Innovation through Labor Mobility: Evidence from Non-Compete Agreements

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Abstract

Much of the United States workforce is subject to non-compete agreements. Proponents argue that non-competes provide innovation incentives that outweigh negative worker outcomes like suppressed wages. In reality, the causal effect of non-competes on innovation is an open empirical question. Leveraging plausibly exogenous state-level changes in the enforceability of non-compete agreements, we find a significant negative effect on innovation: a 13% decrease in patenting for an average-sized increase in enforceability. Further analysis shows that this effect manifests primarily for incumbents rather than entrants. Moreover, our work suggests a central role for labor mobility as a channel of idea diffusion that increases overall innovation, with inventor mobility expected to fall alongside patenting by 22% for an increase in enforceability of the mean size in our sample.

Keywords: Non-Compete Agreements, Innovation, Labor Mobility, Knowledge Diffusion

JEL Codes: O31, O33, J08, J21, E24, K31

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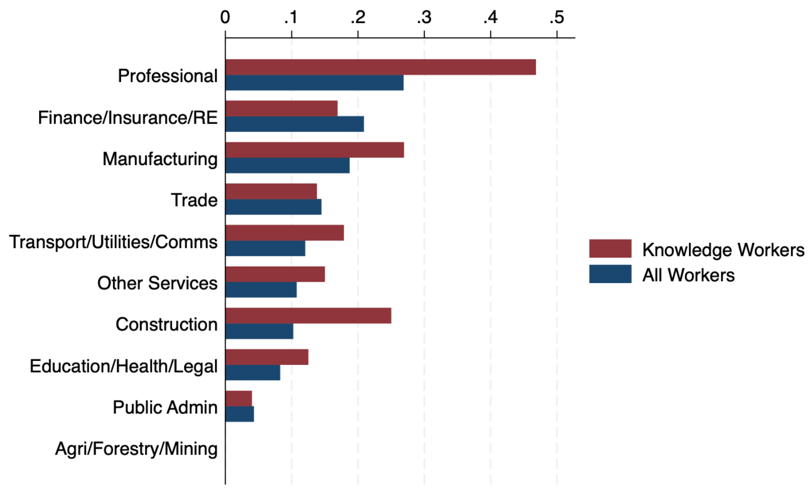
1 Introduction

A large share of the United States workforce is currently subject to non-compete agreements (NCAs). These restrictive covenants in employment contracts prevent employees from joining a rival firm or starting a new firm within the same industry for some duration post-separation (as specified in the clause). A 2017 survey of firms (Colvin and Shierholz, 2019) suggests that 30-45% of the US private sector workforce is subject to an NCA. Moreover, recent data from the US Bureau of Labor Statistics (2019) (BLS) National Longitudinal Survey of Youth 1997 Cohort (NLSY97) suggest that these agreements are especially prevalent among professional sector "knowledge workers" – the executives, managers, computer specialists, engineers, researchers, and scientists whom we might expect to be the most involved in innovative work.[1] As **Figure 1** shows, nearly 50% of professional sector knowledge workers report being subject to an NCA. This statistic is particularly striking because it is likely underestimated. A 2014 survey of private sector workers indicates that employees have very little scope to negotiate these clauses, with many of them reporting they were not even *informed* about the clause until after their employment commenced (Starr et al., 2021). Thus, employee self-reporting of NCAs is likely to understate the true prevalence of NCAs.[2]

[1] For more details on the methodology of the survey and question, see Rothstein and Starr (2022).

[2] This is consistent with the fact that higher rates of NCA use are reported by firms in surveys like Colvin and Shierholz (2019).

Figure 1: NCA Prevalence Across Industries and Worker Type



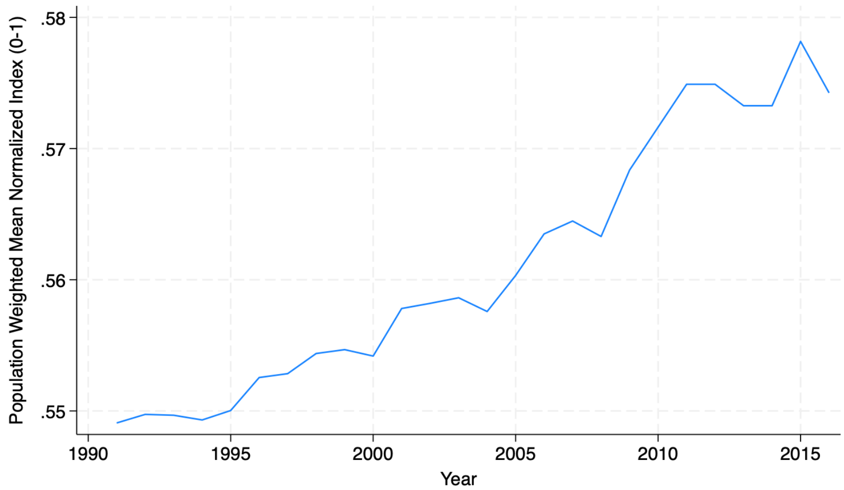
Share of workers from the NLSY 1997 cohort subject to NCAs by industry and worker type from 2017-2019. Knowledge workers refer to workers who serve in roles such as executives, managers, computer specialists, engineers, architects, scientists and researchers (CPS occupation codes 0010-2000). Data source: NLSY97.

Over the last few decades, the use of NCAs has grown in the US.[3] Furthermore, this trend coincides with an increase in the legal enforceability of NCAs in many states. **Figure 2** shows a population-weighted national average of an index measuring NCA enforceability, which we discuss in additional detail below. The index shows that enforceability increased consistently (i.e., was more favorable to employers than employees) over the same time period. More recently, however, some states have begun to restrict the enforceability of NCAs, and NCAs have become a hotly contested public policy issue.[4] And the Federal Trade Commission (FTC) recently voted 3-2 to approve a rule to ban NCAs at the federal level.

[3]For example, between 2002 and 2013, there was a near-doubling in the number of legal cases decided where an employer sued a former employee to enforce an NCA – from 390 to 760 lawsuits (Simon and Loten, 2013).

[4]**Appendix Figure 1** shows the recent trends in enforceability.

Figure 2: Trends in NCA Enforceability



Weighted average state-level NCA enforceability by year. For consistency with the empirical results presented below, states are weighted by share of population in the previous year. Data sources: Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the index are available in Section 2.1 and Appendix Section B.

In this paper, we focus on an oft-touted benefit of NCAs: their potentially positive effect on innovation. As Kitch (1980) summarizes, if "the courts leave employees free to leave the firm and exploit the information in competition with the firm[,] this competition eliminates the return that would otherwise generate the incentive for investment in the production of that information." There is a common presumption in the legal literature and related policy discussions that it is this benefit to innovation that must be traded off against negative impacts on workers – e.g., reduced wages and mobility. However, there are reasons to think that NCAs might *harm* innovation. For example, NCAs could have a negative effect on entry when new firms cannot hire the workers they need because those workers are covered by incumbent NCAs. NCAs may also negatively affect productivity and innovation by reducing the cross-firm flow of new ideas and technologies that facilitate diffusion and innovation spillovers. Therefore, the net impact of NCAs on innovation is *a priori* ambiguous and ultimately an empirical question. Accordingly, in this paper we tackle two questions as follows. First, what is the net impact of a change in NCA enforceability on innovation? And second, what channels explain this effect?

To estimate the effect of NCAs on innovation, we leverage judicial rulings and legislative policy changes as sources of plausibly exogenous variation in the state-level enforceability of NCAs.[5] And we measure innovation and inventor mobility using detailed US Patent and Trademark Office (USPTO) data on patent filings. Our analysis begins with a case study to investigate the effects of a single judicial change in Ohio that strengthened NCA enforceability. We find that patenting in Ohio fell significantly relative to a synthetic control, and that this decline was paralleled by a decrease in within-state inventor moves. We then estimate the effect of state-level changes in NCA enforceability on innovation using all of the variation in enforceability nationwide from 1991 to 2016 based on a state-year difference-in-differences approach.

The case study considers the effect of one plausibly exogenous point of variation in NCA enforceability: a decision handed down by the Supreme Court of Ohio in March 2004 that suddenly and significantly expanded the circumstances under which NCAs were enforceable in the state of Ohio. We find that both in-state inventor moves and Ohio patenting fall significantly following this decision.

Our headline results build upon this case study to consider the average impact of all 26 changes in NCA enforceability from our baseline sample, rather than just one. Using staggered difference-in-differences estimation, this more comprehensive analysis again identifies an economically and statistically significant decline in patenting due to increases in NCA enforceability. For an increase of the mean size in our sample, for example, in-state patenting would be expected to decrease by 13%. Thus, contrary to the hypothesis laid out by NCA proponents in typical policy debates, the net effect of NCAs on innovation appears to be negative.

By focusing on NCA *enforceability* rather than observed use, we avoid the issue of highly innovative firms' endogenous choice to use NCAs. We also avoid the issue of endogenous

[5] This approach is in line with Garmaise (2009), Marx et al. (2009), Chen et al. (2018), Hausman and Lavetti (2021), Johnson et al. (2023b), and others.

firm location choice (i.e., that highly innovative firms choose to locate in states with high NCA enforceability) by focusing on *intertemporal variation* in the most restrictive terms that firms could use (i.e., that are legally enforceable in their state). These state-level changes in NCA enforceability are also plausibly exogenous – primarily reflecting sudden judicial rulings that change the case law of what is and is not legally enforceable in an individual state. Our empirical approach robustly handles the non-absorbing treatments of this setting by including "clean control" and "clean treatment" conditions to ensure that we are estimating a true treatment effect in an apples-to-apples comparison and not, for example, including previously-treated observations in the control group or attributing the patenting effects of multiple close-in-time changes in enforceability to a single treatment.

We also provide a number of robustness checks on the results throughout our paper. For example, we show that our results appear to be driven by true changes in patenting, not simply changes in the propensity of firms to patent a given innovation (i.e., to substitute between patents and trade secrets). Moreover, we show that cross-state spillovers are minimal in this setting and, as a result, neither threaten the validity of our estimated treatment effect nor limit us from using our results to think about the effect of a national policy. We also provide a number of checks in the appendix to show that our results are robust to alternative sample definitions, econometric specifications, and outcome measures.

To understand the mechanisms behind the overall effect of NCAs on innovation, we then turn to consider three specific channels through which NCAs may affect innovation. First, we consider incumbent innovation incentives, whereby NCAs increase innovation when firms can better appropriate the returns to their R&D. Second, we consider firm entry, which could have either a positive or a negative effect of NCAs on innovation, depending on whether improved R&D appropriability outweighs potential barriers to entry (e.g., difficulty recruiting talent), or vice versa. Third, we consider a knowledge diffusion channel, whereby NCAs decrease innovation when the flow of new ideas and technologies is hampered because inventors have a harder time moving between firms. The estimated negative net effect of NCA enforceability on patenting indicates that incumbent innovation incentives are outweighed by one or both of the other channels.

Therefore, we further assess the entry and knowledge diffusion channels individually to disentangle the mechanism(s) behind the overall net effect. The entry channel does not appear to be driving these results. We find no statistically significant effect of changes in NCA enforceability on state-level business applications from the Census Bureau's Business Formation Statistics (BFS). In contrast, we observe a statistically significant decline in inventor mobility that parallels the overall impact on innovation, suggesting that knowledge diffusion may be a key driver of patenting effects in this setting.

This paper contributes directly to a growing literature on NCAs. A number of papers address specific NCA-related labor market outcomes. For example, Lipsitz and Starr (2022) and Young (2024) look at the effect of banning NCAs on wages for low-wage workers in Oregon and Austria, respectively. Balasubramanian et al. (2020) examine the impact of NCA restrictions in Hawaii on the careers of technology workers. And Johnson et al. (2023b) look at the implications of changes in enforceability for earnings and job mobility.[6]

Several papers that consider the impact of NCAs on specific dimensions of innovation are also informative to our analysis here. For example, Conti (2014) finds that higher NCA enforceability is associated with "riskier" R&D. Jeffers (2023) finds that increased NCA enforceability leads firms to increase investment in physical but not intangible capital (e.g., R&D). And Marx et al. (2009) show that an inadvertent repeal of Michigan's NCA policy (previously prohibiting NCAs) led to a decline in the mobility of inventors, particularly those with firm-specific skills. Several papers also focus on the entry dimension, with different settings producing an interesting mix of findings. Carlino (2021) finds no impact of increased NCA enforceability on entry in Michigan, consistent with what we find across all states. In contrast, Starr et al. (2018) and Baslandze (2022) find that higher NCA enforceability

[6]Although not directly related to NCAs, we note that some other literatures discuss similar themes on the effect of labor mobility on related outcomes. For example, Krueger and Ashenfelter (2018) discuss the implications of "no-poach" agreements in franchises, and the occupational licensing literature addresses similar questions of market power in labor markets (e.g., Shapiro (1986), Kleiner (2000)).

reduces spinouts. Johnson et al. (2023a) and Jeffers (2023) also find that stronger NCA enforceability reduces entry based on the Census Bureau's Business Dynamics Statistics and matched employee-employer data from LinkedIn, respectively. Our paper contributes to this debate by bringing in evidence from the Census Bureau's Business Formation Statistics, which cover the universe of prospective new firms. In this context, we find no statistically significant impact of NCA enforceability on new firm entry.

Relatively few papers consider the overall net impact of NCAs on innovation. The closest work to our findings on innovation are concurrent and complementary papers by He (2023) and Johnson et al. (2023a), which affirm our headline patenting results. He (2023) finds that stronger NCA enforceability leads to a 33% decline in firm patent values over assets, while Johnson et al. (2023a) find that an average-sized increase in NCA enforceability leads to 16-19% fewer citation-weighted patents over the following 10 years.[7] In addition to documenting this negative causal effect of non-competes on overall innovation, our paper provides novel evidence on the mechanisms through which that effect operates, highlighting the potential role of inventor mobility as a channel through which innovation spillovers may occur. In this regard, we also contribute to the broader literature on innovation incentives and spillovers.

In this regard, our work is also more broadly related to the literatures on innovation incentives and innovation spillovers. There are a variety of papers focusing on the impact of other policies such as patenting (Acemoglu and Akcigit (2012), Boldrin and Levine (2013), Budish et al. (2015)), taxes (Akcigit et al. (2016), Akcigit et al. (2022)), and other R&D incentives on innovation outcomes (Bloom et al. (2019), Autor et al. (2020)). Several papers have also considered how these policies may operate (or be amplified) via innovation spillovers, which many studies have found to be large (Cohen et al. (2002), Keller (2004), Bloom et al. (2013), Akcigit and Kerr (2018), Akcigit et al. (2018), Matray (2021)). We con-

[7]Note that both unweighted and citation-weighted patents provide useful information about innovation dynamics. Our headline results focus on unweighted patents, rather than citation-weighted patents, because changes in NCA enforceability may also affect citation networks and therefore the weighting scheme. However, we provide citation-weighted robustness checks in Appendix Section C.1. The consistency of findings based on different econometric approaches, sample selection, time horizons, and outcome measures is striking, as discussed in the FTC's recent final rule.

tribute to this broader literature by providing micro-founded evidence on both incumbent innovation incentives and the role of labor mobility as a channel of innovation spillovers.

