Acquiring Human Capital: Do Non-Competes Help? *

Dongryeol Lee[†]

March 14, 2025

Link to the Latest Draft

Abstract

I study how challenging it is to acquire human capital through mergers and acquisitions using non-compete agreements (NCAs) as an identification tool. To obtain quasi-random variation in NCA effects, I exploit state-level differences in NCA enforcement by comparing individuals who work for the same target firm but in different states. I find 11% of acquired employees, whom the acquiring firm aims to retain, leave each year without NCAs. While NCAs help acquiring firms retain target firm employees, they lock in mismatched employees, lowering their productivity. Those bound by NCAs become 60% less productive after mergers compared to their colleagues who are not subject to NCAs. My estimates suggest that removing NCAs would result in 600 additional patents worth $10 billion annually. I show that acquiring human capital becomes easier when it is more firm-specific and team-based, as these features make employees less likely to leave even without NCAs.

Keywords: Mergers and Acquisitions, Human Capital, Non Compete Agreements, Innovation, Entrepreneurship

---

*I am grateful to my dissertation committee, Andrea Eisfeldt (chair), Mark Garmaise, Valentin Haddad, Barney Hartman-Glaser, and Tyler Muir for their continuous support. I thank John Asker, Mikhail Chernov, Thummim Cho, Lars Lochstoer, Sangdong Kim, Lukas Kremens, Shohini Kundu, Mark Rempel, Gregor Schubert, Olav Sorenson, Issac Tucker, and Ivo Welch for helpful comments.

†UCLA Anderson School of Management, Email: dongryeol.lee.phd@anderson.ucla.edu

Human capital represents a significant portion of the value firms acquire through mergers and acquisitions. However, retaining employees after a merger can be challenging, as many leave following merger announcements (Kim (2023)). Non-compete agreements (NCAs) help acquiring firms address this issue by limiting employees' ability to move to competing firms. Given their prevalence—impacting up to 50% of U.S. workers (Colvin and Shierholz (2019))—NCAs have the potential to exert substantial effects on the workforce at around mergers.

My paper addresses the following fundamental question: Do NCAs help firms acquire human capital through mergers and acquisitions. This question helps identify, among the employees acquiring firms wish to retain, how many they lose without NCAs. Furthermore, I explore the consequences of locking in these employees by measuring how their productivity changes after M&A. Then, I examine which features of human capital help firms retain target firm employees even without NCAs. To address these questions, I use worker-level data from LinkedIn and employ a setting in which I compare individuals working for the *same* target firm across different states. I exploit variations in state-level differences in NCA enforceability, which defines the most restrictive terms that could legally be applied. This unique empirical strategy can provide causal estimates even when mergers and the firm's use of NCAs are potentially endogenous since NCA enforceability conditional on a merger is plausibly exogenous.

For instance, consider a target firm that operates two branches: one in Massachusetts, where NCAs are enforced, and the other in California, where they are not, as illustrated in Figure 1. The merger disrupts match quality, prompting some employees to consider leaving. However, acquiring firms can enforce NCAs to prevent employees from the Massachusetts branch from leaving, unlike those in the California branch. By computing the differences in departure rates between these branches, I can quantify how many employees acquiring firms aim to retain but lose without NCAs.

To estimate the impact of NCAs on employee retention, I use a difference-in-difference regression comparing employees from the same target firm across states with different levels of NCA enforcement before and after merger announcements. I find employees are significantly less likely to leave after mergers if they are from states where NCAs are enforced. Employees from states with the strictest NCA enforceability are 11% less likely to depart post-mergers than those in states with no enforceability. Given that the average departure rate in my sample is 17%, this coefficient estimate corresponds to a 60% lower likelihood of departure.

The lower departure rates in the branch where NCAs are enforced imply acquiring firms lose a significant fraction of employees they wish to retain without NCAs. The 11% departure rate represents a lower bound, as it assumes all employees the acquiring firm aims to retain have signed

