How Reduced Labor Mobility Can Lead to Inefficient Reallocation of Human Capital

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Abstract

This paper analyzes the effect of non-compete agreements on career trajectories of highly skilled employees, namely around 0.5 million inventors in the US. Inventors evade non-compete agreements by moving to a different firm in another industry. I identify causal effects using staggered changes in non-compete agreement enforceability across US states. Inventors are 67% more likely to move to another industry after an increase in non-compete agreement enforceability. The effect is stronger for highly skilled and younger inventors. Non-compete agreement induced industry movers subsequently experience a decrease in productivity of 30%. In contrast, inventors who move across industries in absence of labor market frictions are subsequently 16% more productive. Overall, labor market frictions lead to inefficient reallocation of human capital in the economy.

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1. Introduction

I analyze career choices of employees in response to non-compete agreements, which are covenants that restrict employees from working for competitors during and after employment. Employers commonly use non-compete agreements to retain valuable human capital within firm boundaries. There is an ongoing debate in economics, finance, and among policy makers about benefits and drawbacks of these agreements.[1] They can benefit employees, because of increased incentives for employers to invest in employees' human capital. However, the cost is reduced labor mobility (Marx et al. 2009).

I find, in contrast to reduced labor mobility, that more enforceable non-compete agreements *increase* inter-industry mobility. Employees evade non-compete agreements by moving to a firm in another industry. Future productivity of industry movers decreases, which is consistent with the interpretation that increased enforcement of non-compete agreements leads to inefficient reallocation of human capital.

My analysis uses data of around 0.5 million US corporate inventors from 1976 to 2018. Patent data provides a suitable laboratory to study non-compete agreements and the efficient allocation of labor in the economy for three reasons: First, patents provide the precise location of inventors and detailed employment histories. The corporate employers of these inventors provide measures of industry affiliation. Second, inventors are highly skilled individuals and, as such are likely affected by non-compete agreements. Third, patent data provide a time series measure of productivity (e.g. citations received, economic value based on market reactions to patent grants) on a granular level.

Staggered changes of non-compete agreement enforceability across U.S. states provide

[1] Among others, see Garmaise (2011); Jeffers (2017); Starr (2019); Marx and Fleming (2012); Samila and Sorenson (2011); He (2021). There are recent policy proposals, e.g. https://www.whitehouse.gov/briefing-room/statements-releases/2021/07/09/fact-sheet-executive-order-on-promoting-competition-in-the-american-economy/

variation for estimating causal effects. More precisely, 21 states experienced changes in non-compete enforcement either in the form of precedent-setting court cases or state laws. Non-compete agreements are often limited in terms of geography, duration, and important for this paper: industry scope. An increase in enforcement effectively means that non-compete provisions are potentially more legally binding. Employees effectively have fewer outside employment options at competitors in the same industry.

In a staggered difference-in-differences regression, increases in non-compete agreement enforcement are positively related to the probability that an inventor moves to another SIC 3-digit industry. 2 in 100 additional inventors move to another firm in another industry after an increase in non-compete enforcement. This implies a 67% increased probability of an industry move. This regression, using inventor and year fixed effects, exploits the staggered timing of 9 non-compete enforcement increases across states. There is no effect for decreases in non-compete agreement enforcement.

Econometric theory provides guidance on the regression design: I compare treated inventors (i.e., those exposed to an increase in non-compete agreement enforceability) to never-treated in an event study framework (Baker et al. 2022; Borusyak et al. 2021, de Chaisemartin and d'Haultfoeuille 2021, Callaway and Sant'Anna 2021, Sun and Abraham 2021). I match treated inventors to control inventors based on their quality as measured by number of patents and citations received, as well as age. Moreover, to proxy for technological shocks, I also match on patent technology.

The effect of increased non-compete agreement enforcement on inter-industry mobility is not immediate, but rather long run and increasing over time. Consistent with a causal interpretation of the results, there are no pre-trends.

If increases in non-compete enforcement cause inter-industry mobility, then inventors that are likely bound to a non-compete agreement should drive this result. Unfortunately individual contracts of inventors are unobserved. However, I compute a proxy as follows: First, I obtain all annual and quarterly (10-K and 10-Q) reports of the employers in the sample from 1994-2020. These reports often include contract information and non-compete agreements of senior employees. I hand collect this data and compute a dummy equal to one if a firm relies on non-compete agreements. The assumption is that this firm-level variable to some extent proxies for the presence of non-compete agreements on an inventor level. I estimate a triple-difference regression, and indeed, the effect seems to be present only for inventors whose employers do rely on non-compete agreements. This is in line with a causal interpretation of the results.

The natural follow-up question to ask is: What is the effect on productivity if inventors move across industries in response to increased enforcement of non-compete agreements? On one hand, it might be beneficial to society if increased inter-industry mobility leads to more idea recombination, and thus more innovation. On the other hand, inventors might perform worse after a non-compete agreement induced industry move. In the following I refer to an across industry move following an increase in non-compete agreement enforcement as a *constrained* industry move. In a difference-in-differences regression, constrained industry movers perform 30% worse compared to inventors who stay. There are no productivity differences (pre-trends) between inventors who stay and leave before one subset moves across industries. Looking at within-inventor variation, they perform 22% worse compared to themselves after an industry move. The results also hold in an instrumental variable regression, when instrumenting industry move with state-level increases of non-compete agreement enforcement. This ultimately measures the local average treatment effect on future innovation output of those inventors who move industries. This is the subset of individuals we are interested in. These so-called compliers are precisely those inventors that are forced to move industries because of an increase in non-compete enforcement. The main variable of interest for this analysis is the economic value of patents, measured as stock market reaction to patent grants. The results are similar when using citation-weighted patents. Constrained industry movers are significantly less productive after they move. This evidence is consistent with the hypothesis that labor market frictions lead to inefficient reallocation of labor.

In contrast to the evidence before, unconstrained industry mobility *increases* future productivity of inventors. I define unconstrained industry mobility as an inventor moving to another firm in another industry without considering non-compete enforcement changes. Inventors who move to another industry are subsequently 16% more productive. This effect holds when comparing inventors to similar ones who do not move. They are also more productive after moving to another industry compared to themselves. I also compare inventors who move to another industry to inventors who move to another firm but within industry. Inventors who move across are subsequently 28% more productive compared to inventors who move within industries. While I do not claim causal effect of inter-industry mobility on productivity, the analysis highlights a sharp distinction: Moving to another industry (unconstrained) seems to increase productivity, however moving to another industry after an increase in non-compete agreement enforcement (constrained) reduces productivity.

The last question I ask is: What type of inventors are more likely to move industries to evade non-compete agreements? I first use birth record data and look at heterogeneity with respect to age. The effect is stronger for younger and for high-quality inventors. These individuals have higher opportunity cost and are likely to possess possibilities to move to employers in other industries, whereas this is not the case for older and low quality inventors.

This paper contributes to several strands of literature. First, to the literature on the real effects of non-compete agreements. Previous research has shown that non-compete agreements lead to lower labor mobility (Fallick et al. 2006; Marx et al. 2009; Balasubramanian et al. 2020), as well as a brain drain of enforcing states (Marx et al. 2015). I add an industry dimension and highlight that inter-mobility increases, as inventors evade non-compete agreements by moving to a firm in another industry. The paper is thus closely related to Marx (2011), who provides survey evidence consistent with the empirical results presented in this paper. My setting additionally allows to focus on long run employment effects and an important outcome for society: innovation.

I also add to the allocation of labor literature (Babina et al. 2020; Babina 2020; Hombert and Matray 2017; Hombert and Matray (2018)). I show how labor market frictions can lead to inefficient reallocation of labor in the economy, which is a likely unintended consequence for policy makers in the context of non-compete agreement enforcement. Lastly I add to the literature on firm and industry boundaries and the productivity of labor (Seru 2014; Hacamo and Kleiner 2022). I highlight an important distinction: unconstrained inter-industry mobility seems to be beneficial for society, however constrained inter-industry mobility is not.

2. Data

The research question looks at how high-skilled employees react to an increase non-compete agreement enforcement. The next section describes the data sources needed to construct a panel on an inventor-year level. The next section gives details on non-compete agreements and state-level enforcement.

2.1. Employment Histories of Corporate Inventors

I obtain data on corporate innovation from 1976 until 2020 from two sources. Patents matched to firms from Kogan et al. (2017), commonly referred to as KPSS, as well as from the DISCERN database of Arora et al. (2021).[2] This provides a list of patent numbers and a corporate identifier.

The next step is to match individual inventors to these patents. The United States Patent and Trademark Office (USPTO) provides detailed data on patents. Most importantly for this paper, the USPTO provides disambiguated inventor-level data.[3] The USPTO does not require inventors to provide a unique identifier when filing patents. Thus, disambiguated data allows to track individual inventors over time. I add to this data web-scraped information such as the age and the gender of inventors, obtained from birth records of US inventors (Kaltenberg et al. 2021).

Innovation is an ideal laboratory for several reasons: First, the universe of corporate patenting provides tractable employment histories of inventors based on granted patents.[4] Second, patent documents also capture the location (on a city level) of each inventor listed on a patent. This greatly improves measurement for empirical research that uses location-based variation in treatment. Previous studies often proxy for location using the headquarter location of the employer corporation. Third, corporate innovation allows to use industry affiliation such as SIC and NAICS industry codes, as well as text-based industry classifications (Hoberg and Phillips 2010; Hoberg and Phillips 2016). Fourth, patent data provides a useful performance metric on a patent basis. A researcher can thus observe the number of patents, the number of citations received[5] Lerner and Seru

[2] The KPSS data with matched patent data is updated until the end of 2020 and available here: https://github.com/KPSS2017/Technological-Innovation-Resource-Allocation-and-Growth-Extended-Data; The DISCERN database includes patents matched to firms (including subsidiaries) until 2015 and is available here: https://zenodo.org/record/4320782

[3] Which builds on previous efforts such as the NBER patent citation data file as well as disambiguated inventor-level data of Li et al. (2014).