This paper proceeds as follows. In Section 2, we describe the data used in this paper, including how we quantify changes in the enforceability of NCAs by extending a state-level annual index capturing judicial rulings and legislative policy changes over time. In Section 3, we introduce a case study to fix ideas before outlining our empirical approach for the all-state analysis that uses staggered difference-in-differences estimation. In Section 4, we present our all-state results that document the negative local average treatment effect of an increase in NCA enforceability on innovation. In Section 5, we discuss our conceptual framework for the mechanisms through which NCA enforceability might affect innovation and examine the empirical evidence on the possible drivers of the observed overall effect. This analysis suggests that inventor mobility, rather than entry, appears to be driving a substantial part of the observed decline in patenting. In Section 6, we consider other potential factors at play in this setting, such as spillovers across states, and find that our baseline estimates are unaffected. Finally, in Section 7, we briefly consider national policy counterfactuals.

2 Key Data

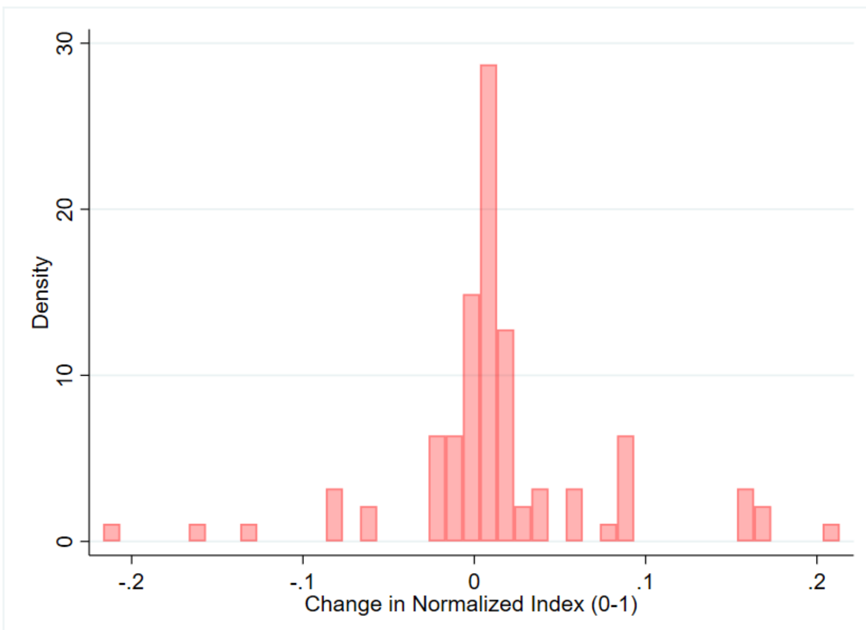
2.1 Index of State-Level Changes

In order to estimate the impact of NCAs on innovation, we track and quantify state-level changes in NCA enforceability by following Bishara (2011) and utilizing an index of state-level enforceability. Bishara (2011) develops a set of seven questions about enforceability, each of which is scored out of ten and then weighted by importance to create an index of state-level NCA enforceability over time out of a total possible score of 600.[8] Overall, a score of 600 means that NCAs are very enforceable, and a score of 0 means that NCAs are hardly enforceable at all.

[8]As an additional reference, the full text of these questions is detailed in Appendix B.

Bishara (2011) calculates this index for 1991 and 2011, and Marx (2022) extends the index to cover the years between 1991 and 2014. This paper newly extends the annual index through 2022 using a catalog of NCA enforceability changes broken down by state (e.g., due to state supreme court rulings or statutory changes) and focusing on changes that affect all workers rather than a subset of occupations (e.g., low-wage workers).[9] We then normalize the index on a 0 to 1 scale and focus our results on the years 1991-2016, which ensures a reasonable horizon of post-treatment effects.[10] **Figure 3** shows the distribution of non-zero changes to enforceability that we observe in the normalized index during this period.[11]

Figure 3: Distribution of Non-Zero Changes in NCA Enforceability



State-level changes in NCA enforceability, as measured by the normalized index, across years. Data sources: Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Additional details on the index are available in Appendix Section B.

Note that we observe both large and small changes on either side of zero. However, the majority of our changes in this period are "strengthenings" in NCA enforceability –

[9] This is consistent with the goal of this paper, as limits on NCA enforceability with respect to low-wage workers are less likely to affect professional knowledge workers involved in innovation.

[10] This also allows us to account for lags in the patent system (and the time necessary for forward citations to accrue) that would render recent data less complete and subject to greater mismeasurement.

[11] **Appendix Figure 2** shows the distribution of changes for the wider time period.

i.e., changes greater than zero – which are consistent with the general trend we observed above toward greater NCA enforceability during this period. The average change in this time period is 0.09 points in our baseline sample (as defined below), which approximately corresponds to a 9 percentage point change in enforceability.

2.2 US Patent Filings

We use patent filings from the USPTO (2022) PatentsView project to measure innovation. Patents are not a perfect measure of innovation – e.g., there are innovations that firms do not patent, either because they are not eligible to be patented or because the firm would prefer to keep the technology as a trade secret. Nonetheless, like much of the innovation literature, we contend that patents are a useful proxy for broader innovation, and one for which we have detailed disambiguated data for the universe of filings in the US since 1976. Although NCAs may not be directly relevant to protecting ideas already under patent, they have a material effect on firms' abilities to protect "tacit knowledge" – e.g., the work-in-progress research, follow-on innovations, research methods, negative knowledge, unpatentable byproducts of research, etc., which are generated alongside patent disclosures.

For the analysis discussed below, we restrict our sample to granted patents, with an identified filing location within the US, and we take each granted patent's date of application to construct our panel of state-level patenting by year. We also focus our baseline analysis on patents filed by US corporations.[12] And we restrict our sample to 1991-2016, as lags due to patent examination mean that post-2016 data are likely to be incomplete.[13] In addition to information about patenting inventors, the USPTO provides information about assignees (which are typically inventors' affiliated companies), inventor and assignee locations, technology fields, and citations. For our baseline estimates, we take the location of the assignee

[12] We focus on corporate patenting – and exclude, e.g., government and academic patenting – to align with our interest in how NCAs affect firm incentives and innovative productivity.

[13] Data after 2016 are likely to be incomplete at least in terms of forward citations if not also in terms of conversion from application to granted patent in the most recent years.

as the location of the patent.[14] However, in Appendix Section D.1 we also replicate our analysis when using inventor location as the patent location and also when restricting our sample to patents where the assignee and inventor locations are the same; we find that our results are robust to all specifications.

In the appendix we also use different citation measures to weight patents by approximations of their value rather than assuming (as we would when focusing on patent counts alone) that all patents are equally valuable or innovative. We find that our results are robust to this type of value-weighting. Forward citations are a common proxy for impact in the existing patent literature; they measure how many subsequent patents cite the patent in question and therefore are thought to approximate the extent to which a given patent has led to follow-on inventions.[15] Backward citations are a slightly less common measure, but recent research suggests that they may actually capture technological "value" more effectively;[16] they measure how many *previous* patents were cited by the patent in question, with the intuition that having fewer backward citations indicates a more original invention. For our novelty weighting, we weight patents by $1/(\text{backward cites} + 1)$. At the patent level, backward and forward citations are also winsorized to keep a single (either true or mismeasured) outlier patent from driving the results.

Using these data, we construct a panel for the entire sample period on inventors and assignees linked to their patents (and each patent's citations) as well as inventors linked to their assignee firms across years. We see this inventor-specific mobility data, which looks at when a given inventor switches from patenting with one firm to patenting instead with a different firm, as close to our primary object of interest when it comes to mobility and as a

[14] It does not appear to be the case that assignee locations are simply each assignee's headquarters. Rather, a review of the disambiguated assignees suggests that assignees list patent-specific locations (e.g., the office where the work was done) as their location on patent applications. See Appendix Figure 5 for an example of this.

[15] Although forward-weighted citations are a popular measure in the innovation literature, we de-prioritize them in the analysis here because of the potential for NCAs to affect not only innovative activity but also subsequent citation networks. For example, a patent by a firm whose inventors are less likely to move may have a lower propensity to be cited, all else equal. Therefore, we prioritize the patent count results as our baseline specification instead and would caution against over-indexing on citation-weighted results.

[16] E.g., see the discussion in Jaffe and de Rassenfosse (2017).

unique contribution to this literature.[17]

2.3 Business Formation Statistics

As discussed above, we also utilize data on firm entry from the US Census Bureau (2022) BFS, which reports information on new business applications and formations. We observe the total applications for an Employer Identification Number (EIN) submitted by entrepreneurs and corporations for each state.[18] In addition to applications, the BFS data also report information on business conversion/formations. Specifically, for quarter t, it reports how many applications are converted to formed businesses by period $t + 4$ quarters or $t + 8$ quarters. BFS data are reported at a quarterly frequency from 2004 to the present. For purposes of the data analysis discussed below, we aggregate applications to the yearly level to match the index of state-level enforcement of NCAs.

3 Empirical Approach

3.1 Identification

There are a number of challenges to estimating the causal impact of NCAs on innovation directly. For example, the use of NCAs by firms is very likely to be endogenous to their future innovation. For example, if a firm anticipates that they are likely to have valuable innovations in the future, they may be more incentivized to include NCAs in their employment contracts. Ignoring this endogeneity could bias any estimates of the impact of NCAs on innovation, as it would spuriously suggest a positive relationship between the two due to the reverse causality of anticipated innovations on NCA use. For this reason, even where direct information

[17]Note that the outside option to such a move would include staying at the initial firm as well as moving to a non-patenting firm or to a patenting firm but in a non-patenting role.

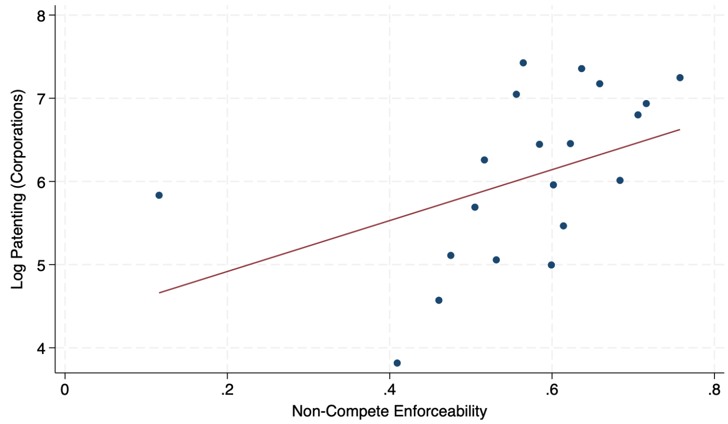
[18]The data exclude applications for tax liens, estates, trusts, or certain financial filings, applications with no state-county geocodes, applications with certain NAICS codes in sector 11 (agriculture, forestry, fishing and hunting) or 92 (public administration) that have low transition rates, and applications in certain industries (e.g. private households, civic and social organizations).

on NCA use and terms is available (for example, for the executives of listed firms), this information is not helpful for identifying the causal effect of NCAs on innovation.

To deal with this endogeneity, we instead focus on NCA *enforceability.* Rather than focusing on the actual employment contract terms that firms do use, this allows us to focus on variation in the most restrictive terms that they *could* use (i.e., that are legally enforceable in their state). Even enforceability may not be exogenous to firms in the cross-section, though, given that a firm's location choice is likely endogenous. It may be that highly innovative firms choose to locate in states with high NCA enforceability, for example, but that does not necessarily imply the high degree of NCA enforceability *causes* their innovation. **Figure 4** shows the naive correlation between NCA enforceability and patenting across states. The type of spurious positive correlation shown there might have contributed to the popular narrative that NCAs support innovation. However, our results below show that the direction of this relationship is actually *reversed* when we account for this type of endogeneity.

Instead, we use state-level *changes* in NCA enforceability as our independent variable, which we argue are plausibly exogenous. These changes largely reflect judicial rulings that change the case law of what is and is not legally enforceable. Some changes also result from state legislation, which directly alter a state's statutes on NCA enforceability, overriding past legislation as well as past case law. Even in cases where we might worry that legislatures may change NCA policy in response to state-level conditions, Johnson et al. (2023b) shows that these changes are not predictable using a range of state-level characteristics.

Figure 4: Bin-Scatter of Log Patenting on NCA Enforceability



Bin scatter plot that groups states into bins by NCA enforceability and then plots the mean of log corporate patenting in each bin along the y-axis. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

3.2 Case Study: *Lake Land v. Columber* Decision

To fix ideas and provide intuition on the setting, we first consider a case study before jumping into a broader analysis of the effects of non-compete enforceability on innovation across all states. In March 2004, the Supreme Court of Ohio expanded the circumstances under which an NCA would be enforceable. In *Lake Land Employment Group of Akron, LLC v. Columber*, the court overturned the previous Ohio rule that an employee who signed an NCA after the commencement of employment had to be compensated for the agreement to be enforceable, ruling instead continued employment would constitute adequate consideration to enforce an NCA going forward. This represented a sudden and substantial increase in the enforceability of Ohio NCAs.[19] This change in enforceability was both unanticipated and would have meant that Ohio firms could relatively costlessly distribute NCAs to their

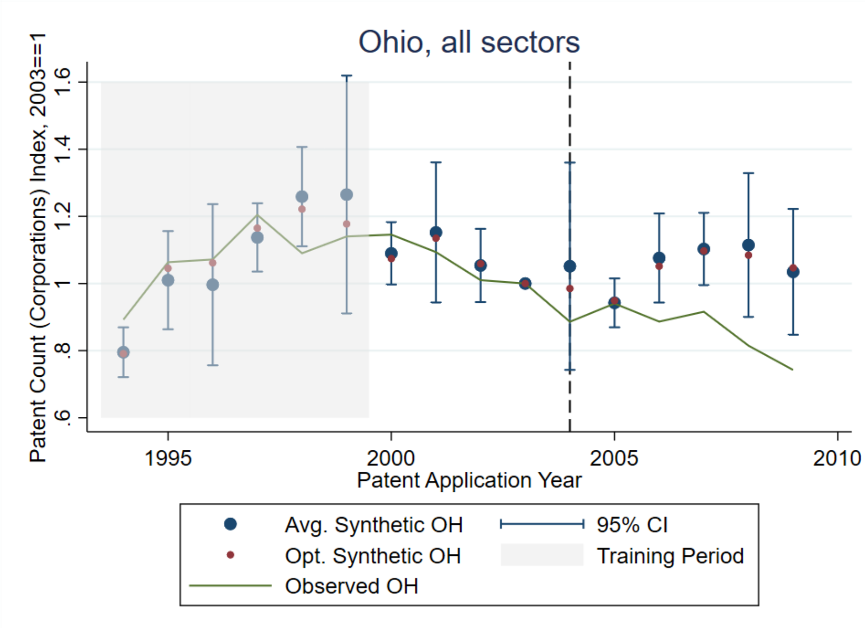
[19]This judicial ruling relates to the Bishara index question: "Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun? Will continued employment provide sufficient consideration after the employment relationship has begun?"

employees the next day (whereas they would have previously needed to compensate those employees in order for the agreements to be enforceable).

Turning now to how this change might have affected inventors and corporations' patenting, **Figure 5** compares Ohio patenting before and after the change to that of a synthetic control for Ohio constructed from states in our sample that never had a change in their NCA enforceability.[20] The green line in the figure documents observed Ohio patenting each year. The red dots represent the optimal synthetic control when we include all control states in the set of potential controls. The blue dots represent the average optimal synthetic control from 500 permutations where we randomly omit control states from the set of potential controls; from this, we can also construct standard errors and the 95% confidence intervals shown in the blue bars. This comparison suggests that Ohio patenting fell relative to the synthetic control after the *Lake Land* decision in 2004.