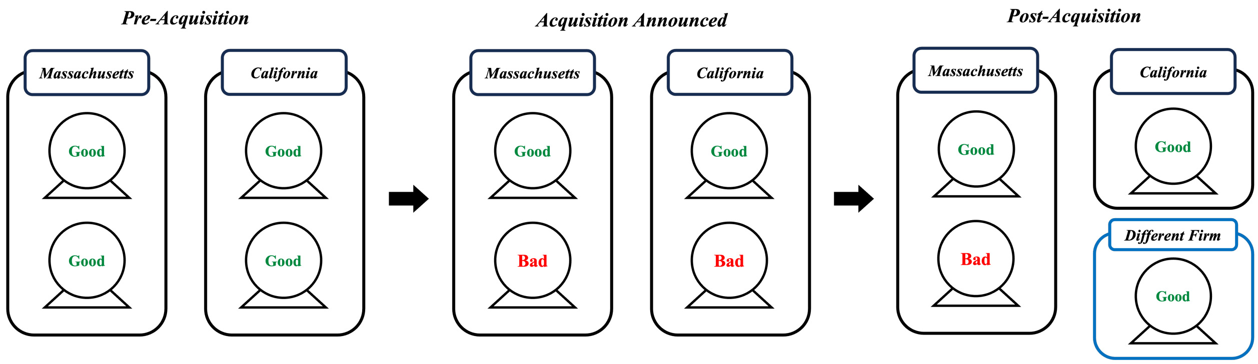

Figure 1: **Pre- vs Post-Acquisition Employee Match.** This figure illustrates the impact of non-compete agreements (NCAs) on employee-employer match quality among employees from two branches within the target firm. It compares branches where NCAs are enforced (*Massachusetts*) with those where they are not (*California*). The figure shows how it is more difficult for employees to leave the firm after a merger, even when their match quality declines if they are from Massachusetts, where NCAs are binding.

NCAs. If all post-acquisition departures were due to firm-wide layoffs, there should be no significant difference in departure rates across branches. The fact that departure rates across branches widen substantially after acquisitions and that employers can always terminate the employment contract suggests that the acquirer intends to retain these employees by enforcing NCAs. In fact, acquiring firms are willing to pay a higher premium for targets when they can enforce NCAs to more employees, recognizing the value of retention.

While NCAs help acquiring firms retain target firm employees, they lock in mismatched employees lowering their productivity. To examine how NCAs can reduce productivity, I use the same setting but compare productivity using individual-level patent data. Figure 1 suggests the acquiring firm retains poorly matched employees in the Massachusetts branch, where NCAs are enforced, which leads to a decline in their productivity due to the mismatch. Consistent with this, I find that employees from branches with stricter NCA enforcement become significantly less productive after the merger. My estimates suggest that these employees would have produced 600 new patents annually worth \$10 billion in the absence of NCAs. These estimates represent a 26% gain relative to the current post-acquisition innovation levels. This tremendous loss of innovations suggests NCAs lead to inefficient allocation of human capital across firms after mergers.

At first glance, locking in mismatched employees by enforcing NCAs may seem counterintuitive. However, an employee's enhanced productivity at another firm provides no direct benefit to the acquiring firm. One could imagine a scenario in which an employee, recognizing their higher productivity potential elsewhere, attempts to pay an upfront transfer fee to leave. Clearly, contracting frictions exist, but I take these frictions as given in this paper, as such transactions rarely occur in practice.

Furthermore, acquiring firms may retain mismatched employees in high-NCA states for several reasons. First, they do so to prevent strengthening competitors, as departing employees could be more productive elsewhere, potentially benefiting competing firms. I find the effects of NCAs are more pronounced once target firms operate in more competitive industries. Second, firms may seek to expand their business into new locations. Harford et al. (2023) argue that whether acquiring firms retain or close a specific branch of the target firm depends on their existing presence in that location. I show the effects of NCAs are weaker once there is a geographic overlap between the acquiring and target firms in a given state. Third, in high-NCA states, replacing mismatched employees is difficult since workers at other firms are also likely bound by NCAs. Thus, they are more willing to hold on to mismatched employees as poaching talent from other firms is challenging. I find the effects of NCAs are stronger when there is a high demand for talent in a given state.

Next, I ask which worker/firm pairs are more likely to suffer from the mismatch during M&As. This question helps identify which features of human capital help firms in retaining target firm employees even without NCAs. First, I find NCAs become less relevant if human capital is more firm-specific, as employees' skills are worth less in other firms and they prefer to stay where their human capital is established (Jovanovic (1979)). For instance, in firms where employees actively collaborate as a team, they may be less inclined to leave. Thus, acquiring human capital becomes easier once it is more firm-specific and team-based.