[4] The caveat here is that non patented innovation is unobserved and thus total labor mobility is underestimated

[5] Newer patents mechanically have less time to accumulate citations than older patents. In order to mitigate this problem I follow Hall et al. (2005), Dass et al. (2017), and Lerner and Seru (2021). When using citation as a measure of innovation output, I adjust all cumulative citations received until January 2022 and perform a truncation adjustment by adjusting with respect to year and technology class.

(2021), and the economic value of patents Kogan et al. (2017). Importantly, this measure is available before and after an employment change. Last, human capital intensive jobs such as inventors are likely to be affected by non-compete agreements, which will be discussed in more detail in the next section.

2.2. Data on Non-Compete Agreement Enforcement Changes

What exactly are non-compete agreements? Non-compete agreements usually put limitations on industry, geographic reach (which ranges from a well defined radius, a state, country or even worldwide), and duration (mostly 1-2 years) of employees. The Appendix lists some examples on non-compete clauses in executive or director contracts. Microvision states in the annual statement that the firm relies on non-compete agreements. Nuance Communications explicitly mentions that they prohibit employees "from working for an employer who is engaged in activities or offers products that are competitive with the activities and products of the company."

I summarize changes in state-level non-compete agreement enforcement in Table 1. The data relies on Ewens and Marx (2018), who provide an extensive discussion on court rulings and legislative changes from 1985-2016.[6] Kini et al. (2021) is the second source of data. They extend a score of non-compete enforceability across states originally developed by Garmaise (2011) to the years 1992-2014.

What happens when non-compete agreements are more enforceable? Restrictions included in a non-compete agreement and what is ultimately enforceable can differ strongly. States such as California and North Dakota are famously opposed to enforcing non-compete agreements. Florida is on the other end of the spectrum and enforces non-compete agreements most strictly. Often, non-compete agreements are enforceable conditional on passing a "reasonableness" test. After a 1996 legislative change, non-compete

[6] The data is available here: https://github.com/michaelewens/Non-compete-Law-Changes

agreements in Florida need to protect "legitimate business interests" in order to be enforceable. This clarified previous uncertainty and shifted power towards employers.[7]

For some specifications, I use data on firm-level reliance on non-compete agreements. I obtained form 10-K and form 10-Q filings from EDGAR. These were parsed and stripped of figures, pictures and html tags. I obtain identifiers from historical Compustat from WRDS servers, as well as a historical CIK-CUSIP mapping.[8] Form 10-K and form 10-Q filings commonly include non-compete agreements of senior employees at a firm. The information was used to construct a panel of US corporations with an indicator variable equal to 1 if the corporate employer mentions the use of a non-compete agreement either in an executive/board contract or mentions the reliance on non-compete agreements in the annual statement. I perform a manual collection and obtain a panel on a firm-year level whether a firm relies on the use of non-compete agreements. In my sample, 54% of firms rely non-compete agreements. This is close to the other survey and empirical evidence. Starr et al. (2021) finds that almost a fifth of all employees in the US are bound by non-compete agreements. The share of non-compete contracts for technical workers is around 50% (Marx 2011), 62.5% for CEOs with employment contracts (Kini et al. 2021), and 70% for executives (Garmaise 2011).

[7]There are many other examples on how non-compete agreements become more enforceable. For example, the Ohio Supreme Court decided in 2004 that a sufficient consideration to uphold a non-compete agreement was continued employment. Another example is Idaho, which changed to a so-called "blue pencil" rule where a judge can modify the contract to make it more reasonable whereas in other states one invalid part of a non-compete clause renders the whole agreement void. Interested readers should refer to Marx and Fleming (2012) for history, background as well as other work by Matt Marx. Ewens and Marx (2018) provide extensive details on individual court cases and legislative changes

[8]This mapping was obtained from the website of Ekaterina Volkova: https://sites.google.com/view/evolkova/data-cik-cusip-link

2.3. Sample Construction and Descriptive Statistics

The sample construction starts with all corporate innovation from the two sources mentioned previously. This gives a mapping with a unique identifier for each corporation and the patent number assigned by the USPTO. In principle, data on corporate patents is available from 1926, however the USPTO provides digitized patent information with disambiguated inventor data from 1976 onwards, which marks the start of the sample. In a next step, I merge the inventors of all corporate owned patents with the disambiguated inventor data. The resulting dataset is a panel on an inventor-year level.

I identify industry employment changes as follows: The inventor files two subsequent patent applications for a different employer with a different industry affiliation. I follow the previous literature (Song et al. 2003; Marx et al. 2015 and use the yearly midpoint between two subsequent patents to proxy for the change in employment.[9] The application year rather than the grant year is used, in order to have a more timely measure of innovation creation[10] and employment changes. There are two data cleaning steps, both of which do not change the results when removed: First, there are many inventors that assign their patents to many different firms. I remove inventors with more than five employer. It is unlikely that these inventors have standard employment contracts, but are rather part of a joint venture or other research and development co-operations. Second, I remove inventors that only have one patent. All regressions include inventor fixed-effects, so these inventors would not provide any meaningful variation on labor market employment.

Table 2 shows descriptive statistics. The timeframe is from 1976-2018. In total, the

[9] Patent-based measures of employment histories thus include measurement error. On average, there is a gap of 0.9 years between two subsequent patents filed by the same inventor. The median number of years between two filings is zero. When alternatively limiting the sample to patent filings with at most one year between two subsequent patents, the results become stronger.

[10] This avoids a lag between applying for and being granted a patent, which is 4 years at the median.

panel includes 3,803,179 inventor-year observations. This includes data of around 1.5 Million patents of roughly 0.5 Million inventors. The sample includes 6,345 listed firms as employers. An industry move - defined as an inventor moving between two firms and changing industries defined a SIC 3-digit level - appears in 3% of all inventor-year observations. I compare this to the previous numbers in the literature such as Melero et al. (2017) who show based an patent application data, that inventors move employers (without considering industries) at a rate of 10% per year. The mean age of an inventor is 43 and the median is 42. The mean number of patents granted is 10.1 and the number of truncation adjusted citation-weighted patents is 7.1. These moments are well aligned with the literature.

3. Staggered State-Level Changes in Non-Compete Enforcement

3.1. Panel Baseline

Using staggered changes in non-compete agreement enforcement across US states, I estimate the following panel regression:

$$IndustryChange_{i,s,t+1} = \beta \times NCA_{s,t} + \theta_i + \phi_t + \epsilon_{i,s,t} \tag{1}$$

where i represents inventor i, located in state s, in year t. The dependent variable $IndustryChange_{i,s,t+1}$ is defined as equal to one if an inventor moves between two firms with different 3-digit SIC industry codes.[11] I separate the treatment indicator into $NCAIncrease \times Post$ and $NCADecrease \times Post$ whether state s decreased, or increased the enforceability of non-compete agreements. Panel A and B of Table 1 provide an overview of these events. The variables θ and ϕ are inventor and year fixed-effects, re-

[11] Alternatively, I use SIC 2-digit, NAICS 4-digit as well as textual-based industry definitions following Hoberg and Phillips (2010), Hoberg and Phillips (2016)

spectively. Year fixed-effects account for year-specific shocks to mobility. Inventor fixed-effects control for constant unobserved factors on the inventor level. I cluster standard errors on a state level, which is the level of treatment. Different levels of clustering do not change the results. Essentially, the methodology estimates a difference-in-differences regression, that compares inventors located in states with a change in the enforcement of non-compete agreements with those that did not.

Table 3 shows the results. An increase in non-compete agreement enforcement leads to an increase in industry mobility of 2%. In terms of economic magnitudes, an increase in non-compete enforcement thus leads to 2 in 100 additional inventors changing SIC 3-digit industries. Given that the average industry mobility in the sample is 3% per year, this implies a 67% increased probability that an inventor moves to another SIC 3-digit industry after a change in non-compete agreement enforcement. Looking at Panel B, there is no effect when looking at decreases in non-compete agreement enforceability.

This analysis is based on staggered difference-in-differences across 15 states, 9 of which experienced an increase and 6 a decrease. A necessary condition for a causal interpretation is that treated and untreated inventors share parallel trends. In order to test this assumption, as well as incorporate recent developments in the econometrics literature, the following section explicitly looks at the timing of the treatment effect and dynamic effects.

3.2. Event Study and Dynamic Effects

I estimate the following event study regression:

$$IndustryChange_{i,s,t+1} = \sum_{k=-5}^{k=+10} \delta_k \times D_k + \sum_{k=-5}^{k=+10} \beta_k \times D_k \times NCAIncrease_{s,t} + \theta_i + \phi_t + \epsilon_{i,s,t} \tag{2}$$

Where D_k indicate time dummies relative to the event. The coefficients of interest are β_k which capture the treatment indicator interacted with 4 pre-treatment dummies and 10 post-treatment dummies. All coefficients are estimated relative to one year before treatment.

I use matching to compare treated and control inventors. I match inventors based on year of activity (whether they are currently employed at a firm), age of the inventor, lagged number of patents, lagged total citations, both to capture a similar quality of inventor. I also include patent technology to guarantees that treatment and control inventors are exposed to similar technological shocks. I match the three nearest neighbors with replacement using the Mahalanobis distance. The analysis again includes inventor as well as year fixed effects. I cluster standard errors on the inventor and year level.

A two-way fixed effect estimation of a staggered difference-in-differences design are weighted averages of all possible two-group difference-in-differences estimators (Goodman-Bacon 2021). A potential problem are dynamic treatment effects when we compare early-treated to late-treated inventors Baker et al. (2022). I follow recent econometric theory when exploiting the state-level changes of Table 1. The analysis therefore only compares treated with never-treated inventors. Thus, I compare inventors based in states that experienced increased enforcement of non-compete agreements with clean controls: those inventors that did not experience any changes during the sample period. I use a number of recently proposed estimators such as Borusyak et al. (2021), de Chaisemartin and d'Haultfoeuille (2021), Callaway and Sant'Anna (2021), and Sun and Abraham (2021).