[20] This figure normalizes patenting to a state's patenting to its 2003 level for visibility.

Figure 5: Ohio Patenting – Synthetic Control Comparison



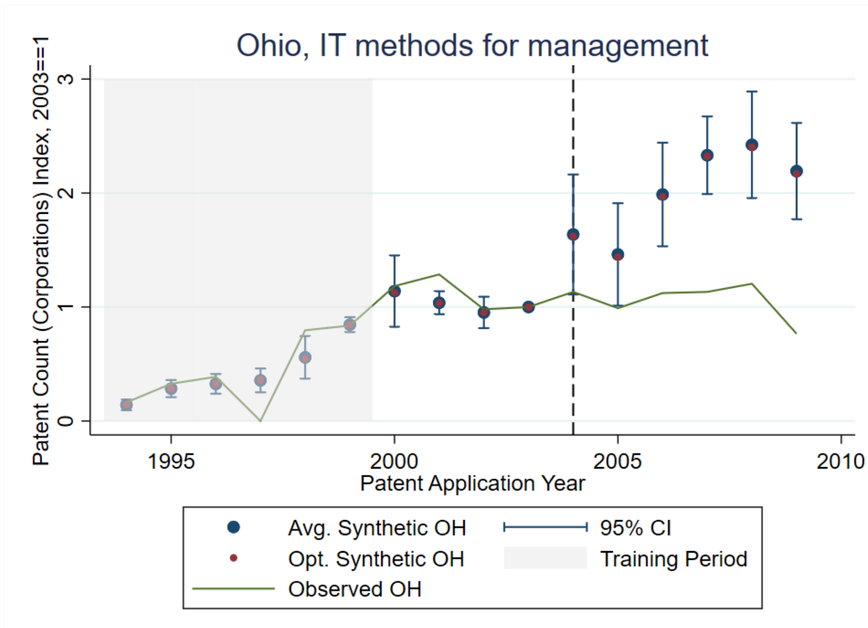
Comparison of observed Ohio patenting to synthetic Ohio patenting. The green line visualizes the trend in observed Ohio patenting. The red dots show the optimal synthetic control when all never-treated states are included as potential controls. The *synth* command in Stata constructs optimal weighted combination of untreated units in the training period as: 18% DC, 5% Mississippi, 5% West Virginia, 5% South Dakota, 5% Indiana, 4% New Jersey, 4% Tennessee, 4% New York, 4% Alabama, 4% Missouri, 3% Pennsylvania, 3% Colorado, 3% Virginia, 3% Oklahoma, 3% North Dakota, 3% Rhode Island, 3% Nebraska, 3% Utah, 3% Wyoming, 3% North Carolina, 3% New Hampshire, 3% Minnesota, 2% Montana, 2% New Mexico, 2% Washington, and 2% Nevada. The blue dots show the average synthetic control when we run 500 permutations of the analysis while randomly omitting various potential controls from the set under consideration. This approach also allows us to calculate the 95% confidence intervals show in the blue bars by bootstrapping the standard errors. All state-level patenting trends are indexed to 2003 levels. Data source: PatentsView. Details on the patent data are available in Section 2.2.

We can verify that this observed drop in Ohio patenting is not driven by industry compositional effects by also examining industry-level impacts.[21] By focusing on a high-tech industry, we can also rule out that the effect in **Figure 5** is simply driven by Ohio's exposure to the transport industry during the Great Recession. For example, **Figure 6** shows that there is also a sharp negative effect on Ohio patenting in information technology (IT) methods for management following the *Lake Land* ruling. The IT methods for management WIPO field relates to the International Patent Classification for "data processing methods,

[21] The World Intellectual Property Organization (WIPO) provides granular technology field information.

specifically adapted for administrative, commercial, financial, managerial, supervisory, or forecasting purposes." That is, this field represents software for these special purposes.[22] It is intuitive that experience with and tacit knowledge around software would be valuable in a fast-moving and frontier field.[23]

Figure 6: Ohio IT Methods Patenting – Synthetic Control Comparison



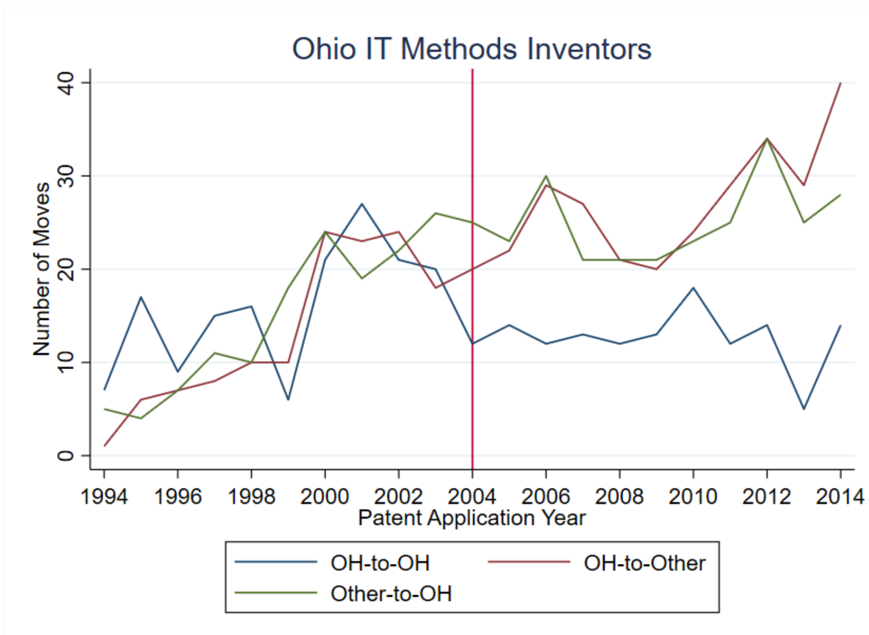
Comparison of observed Ohio patenting to synthetic Ohio patenting, now limited to the IT methods for management field defined by WIPO. The green line visualizes the trend in observed Ohio patenting. The red dots show the optimal synthetic control when all never-treated states are included as potential controls. The *synth* command in Stata constructs optimal weighted combination of untreated units in the training period as: 8% Indiana, 8% New Hampshire, 7% Minnesota, 7% Tennessee, 6% Missouri, 6% North Carolina, 6% New Jersey, 6% Pennsylvania, 6% New York, 6% District of Columbia, 5% Utah, 5% Virginia, 5% Washington, 5% Oklahoma, 5% Colorado, 5% Nevada, and 5% Nebraska. The blue dots show the average synthetic control when we run 500 permutations of the analysis while randomly omitting various potential controls from the set under consideration. This approach also allows us to calculate the 95% confidence intervals show in the blue bars by bootstrapping the standard errors. All state-level patenting trends are indexed to 2003 levels. Data source: PatentsView. Details on the patent data are available in Section 2.2.

[22]Note that the key inquiry in determining software patentability is whether the claim is directed to an abstract idea. If not, then the software is eligible. However, if it is, the technology is ineligible without additional elements that "transform the abstract idea to a new and useful end" (Alice v. CLS Bank, 2014). For example, simply adding a computer to an abstract idea is not transformative (Alice v. CLS Bank, 2014). However, "non-conventional and non-generic arrangement of known, conventional pieces" constituting "a technical improvement over prior art ways" may transform the abstract idea (BASCOM v. AT&T, 2016).

[23]These statistics also likely understate the amount of innovative activity going on, as only a subset of software is patentable.

To further confirm that it is the *Lake Land* decision driving these effects and not just some other event that happened around the same time, we can look directly at in-state inventor mobility in Ohio during this same time period.[24] **Figure 7** shows the number of moves by IT methods inventors (i) within Ohio; (ii) out of Ohio; and (iii) into Ohio. Although the three series move together in the pre-period, the number of within-state moves drops sharply below the number of across-state moves in 2004 and afterwards.

Figure 7: Moves by Ohio IT Methods Inventors – by Origin and Destination



Comparison of the number of moves each year by Ohio IT methods for management inventors who either moved within, out of, or into Ohio. A move is counted in this figure whenever we observe an inventor patenting historically at one firm and then switching to patenting at a new firm in year t. Data sources: PatentsView; clean organization lookup created by this paper. Details on the patent data are available in Section 2.2. The clean organization lookup is discussed in additional detail in Footnote 35.

[24] As described above, we define inventor mobility here as how many times we observe an inventor patenting historically at one firm and then switching to patenting at a new firm in year t. Inventor mobility thus captures *moves between patenting-related jobs at innovative firms.* Even a decline in moves driven by inventors who move to non-patenting jobs and thus out of our sample would be relevant to our analysis because this such a response would still take the inventor out of the innovative ecosystem. Nonetheless, in our all-state analysis below, we consider the *share of inventors in the sample* who move and continue to see a proportional and statistically significant decline in inventor moves.

This is precisely what one would expect an increase in NCA enforceability to accomplish, as NCAs generally have limited within-state geographic scope. These coincident drops in in-state inventor mobility and in-state patenting suggest that *Lake Land* played a significant role in changing the innovative landscape of Ohio by restricting worker mobility.

3.3 All-State Estimation

For our main results, we adopt a staggered difference-in-differences estimation approach: local projections difference-in-differences (LP-DiD) developed by Dube et al. (2023). This estimator is similar to other popular estimators like Callaway and Sant'Anna (2021) and Borusyak et al. (2021b) and shares their desirable features of, for example, avoiding the negative weights bias under staggered heterogeneous treatments of two-way fixed effects models; however, it also uniquely permits non-absorbing and non-binary treatments like we have in our setting here. Our estimating equation is the following:

$$\begin{aligned} y_{i,t+h} - y_{i,t-1} = {} & \beta_h \cdot \mathbb{I}_{it} \cdot \Delta X_{it} && \text{treatment (change in index)} \\ & + \delta_{t+h} - \delta_{t-1} && \text{time effects} \\ & + \epsilon_{it+h} && \text{for } h = -H, ..., H, \end{aligned} \tag{1}$$

where we restrict the sample to observations that are either (i) clean controls – i.e., not-yet or never treated states; or (ii) clean treatments – i.e., state-years with only one treatment in the past H years and with treatments greater than a threshold c.[25] In this specification, $y_{i,t+h} - y_{i,t-1}$ is the difference in the relevant outcome variable (log patents, log business applications, etc.). ΔX_{it} is the change in our continuous treatment variable – the normalized state-level index of NCA enforceability. Dube et al. (2023) allows for outcome lags to be included on the right-hand side of the estimating equation.[26] Although not part of our

[25] For additional information on which states have strengthenings, weakenings, or neither in our baseline sample, see **Appendix Figure 6**.

[26] This is the local projections piece of the LP-DiD estimating equation. Controlling for pre-treatment values of time-varying covariates, including outcome dynamics, on the right-hand side allows a weaker parallel

baseline specification, we include a version of our results with these lags in the appendix to show robustness. Finally, we also control for time-fixed effects, δ. For our baseline specification we use: $H = 5$ years and $c = 15/600$.[27] We cluster standard errors at state-level, and for our baseline results we weight each state by its population share in the previous year. In the appendix, we also present our results unweighted, and weighted by their patent share in the previous year, and show our findings are robust to either alternative weighting scheme.

The primary assumption necessary for the validity of the LP-DiD estimator is captured in the clean treatment and control conditions noted above, which requires that in order to be included as either a treatment or control observation, a state must be either newly or not-yet treated such that it is not experiencing some previous dynamic treatment effect from a previous change.[28] With these restrictions, we have 26 total treatments for our baseline 1991-2016 sample, of which 17 are instances in which NCA enforceability increased ("strengthenings"), and 9 are instances where enforceability decreased ("weakenings"). We also include a number of other checks in the appendix to show that our results are robust to different permutations of this analysis.[29]

4 All-State Results

Figures 8 and 9 present our headline results of the impact of all 26 changes in NCA

trends assumption than is standard to the typical DiD approach.

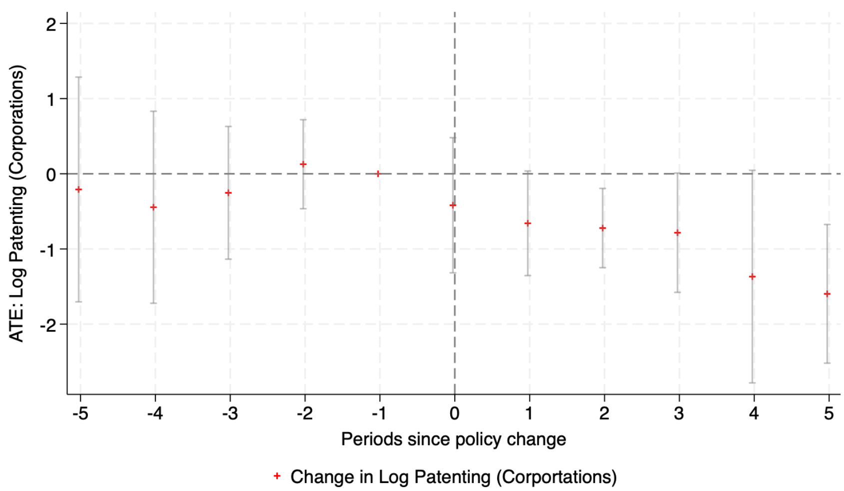
[27]This low $c = 15/600$ threshold is conservative, as we show that setting a higher threshold further strengthens our estimated effects, supporting the generalizability of our conclusions to other potential policy changes.

[28]Using a previously treated unit that is still experiencing lagged time-varying and heterogeneous treatment effects as a control would introduce bias. The LP-DiD methodology avoids this by restricting the sample so that "unclean" observations are not included.

[29]In particular, we additionally restrict our sample such that we have a balanced panel of 13 treatments, of which 10 are strengthening and 3 are weakening. Under this specification our estimates are very similar in magnitude and more tightly estimated than in our baseline, suggesting the broader sample is conservative. However, we maintain the unbalanced as our baseline to ensure consistent and sufficient samples for the BFS results, for which we have a reduced time period of data availability. Below, we also show results from the analysis when using a higher threshold of $c = 50/600$. This reduces our sample to 10 strengthenings and 4 weakenings, and again results in a similar and more precisely estimated effect than in our baseline specification.

enforceability in our baseline sample. The peak decline in patenting that we observe occurs after 5 years. These coefficients imply an economically significant decline in patenting due to increases in NCA enforceability. For example, they suggest that a strengthening of NCA enforceability from the median enforcement level observed in our sample to the maximum enforcement level observed in our sample (i.e., a move on the normalized index from 0.59 to 0.80) would decrease patenting by about 28% after 5 years.[30] For a more modest change in enforcement (e.g., of the average size observed in our sample), our estimates suggest that an increase of 0.09 points in our normalized index would decrease patenting by about 13%.

Figure 8: Estimated Effect of Changes in NCA Enforceability on Log Patent Count



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

We estimate Equation 1 by first regressing the outcome measure on the year effects and then regressing the residual on the treatment. This implementation allows us to estimate

[30]Note: predicted change is equal to $\exp(\beta_5 * \Delta X) - 1$.

calendar year fixed effects that are constant across ts and hs rather than estimating these effects specific to time horizons and treatment years. As a result, the R-squared terms shown in the tables below reflect only the share of the variation in the residual that is explained by the treatment variable, and not the share of the variation in the outcome that is explained by the year fixed effects.