I use three measures to examine how the firm-specificity of human capital can drive the mismatch. First, I use the length of firm-specific training periods. Second, I use the likelihood of collaborators' departure, where collaborators are defined as individuals with whom employees have coauthored patents. I compute their departure likelihood based on the average NCA enforceability in their respective states. Third, I use the firm's investment in organization capital, which refers to a production factor tied to a firm's key talent, with its efficiency being uniquely firm-specific (Eisfeldt and Papanikolaou (2013)). Thus, target firms that invest heavily in organization capital tend to develop more firm-specific human capital. Across all three measures, I find the impact of NCAs is smaller when employees possess more firm-specific human capital. These findings provide guidance for firms on which targets to pursue if their goal is to acquire human capital through M&As.

Second, I find that the impact of NCAs is more pronounced among employees with higher levels of human capital, as they have higher outside options. In fact, the impact of NCAs on productivity is more pronounced among employees who were more productive during the pre-merger period. To further explore this, I use two additional measures to assess the level of human capital: social skills and the extent to which their tasks are routine. Deming (2017) shows that employees with high social skills who perform non-routine tasks are especially well-rewarded in the labor market. Consistent with his findings, I find that employees are more likely to suffer from the mismatch caused by NCAs when they possess higher social skills and perform non-routine tasks. These findings further demonstrate the challenge of acquiring human capital through M&As, as retaining the most talented and productive employees is particularly difficult.

I rule out potential alternative channels by conducting a series of robustness checks. First, I run a placebo test to examine whether my findings may be driven by other factors related to merger announcements or NCAs aside from mismatch. I estimate the same empirical models with failed merger attempts and find no significant effects. This result suggests that match quality is crucial for explaining my main findings and that NCAs affect productivity only through completed M&A deals, which introduce shocks to match quality. Using employees from these failed mergers as an alternative control group, I further confirm that NCAs affect productivity only once the merger is completed. This result reinforces the findings that NCAs decrease productivity, specifically among employees with reduced match quality as a result of completed M&As. Additionally, I examine whether firms have different patenting policies based on the enforcement of NCAs as firms might feel less compelled to patent their ideas in areas with stricter NCAs. However, I show that the local enforceability of NCAs does not influence patenting decisions.

Overall, my findings suggest while NCAs help acquiring firms retain employees after a merger, they lead to a misallocation of human capital, lowering innovation and entrepreneurship. These findings are not only relevant to academic literature but also offer policy insights into the ongoing debate over banning NCAs. Recently, there has been a vigorous debate regarding the value of NCAs among policymakers following the Federal Trade Commission's (FTC) proposal to ban them, which was nullified by a court decision in August 2024[1]. This paper highlights the need to consider the broader economic effects of NCAs on labor markets, innovation, and entrepreneurship as part of this debate.

Related Literature and Contribution

This paper is most closely related to the literature on the economic impact of non-compete agreements (NCAs). A vast literature has documented NCAs are commonly used among a wide range

[1] In April 2024, FTC proposed a rule banning the use of NCAs nationwide, which was nullified by a court decision in August 2024 and became effective in September 2024. Under the Administrative Procedure Act, this court's decision applies nationwide. Link to the Ryal LLC vs Federal Trade Commission case.

of occupations and seniority to restrict labor mobility, particularly among “knowledge workers" who engage in innovative activities (Kaplan and Stromberg (2003); Marx, Strumsky, and Fleming (2009); Garmaise (2011); Marx (2011); Colvin and Shierholz (2019); Lavetti, Simon, and White (2020); Kini, Williams, and Yin (2021); Lipsitz and Starr (2021); Starr, Prescott, and Bishara (2021); Balasubramanian et al. (2022); Johnson, Lavetti, and Lipsitz (2024)). However, how NCAs can affect innovation remains debatable. On the one hand, NCAs can encourage firms to invest in their human capital by reducing the risk of losing talented employees, thereby boosting innovations (Conti (2014); Younge and Marx (2016); Jeffers (2024)). On the other hand, restricting labor mobility can lead to less innovation because it slows down knowledge spread (Saxenian (1996); Gilson (1999); Samila and Sorenson (2011); Johnson, Lipsitz, and Pei (2023); Reinmuth and Rockall (2024)). Furthermore, many papers find NCAs can decrease entrepreneurial activities (Stuart and Sorenson (2003); Hellmann (2007); Samila and Sorenson (2011)), although startups from states where NCAs are enforced are more likely to survive and grow faster due to stricter screening processes (Starr, Balasubramanian, and Sakakibara (2018)).