Figure 1 visualizes the results from Equation 2. They are well-aligned with the baseline evidence. There is no sudden jump in the probability that an inventor changes industries, but rather a steady increase over time that is statistically significant starting from year 3 after the treatment. In year 4, the effect is around 0.01, which is a 33% increase in the probability that an inventor moves industries. The alternative estimators are close to the OLS estimates. Figure A5 shows that there is no effect when looking at decreased non-compete agreement enforcement.

3.3. Does Increased Non-Compete Enforcement Cause *Industry Mobility?*

In order to interpret the results as causal, the critical assumption is that treatment and control inventors are equally likely to change industries in absence of treatment. As a necessary feature, I can assess whether treated and control inventors experience parallel trends. Reassuringly, the event study in Figure 1 shows no detectable pre-trend in the years before treatment. The delayed effect raises the question whether on-compete enforcement increases *should* lead to immediate labor market effects.

There are several reasons for a delayed response: E.g. the Florida law change in 1996 was explicitly only applicable to contracts signed after July 1, 1996.[12] This would mean that only employees that start working after this date are exposed to increased non-compete agreement enforcement. To increase the chances of legal protection, Ewens and Marx (2018) note that employers commonly require their employees to sign updated employment contracts, which might therefore not lead to immediate responses. For the Georgia 2010 case, Ewens and Marx (2018) interviewed an employment attorney, who stated: "when the new law went into effect (including our firm) and many employers revised their employment and restrictive covenant agreements to take advantage of the law". This practice would not lead to an immediate reaction. Setting the legal point of view aside, there are additional considerations for a delayed response from the point of view of employees. Inventors willing to move might not be well aware of the details of their non-compete agreement. They might find out years after about the increased enforcement of non-compete agreements.

[12] However Ewens and Marx (2018) note that continued employment suffices as consideration.

A potential problem for a causal interpretation is whether state legislative changes are correlated with other factors that determine industry mobility. State legislative changes might be problematic if the desired policy change is anticipated. There are two reasons why this is unlikely to be a threat to identification in my setting. First, Jeffers (2017) shows that the state-level shocks are unrelated to macroeconomic conditions and cannot be easily predicted. Given the focus on inter-industry mobility, the positive effect on industry changes of inventors is a plausible unintended consequence of regulatory changes. Nevertheless, the analysis is repeated and is robust when only considering court cases, which are arguably more exogenous compared to state legislative changes.

Overall, the findings are consistent with interview evidence of Marx (2011) where employees admit to taking career detours given that their non-compete agreement prohibited them from working in a similar industries for the next 1-2 years. Marx (2011) interviewed one speech recognition professional who left the industry after being fired by his coauthor. "Well, if I'm ever gonna leave, what would I do for 2 years if I couldn't do speech recognition?"

3.4. Is the Effect Stronger in the Presence of Non-Compete Agreements?

If indeed non-compete enforcement changes lead to increased inter-industry mobility of inventors, then we would expect this effect to be stronger for inventors that are in fact bound to a non-compete agreement. Unfortunately individual level non-compete agreements of inventors are unobserved. However, employers might differ on how much they rely on non-compete agreements.

I formally test whether increased enforcement of non-compete agreements leads to more industry mobility especially for those inventors employed at firms that use non-compete agreements. For this purpose, a triple difference-in-differences regression is run which compares inventors exposed to the treatment to those exposed to the treatment but also employed at firms using non-compete agreements as follows:

$$
\begin{aligned}
IndustryChange_{i,s,j,t+1} = \beta{\times}NCAIncrease_{s,t}{\times}Post_{s,t}+ \\
\delta{\times}NCAIncrease_{s,t}{\times}Post_{s,t}{\times}NCA_{j,t}+\theta_i+\phi_t+\epsilon_{i,s,j,t}
\end{aligned} \tag{3}
$$

where NCA is an indicator variable equal to one if the employer heavily relies on non-compete agreements. Table 4 shows the results. First we look at the baseline effects of NCA. The variable is positively related to the probability that an inventor moves across industries. The interaction coefficient $NCAIncrease{\times}Post{\times}NCA$ is positive and significant. The difference-in-differences term $NCAIncrease{\times}Post$ is now insignificant. This means that the observed effect seems to be confined to inventors that are likely bound by non-compete agreements. This is aligned with a causal interpretation of the results.

4. Constrained Industry Mobility and Productivity

What are the effects on productivity if inventors are moving industries in response to non-compete enforcement increases? On one hand, it might be beneficial to society if increased inter-industry mobility leads to more idea recombination, and thus more innovation. On the other hand, inventors might perform worse after a constrained industry change. To shed light on this question, I first run a simple difference-in-differences regression as follows:

$$
Innovation_{i,t} = \sum_{k=-5}^{k=+10} \delta_k{\times}D_k+\sum_{k=-5}^{k=+10} \beta_k{\times}D_k{\times}IndustryChange_{i,t}+\theta_i+\phi_t+\epsilon_{i,t} \tag{4}
$$

where D_k again indicate time dummies relative to the event. The coefficients of interest are β_k which capture the treatment indicator interacted with 4 pre-treatment dummies and 10 post-treatment dummies. In this case the treatment indicator is defined as equal to one if the inventor moves across industries after an increase in the enforcement of non-compete agreements. The sample is composed of all inventors that are exposed to higher enforcement of non-compete agreements. The dependent variable of interest is defined as the natural logarithm plus one of the economic value of patents following Kogan et al. (2017). Alternatively, it is defined as the natural logarithm plus one of citation-weighted patents. The difference-in-differences regression thus estimates the productivity difference before and after the enforcement change of those inventors that move across industries versus those inventors that do not move.

Figure 2 displays the result. Inventors who move industries experience a sudden drop in productivity following an industry move. This effect is measured relative to those inventors that do not move. This analysis exploits the variation of those inventors who move in response to an increase in non-compete enforcement increase. There is no pre-trend, which indicates that inventors do not meaningfully differ before moving across industries. These results are consistent with the interpretation that moving across industries after an increase in the enforcement of non-compete agreements leads to a decline in productivity. To interpret the drop in productivity, we see a decline of $e^{(-0.35)}-1 = 29.5\%$ for the economic value of patents and $e^{(-0.08)}-1 = 7.7\%$ for citation-weighted patents. In terms of actual economic value lost, the average patent is worth around 3 million USD, so we observe a decrease in economic value of patents of around 0.9 million USD per inventor per year.

The specification allows for one additional observation. The difference-in-differences regression allows to compare the productivity of those inventors who stays relative to those that leave. A testable hypothesis is whether inventors who leave are significantly less productive. Increased enforcement of non-compete agreements might induce firms to keep high quality inventors and invest more in human capital. We would then expect low quality inventors to move industries. Looking at pre-trend in figure 2 shows that this does not seem to be the case. Inventors who leave and inventors who stay are not meaningfully different before moving.

One caveat of the previous regression is that it introduces an unwanted source of endogeneity through an ex-post observed preference. The difference-in-differences compares those inventors who move to those that did not. The above effect is thus estimating an effect in the cross-section which is only valid if the two groups of inventors are randomly allocated. This is unlikely to be the case. The next specification thus looks only at within inventor variation over time. If indeed an industry move is responsible for the drop if productivity, then an inventor should perform worse compared to herself. This analysis is done in figure A6. We see that there is a sharp and permanent drop in productivity around the time of industry change. The effect stays negative until 9 years after an industry change. In terms of economic magnitudes, the effect is comparable to the cross sectional comparison. Inventors are 22% less productive compared to before an industry move.

The next specification will explore the same question, however using an instrumental variable regression. What we are ultimately interested in is the effect of those inventors that change industries on future innovation output. In a two stage least squares regression, this subset of inventors in the sample are so-called compliers. Compliers are precisely the inventors that are induced to move to another industry due to an increase in non-compete agreement enforcement. The instrument in this setting is $NCAIncrease \times Post$. For this purpose, I use equation 1 as a first stage regression. In the second stage, I use the instrumented industry change and look at two separate innovation output measures: Economic value of patents and citation-weighted patents.

The instrumented industry change indeed leads to significantly lower future innovation. When looking at the first stage, the instruments pass conventional thresholds. Also the second stage passes stricter t-ratio inference thresholds (Lee et al. 2021).

Table 5 shows the results. In terms of economic significance, an increase in non-compete agreement enforcement reduces the economic value of patents by 67%. Panel B shows the results for citation-weighted patents as the outcome variable. Here, the productivity declines by 17%. The results of citation-weighted patents are consistent, but only significant at the 5% level.

This evidence is subject to the crucial exclusion restriction assumption. Non-compete agreements should not affect innovation of inventors other than through industry mobility. On the one hand, stricter non-compete agreement enforcement might lead to less innovation because of less mobility and idea recombination. On the other hand, Jeffers (2017) shows that corporate investment increases which might have potential benefits for innovation. However, in this particular setting, it is unclear how an increase in non-compete enforcement can have an effect on future innovation other than through an allocation mechanism. The presented effect here in conditional on an inter-industry move. Given that the inventor *did in fact move* across industries, the inventor is unlikely to be affected by the increased enforcement of non-compete agreements.

Overall, all specifications point in the same direction: Inventors are less productive when they move across industries in response to an enforcement in non-compete agreements. The results are measured either relative to inventors who stay in the industry or alternatively compared to within the same inventor. The results of instrumented industry change support this conclusion. This evidence is consistent with anecdotal evidence of Marx (2011). Respondents who took career detours reported estrangement from their professional networks, reduced compensation and degeneration of skills. These effects can well lead to reduced productivity in an innovation-related job.