Figure 9: Estimated Effect of Changes in NCA Enforceability on Log Patent Count

Year Relative to Treatment:	-5	-4	-3	-2	-1	0	1	2	3	4	5
Treatment: Change in Index	-0.209	-0.445	-0.253	0.127	.	-0.419	-0.659	-0.722	-0.784	-1.368	-1.60
	(0.763)	(0.651)	(0.450)	(0.302)	.	(0.459)	(0.355)	(0.269)	(0.405)	(0.721)	(0.471)
Year FE	X	X	X	X	X	X	X	X	X	X	X
Constant	X	X	X	X	X	X	X	X	X	X	X
Observations	541	584	629	668	719	712	712	710	708	704	689
R-squared	0.000	0.002	0.001	0.000	.	0.004	0.004	0.004	0.003	0.007	0.006

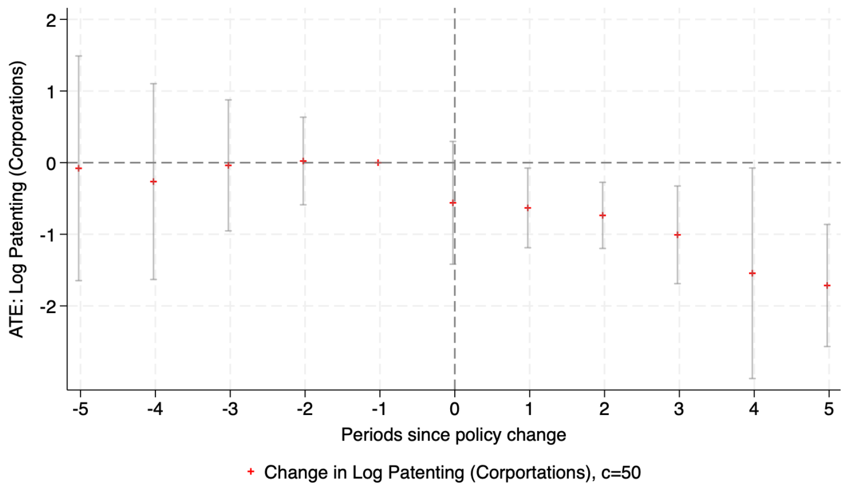
Standard errors clustered by state are shown in parentheses.

Note: States are weighted in year *t* by their population share in year *t-1*.

See **Appendix Figures 7 and 8** for analogous impact and novelty-weighted results.

Specifying a higher minimum threshold (c) for clean treatments results in marginally larger treatment effect estimates and smaller standard errors. See **Figure 10**. That is, the estimated treatment effect is even more statistically significant when we focus on larger (and therefore likely better measured and more salient changes). These results (and the robustness checks included in the appendices) suggest that there is an economically and statistically significant negative impact on patenting following an increase in NCA enforceability.

Figure 10: Estimated Effect of Changes in NCA Enforceability on Log Patent Count (C=50/600)



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 50/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

4.1 Product Patents versus Process Patents

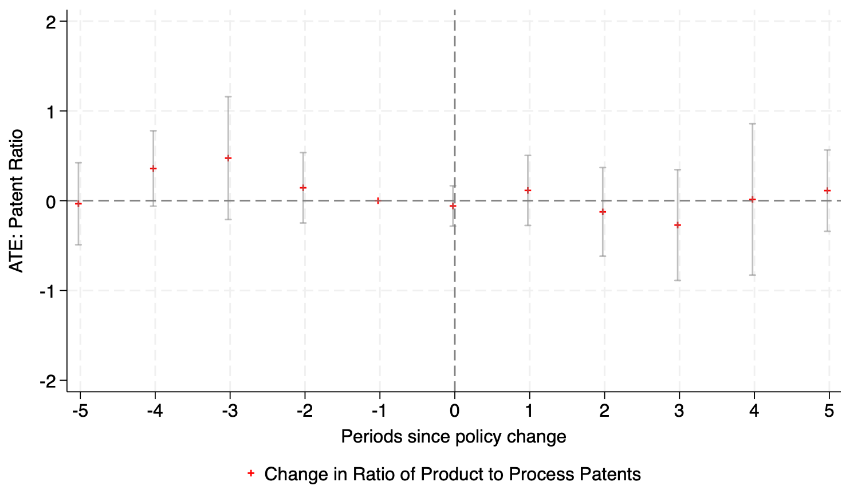
An additional challenge in identifying the relationship between NCAs and innovation is that NCAs may affect not only innovation directly, but also the propensity of firms to patent a given innovation. That is, if firms view NCAs and patents as substitutes in protecting their inventions and ideas, we might pick up substitution effects alongside innovation effects if firms increase their reliance on trade secrets under stronger NCA enforceability. Such substitution is unlikely to be an issue here, though. Discussions with legal scholars have revealed that the risk of not patenting an eligible invention is generally too large for firms to adopt such a strategy, even under fully enforceable NCAs – e.g., if a competitor independently invents and patents your same invention, it obtains the right to exclude you from use of the invention. And, even short of a competitor *patenting* your invention, independent discovery and reverse engineering would destroy any right you have to exclude others. Consistent with this reasoning, Greenwood et al. (2024) finds that firms do not *decrease* their reliance on on trade secrets after NCAs are made *less* enforceable.

As empirical evidence, we can consider the treatment effect separately for 'product' and 'process' patents to confirm that our estimates reflect the true impact of NCAs on *innovation* rather than substitution.[31] To the extent that product innovations are hard to protect through trade secrets alone due to the potential for reverse-engineering, firms are unlikely to choose not to patent eligible product innovations even in the face of high NCA enforceability. Substitution between trade secrets (protected by NCAs) and patents may be more of a concern for process innovations. Our results in **Figure 11** show, however, that the mix of product and process patents granted within a state does not change in response to changes in NCA enforceability, such that it does not seem to be substitution away from (process) patenting that generates our results. If firms are simply using patents as a substitute for trade secrets that become harder to protect in the presence of weaker NCA enforcement, rather than actually conducting additional R&D and innovation, then we should see most (if not all) of our effect operating through the process-only patents channel such that the relative frequency of product- and process-only patents changes too. However, we can see that the relative frequencies are unaffected and that it does not appear as though process patents are driving our baseline results discussed above.

Moreover, we observe real simultaneous changes in inventor moves in the channel-specific results discussed below, suggesting that more fundamental changes are happening in innovative industries in response to NCA enforceability.

[31] Patents are categorized as process or product patents based on an index by Heinrich et al. (2022). We do not take a stance here on the appropriate classification of mixed patents.

Figure 11: Estimated Effect of Changes in NCA Enforceability on the Ratio of Product-Only Patents to Process-Only Patents



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the ratio of product-only patents to process-only patents in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates; Heinrich et al. (2022) patent categorizations. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

5 Channels Analysis

We consider three specific channels through which NCAs may affect innovation. First, the most common explanation for why NCAs might increase innovation is **incumbent innovation incentives**. Just as patents are thought to encourage innovation by allowing firms to better appropriate the returns on R&D expenditures (e.g., by allowing them to charge higher markups on innovative products for a period of time following a novel invention), NCAs may also incentivize incumbent innovation by helping firms protect their confidential information (e.g., trade secrets, tacit knowledge, negative knowledge, etc.) and maintain a competitive advantage over rivals by restricting the flow of information.

However, incumbent firms are not the only source of innovation. Our second channel, **firm entry**, is important as well. Garcia-Macia et al. (2019) estimates, for example, that firm entry accounts for approximately 20% of total innovation in the US. But, without empirical analysis, the effect of NCAs on entry is theoretically ambiguous. On the one hand, NCAs might have a positive effect on firm entry to the extent that they increase the expected profits of a successful entrant (e.g., as with incumbents: by allowing firms to earn supra-normal profits on their innovations or by reducing the wages and bargaining power of workers). However, NCAs could also have a negative effect on entry, as they directly impose barriers to entry. For example, a worker covered by an NCA is legally prohibited from entering the market as a rival firm. Moreover, even if a would-be entrepreneur is not themselves covered by an NCA, they may still find it difficult to enter the market if most workers in the industry are covered by NCAs such that it proves difficult to recruit talent. Among other things, this paper assesses the net impact of NCAs on this entry channel, which is otherwise uncertain.

Third, NCAs may negatively affect productivity and innovation by reducing **knowledge diffusion** – i.e., the flow of new ideas and technologies between firms. This channel impacts the ability of other firms to learn about and improve upon the new technologies produced by rivals.[32] As discussed above, if workers moving between firms is an important mechanism through which innovations spread and diffuse, then direct limits on employee mobility will hamper this channel of knowledge flows.

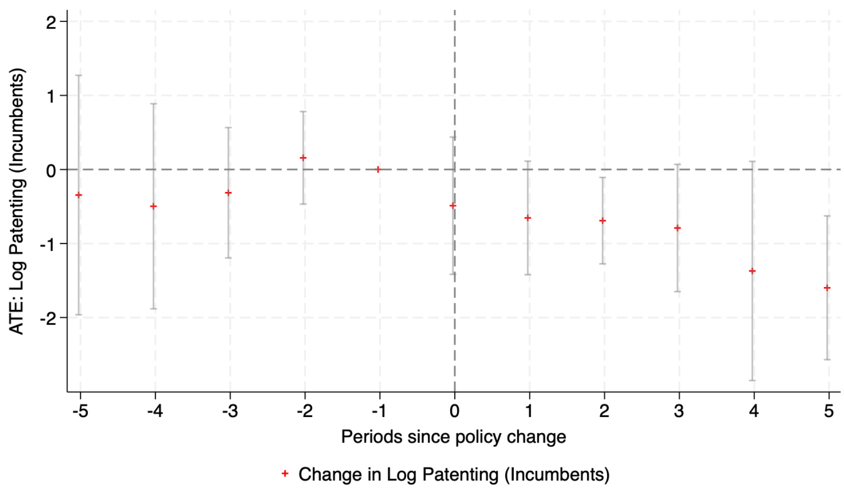
The estimated negative net effect of NCA enforceability on patenting discussed above indicates that incumbent innovation incentives are outweighed by one or both of the other channels. Therefore, to start untangling which channel/mechanism is driving these results, we first split our headline results to look at the patenting of firms who have previously patented ("incumbents") – i.e., excluding first-time entrants. **Figures 12 and 13**[33] show

[32] This channel has been pointed to as a potential explanation behind Silicon Valley's growth, for example.

[33] As discussed above, the R-squared terms shown in the table below reflect only the share of the variation in the residual that is explained by the treatment variable, and not the share of the variation in the outcome that is explained by the year fixed effects.

the estimated effect of NCA enforceability on the patenting of incumbents. Incumbents see a decline in patenting similar to that of our headline estimates, suggesting that, at the very least, not all of the observed decline in overall patenting can be explained by reduced entry. Rather, these results show that *incumbent* firms themselves are innovating significantly less following an increase in NCA enforceability.

Figure 12: Estimated Effect of Changes in NCA Enforceability on Log Patent Count - Incumbents (Firms Who Have Previously Patented) Only



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting by incumbents in year h relative to the time of the policy change ($h = 0$). Incumbents are defined as corporations with previous observed patenting. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 13: Estimated Effect of Changes in NCA Enforceability on Log Patent Count - Incumbents (Firms Who Have Previously Patented) Only

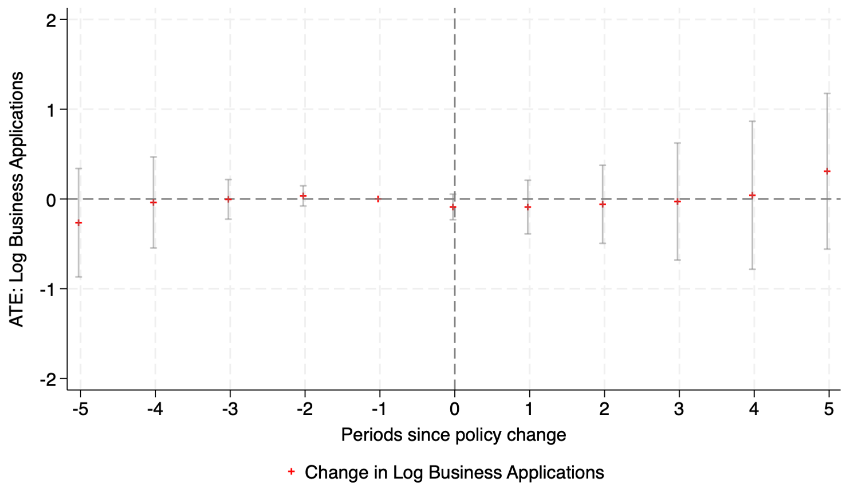
Year Relative to Treatment:	-5	-4	-3	-2	-1	0	1	2	3	4	5
Treatment: Change in Index	-0.345	-0.497	-0.315	0.157	.	-0.489	-0.654	-0.691	-0.791	-1.371	-1.598
	(0.825)	(0.707)	(0.449)	(0.319)	.	(0.473)	(0.391)	(0.298)	(0.438)	(0.755)	(0.495)
Year FE	X	X	X	X	X	X	X	X	X	X	X
Constant	X	X	X	X	X	X	X	X	X	X	X
Observations	541	584	629	668	719	712	712	710	708	704	689
R-squared	0.001	0.002	0.001	0.000	.	0.004	0.003	0.003	0.003	0.006	0.006

Standard errors clustered by state are shown in parentheses.
Note: States are weighted in year *t* by their population share in year *t-1*.

We can further assess the magnitude of the entry channel by using the Census BFS data discussed above. **Figures 14 and 15**[34] show the estimated effect of changes in NCA enforceability on business applications. These results again show no statistically significant effect of changes in NCA enforceability on entry – and certainly not a *decline* of the magnitude we see for patenting overall. This could be consistent with the negative barriers to entry channel being cancelled out (at least partially) by the positive profitability channel from our theoretical framework outlined above.

[34] As discussed above, the R-squared terms shown in the table below reflect only the share of the variation in the residual that is explained by the treatment variable, and not the share of the variation in the outcome that is explained by the year fixed effects.

Figure 14: Estimated Effect of Changes in NCA Enforceability on Log Entry (Business Applications)



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log state-level count of business applications in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: Census BFS; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the business applications data are available in Section 2.3. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 15: Estimated Effect of Changes in NCA Enforceability on Log Entry (Business Applications)

Year Relative to Treatment:	-5	-4	-3	-2	-1	0	1	2	3	4	5
Treatment: Change in Index	-0.265	-0.039	-0.004	0.034	.	-0.089	-0.090	-0.059	-0.029	0.041	0.309
	(0.308)	(0.259)	(0.112)	(0.057)	.	(0.073)	(0.152)	(0.222)	(0.333)	(0.421)	(0.442)
Year FE	X	X	X	X	X	X	X	X	X	X	X
Constant	X	X	X	X	X	X	X	X	X	X	X
Observations	110	144	175	210	244	244	240	238	238	238	236
R-squared	0.010	0.000	0.000	0.001	.	0.003	0.002	0.001	0.000	0.000	0.007

Standard errors clustered by state are shown in parentheses.
Note: States are weighted in year *t* by their patent share in year *t-1*.

These results are unchanged when considering either four-quarter business formations or eight-quarter business formations, as shown in the appendix. In summary, firm entry does not seem to explain the estimated negative effect of NCAs on innovation.