This paper makes two key contributions to the existing literature. First, it empirically estimates the consequences of retaining employees whose match quality with the employer has declined following mergers, a question not previously addressed in other studies. Second, it exploits within firm geographic variations to quantify the impact of NCAs on employee retention and productivity. With this unique empirical strategy, the paper provides precise causal estimates that demonstrate how NCAs reduce productivity and entrepreneurship following mergers.

This paper also contributes to the literature on M&As, particularly regarding how well acquiring firms can transfer assets from target firms (Asquity (1983); Agrawal et al. (1992); Moller et al. (2003); Siegel and Simons (2010); Kim (2022, 2023)). My paper focuses on the transferability of human capital when acquiring firms enforce NCAs. With the rise of “acqui-hires," where mergers increasingly focus on acquiring key talent, examining the impact of NCAs in the context of mergers is essential (Ouimet and Zarutskie (2020); Ng and Stuart (2022)). I find that while NCAs can help transfer employees from target firms to acquiring firms, mismatched employees become less productive than they could be otherwise. This decline in productivity may be attributable to the acquirer’s "killer acquisitions" strategy to gain greater market power (Cunningham, Ederer, and Ma (2021); Kamepalli, Rajan, and Zingales (2021)).

1 Data and Empirical Strategy

1.1 Institutional Background: Non-Compete Agreements Enforceability

Non-compete agreements (NCAs) are contractual clauses that restrict employees from joining or starting a competing firm within a certain period and geographic area after leaving their current employer. It has been documented that up to 50% of the US labor force is subject to an NCA, particularly among "knowledge workers,"[2] who engage in innovative activities (Leonard (2001); Kaplan and Stromberg (2003); Colvin and Shierholz (2019); Starr, Prescott, and Bishara (2021)). Starr, Prescott, and Bishara (2021) find that most NCAs last up to two years (64.7%) and impose geographic restrictions, ranging from specific cities or states to the entire United States. Moreover, approximately 60% of employees learn about the NCA before accepting a job offer, while 30% are informed afterward, which can reduce their ability to negotiate. In fact, only 10% of employees attempt to negotiate the terms of their NCAs, with limited room to do so.

Given the prevalence of NCAs among US employees, many studies have confirmed NCAs effectively suppress worker mobility (Marx, Strumsky, and Fleming (2009); Garmaise (2011); Marx (2011); Lavetti, Simon, and White (2020); Balasubramanian et al. (2022); Johnson, Lavetti, and Lipsitz (2024); Jeffers (2024)). This widespread use across different sectors underscores the importance of understanding the economic impacts of NCA enforceability, as these agreements can have far-reaching implications for both workers and employers.

The enforceability of NCAs is determined by state employment law, which varies significantly across the United States. Each state's legal framework, shaped by judicial rulings and legislative actions, dictates the terms under which an employer can enforce an NCA. This enforceability acts as the upper bound of NCAs in employment contracts, defining the most restrictive terms that could legally be applied within a state. Bishara (2011) develops a theoretically grounded state-level index to quantify NCA enforceability, using a scale from 0 (completely unenforceable) to 1 (easily enforceable). This index covers various aspects, including the permissible scope of NCAs, the consideration required for support, and the conditions under which an NCA might be considered unreasonable. To create an overall enforceability score, Bishara (2011) proposes a weighted sum of these dimensions, with weights reflecting the relative importance of each component based on his expertise.[3] He finds a sizable cross-sectional variance in NCA enforceability across states, reflecting diverse legal interpretations and policy priorities.

[2] Knowledge workers are defined as executives, managers, computer specialists, engineers, researchers, and scientists.

[3] Table A.1 provides the complete list of the seven quantifiable dimensions used to measure NCA enforceability and their respective weights, as outlined in Bishara (2011)'s methodology.