5. Unconstrained Industry Change Increases Productivity

The previous section presented results how a forced industry change leads to lower productivity. This raises the question: Are inventors also less productive if they voluntarily move across industries? By and large, the evidence points in the opposite direction: Inventors perform better when moving to another industry. I perform similar analyses compared to the last section, with one crucial difference: the sample does not only include inventors moving across industries after an increase in non-compete enforcement. The sample is instead composed of all observations where an inventor moves across industries. In the following, I refer to such inter-industry mobility as unconstrained industry mobility.

To make the assessment that unconstrained industry mobility leads to higher productivity, I again estimate equation 4. The treatment indicator $IndustryChange$ is equal to one for those inventors that move across industries. The first specification is thus essentially a difference-in-differences which compares inventors that move across industries compared to those that do not. For this, I re-run the matching algorithm and compare similar inventors based on age, technology, number of patents, and number of citations. Figure 3 shows the results. Around the time of industry change, we see a continued increase in productivity of those inventors who move across industries. In terms of economic significance, moving to another industry increases productivity by 16%, as measured by economic value of patents. The effect is equal to 10.5% for citation-weighted patents. This effect is cross sectional compared to those inventors that stay. For the economic value of patents there is no pre-trend. However for citation-weighted patents, there are small indications of a pre-trend, no effect immediately after the industry change, but significant positive effects from year 6 onwards.

The second specification again looks at within inventor. The setting exploits the timing of industry change and looks at the productivity of inventors before and after this change. The results are reported in A7. Again there are significant positive effects after an industry change. However most notable, there are significant pre-trends. The results seem to suggest that inventors who move to another industry are on an upward slope before the industry move. This highlights severe problems when interpreting the effects as causal. Inventors do not randomly move to another industry.

Because of this findings, I introduce a third specification. I will look at the productivity effect of inventors that move across 3-digit SIC industries relative to those who move within the same 3-digit SIC industry. The sample is thus fully composed of industry movers. Inventors who moved across SIC 3-digit industries are defined as the treatment group. The control group are inventors who moved to another firm, but within the same SIC 3-digit industry. The regression thus compares two inventors that moved employers, however only one of them across industries. The variable of interest $IndustryChange$ captures those that changed across SIC 3-digit industry. The regression estimates productivity effects relative to when both groups of inventors move industries.

Figure 4 displays the result. The coefficients before industry change are not meaningful different to zero and no pre trend is visible. After inventors move across SIC 3 digit industries, they experience a sharp increase in economic value of patents. The effect is around 28% for the economic value of patents. This effect is relative to those that also changed employers, however within the same SIC3-digit industry. This specification can reduce some concerns on endogeneity with respect to moving employers. The specification speaks to benefits moving to another industry compared to moving to another firm in the same industry. However, to the extent that unobserved factors are correlated with the decision to move to an industry that is far away from the previous industry, the analysis cannot make any causal claims.

Moving to another industry seems to be related to increased productivity of inventors. This is consistent with the literature on idea recombination and the positive effects on a specific form of labor mobility. Significant pre-trends make causal statements impossible and I stay agnostic on potential reasons for the productivity increase, as this is beyond the scope of this paper. However, I highlight this as a possible research avenue for further research. To sum up, there seems to be a sharp contrast between moving across industries unconstrained and after an increase in the enforcement of non-compete agreements. The evidence is consistent with the hypothesis that an increase in the enforcement in non-compete agreements leads to inefficient reallocation of human capital.

6. Channels

6.1. Low vs. High Quality Inventors

Assuming that inventors do in fact evade their non-compete clause and change industries in response to an increase in non-compete agreement enforcement. A testable hypothesis would be that this only affects low quality inventors. Assuming firms invest more in their human capital (as employees are more closely tied to the firm) then they might let go of low quality inventors. In the regressions, we would then see increased industry mobility of low quality inventors, but no change for high quality inventors.

I run the baseline regression of Equation 1 and additionally interact the variable of interest ($NCAIncrease \times Post$) with two measured of inventor quality. The first is the total number of patents that were granted up until the year before treatment. The second is the total number of citations received for patents granted until the year before treatment. Table 6 shows the results. The coefficient of $NCAIncrease \times Post$ is still positive and significant, but the effect for high quality inventors is still positive and significant, when looking at long-run outcomes. I can therefore reject the hypothesis that only low quality inventors are moving industries in response to non-compete agreement enforcement increases.

6.2. *Young vs. Old Inventors*

Using the rich data available, I analyze heterogeneous effects of inventor age on mobility. I use the age of each inventor in the filing year and group inventors into 5 equally sized brackets. Bracket one includes all inventors below 35, the second one those between 35 and 42, the third one between 42 and 46, the fourth between 46 and 52 and the fifth includes all inventors above 52. I then look at mobility effects depending on age bracket. Table 7 shows the result. First we look at the coefficients on the age brackets. Mobility seems to be highest for middle aged inventors. Since the coefficients are relative to the middle age bracket, we see that brackets two and four do not meaningfully differ from the base age category. Brackets 1 and 5, those including the youngest and oldest inventors seem to be less mobile.

Next, we look at the coefficient on $NCAIncrease \times Post$ which is additionally interacted with the four age brackets, again relative to the third. The bracket containing the oldest inventors seems to be least affected by the enforcement increase. The bracket including the youngest inventors seems to be most affected. Increases in non-compete agreement enforcement thus seem to lead to inter-industry mobility especially for young and less so for old inventors.

7. Conclusion

Employees evade their non-compete agreements by moving across industries. Non-compete enforcement increases have a positive causal effect on the probability that an inventor moves across industries. This contrasts earlier results that non-compete agreements limit labor market mobility. The effect is only present for those inventors in firms with non-compete contracts observed on a firm level. Stronger non-compete enforcement leads to inefficient reallocation of human capital in our economy. Inventors that move industries are subsequently less productive. This contrasts results whereas unconstrained inventors that move industries are more productive. This paper highlights negative externalities of human capital reallocation in response to labor market frictions.

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Table 1 – Overview of State-Level Changes in Non-Compete Enforceability

This table provides an overview of changes of enforceability of non-compete agreements. The changes are based on Ewens and Marx (2018) as well as Kini et al. (2021). Ewens and Marx (2018) gather data from Malsberger et al. (2016) and among other things consult lawyers. Kini et al. (2021) extend a score of non-compete enforceability across states originally developed by Garmaise (2011) to the years 1992-2014. To do so, they use data provided by the law firm Beck Reed Riden LLP. Those two sources together comprise the most comprehensive list of changes during the years 1985-2016. Panel A includes states that increased the enforceability of non-compete agreements. Panel B includes decreases. Panel C includes states that had several changes in the enforceability of non-compete agreements. Brackets in Panel C indicate the direction of the change, (+) equal to an increase in enforceability.

State	Case	Year
Panel A: Increase of Non-Compete Agreement Enforcement		
AL	Alabama legislature	2016
AR	Arkansas legislature	2016
FL	Florida legislature	1996
GA	Georgia legislature	2011
ID	Idaho legislature	2008
MI	Michigan Antitrust Reform Act (MARA), see Marx et al. (2009)	1985
OH	Lake Land v. Columber	2004
VT	Summits 7 v. Kelly	2005
VA	Assurance Data Inc. v. Malyevac	2013
Panel B: Decrease of Non-Compete Agreement Enforcement		
MT	Wrigg v. Junkermier	2009
NH	New Hampshire legislature	2011
NV	Golden Rd. Motor Inn. v. Islam	2016
OR	Oregon legislature	2008
SC	Poynter Investments v. Century Builders of Piedmont	2010
UT	Utah legislature	2016
Panel C: Repeated In-/Decreases of Non-Compete Agreement Enforcement		
CO	Luncht's Concrete Pumping v. Horner (+)	2011
CO	see Kini et al. (2021) (-)	2013
IL	Fire Equipment v. Arredondo (+)	2011
IL	Fifield v. Premier Dealership Servs. (-)	2013
KY	Gardner Denver Drum v. Peter Goodier and Tuthill Vacuum and Blower Systems (+)	2006
KY	Creech v. Brown (-)	2014
LA	Shreveport Bossier v. Bond (-)	2001
LA	Louisiana legislature (+)	2003
TX	Light v. Centel Cellular (-)	1994
TX	Baker Petrolite v. Spicer (+)	2006
TX	Mann Frankfort Stein & Lipp Advisors v. Fielding (+)	2009
TX	Marsh v. Cook (+)	2012
WI	Star Direct v. Dal Pra. (+)	2009
WI	Runzheimer International v. Friedlen (-)	2015

Table 2 – Summary statistics on an inventor level
Variable definitions are provided in the Appendix.