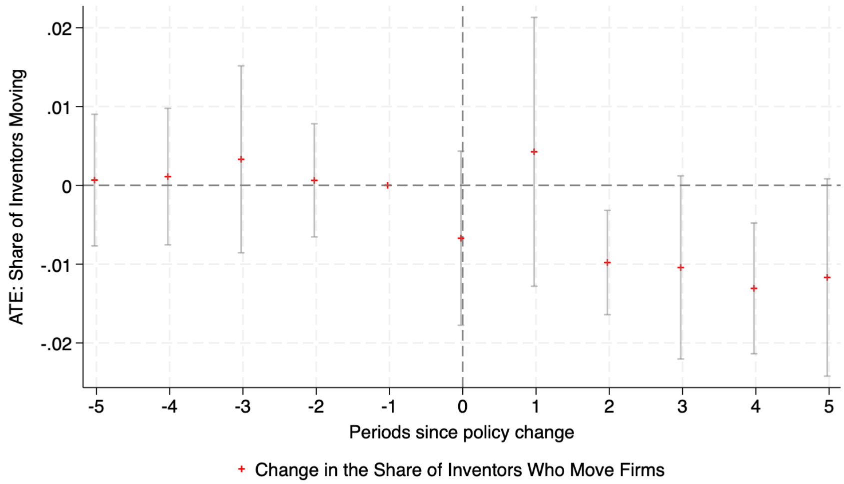
In contrast, the observed impact on patenting is paralleled by a real impact on inventor mobility. We measure inventor moves from the patent data using disambiguated inventor and assignee names.[35] An example mover is included for reference in **Appendix Figure 3**. See **Figures 16 and 17**[36] for inventor mobility results that suggest a strengthening from median to max enforceability (i.e., 0.59 $\rightarrow$ 0.80) would cause a 0.2pp (from a base rate of 0.5pp, or 51%) decrease in the share of inventors moving firms after 5 years. A strengthening of average size in our data (i.e., 0.09) corresponds to a 0.1pp (or 22%) decrease in inventor moves after 5 years. That inventors are strongly affected alongside patenting supports the role of labor mobility as a channel of knowledge diffusion.[37]

[35] To ensure that we are accurately measuring inventors who move across firms, we also contribute a novel clean organization lookup that further disambiguates firm names from the USPTO data. For example, USPTO's disambiguated assignee organizations include organization names like: DARTMOUNTH COLLEGE, TRUSTEES OF DARTMOUTH COLLEGE, THE TRUSTEES OF DARTMOUGH COLLEGE, THE TRUSTEES OF DARTMOUTH COLLEGE AND DARTMOUTH-HITCHCOCK CLINIC, TRUSTEES OF DARTMOUTH, and TRUSTEES OF DARTMOUTH UNIVERSITY. It would be incorrect to consider an inventor we see attached to multiple of these organization names as moving. Therefore, through work involving ChatGPT, Gemini, and manual effort, we have assembled a clean lookup that consolidates these firm names into a clean ID from which we can accurately measure inventor moves.

[36] As discussed above, the R-squared terms shown in the table below reflect only the share of the variation in the residual that is explained by the treatment variable, and not the share of the variation in the outcome that is explained by the year fixed effects.

[37] It also helps us understand the timing of the patenting effects, which is fairly immediate and growing over time. The immediacy of the initial effect is less surprising given these equally immediate impacts on mobility and the fact that existing research shows a "strong *contemporaneous* relationship between R&D expenditures and patenting" (emphasis added) (Hall et al. (1986); Pakes and Griliches (WP)).

Figure 16: Estimated Effect of Changes in NCA Enforceability on Share of Inventors Moving



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the share of inventors moving firms in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. Inventor moves are defined as above and now also within state and industry given that these are the types of moves targeted by NCAs. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates; clean organization lookup. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B. The clean organization lookup is discussed in additional detail in Footnote 35.

Figure 17: Estimated Effect of Changes in NCA Enforceability on Share of Inventors Moving

Year Relative to Treatment:	-5	-4	-3	-2	-1	0	1	2	3	4	5
Treatment: Change in Index	-0.001	-0.002	0.002	-0.002	.	-0.008	0.004	-0.011	-0.010	-0.015	-0.012
	(0.004)	(0.004)	(0.006)	(0.004)	.	(0.006)	(0.009)	(0.004)	(0.006)	(0.004)	(0.007)
Year FE	X	X	X	X	X	X	X	X	X	X	X
Constant	X	X	X	X	X	X	X	X	X	X	X
Observations	552	595	639	683	727	727	723	721	721	721	719
R-squared	0.000	0.000	0.000	0.000	.	0.005	0.001	0.006	0.004	0.009	0.005

Standard errors clustered by state are shown in parentheses.
Note: States are weighted in year *t* by their population share in year *t-1*.

6 Other Considerations

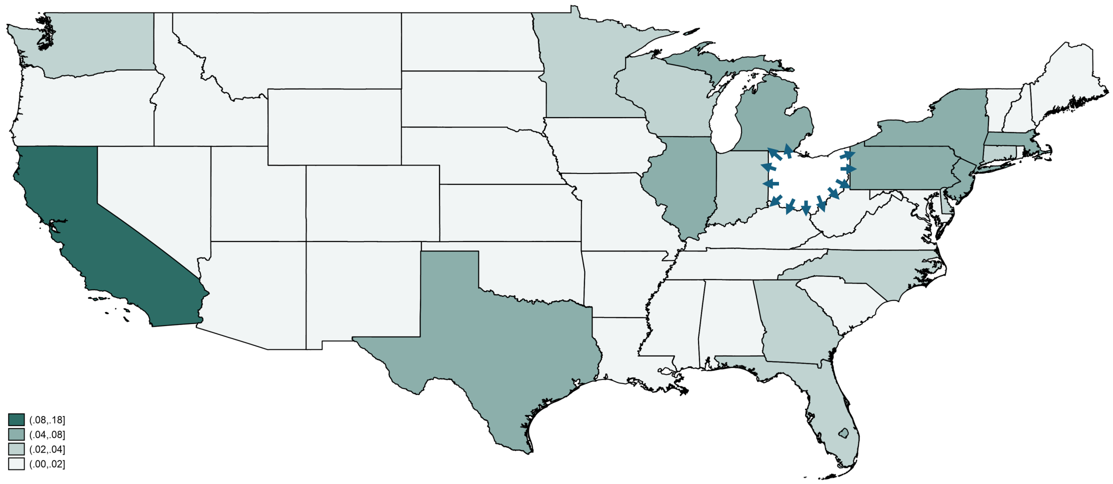
6.1 Accounting for Spillovers

So far, we have estimated the impact of state-level changes in NCA enforceabiliity. To extrapolate to the expected effects of a national policy (e.g., the FTC's recent ban), we additionally need to account for the potential impact of cross-state spillover effects.[38] Accounting for certain spillover effects could intensify our estimated net effect. For example, accounting for how declines in patenting in Ohio would inhibit follow-on innovation in other states would increase our estimated magnitudes. However, accounting for other spillover effects could dampen our net effect. For example, if workers move states to escape NCAs (following an increase in enforceability in their initial state of residence), they may increase innovation in their destination state. To ensure that our estimated effect is conservative, we now also control for out-of-state migration effects, which may dampen our baseline estimated treatment effect.

To do so, we control for the relative exposure of one state to all other states' movers. Helpful in this step is the fact that inventor migration patterns have stable determinants (e.g., distance, industry mix, demographics, and relative economy size). For example, **Figure 18** shows the relative shares with which inventors moving out of Ohio end up in other states. California is the most common, as might be expected based on factors like the relative size of its inventive economy. However, states like Wisconsin, Illinois, Indiana, and Pennsylvania are also well-represented, as might be expected based on factors like distance of migration. **Figure 19** shows that these migration shares (conditional on out-of-state migration) are relatively constant over time.

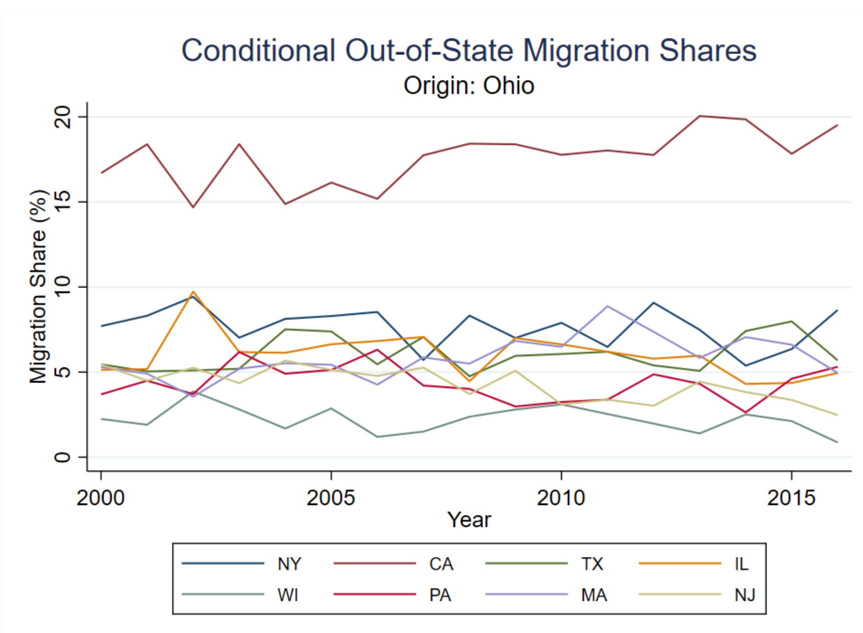
[38] Doing so will also allow us to confirm the validity of SUTVA in our baseline estimates above.

Figure 18: Example Mover Destinations (Ohio)



Map of destination states for Ohio inventors who move out of the state. Darker colors indicate states that are more frequent recipients of Ohio inventors, as detailed in the legend. Inventor moves are defined as above. Data sources: PatentsView; clean organization lookup. Details on the patent data are available in Section 2.2. The clean organization lookup is discussed in additional detail in Footnote 35.

Figure 19: Mover Destination Stability (Ohio)



Plot of the annual share of Ohio inventors who move out of state who end up in the destination states denoted in the legend. For visibility, only the 8 most common destination states are plotted. Inventor moves are defined as above. Data sources: PatentsView; clean organization lookup. Details on the patent data are available in Section 2.2. The clean organization lookup is discussed in additional detail in Footnote 35.

Given this, we can define a gravity-style control for the cumulative exposure of state i to all other states' changes in enforceability.[39] Specifically, we can specify the following term to capture state i's exposure to movers out of other states due to other states' changes in enforceability as a percentage of i's initial inventor population:

$$\lambda_{i,t+h} := 100 \cdot \left(\sum_{\forall o \neq i} \underbrace{wm_{oi,t-1}}_{i\text{'s exp. to } o} \cdot \underbrace{\mathbb{I}_{o,t} \cdot m_{o,t-1} \cdot [M_{o,t+h} - M_{o,t-1}]}_{\text{change in \# movers out of } o} \right) / \underbrace{P_{i,t-1}}_{\text{\# inventors in } i}, \qquad (2)$$

where $wm_{oi,t}$ is the share of inventors who move out of state o in year t that end up in state i; $m_{o,t}$ is the level count of inventors moving out of state o in year t; $M_{o,t}$ is the log count of inventors moving out of state o in year t; and $P_{i,t}$ is the level inventor population in state i in year t.

With this term in mind, we can predict the change in the number of inventors moving out of state that is induced by a change in NCA policy in the first stage of the following two-stage regression, and then use that predicted change to include a predicted $\hat{\lambda}$ as a covariate in the second stage (which is a version of our baseline specification):[40]

1. $$M_{o,t+h} - M_{o,t-1} = \theta_h \cdot \mathbb{I}_{o,t} \cdot \Delta X_{o,t} + \gamma_{t+h} - \gamma_{t-1} + \varepsilon^1_{o,t+h} \qquad (3)$$
2. $$Y_{i,t+h} - Y_{i,t-1} = \beta_h \cdot \mathbb{I}_{i,t} \cdot \Delta X_{i,t} + \delta_{t+h} - \delta_{t-1} + \rho_h \cdot \hat{\lambda}_{i,t+h} + \varepsilon^2_{i,t+h} \qquad (4)$$

Here, ρ is a parameter to test for the presence of spillovers through inventor moves.

This analysis suggests that there are no statistically significant spillover effects from migration. See **Figure 20**.[41] This result is consistent with existing literature that finds that accounting for individual workers' out-of-state migration does not change optimal state-level

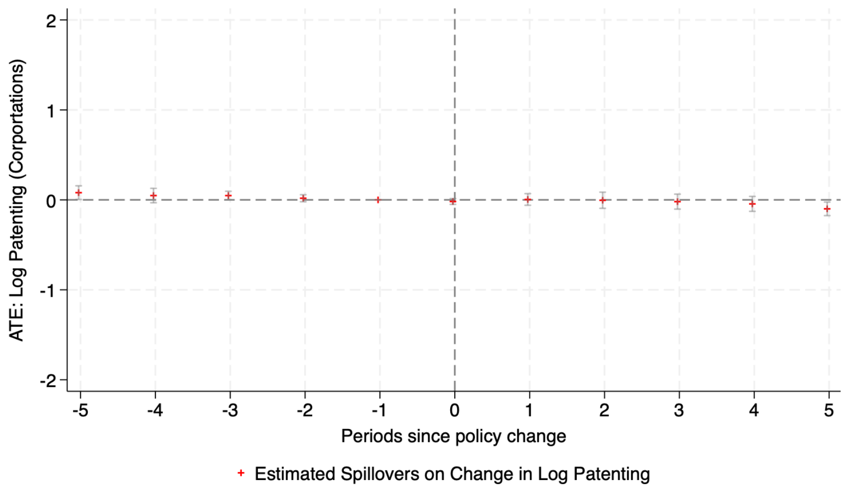
[39]This control is in the style of existing work such as Dubé et al. (2017); Borusyak and Hull (2023); Borusyak et al. (2021a); Peri et al. (2015); Kerr and Lincoln (2010); and Card (2001). However, it is somewhat novel in that it captures exposure to multiple events rather than a singular event.

[40]The two identifying assumptions necessary for this approach are: (i) treatment in state i affects state j patenting only proportionally to historical migration flows; and (ii) treatment in state i is exogenous to things happening in state j.