Building on this foundational work, Johnson, Lavetti, and Lipsitz (2024) extend the original index developed by Bishara (2011) by creating a state-year panel dataset covering the period from 1991 to 2014. While Bishara (2011)'s index quantifies NCA enforceability for 50 states and Washington D.C. at two specific points in time (1991 and 2009), Johnson, Lavetti, and Lipsitz (2024) aim to capture the dynamic nature of NCA enforceability over time. They construct this series by compiling and analyzing additional data from state-level judicial rulings and legislative changes between 1991 and 2014. This approach allows them to construct a continuous panel reflecting annual variations in NCA enforceability for each state, thereby providing a more comprehensive understanding of how changes in legal environments impact economic and labor market outcomes over time. Their work builds on Bishara (2011)'s methodology, maintaining the same dimensions and weightings but updating the index annually to reflect the evolving legal landscape regarding non-compete agreements. This extended dataset offers researchers a valuable tool for examining the effects of NCA enforceability in a more detailed and temporally sensitive manner. It has already been used in academic research to explore its relationship with innovation. For instance, Johnson, Lipsitz, and Pei (2023) and Reinmuth and Rockall (2024) focus on changes in the legal landscape rather than just the existence of NCAs and find that an increase in the enforceability of NCAs is associated with a significant reduction in patenting and inventor mobility.

The enforceability of non-compete agreements has recently become a widely debated issue in the United States. In April 2024, the Federal Trade Commission (FTC) issued a rule banning the use of non-compete agreements nationwide, arguing that such agreements suppress wages, hinder innovation, and limit economic mobility (FTC (2024)). However, the FTC's decision has already faced legal challenges. A recent lawsuit in Texas in August 2024 nullified the FTC's ban, arguing that the rule oversteps the agency's regulatory authority and infringes on states' rights to regulate employment (Michaels (2024)). The FTC announced shortly after the court's decision that it is seriously considering an appeal. My paper offers important insights into this ongoing legal debate, emphasizing the need to evaluate the effects of NCAs within the context of mergers.

1.2 Stylized Facts on NCA Enforceability

In this subsection, I present some empirical facts before moving on to the main analyses. Figure 2 shows substantial cross-sectional heterogeneity in NCA enforceability across U.S. states. The level of enforceability varies significantly, with some states (e.g. Florida, Connecticut, New Jersey) enforcing NCAs more strictly than others (e.g. California), highlighting the importance of geographic differences in understanding the effects of NCAs. It also plots the distribution of target firm employees included in this paper's analyses and shows the sample is well-distributed across the state and not concentrated in specific regions.

Figure 3 presents a scatter plot of NCA enforceability and average departure rates across U.S. states in 2014, computed using LinkedIn data. It shows a negative relationship between NCA enforceability and average departure rates by state, although most states fall within the confidence intervals. However, Figure 4 shows no strong relationship between NCA enforceability and productivity, as measured by the average number of patents per individual. Despite the variations in NCA enforceability across states, the productivity of individuals does not exhibit a clear correlation with the level of enforceability. In the main analysis, I show how NCAs affect productivity only in the presence of shocks to employer-employee match quality, such as acquisitions.

1.3 Empirical Strategy

To answer the main questions in the paper, I propose a difference-in-difference regression consistent with the framework as in Figure 1. Specifically, I estimate the following,

$$y_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot \left(\text{NCA Enforceability}_{ijst} \times \text{Post}_t\right) + \varepsilon_{ijst}, \tag{1}$$

where NCA Enforceability$_{ijst}$ is the degree of the non-compete agreement's enforceability one year prior to the merger announcement, and Post$_t$ is equal to 1 from the announcement. I use NCA enforceability that varies across states and years from Johnson, Lavetti, and Lipsitz (2024), which ranges from 0 (completely unenforceable) to 1 (the strictest enforceability observed in our sample). Along with the individual (α_i) and time fixed effects (α_t), I also include the target firm fixed effects (α_j), which allows me to exploit the variation of the departure rate among people *within the same firm* but across branches in different states. Thus, this approach essentially compares the outcomes between branches with and without NCA enforcement around the merger announcement as illustrated in Figure 1.

A potential concern is that different types of employees could be allocated to each branch. To address this concern, I employ two additional fixed effects. First, I use Metropolitan Statistical Area (MSA) fixed effects to exploit variation within each MSA. This approach is particularly useful since MSAs often span multiple states. For instance, the New York MSA includes New York, New Jersey, and Pennsylvania, states with drastically different levels of NCA enforceability. I also use occupation-level fixed effects based on job titles, defined by the first two digits of the O*NET code[4]. The O*NET code is an 8-digit occupational classification system that describes the specific type of occupation each person has. Job title fixed effects allow me to study variation within each broad occupational classification, such as those who work in computer science or engineering. Additionally, I demonstrate later that individuals from different branches do not differ

[4]Table A.2 present the list of job titles for the inventors in my sample.

in productivity before the merger announcements.

