level. 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 A1: The Effect of NCA Enforceability on Various Measures of "True" Innovation at the State-level

| | (1) Top 1% | (2) Top 5% | (3) Top 10% |
|---|---|---|---|
| NCA Score | -1.38 | -1.39 | -2.05*** |
| | (2.51) | (1.07) | (.752) |
| Mean DV | 9.9 | 51.6 | 105.8 |
| Effect of Mean Change | -10.6% | -10.6% | -15.3% |
| N | 2700 | 2700 | 2700 |
| | (4) Breakthrough | (5) Non-Breakthrough | (6) Pharma/Med Equip |
| NCA Score | -3.70** | -1.36*** | -2.52* |
| | (1.50) | (.460) | (1.43) |
| Mean DV | 169.3 | 747.3 | 33.94 |
| Effect of Mean Change | -25.9% | -10.4% | -18.5% |
| N | 1332 | 1332 | 5250 |

Standard error clustered at state × subexperiment level in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes.* Each column reports estimates from a Poisson pseudo-likelihood regression model, weighted by the count of normalized citation-weighted patents before the treatment year in each state in each subexperiment. All regressions include year × subexperiment and state × subexperiment fixed effects. See Equation 1 for details. In Columns 1–3, the dependent variable is the number of state-year patents with forward citations in the top 1, 5, and 10% of the distribution. In Columns 4 and 5, 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 4 and 5 is smaller than in Columns 1–3. In Column 6, 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.

Table A2: The Effect of Exposure to NCA Enforceability on CPCs' Patenting

| Dependent variable: | Annualized Percent Change in: Citation-weighted patents | Annualized Percent Change in: Unweighted patents | Total Count of: Citation-weighted patents | Total Count of: Unweighted patents |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| $\Delta$ NCA Exposure | -2.841*** | -2.456*** | | |
| | (0.612) | (0.449) | | |
| Initial NCA Exposure | | | -3.03*** | -5.10*** |
| | | | (1.01) | (1.74) |
| % change in patents if mean score increase | -23.0 | -19.9 | -21.8 | -33.8 |
| N | 486 | 489 | 492 | 492 |
| Section-year FE | Y | Y | Y | Y |
| Subsection FE | N | N | Y | Y |
| Specification | OLS | OLS | Poisson | Poisson |

*Notes:* Columns 1 and 2 display an estimate of $\beta$ from Equation 3. The unit of observation is a CPC–10-year-period. $\Delta$ *NCA Exposure* is a CPC's change in NCA exposure over the 10-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 10-year period, relative to the number of patents in the prior 10-year period. Columns 3 and 4 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 105-year period, and *Initial NCA Exposure* is the CPC's effective NCA exposure in the first year of the 10-year period.

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

Table A3 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 two treatment states with out-of-support baseline patenting (California and Washington) that lacked a suitable control group. In Column 3, we estimate the baseline model except that we weight obervations 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 3 we replace our $Enforceability$ measure, a continuous variable (between 0 and 1), to instead be a dichotomous variable. That is, for treated states whose focal leads to 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 ($p = 0.010$). Considering that the average size (in absolute value) of law changes in our estimation sample was 0.081), the implied effect of enforceability on patenting is $-0.104/.081 = -1.28$, which is comparable to our 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. The estimate for negative changes is not quite statistically significant ($p = 0.145$), 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 instead estimate the effect of enforceability using a two-way fixed effects regression instead of our stacked design, omitting California and Washington (Column 9) and not omitting them (Column 10). In all cases, the coefficient remains statistically significant and qualitatively simiar to our baseline estimate.

Table A3: 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*** | -4.82*** | -2.89*** | | -4.25*** |
| | (.736) | (.944) | (.726) | | (.676) |
| Binary Score | | | | -.104** | |
| | | | | (.0406) | |
| Mean Dep Var | 10.14 | 11.49 | 14.13 | 10.14 | 10.02 |
| N | 246,798 | 281,352 | 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 | -1.45*** | -3.18*** | -2.00*** | -3.50* |
| | (.95) | (.322) | (.893) | (.276) | (2.01) |
| Mean Dep Var | 10.41 | 1.19 | 10.62 | 13.41 | 24.44 |
| N | 231,910 | 248,925 | 227,887 | 19,787 | 78,401 |

Standard errors in parentheses

Standard error clustered at state × subexperiment level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

*Notes.* In Column (4), the mean of binary changes in the sample is 0.05 and the standard deviation is 0.22.