[41]Note that the effect of the spillovers is insensitive to state i's treatment period, which makes sense because the spillovers are timed according to *other* states' treatment periods.

policies, such as optimal state-level income taxation (Mazerov (2023)). Accordingly, the estimated net effect of a change in NCA enforceability on patenting is unchanged from our baseline specification shown above.[42] See **Figure 21**. Details on the first stage results of this regression analysis can be found in **Appendix Figure 4**.[43]

Figure 20: Estimated ρ

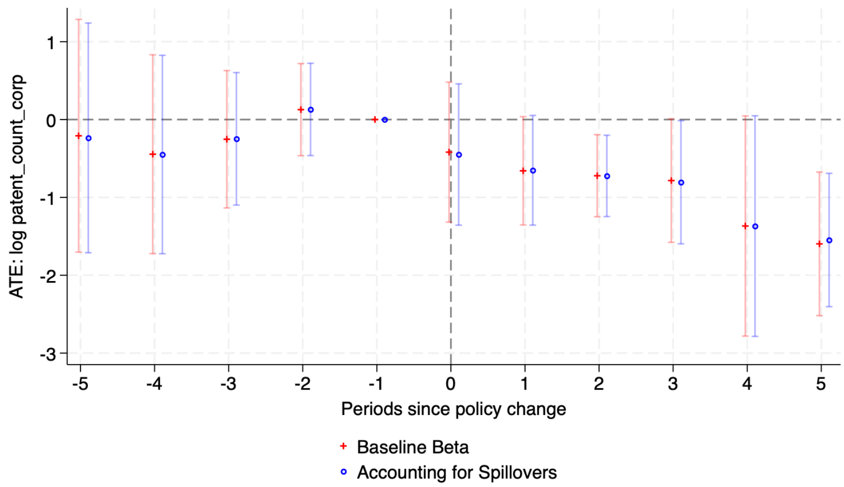


Plot of the estimated average treatment effect of a 1 percent increase in the number of inventors in a destination state because of changes in NCA enforceability in other origin states on the log of destination state-level corporate patenting in year h relative to the time of the destination state's policy change ($h = 0$). Details on the econometric specification can be found in Equations 2, 3, and 4. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates; clean organization lookup. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B. The clean organization lookup is discussed in additional detail in Footnote 35.

[42] Note that this is consistent with but conservative relative to Johnson et al. (2023a)'s conclusion that economy-wide losses are *larger* than state-level estimates.

[43] There, we see no statistically significant impact of change in NCA enforceability on out-of-state moves, with F-stat of the treatment indicators $\in [0, 3]$ for all $h \in [-5, 5]$.

Figure 21: Updated Estimated Effect of Changes in NCA Enforceability on Log Patent Count



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$) before accounting for spillovers (in red) and after accounting for spillovers (in blue). Details on the econometric specification can be found in Equation 1 (for the red "baseline" series) and in Equations 2, 3, and 4 (for the blue "accounting for spillovers" series). Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates; clean organization lookup. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B. The clean organization lookup is discussed in additional detail in Footnote 35.

6.2 Limitations

The preceding analysis raises a number of additional considerations that are beyond the scope of this paper due to data limitations, but which we hope to explore in future work. Firstly, another potential mechanism that we do not include in our channels analysis is any potential discouragement effect of increased NCA enforceability on incumbent innovations. It is possible that more enforcement of NCAs could discourage inventors from working as hard as before (either directly when inventors are forced to stay in a job that they would like to leave or indirectly due to the lower pay from stronger NCAs that has been identified by others in the existing literature). However, in the patent data we cannot observe, for example, compensation or hours worked, so it is hard to speak to this channel directly.

As previewed above, our focus on patented inventions necessarily excludes a potentially significant portion of innovative activity. Many innovations may not be patented for various reasons such as eligibility, and if the drivers of patented versus non-patented innovations differ, our conclusions may not fully capture the impact of NCA enforceability on overall innovation. Future work that explores non-patent measures of innovation would be a valuable addition to this area of research.

Additionally, an important caveat when extrapolating our results to policy changes that would be larger than those considered in our analysis (recall: the average change in our sample is $\sim$0.09 points on the normalized index) is that the reduced-form nature of our approach limits our ability to assess potential non-linearities in the relationship between NCA enforceability and innovation. The treatment effect may depend both on the starting level of enforceability and the magnitude of the change. Indeed our results find a larger treatment effect for larger changes (possibly suggesting a role for salience), but it could also be true that, while NCAs dampen innovation locally, the sign of the treatment effect reverses as enforceability approaches zero. This could happen, for example, if the strength of the various channels changes across the range of enforceability. For this reason, extrapolating our local estimates to broader contexts requires caution. We do not claim in this paper that the optimal level of NCA enforceability is zero; rather, we only claim that, on average, the current level of enforceability appears to be too high to maximize innovation. Future research that employs structural models and richer data would allow for a more robust understanding of these trends.

Finally, this study examines the effects of NCA enforcement largely at the state and national level. This is likely to mask significant heterogeneity between different workers, firms, and industries, all of which may be insightful into how these channels operate and what the key determinants of knowledge diffusion are. Although we do not explore this potential heterogeneity in this paper, this would likely be a valuable area of future research.

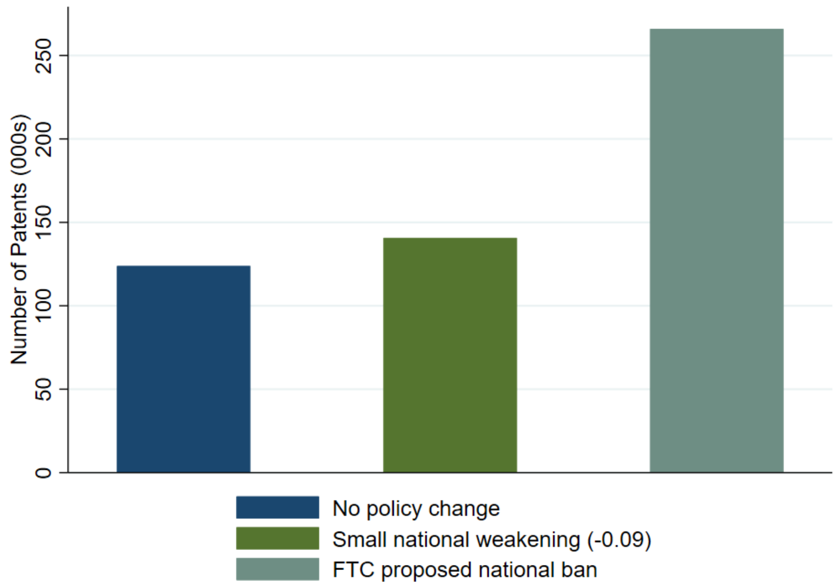
7 Discussion

Given these results, we can turn to think next about the effect of a federal policy that weakens NCA enforceability nationwide. Our preferred policy counterfactual is one that matches up well with the average treatments observed in our data. Therefore, we consider what the effect of a 0.09 point weakening in our normalized index would do to patenting in every state across the country.[44] Given the lack of significant spillovers in the other direction above, we use our baseline β estimate from 5 years after a hypothetical change for this exercise. The second bar of **Figure 22** shows what the effect of this federal decrease in national enforceability would do to patenting based on our point estimates – predicting a 14% increase in patenting nationwide.

One could take our results a step farther and extrapolate linearly to think about what these results might imply for the effect of the FTC's recent ban on NCAs. For the reasons discussed above, we heavily caveat such extrapolation. However, the sign and economically significant magnitude of the predicted effect should encourage policymakers to seriously consider the potential upside to innovation of such a ban. The third bar of **Figure 22** shows what the effect of this federal ban would do to patenting based on our point estimates. Extrapolating linearly, our results predict a 115% increase in patenting nationwide.

[44] In the case that a state's 2022 enforceability is already less than 0.09, we take that state to zero enforceability for this counterfactual.

Figure 22: Back-of-the-Envelope Calculation of the National Effect of Federal Rules that Decrease NCA Enforceability



The first (navy) bar shows the number of patent applications filed in 2016. The second (green) bar shows the predicted effect of a nationwide decrease of NCA enforceability equivalent to a 0.09 point change in the normalized enforceability index using our baseline estimated β when $h = 5$ (or five years after a change in enforceability). The third (teal) bar shows the analogous predicted effect of a nationwide ban of NCAs. For these counterfactual policy changes, we take a state's 2022 score on the normalized enforceability index as its initial condition. In the case that a state's 2022 enforceability is already less than 0.09, we take that state to zero enforceability for the second counterfactual prediction. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

8 Conclusion

This paper suggests that NCAs have a significant negative net impact on innovation, in contrast to what is often assumed in policy discussions. The impact is not only statistically significant but also economically significant: a strengthening from the median to maximum observed enforceability in our sample (i.e., 0.59 to 0.80 on our normalized index) is associated with a 28% decrease in patenting after five years. Even for a more modest change in enforceability, our estimates suggest that an increase of 0.09 points in our normalized index (the mean observed change) would decrease patenting by 13%.

This effect is not simply driven by NCAs restricting entry. In fact, our results find no statistically significant impact on entry from changes in NCA enforcement. This result is consistent with the idea that more enforceable NCAs might simultaneously introduce positive profitability incentives for entry and negative barriers to entry that cancel each other out overall. Our work here suggests that much of the effect may instead be coming from the knowledge diffusion channel, which implies an important role for labor mobility in innovation.

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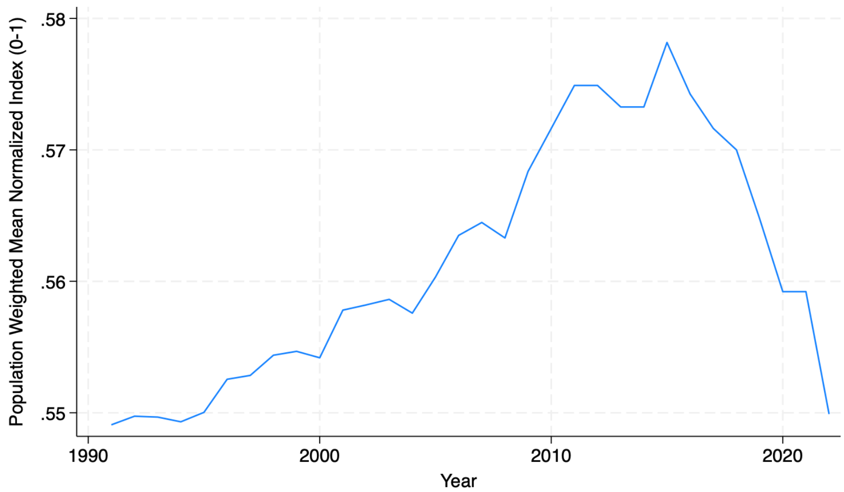
For Online Publication

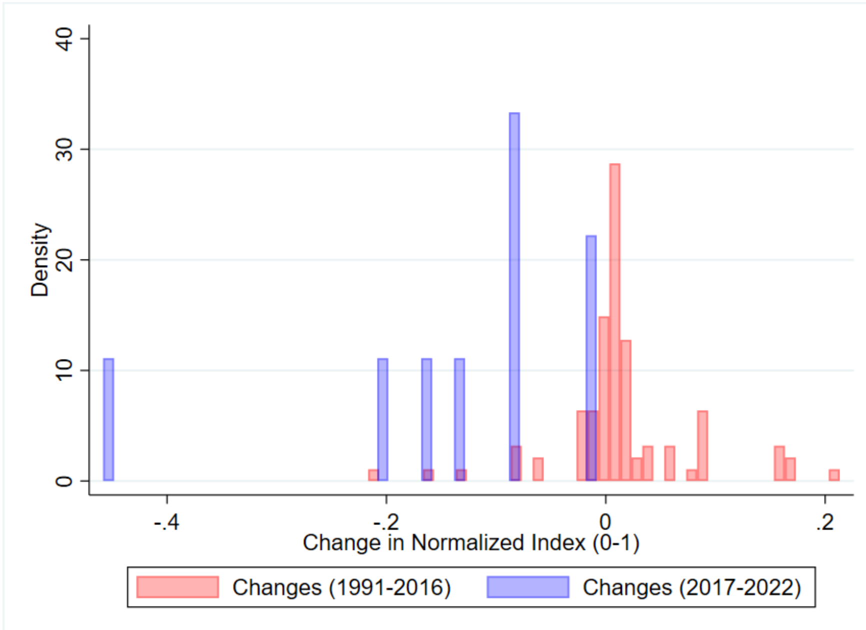
A Supplementary Figures

Figure 1: Trends in NCA Enforceability



Population-weighted average state-level NCA enforceability by year. Data sources: Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 2: Distribution of Non-Zero Changes in NCA Enforceability



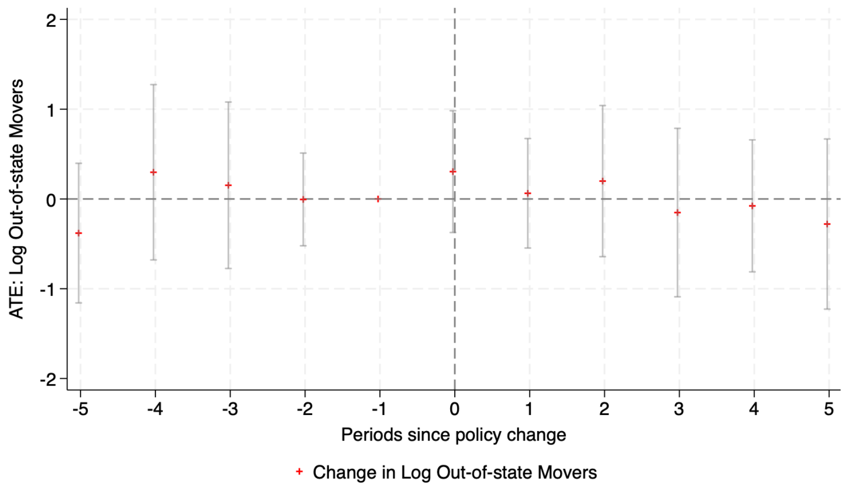
State-level changes in NCA enforceability, as measured by the normalized index, across years. Data sources: Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 3: Example Moving Inventor

Year	Inventor ID	Assignee #1	Assignee #2	Assignee #3	Assignee #4
1991	fl:ja_ln:hughett-1	X			
1992	fl:ja_ln:hughett-1	X			
1993	fl:ja_ln:hughett-1	X [Exit]			
1999	fl:ja_ln:hughett-1		X [Enter]		
2000	fl:ja_ln:hughett-1		X		
2003	fl:ja_ln:hughett-1		X [Exit]	X [Enter/Exit]	
2005	fl:ja_ln:hughett-1				X [Enter]
2008	fl:ja_ln:hughett-1				
2009	fl:ja_ln:hughett-1				X
2010	fl:ja_ln:hughett-1				X
2011	fl:ja_ln:hughett-1				X
2012	fl:ja_ln:hughett-1				X
2013	fl:ja_ln:hughett-1				X
2014	fl:ja_ln:hughett-1				X
2016	fl:ja_ln:hughett-1				X

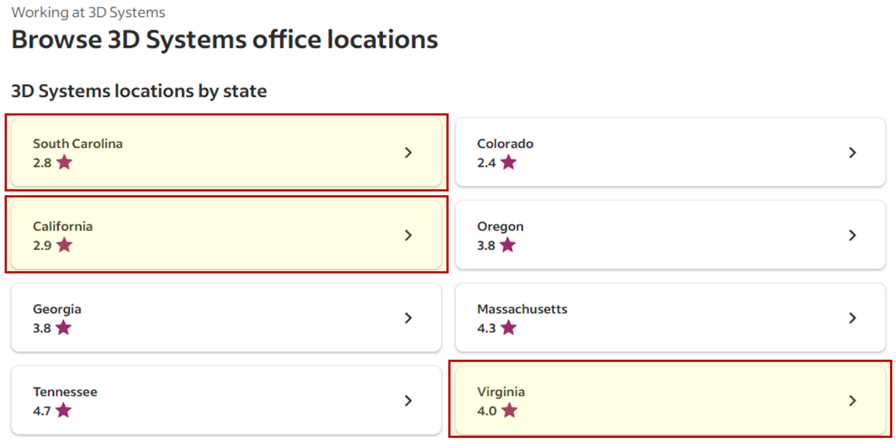
Example of an inventor switching between patenting at different firms over time. Data source: PatentsView. Details on the patent data are available in Section 2.2.