1.4 Data Sources

1.4.1 Non-Compete Agreements Enforceability

I use non-compete agreement (NCA) enforceability data from Johnson, Lavetti, and Lipsitz (2024), which extends the work of Bishara (2011). Bishara (2011) computes the degree of enforceability based on seven quantifiable dimensions for 50 states and Washington D.C. for two years (1991 and 2009), using a series of legal texts, 'Covenants Not to Compete: A State by State Survey.' Table A.1 provides the complete list of these seven dimensions and their respective weights used to measure the composite score of NCA enforceability. Building on this dataset, Johnson, Lavetti, and Lipsitz (2024) have constructed a state-year panel from 1991 to 2014 using the same methodology. I use their composite NCA Enforceability score, which scales from 0 (completely unenforceable) to 1 (the strictest enforceability observed in our sample).

1.4.2 LinkedIn Revelio Data

LinkedIn Revelio data provides detailed individual-level information on employment history, including the specific branches where individuals have worked. This data is crucial for studying worker mobility as it allows me to track where people work, when they change jobs, and the duration of their employment at each firm. Once I convert this data into a panel format, I occasionally encounter cases where an individual has affiliations with multiple firms within the same year. In such instances, I count the years the individual has worked at each firm throughout their lifetime and retain the firm with the higher count. This approach helps identify the primary employer, addressing potential data entry errors or situations where an individual holds multiple jobs simultaneously.

LinkedIn Revelio also provides an O*NET code for each job, enabling me to categorize jobs and use various occupation-related measures, such as the average training period required to perform tasks. I infer the age of each individual by assuming that their bachelor's degree entrance year corresponds to when they were 19 years old, using the earliest listed degree when multiple bachelor's degrees are present. The dataset includes stock ticker information and exchange listings, allowing me to merge this data with other datasets. For my analysis, I restrict the sample to public firms traded on U.S. stock markets, providing a focused view on worker mobility and employment patterns within publicly traded companies.

1.4.3 Patents: Measuring Individual Productivity

I use patent data to measure the productivity of individuals, utilizing the USPTO PatentsView disambiguated data (Link). This data provides comprehensive details about patent authors, including their full names and the companies they were affiliated with during the patent application process. I construct an author-year panel by counting the number of patents granted to each author annually. To complement this data, I use forward citations and the market value of patents from Kogan et al. (2017). The data from Kogan et al. (2017) provides the market value of patents for each patent number, as well as the affiliated companies' PERMNO, which enables me to retrieve the PERMNO of all public firms listed in the PatentsView dataset. I merge this panel with LinkedIn Revelio data by matching individuals' first names, last names, affiliated companies, and the corresponding year. Using the patent data, I use three proxies to measure the productivity of individuals: (1) the total number of patents granted within a year, (2) the total number of forward citations received, and (3) the sum of the market value of patents granted.

1.4.4 Others and Sample

I use SDC Mergers for M&A transactions data. For my main analyses, I use completed merger deals and include incomplete mergers for additional analyses. I use Center for Research in Security Prices (CRSP) for stock price data and annual accounting data from CRSP/Compustat Merged (CCM).

Table 1 tabulates the number of individuals in my sample and their key characteristics. The sample spans from 1991 to 2014, covering 513,847 individuals, 3,555,203 individual-year observations and 301 M&A deals. The sample consists of 7,652 individuals, 66,116 individual-year observations and 105 M&A deals after merging the data with PatentsView, representing a subset of inventors. The sample primarily consists of young, highly educated workers, reflecting LinkedIn's user base, which tends to include white-collar professionals focused on career advancement. Nearly all individuals in the sample hold at least a bachelor's degree; among inventors, 63% have obtained a master's degree or higher. Hence, these employees are more likely to sign NCAs and would be subject to NCAs' restrictions if the states they reside in enforce NCAs.

2 Main Results

2.1 Non-Compete Agreements Limiting Departure

Are employees less likely to leave the firm after mergers if they work in states with high NCA enforceability? If so, to what extent does NCA enforceability affect retention rates? To test this hypothesis, I estimate the following difference-in-difference regression,

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst}, \tag{2}$$

where Departure_{ijst} is 1 if an individual i who is from target firm j joins a new firm in year t and 0 otherwise. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement, and Post_t is equal to 1 from the announcement. The coefficient β estimates the fraction of employees an acquiring firm will likely lose if it cannot enforce NCAs on target firm employees.