Variable	N	Mean	SD	Min	25%	50%	75%	Max
SIC-3 Industry Change	3,803,179	0.030	0.17	0	0	0	0	1
SIC-2 Industry Change	3,803,179	0.027	0.16	0	0	0	0	1
NAICS-4 Industry Change	3,803,179	0.032	0.18	0	0	0	0	1
Hoberg Industry Change	3,803,179	0.027	0.16	0	0	0	0	1
Female Inventor	3,009,539	0.07	0.26	0	0	0	0	1
Age of Inventor	3,803,179	43.02	11.55	18	35	42	50	100
Number Patents	3,803,179	10.09	14.63	0	3	6	11	1,076
Total Citations	3,803,179	7.07	35.70	0	0.44	1.86	6.04	18,034.14

Table 3 – Baseline Panel Regression: Staggered State-Level Changes in Non-Compete Agreement Enforcement
This table reports the two way fixed effect panel regression of equation 1. The sample is on an inventor-year level. $IndustryChange_{t+1}$ is a dummy variable equal to one if the inventor changes to a firm in a different industry. I split the treatment indicator into $NCAIncrease$ and $NCADecrease$ whether the state decreased, or increased the enforceability of non-compete agreements. In column (1) industry is defined on a SIC 3-digit level, in column (2) on a SIC 2-digit level, in column (3) on a NAICS 4-digit level and in (4) using Hoberg and Phillips (2016) text-based industry definitions. Variable definitions are provided in Appendix 1. Standard errors are clustered by State and Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dependent variable:	$IndustryChange_{t+1}$			
	(1)	(2)	(3)	(4)
Panel A: Non-Compete Agreement Enforcement Increase				
$NCAIncrease \times Post$	0.02***	0.01***	0.02***	0.02***
	(3.53)	(3.71)	(4.14)	(4.09)
Observations	3,803,179	3,803,179	3,803,179	3,803,179
R-squared	0.11	0.11	0.12	0.11
Industry Definition	SIC 3-digit	SIC 2-digit	NAICS 4-digit	Hoberg TNIC
Inventor FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Panel B: Non-Compete Agreement Enforcement Decrease				
$NCADecrease \times Post$	-0.00	0.00	-0.00	-0.00
	(0.05)	(0.03)	(-0.04)	(-0.19)
Observations	3,803,179	3,803,179	3,803,179	3,803,179
R-squared	0.11	0.11	0.11	0.11
Industry Definition	SIC 3-digit	SIC 2-digit	NAICS 4-digit	Hoberg TNIC
Inventor FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Figure 1 – Staggered State-Level Changes in Non-Compete Agreement Enforcement: Event Study and Dynamic Effects

This figure reports the result of the difference-in-differences event study of equation 2. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies, interacted with a treatment indicator equal to one if the state increases non-compete agreement enforcement. The y-axis shows the coefficient on a regression on the variable $IndustryChange$, which is a dummy variable equal to one if the inventor moves to a firm in a different SIC 3-digit industry in that year. The sample compares treated to never-treated inventors. Inventors are propensity score matched based on employment year, age, number of patents, number of citations and patent technology class. I match the three nearest neighbors with replacement using the Mahalanobis distance. Variable definitions are provided in Appendix 1. All regressions include Inventor and Year fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

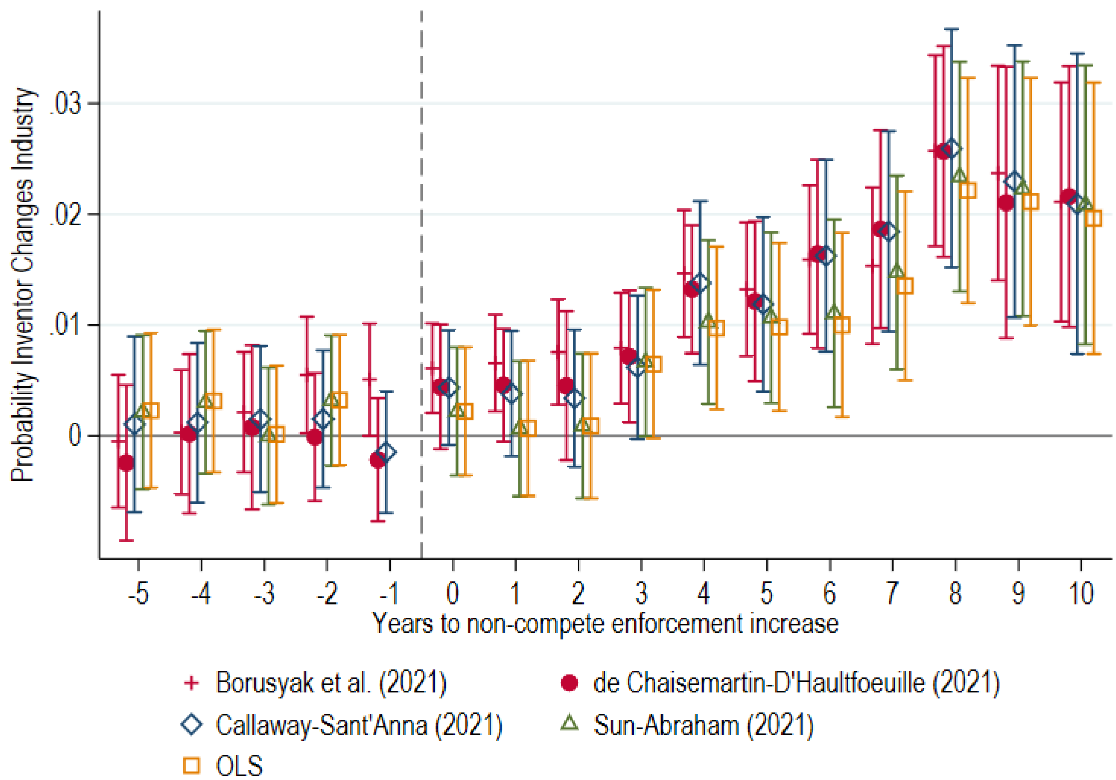


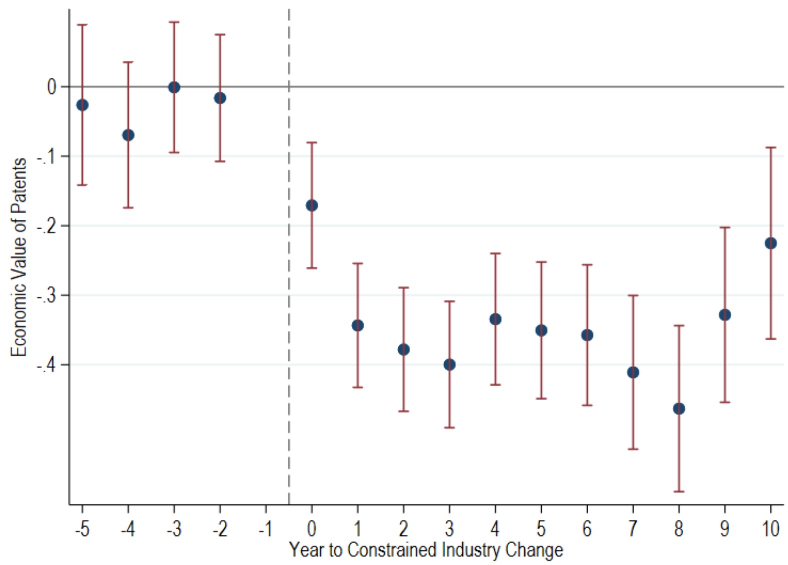
Table 4 – Triple difference-in-differences: Inventors Employed at NCA Firms

This table reports the triple-difference-in-differences fixed effect panel regression of equation 3. The sample is on an inventor-year level. $IndustryChange_{t+1}$ is a dummy variable equal to one if the inventor moves to a firm in a different industry. $NCAIncrease$ is a dummy variable equal to 1 if the state increased the enforceability of non-compete agreements. This variable is interacted with a proxy for firm-level use of non-compete contracts, based on information from form 10-Ks and 10-Qs. The variable is equal to one if the firm states that it relies on NCA or whether senior employees sign NCAs. In column (1) industry is defined on a SIC 3-digit level, in column (2) on a SIC 2-digit level, in column (3) on a NAICS 4-digit level and in (4) using Hoberg and Phillips (2016) text-based industry definitions. Variable definitions are provided in Appendix 1. Standard errors are clustered by Inventor and Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dependent variable:	$IndustryChange_{t+1}$			
	(1)	(2)	(3)	(4)
$NCAIncrease \times Post$	0.00	0.00	0.00	0.00
	(0.86)	(0.12)	(0.21)	(1.36)
NCA	0.00***	0.00***	0.00***	0.00***
	(7.50)	(9.18)	(8.89)	(5.78)
$NCAIncrease \times Post \times NCA$	0.01***	0.01***	0.01***	0.01***
	(3.92)	(3.92)	(3.35)	(3.20)
Observations	2,668,634	2,668,634	2,668,634	2,668,634
R-squared	0.14	0.14	0.15	0.14
Industry Definition	SIC 3-digit	SIC 2-digit	NAICS 4-digit	Hoberg TNIC
Inventor FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Figure 2 – Productivity Effects of NCA-Induced Industry Changers: Move vs. Stay
This figure reports the result of the difference-in-differences event study of equation 4. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies, interacted with a treatment indicator equal to one whether the inventor moves across SIC3-digit industries. Time dummies are relative to the year of industry move. The y-axis shows the coefficient on a regression on the variable *Innovation*, which is in Panel A the economic value of patents following Kogan et al. (2017), and in Panel B it is the natural logarithm plus one of citation-weighted patents. The sample is composed of all inventors exposed to an increase in non-compete agreement enforcement. Variable definitions are provided in Appendix 1. All regressions include Inventor and Year fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

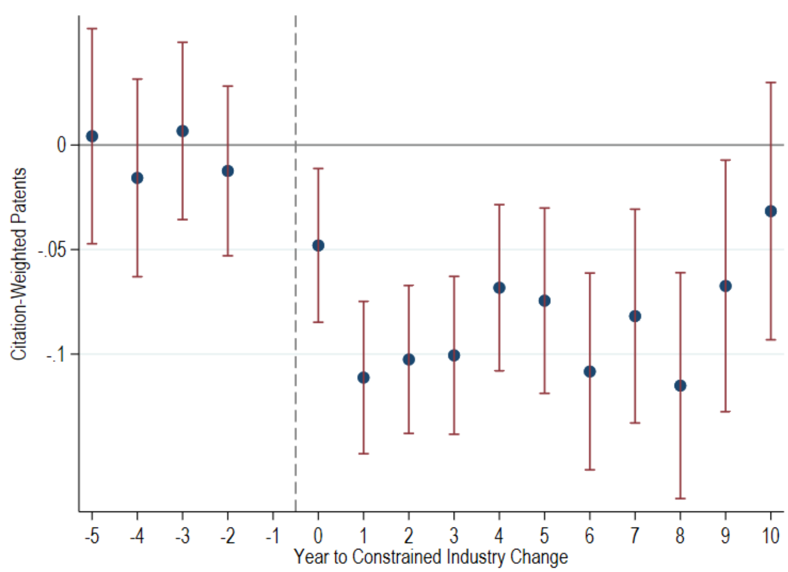
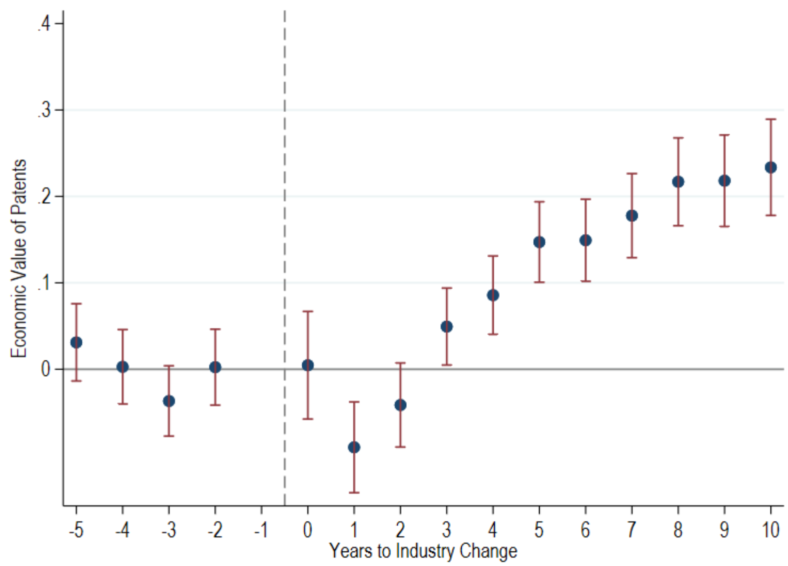