Figure 4: First Stage Results of Spillovers Regression



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of out-of-state movers in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 3. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates; clean organization lookup. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B. The clean organization lookup is discussed in additional detail in Footnote 35.

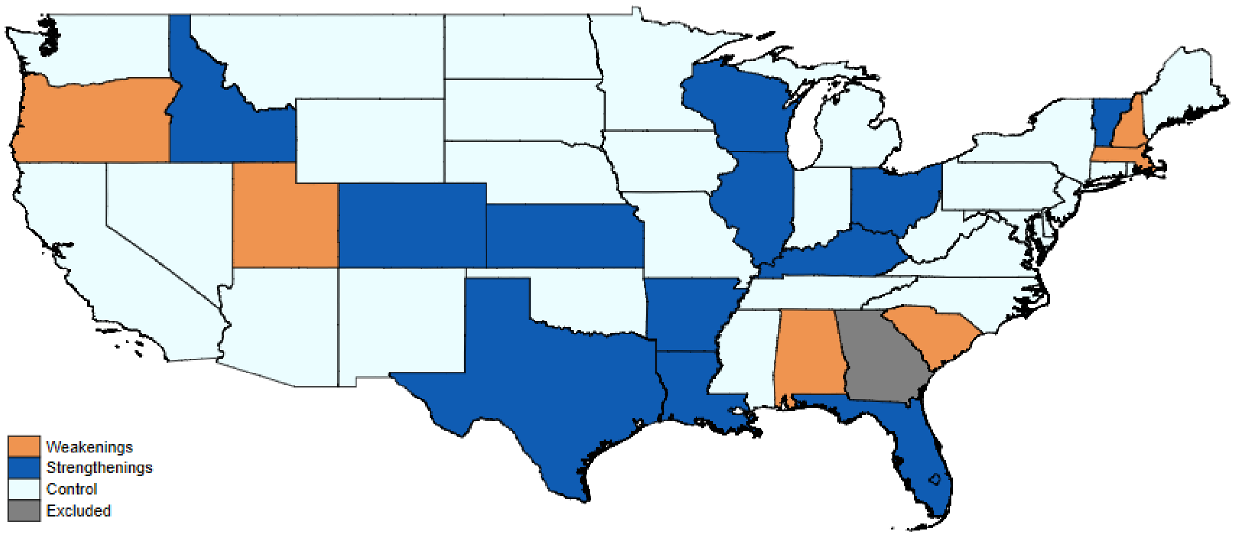
Figure 5: Example Assignee Locations



assignee_s~e	organization	new_id	patent_count
CA	3D System, Inc.	383	11
CA	3D Systems, Inc.	383	540
SC	3D Systems, Inc.	383	488
SC	3D Systems, Incorporated	383	7
VA	3D Systems, Incorporated	383	4

Example of a firm with multiple office locations also assigning patents to multiple locations in the patent data both within the same raw organization name(s) and after receiving a clean organization ID from the lookup created by this paper. Data sources: Indeed.com (link); PatentsView; clean organization lookup created by this paper. Details on the patent data are available in Section 2.2. The clean organization lookup is discussed in additional detail in Footnote 35.

Figure 6: Map of Baseline Sample



Map of state assignments to treatment and control groups. Note that states listed as treated may also be controls during their not-yet treated period or excluded in subsequent periods (e.g., if another treatment occurs). Data source: Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the index are available in Section 2.1 and Appendix Section B.

B Bishara (2011) Index Questions

1. Is there a state statute of general application that governs the enforceability of covenants not to compete? (Weight = 10)

   - Score = 0: statute that disfavors enforcement
   - Score = 5: no statute or statute that is neutral in its approach to enforcement
   - Score = 10: statute that favors strong enforcement

2. What is an employer's protectable interest and how is that defined? (Weight = 10)

   - Score = 0: strictly defined limited protectable interest
   - Score = 5: balanced approach to defining a protectable interest
   - Score = 10: broadly defined protectable interest

3. What must plaintiff be able to show to prove the existence of an enforceable covenant not to compete? (Weight = 5)

- Score = 0: strong burden of proof on the employer
- Score = 5: balanced approach to the burden placed on the employer
- Score = 10: weak burden of proof on the plaintiff employer

4. Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant? (Weight = 10)
   - Score = 0: start of employment is never sufficient
   - Score = 5: start of employment is sometimes sufficient
   - Score = 10: start of employment is always sufficient
5. Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun? Will continued employment provide sufficient consideration after the employment relationship has begun? (Weight = 5)
   - Score = 0: neither continued employment nor a beneficial change in terms would be sufficient consideration
   - Score = 5: only a beneficial change in terms was sufficient to support a covenant not to compete
   - Score = 10: continued employment is always sufficient
6. If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenant enforceable? If so, under what circumstances will the courts allow reduction and in what form? (Weight = 10)
   - Score = 0: strictly defined limited protectable interest
   - Score = 5: balanced approach to defining a protectable interest
   - Score = 10: broadly defined protectable interest
7. If the employer terminates the employment relationship, is the covenant enforceable? (Weight = 10)
   - Score = 0: not enforceable if the employer terminates
   - Score = 5: enforceable only in some circumstances
   - Score = 10: always enforceable if the employer terminates

C Additional Results

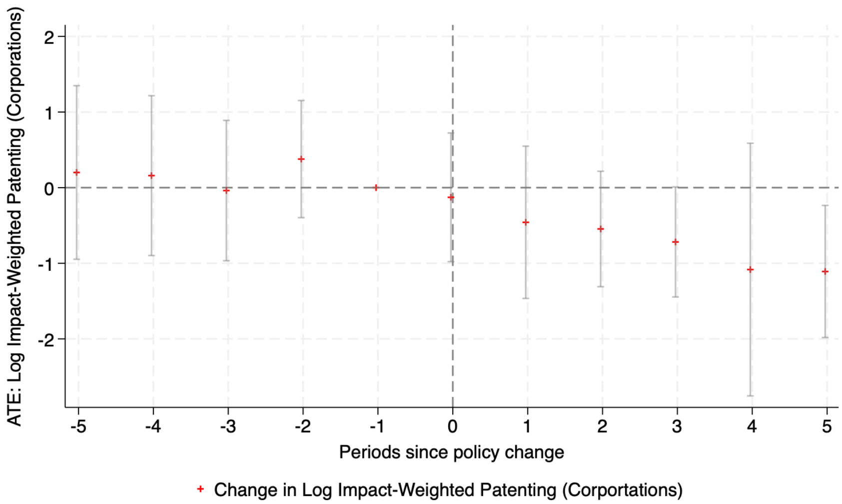
C.1 Impact and Novelty-Weighted Patent Outcomes

Forward citations are a common proxy for impact in the existing patent literature; they measure how many subsequent patents cite the patent in question and therefore are thought to approximate the extent to which a given patent has led to follow-on inventions. Backward citations are a slightly less common measure, but recent research suggests that they may actually capture technological "value" more effectively;[45] they measure how many *previous* patents were cited by the patent in question, with the intuition that having fewer backward citations indicates a more original invention.[46]

[45]E.g., see the discussion in Jaffe and de Rassenfosse (2017).

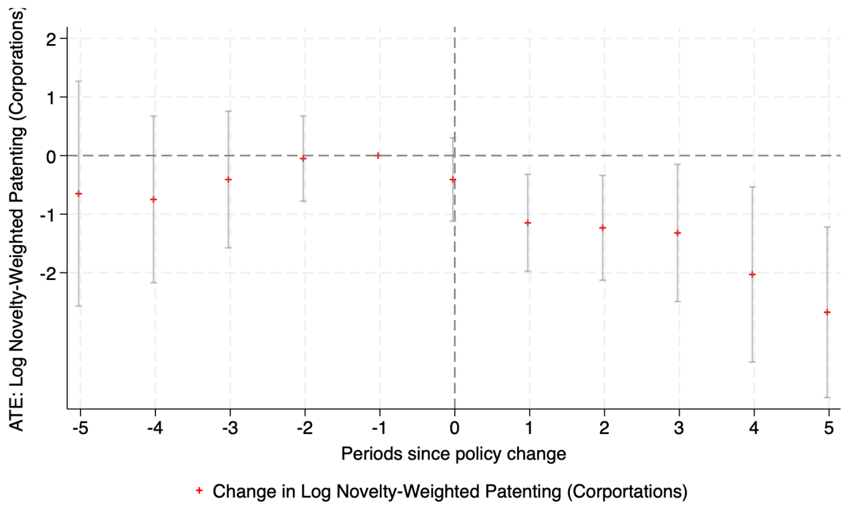
[46]For our novelty weighting, we weight patents by $1/(\text{backward cites} + 1)$. At the patent level, backward and forward citations are also winsorized to keep a single (either true or mismeasured) outlier patent from driving the results.

Figure 7: LP-DiD Coefficient Estimates: Log Impact-Weighted Patent Count



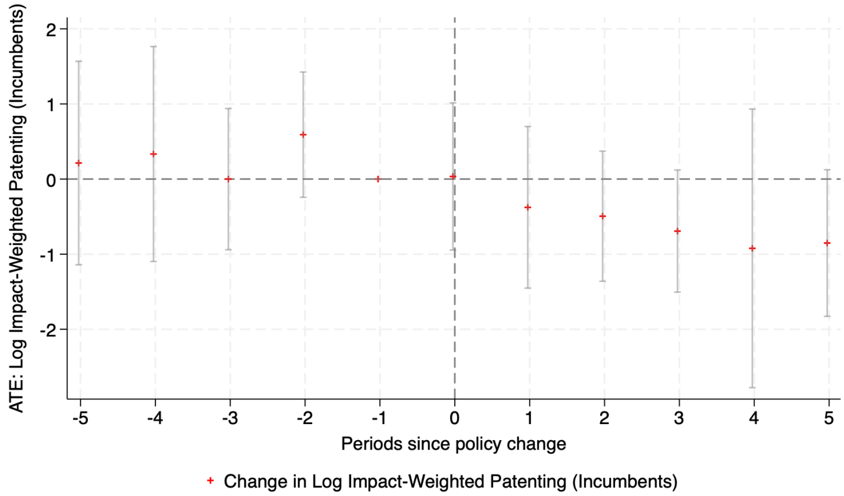
Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting weighted by forward citations in year h relative to the time of the policy change ($h = 0$). Forward citations measure how many subsequent patents cite the patent in question and therefore are thought to approximate the extent to which a given patent has led to follow-on inventions. However, we de-prioritize them in this analysis because of the potential for NCAs to affect not only patenting but also citation networks. Forward citations are windsorized at the 1st and 99th percentiles. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 8: LP-DiD Coefficient Estimates: Log Novelty-Weighted Patent Count



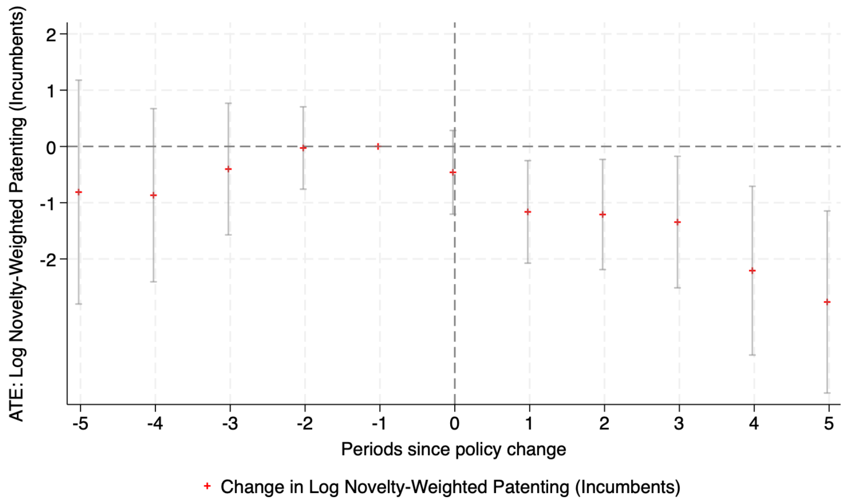
Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting weighted by inverse backward citations in year h relative to the time of the policy change ($h = 0$). Backward citations measure how many previous patents were cited by the patent in question, with the intuition that having fewer backward citations indicates a more original invention. Inverse backward citations are defined to be equal to $1/(\text{backward cites} + 1)$. Backward citations are windsorized at the 1st and 99th percentiles. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 9: LP-DiD Coefficient Estimates: Log Impact-Weighted Patent Count - Incumbents Only



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting by incumbents weighted by forward citations in year h relative to the time of the policy change ($h = 0$). Incumbents are defined as corporations with previous observed patenting. Forward citations measure how many subsequent patents cite the patent in question and therefore are thought to approximate the extent to which a given patent has led to follow-on inventions. However, we de-prioritize them in this analysis because of the potential for NCAs to affect not only patenting but also citation networks. Forward citations are windsorized at the 1st and 99th percentiles. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 10: LP-DiD Coefficient Estimates: Log Novelty-Weighted Patent Count - Incumbents Only

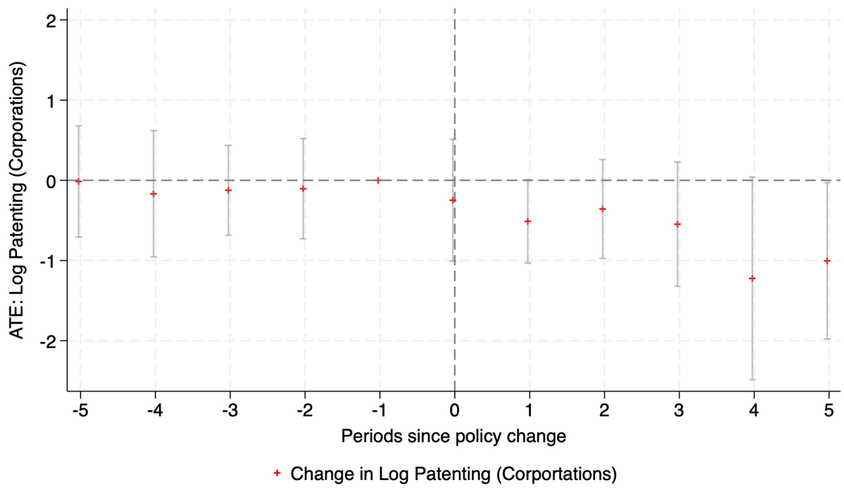


Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting by incumbents weighted by inverse backward citations in year h relative to the time of the policy change ($h = 0$). Incumbents are defined as corporations with previous observed patenting. Backward citations measure how many previous patents were cited by the patent in question, with the intuition that having fewer backward citations indicates a more original invention. Inverse backward citations are defined to be equal to 1/(backward cites + 1). Backward citations are windsorized at the 1st and 99th percentiles. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