I choose a linear probability model (LPM) using OLS to estimate Equation (2), over a logit model as the primary method since I am handling multiple fixed effects. When using fixed effects in logistic regression, groups with no variation in the dependent variable are dropped from the analysis because the model cannot estimate the likelihood function for these groups. In fact, Timoneda (2021) finds LPM with fixed effects produces more accurate estimates and predicted probabilities than logit models, particularly when the dependent variable has a low proportion of ones, specifically less than 25%. Since the average departure rate in my setting is 17%, we can expect LPM to yield more accurate estimates. I also estimate a binary logit model for departure in the appendix, and the results are consistent with the LPM estimates, which I will discuss later.

Table 2 presents the results for Equation (2). Estimates are based on an 11-year window around the merger announcement (–5 $\sim$ 5). I compute triple clustered standard errors by the target firm, industry (2-digit NAICS code), and year. The table shows that people working in states with high NCA enforceability are significantly less likely to leave after the merger. Moving from no enforceability to the strictest enforceability leads to an 11% reduction in the departure rate. Given that the average departure rate in my sample is 17%, this reduction is equivalent to a decrease in the departure rate of nearly 60%. The results remain robust with and without MSA and job-title (2-digit O*NET occupation code) fixed effects, which control for unobserved MSA- and occupation-level characteristics. One might be concerned that each target firm could allocate different types of occupations or roles based on locations in their unique way. In this case, MSA and Job Title fixed effects may not fully account for these variations. Thus, in the appendix, I repeat the same analysis using Target Firm $\times$ MSA, and Target Firm $\times$ Job Title fixed effects, and the results remain robust to these changes (Table A.3).

Table A.4 presents the results for the binary logit model. The coefficients on NCA Enforceability $\times$ Post are significantly negative for the logit model as well. The point estimates range from -1.23 to -1.26, which implies that moving from a state with zero enforceability to one with the strictest enforceability would reduce the odds of switching firms by approximately 70%.

These results are important for two reasons. First, they demonstrate that NCAs indeed help acquiring firms to retain target firm employees. 11% represents the lower bound of the fraction of target firm employees that acquiring firms wish to retain using NCAs but lose if they cannot enforce NCAs. In fact, I find acquirers are willing to pay a premium when a larger proportion of employees from target firms are bound by NCAs, recognizing the value of retention (Table A.5). In addition, the results also establish that M&A events are effective shocks to the employer-employee match quality. Given that the difference in departure rates between branches begins to increase significantly following the merger announcement, these results indicate a substantial group of employees whose match quality has declined and who, therefore, want to leave post-merger. This suggests that the acquirer is also retaining poorly matched employees. In the following subsection, I will discuss the consequences of this retention, particularly how their productivity changes.

2.2 Locking in Employees Lowering Productivity

Since it is easier to retain employees from the target firm after the merger once they are bound by higher non-compete agreements enforceability, this subsection explores its subsequent impact on productivity. As illustrated in Figure 1, once the acquirer locks in employees using non-compete agreements, it also retains a subset of employees whose match quality declines. Thus, the average productivity of employees bound by NCAs will decrease relative to those who are less restricted and choose to stay. To test this empirical prediction, I subset the sample to include only inventors who were granted patents (*inventors*)[5]. Using this sample, I estimate the following difference-in-difference regression,

$$\text{Productivity}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst}, \tag{3}$$

where $\text{Productivity}_{ijst}$ is an individual i's productivity measured by the number or value of patents granted. I use three different proxies for patent productivity: a raw count (*No. Patents*), citation-weighted (*Citations*), and market value-weighted (*Market Value*), with the market value of patents sourced from Kogan et al. (2017).

Table 3 presents the results for Equation (3). Panel A shows that people working in high NCA enforceability states become significantly less productive after the merger compared to those in states with lower enforcement, based on employees who were at target firms one year prior to the announcement. This effect is consistent across all three types of productivity measures, and

[5] I repeat the same analysis in Figure 5 and Table 2 using the inventor sample in the appendix, and the results are consistent with those obtained when the sample is not restricted to inventors. Please refer to Figure A.1 and Table A.6.

the results remain robust with MSA and job-title (2-digit ONET code) fixed effects[6]. Given the averages of the three productivity measures – *No. Patents* (0.31), *Citations* (7.28), and *Market Value* (7.01) – the effect is also economically significant. Moving from no enforceability to the strictest enforceability would result in approximately a 40% decline in the number of patents, a 30% reduction in citations, and patents that are worth 60% less in market value.