Table 5 – Productivity Effects of NCA-Induced Industry Changers: Compliers
This table presents a two-stage least squares instrumental variable regression. The sample is on an inventor-year level. Inter-industry move is instrumented with state-level increases of non-compete agreement enforcement. The first stage is equivalent to equation 1. The second stage uses the instrumented change in industry as the explanatory variable. The dependent variable in Panel A is the natural logarithm of one plus the economic value of patents obtained from Kogan et al. (2017). The dependent variable in Panel B is the natural logarithm of one plus citation-weighted patents. The Kleibergen-Paap F-statistic of the first stage is reported. Variable definitions are provided in Appendix 1. Standard errors are clustered by State and Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. *t*-statistics are displayed in parenthesis.

Dependent variable:			$Innovation_{t+1}$		
	(1)	(2)	(3)	(4)	(5)
Panel A: Economic Value of Patents					
$Industry\hat{C}hange_{t+1}$	0.17				
	(0.07)				
$Industry\hat{C}hange_{t+2}$		-1.00			
		(-1.41)			
$Industry\hat{C}hange_{t+3}$			-1.26***		
			(-3.31)		
$Industry\hat{C}hange_{t+4}$				-1.34***	
				(-4.08)	
$Industry\hat{C}hange_{t+5}$					-1.12***
					(-3.96)
Panel B: Citation-Weighted Patents					
$Industry\hat{C}hange_{t+1}$	-0.32				
	(-0.50)				
$Industry\hat{C}hange_{t+2}$		-0.30			
		(-1.20)			
$Industry\hat{C}hange_{t+3}$			-0.29*		
			(-1.94)		
$Industry\hat{C}hange_{t+4}$				-0.25**	
				(-2.31)	
$Industry\hat{C}hange_{t+5}$					-0.19**
					(-2.56)
First stage F-stat	13.1	14.4	15.7	18.0	20.2
Observations	3,803,179	3,771,133	3,708,336	3,622,121	3,518,139
Inventor FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Figure 3 – Productivity of Unconstrained Industry Mobility: Move vs. Stay
This figure reports the result of equation 4. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies, interacted with a dummy variable equal to one if the inventor moves across SIC 3-digit industries. The treatment group is composed of inventors who moved across SIC 3-digit industries. The control group are inventors who moved to another firm, but within the same SIC 3-digit industry. The y-axis shows the coefficient on a regression on the variable *Innovation*, which is in Panel A the economic value of patents following Kogan et al. (2017), and in Panel B it is the natural logarithm plus one of citation-weighted patents. Inventors are propensity score matched based on employment year, age, number of patents, number of citations and patent technology class. I match the three nearest neighbors with replacement using the Mahalanobis distance. Variable definitions are provided in Appendix 1. All regressions include Inventor fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

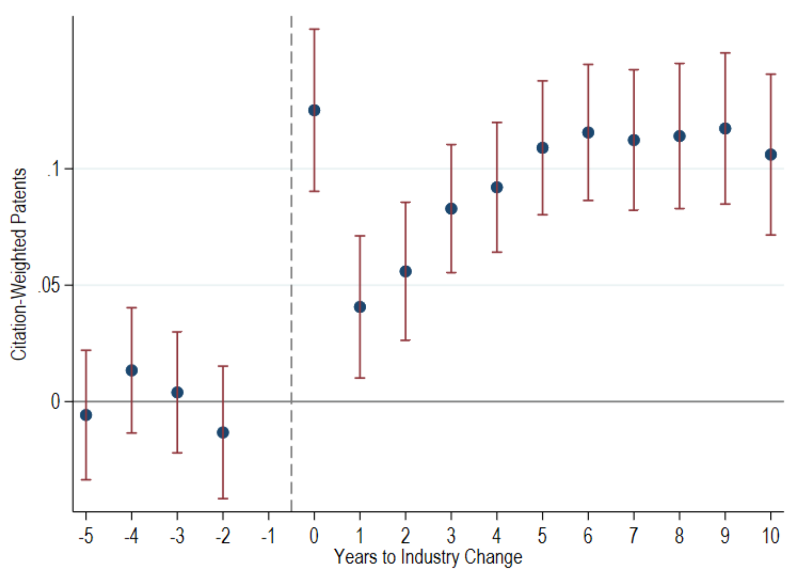
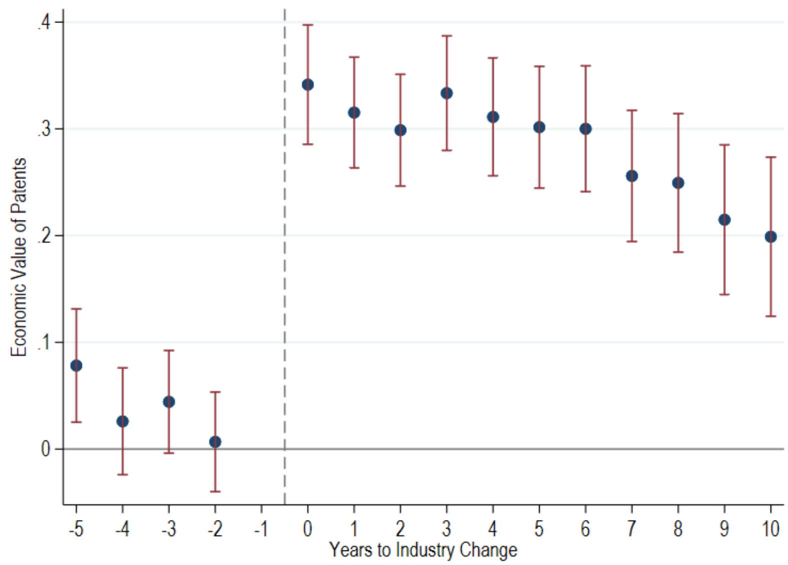


Figure 4 – Productivity of Unconstrained Industry Mobility: Across SIC3 vs. Within
This figure reports the result of equation 4. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies, interacted with a dummy variable equal to one if the inventor moves across SIC 3-digit industries. The treatment group is composed of inventors who moved to another employer in a different SIC 3-digit industry. The control group are inventors who moved to another firm, but within the same SIC 3-digit industry. The y-axis shows the coefficient on a regression on the variable *Innovation*, which is in Panel A the economic value of patents following Kogan et al. (2017), and in Panel B it is the natural logarithm plus one of citation-weighted patents. Variable definitions are provided in Appendix 1. All regressions include Inventor fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

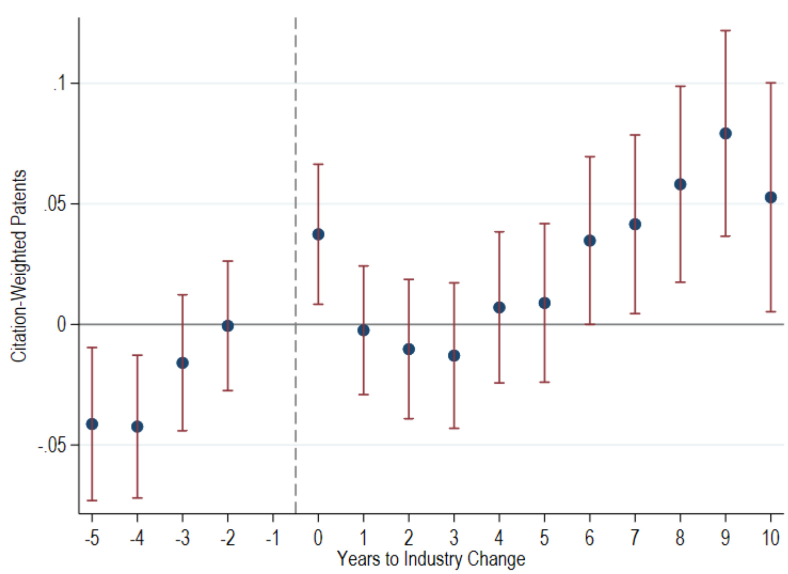


Table 6 – Heterogeneity: High vs. Low Quality Inventors

This table reports the two way fixed effect panel regression of equation 1. The sample is on an inventor-year level. $IndustryChange_{t+1}$ is a dummy variable equal to one if the inventor moves to a firm in a different industry in that year. $NCAIncrease$ is a dummy variable equal to 1 if the state increases the enforceability of non-compete agreements. The variable of interest is interacted with measures of inventor quality, in Panel A with the total number of patent granted up to the year before treatment. Panel B provides an interaction with the total number of citations received on all granted patents up to the year before treatment. Variable definitions are provided in Appendix 1. Standard errors are clustered by Inventor and Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dependent variable:	$IndustryChange_{t+1}$				
	(1)	(2)	(3)	(4)	(5)
Panel A: Interaction with Number of Patents					
$NCAIncrease \times Post$	0.02***	0.02***	0.02***	0.02***	0.01***
	(3.09)	(2.97)	(3.05)	(3.47)	(3.08)
$NCAIncrease \times Post \times Patents$	0.00	0.00***	0.00***	0.00***	0.00***
	(0.74)	(2.84)	(2.99)	(2.76)	(2.74)
Panel B: Interaction with Total Citations					
$NCAIncrease \times Post$	0.02***	0.02***	0.02***	0.02***	0.02***
	(3.16)	(3.14)	(3.17)	(3.53)	(3.16)
$NCAIncrease \times Post \times Citations$	0.00	0.00	0.00**	0.00***	0.00***
	(0.33)	(1.13)	(2.30)	(4.38)	(5.01)
	3,129,907	3,015,725	2,932,584	2,880,100	2,854,998
R-squared	0.13	0.12	0.11	0.11	0.11
Inventor FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES

Table 7 – Heterogeneity: Young Inventors

This table reports the two way fixed effect panel regression of equation 1. The sample is on an inventor-year level. $IndustryChange_{t+1}$ is a dummy variable equal to one if the inventor moves to a firm in a different industry in that year. $NCAIncrease$ is a dummy variable equal to 1 if the state increases the enforceability of non-compete agreements. The variable of interest is interacted with five equal sized age group dummies. The first one includes all inventors below 35, the second one those between 35 and 42, the third one between 42 and 46, the fourth between 46 and 52 and the fifth above 52. The coefficients are all relative to the third group. Variable definitions are provided in Appendix 1. Standard errors are clustered by Inventor and Year. ***, ** and * represents significance at the 1%, 5% and 10% level, respectively. t-statistics are displayed in parenthesis.

Dependent variable:	$IndustryChange_{t+1}$			
	(1)	(2)	(3)	(4)
$NCAIncrease \times Post$	0.02***	0.01***	0.02***	0.02***
	(9.47)	(8.04)	(9.38)	(9.23)
Age [18-34]	-0.00***	-0.00***	-0.00***	-0.00***
	(-2.76)	(-3.42)	(-2.59)	(-3.27)
Age [35-42]	-0.00	-0.00	-0.00	-0.00
	(-0.50)	(-0.43)	(-1.03)	(-1.18)
Age [46-52]	-0.00	-0.00	0.00	-0.00
	(-1.23)	(-0.24)	(0.59)	(-1.13)
Age [53-100]	-0.00***	-0.00***	-0.00	-0.00***
	(-3.91)	(-3.33)	(-0.75)	(-3.12)
$NCAIncrease \times Post \times Age[18-34]$	0.01**	0.01**	0.01**	0.01***
	(2.39)	(2.29)	(2.28)	(2.77)
$NCAIncrease \times Post \times Age[35-42]$	0.00	0.00	0.00	0.00
	(1.57)	(1.20)	(1.31)	(1.44)
$NCAIncrease \times Post \times Age[46-52]$	-0.00	-0.00	-0.00	0.00
	(-0.83)	(-0.30)	(-0.24)	(0.02)
$NCAIncrease \times Post \times Age[53-100]$	-0.00**	-0.00**	-0.00*	-0.00
	(-2.03)	(-2.00)	(-1.69)	(-1.50)
Observations	2,930,140	2,930,140	2,930,140	2,930,140
R-squared	0.11	0.11	0.11	0.11
Industry Definition	SIC 3-digit	SIC 2-digit	NAICS 4-digit	Hoberg TNIC
Inventor FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

APPENDIX

A. Variable Definitions

This section provides the variable definitions and the sources of the data.

1. *IndustryChange* – Equal to one if an inventor moves from one firm to another with a different industry classification. Obtained from employment histories of inventors from patentsview.org, patents assigned to corporations from Kogan et al. (2017) and Arora et al. (2021). SIC and NAICS industry codes are obtained from Compustat. Text-based industry classifications from Hoberg and Phillips (2016) are obtained from Hoberg and Phillips Data Library website:
   https://hobergphillips.tuck.dartmouth.edu/
2. *NCA Increase/Decrease* – Equal to one if the state decreased, or increased the enforceability of non-compete agreements. Obtained from Ewens and Marx (2018) and Kini et al. (2021).
3. *NCA* – Equal to one if the firm has mentioned the use of non-compete agreements either in their annual statement or in employment contracts of senior executives. Obtained from 10-K and 10-Q filings downloaded from EDGAR.
4. *Economic Value of Patents* – The economic value of patents, based on stock market reactions to patent grants. Obtained from Kogan et al. (2017), available here:
   https://github.com/KPSS2017/Technological-Innovation-Resource-Allocation-and-Growth-Extended-Data
5. *Number of patents* – The number of patents of each inventor one year before treatment. Lagged by one year. Obtained from patentsview.org.
6. *Patent Citations* – The number of received (forward) citations of all patents of an inventor one year before treatment. Citations were truncation adjusted using year and technology fixed effects on a patent basis. See Hall et al. (2005) and Lerner and Seru (2021) for details. Obtained from patentsview.org.

7. *Patent technology* – The Cooperative Patent Classification (CPC) section was used, which groups patents into 9 different patent sections. Obtained from patentsview.org.
8. *Inventor Age* – Age of inventor, in years, equal to the (current) application year minus birth year. Obtained from web-scraped birth records matched to US inventors Kaltenberg et al. (2021).
9. *Inventor Female* – Gender of inventor, defined as a dummy variable equal to one if the gender is female with a confidence of 85%. In the data, 75% of inventors are male, 6% are female and 19% are ambiguous. Obtained from web-scraped birth records matched to US inventors Kaltenberg et al. (2021).

Table A1 – Most Frequent Industry Mobility

This table shows the 5 most common industries ranked according to industry mobility. The table lists the departure industry and the joining industry, a brief description of the industry and the fraction of mobility events compared to the total number of mobility events. Variable definitions are provided in the Appendix.

Rank	Leaving Industry (SIC 3)	Joining Industry (SIC 3)	Fraction
1	Office, Computing, Accounting Mach.	Comp. Programming, Data Process.	4.4%
2	Office, Computing, Accounting Mach.	Electronic Components and Accessor.	3.8%
3	Comp. Programming, Data Process.	Office, Computing, Accounting Mach.	2.4%
4	Electronic Components and Accessor.	Comp. Programming, Data Process.	2.3%
5	Communications Equipment	Electronic Components and Accessor.	2.1%

Figure A5 – Staggered Difference-in-Differences

This table reports the result of the staggered difference-in-differences event study of equation 2. The sample is on an inventor-year level. The figure plots the coefficient of $NCADecrease$, which is a treatment indicator equal to one for a state that decreases non-compete enforcement. The y-axis shows the effect on the likelihood that an inventor moves across SIC3 digit industries. The point estimates are normalized to time = -1, the year before treatment. Never-treated inventors are propensity matched based on year, age, number of patents, number of citations and patent technology class. Variable definitions are provided in the Appendix 1. All regressions include Inventor and Year fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the top/bottom 5%.

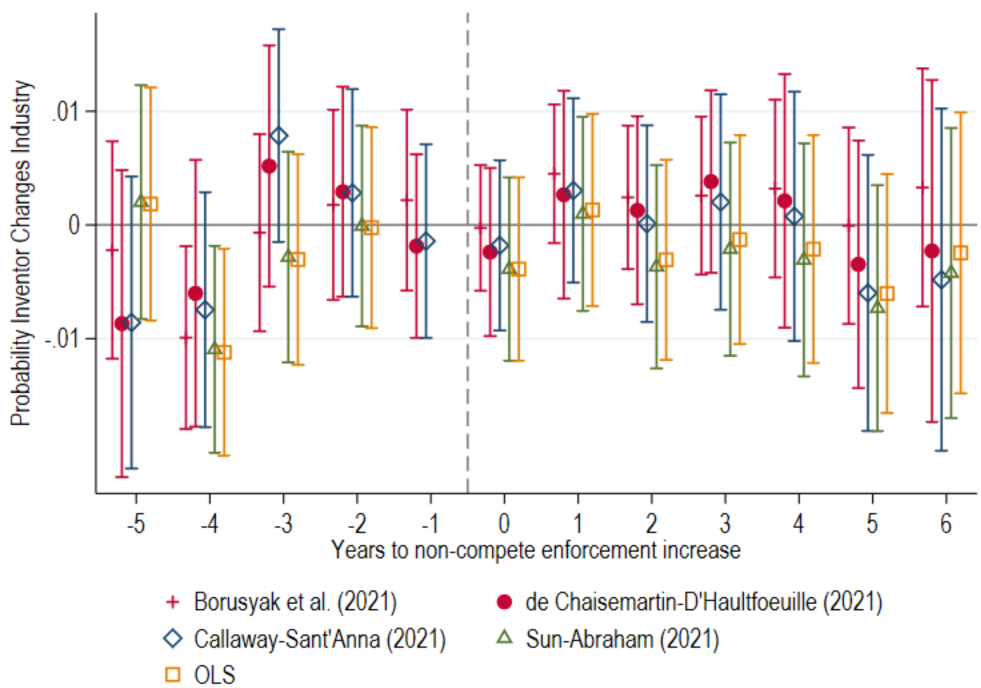
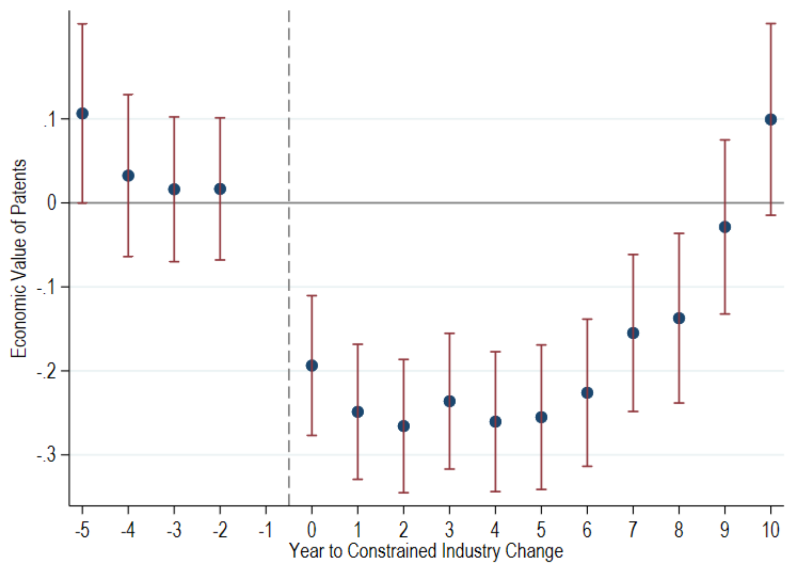