D Robustness Checks

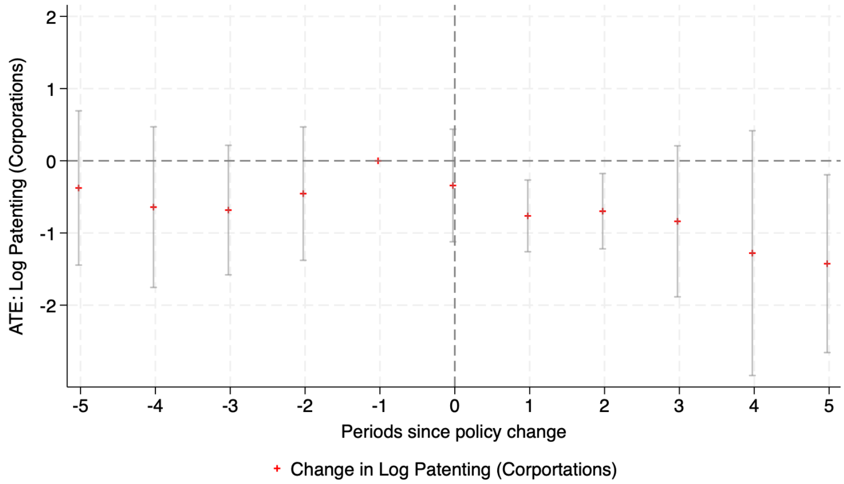
D.1 Alternate Patent Location Definition

Figure 11: LP-DiD Coefficient Estimates: Log Patent Count - Inventor Location



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Uses the location of the first-listed inventor rather than the location of the assignee as the location of the patent. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 12: LP-DiD Coefficient Estimates: Log Patent Count - Shared Location



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Uses the shared location of the first-listed inventor and assignee as the location of the patent. Excludes patents with different author and assignee locations. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

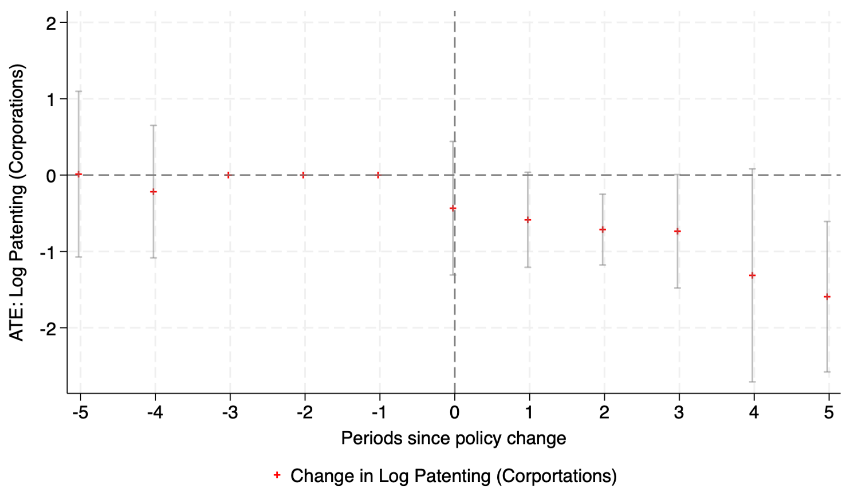
D.2 Example Results with Outcome Lags in LP-DiD

As discussed in Dube et al. (2023), we can also include outcome lags on the right-hand side of the estimating equation to control for pre-treatment values of time-varying covariates:

$$\begin{aligned} y_{i,t+h} - y_{i,t-1} = \beta_h \cdot \mathbb{I}_{it} \cdot \Delta X_{it} &\quad \text{treatment (change in index)} \\ + \sum_{k=1}^{K} \gamma_k^h \cdot y_{i,t-k} &\quad \text{outcome lags} \\ + \delta_{t+h} - \delta_{t-1} &\quad \text{time effects} \\ + \epsilon_{it+h} &\quad \text{for } h = -H, ..., H, \end{aligned} \tag{1}$$

where we include K lags of the outcome variable, which helps control for any concerns about patenting predicting changes in enforceability. Doing so gives very similar results for our patent outcomes (see below).

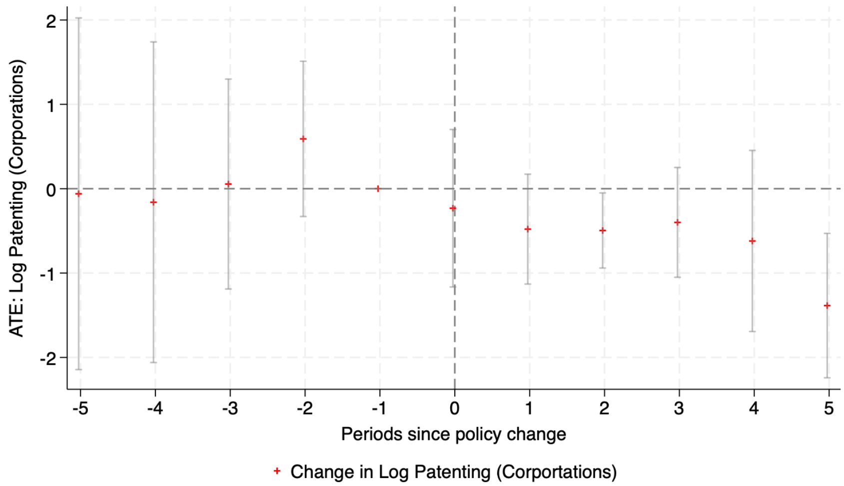
Figure 13: LP-DiD Coefficient Estimates: Log Patent Count - Conditioning on Lagged Outcomes



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

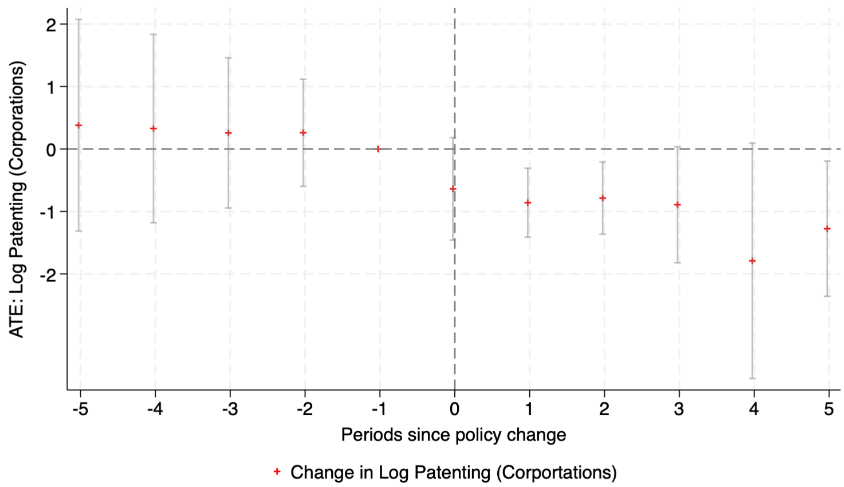
D.3 Example Results with Alternate Weighting

Figure 14: LP-DiD Coefficient Estimates: Log Patent Count - Unweighted



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are not weighted. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

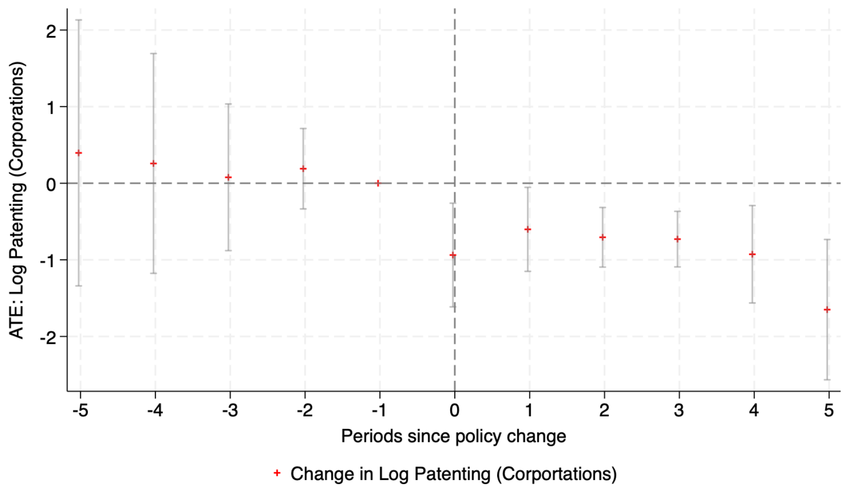
Figure 15: LP-DiD Coefficient Estimates: Log Patent Count - Weighted by Share of Patents



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of patent applications filed in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

D.4 Results with Balanced Panel

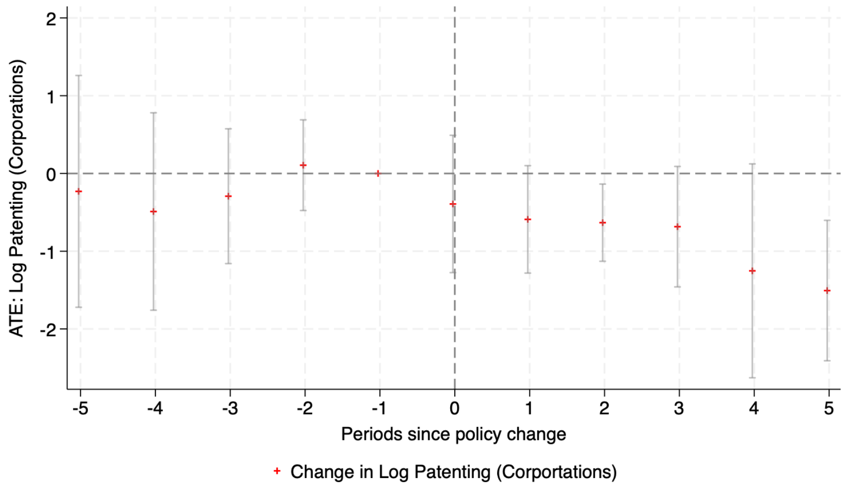
Figure 16: LP-DiD Coefficient Estimates: Balanced Panel



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Limited to a balanced panel where we can observe a full clean 5 years of outcomes on either side of t. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

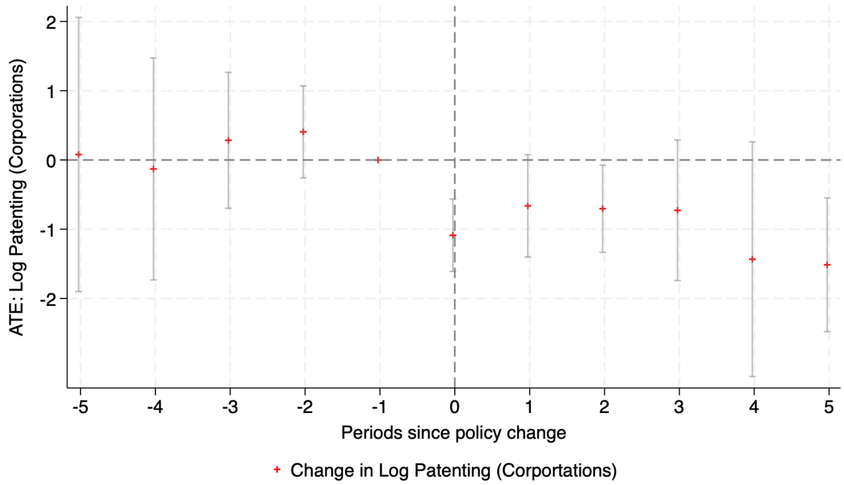
D.5 Alternative Sample Definitions

Figure 17: LP-DiD Coefficient Estimates: Log Patent Count - Excluding California



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Excludes California from the analysis. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

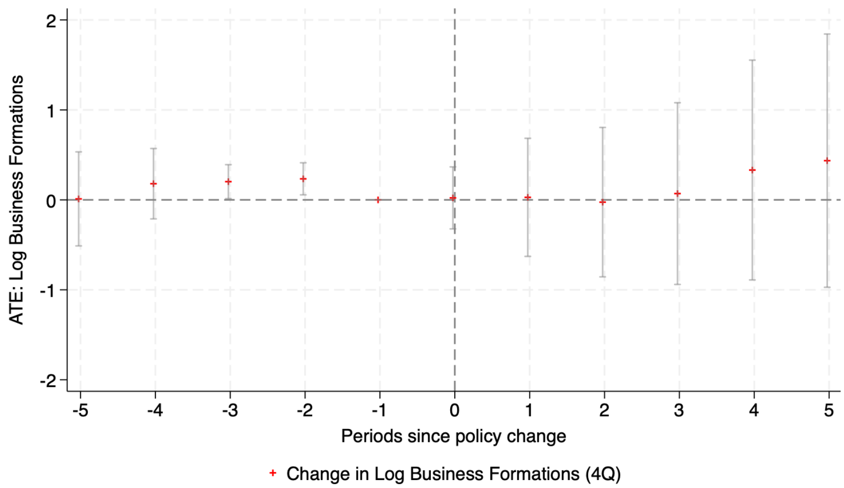
Figure 18: LP-DiD Coefficient Estimates: Log Patent Count - Strengthenings Only



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log of state-level corporate patenting in year h relative to the time of the policy change ($h = 0$). Excludes treatments that are weakenings (i.e., decreases in enforceability) from the analysis. Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: PatentsView; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the patent data are available in Section 2.2. Details on the index are available in Section 2.1 and Appendix Section B.

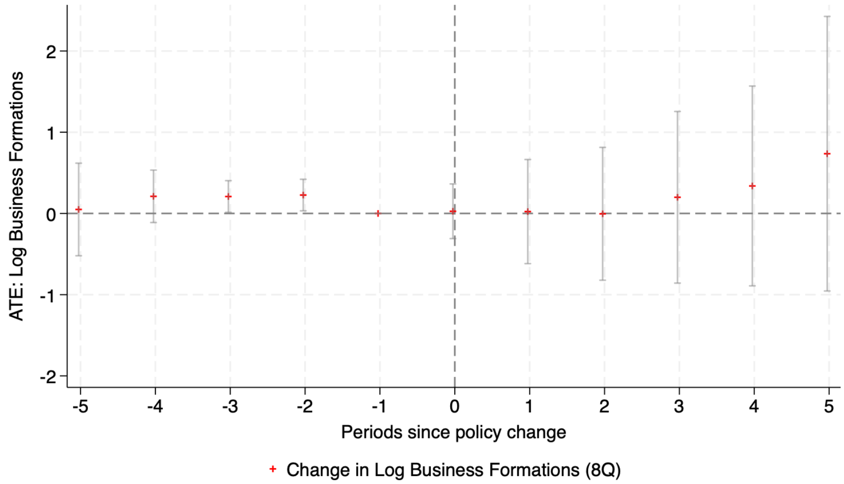
D.6 Entry Robustness Checks

Figure 19: LP-DiD Coefficient Estimates: Log Entry (Four-Quarter Business Formations)



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log state-level count of business formations within four quarters of the year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: Census BFS; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the business formations data are available in Section 2.3. Details on the index are available in Section 2.1 and Appendix Section B.

Figure 20: LP-DiD Coefficient Estimates: Log Entry (Eight-Quarter Business Formations)



Plot of the estimated average treatment effect of a 0 to 1 change in NCA enforceability (according to the normalized index) on the log state-level count of business formations within eight quarters of the year h relative to the time of the policy change ($h = 0$). Details on the econometric specification can be found in Equation 1. Error bars show 95% confidence intervals. Standard errors are clustered at state level. States are weighted by share of population in the previous year. We specify $c = 15/600$ and $H = 5$. Data sources: Census BFS; Bishara (2011) index that summarizes NCA enforceability by state, as expanded by Marx (2022) and this paper, normalized to be [0,1] rather than [0,600]; Census Bureau's Annual Population Estimates. Details on the business formations data are available in Section 2.3. Details on the index are available in Section 2.1 and Appendix Section B.