Figure A6 – Productivity Effects of NCA-Induced Industry Changers: Within Inventor This figure reports the result of the difference-in-differences event study similar to equation 4. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies. The y-axis shows the coefficient of a regression on the variable *Innovation*, which is in Panel A the natural logarithm plus one of the economic value of patents following Kogan et al. (2017), and in Panel B it is the natural logarithm plus one of citation-weighted patents. The sample is composed only of inventors who 1) experience an increase in non-compete agreement enforcement and 2) move across industries subsequently. Variable definitions are provided in Appendix 1. All regressions include Inventor and Year fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents

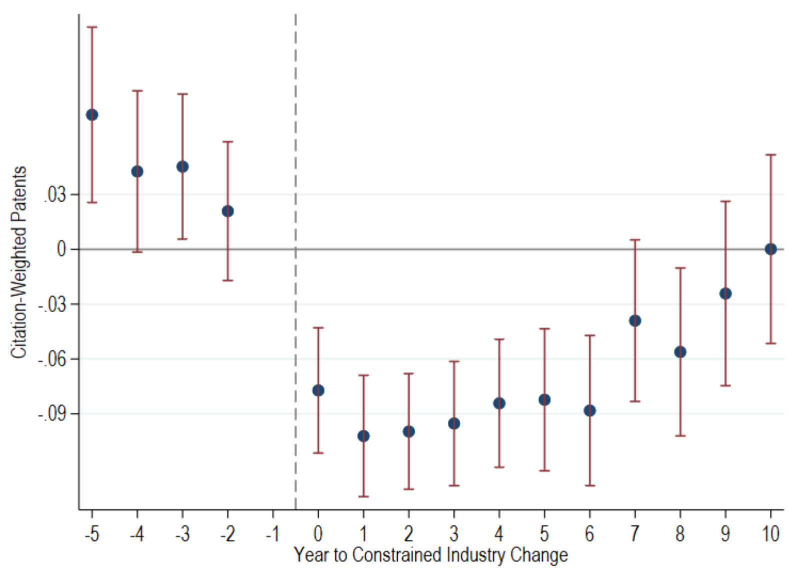
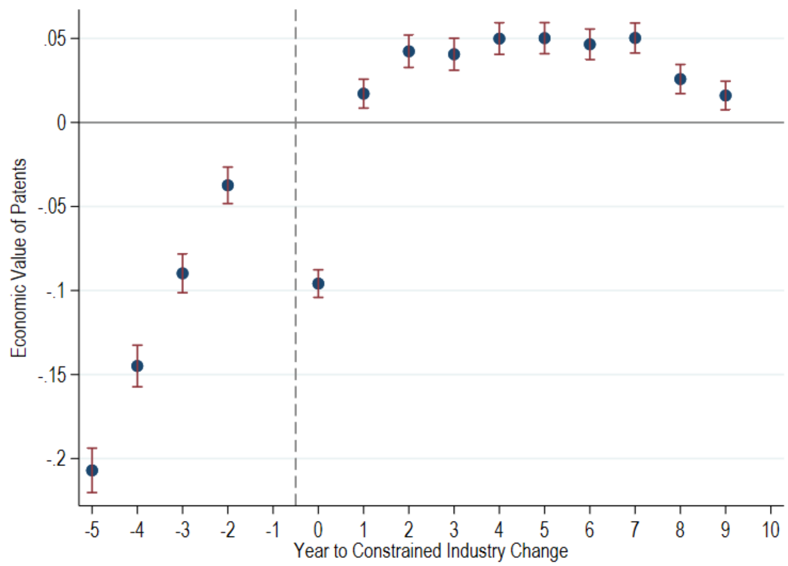
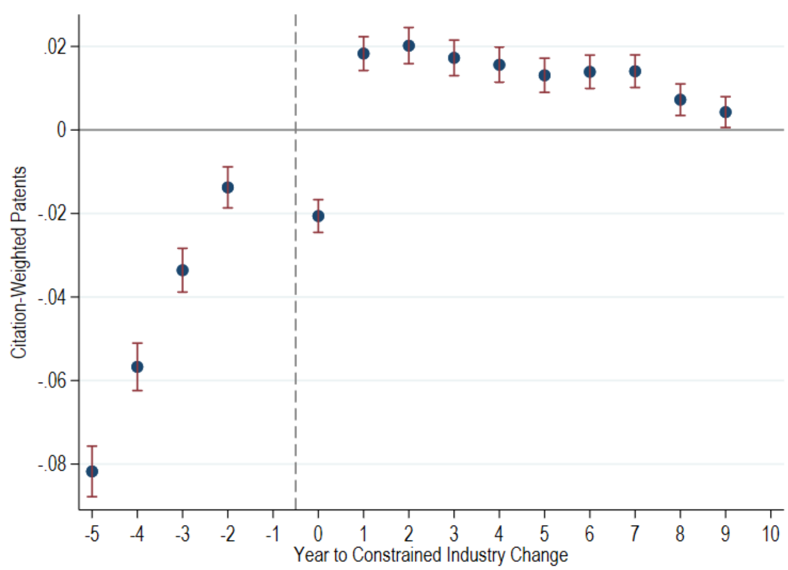


Figure A7 – Productivity Effects of Unconstrained Industry Changers: Within Inventor This figure reports the result of equation 4. The sample is on an inventor-year level. The figure plots the coefficients of pre and post time dummies before and after a SIC 3-digit industry move. The sample is composed of inventors who moved across SIC 3-digit industries. The y-axis shows the coefficient on a regression on the economic value of patents following Kogan et al. (2017). Variable definitions are provided in Appendix 1. The sample is composed only of inventors who move across industries. All regressions include Inventor fixed effects. Standard errors are clustered by Inventor and Year. Confidence intervals are at the 5% level.

Panel A: Economic Value of Patents



Panel B: Citation-Weighted Patents



Appendix B: Examples of non-compete agreements

The following are three samples drawn from the sample of innovating firms (those that are assigned patents), of which 54% have references on the use of non-compete agreements. The universe of 10-K and 10-Q filings were obtained from EDGAR and parsed to make them readable using textual analysis.

NUANCE COMMUNICATIONS INC

"In exchange for the severance pay and other consideration under the Severance Agreement to which Executive would not otherwise be entitled, Executive agrees that for a period of one (1) year after the Termination Date, Executive will not, without the express written consent of the Company, in its sole discretion, enter, engage in, participate in, or assist, either as an individual on your own or as a partner, joint venturer, employee, agent, consultant, officer, trustee, director, owner, part-owner, shareholder, or in any other capacity, in the United States of America, directly or indirectly, any other business organization whose activities or products are competitive with the activities or products of the Company then existing or under development. Nothing in this Agreement shall prohibit Executive from working for an employer who is engaged in activities or offers products that are competitive with the activities and products of the Company so long as Executive does not work for or with the department, division, or group in that employer's organization that is engaging in such activities or developing such products. Executive recognizes that these restrictions on competition are reasonable because of the Company's investment in goodwill, its customer lists, and other proprietary information and Executive's knowledge of the Company's business and business plans."

10-Q filing available here:

https://www.sec.gov/Archives/edgar/data/1002517/000100251714000013/nuan12-31x2013ex104.htm

MICROVISION INC

"We also rely on unpatented proprietary technology. To protect our rights in these areas, we require all employees and, where appropriate, contractors, consultants, advisors and collaborators, to enter into confidentiality and non-compete agreements. There can be no assurance, however, that these agreements will provide meaningful protection for our trade secrets, know-how or other proprietary information in the event of any unauthorized use, misappropriation or disclosure of such trade secrets, know-how or other proprietary information."

10-K filing available here:

https://www.sec.gov/Archives/edgar/data/65770/000113626115000080/body10k.htm

LOCKHEED MARTIN CORPORATION

"This Post Employment Conduct Agreement dated [...] (this "PECA"), together with the Release of Claims being entered into contemporaneous with this PECA, is entered into in consideration of the payment ("Severance Payment") to be made to me under the Lockheed Martin Corporation Severance Benefit Plan for Certain Management Employees ("Severance Plan"). By signing below, I agree as follows:

Covenant Not To Compete - Without the express written consent of the [Chief Executive Officer/Senior Vice President, Human Resources] of the Company, during the [two/one]-year period following the date of my termination of employment with the Company ("Termination Date"), I will not, directly or indirectly, be employed by, provide services to, or advise a "Restricted Company" (as defined in Section 6 below), whether as an employee, advisor, director, officer, partner or consultant, or in any other position, function or role that, in any such case, oversees, controls or affects the design, operation, research, manufacture, marketing, sale or distribution of "Competitive Products or Services" (as defined in Section 6 below) of or by the Restricted Company [...]"

Exhibit of 10-Q filing available here:

https://www.sec.gov/Archives/edgar/data/936468/000119312508156357/dex107.htm