Table 4 further suggests the decline in productivity is more pronounced among productive workers. I interact the difference-in-difference term from Equation (3) with pre-merger productivity, defined by the average productivity from $t-5 \sim t-1$. The table suggests that the productivity decline is more severe among productive workers, as shown by the significantly positive coefficients on the triple difference term. This finding is particularly interesting as it highlights that the adverse effects of NCAs and mergers disproportionately impact the top performers rather than affecting the less skilled employees. Additionally, I estimate the same empirical models as in Table 3 among the subsample of active inventors in the appendix. I define active inventors as the ones who were granted patents one year prior to the announcement. Table A.8 suggests the coefficient estimates are larger than the ones from the baseline model (Table 3), implying the impact of NCAs is indeed greater among productive employees.

To quantify the economy-wide loss of innovations due to NCAs, I use the following back-of-the-envelope calculation,

$$\text{Loss of Innovation} \equiv \sum_i \left(\hat{\beta}(\text{Productivity}) \times \text{NCA Enforceability}_{st} \right), \tag{4}$$

where $\hat{\beta}(\text{Productivity})$ is the coefficient estimate from Equation (3) and NCA Enforceability$_{st}$ is NCA enforceability one year prior to the announcement. The set of individuals subject to this calculation is based on employees who worked for the target firms one year prior to the announcement. This estimate provides the counterfactual measure of additional innovation if all target firm employees were in states with zero NCA enforceability. Based on Table 3, the economy would have approximately 600 new patents annually, worth \$10 billion if NCAs were fully unenforceable. These estimates represent a 26% gain relative to the current post-acquisition innovation levels. Although this back-of-the-envelope calculation does not account for other factors, such as acquiring firms potentially increasing investment in human capital in branches where NCAs are not enforced, which could help them regain productivity. However, given that their productivity declined for the first five years, this implies that the economy experiences a tremendous loss of innovation due to the misallocation of human capital for quite a while. Overall, the results suggest

[6] I repeat the same analysis using Target Firm × MSA and Target Firm × Job Title fixed effects in the appendix, and the results remain robust to these changes (Table A.7)

a significant reduction in the productivity of people bound by stricter enforcement of non-compete agreements compared to those who are not following acquisition.

2.3 Timing of Non-Compete Agreements Impacts

I look at the timing of NCAs' impacts on departures and productivity around merger announcements. My conjecture is that there should be no differences in departure rates or productivity based on NCA enforceability before the announcement, as the difference-in-difference approach requires employees not to experience different trends before the shock. I also expect the impact of NCAs on productivity to gradually increase over time as it takes some time to get patents granted.

To show there is no pre-trend of NCA effects in the difference-in-difference setting, I estimate the following,

$$y_{ijst} = \alpha_i + \alpha_j + \sum_{k=-5}^{5} \beta_k \cdot (\text{NCA Enforceability}_{st} \times \tau_k) + \varepsilon_{ijst}, \tag{5}$$

where τ_k is a dummy variable equal to 1 around the merger announcement year. Figure 5 plots the coefficients on each event time, β_j, as well as their 95% confidence intervals using a dummy variable for departure as the dependent variable. The omitted time period is one year prior to the merger announcement. The figure clearly shows that there is no pre-trend, and the negative impact of NCA Enforceability on the departure rate begins from the merger announcement and continues for up to 4 years. This implies that the M&A event is an effective shock to the match quality between the employer and employees, leading those whose match quality declines to leave the firm following the announcement. In the appendix, I show the same applies to entrepreneurial departure as well: the impact of NCAs only begins to take effect from the merger announcement onward (Figure A.2).

Figure 6 plots the coefficients on each event time, β_j as well as their 95% confidence intervals from Equation (5) using the number of patents as the dependent variable. The figure shows that there is no pre-trend as well, and the negative impact of NCA Enforceability on the productivity begins to appear from the announcement and continues for up to five years. In fact, the coefficient estimates grow in magnitude (from –0.1 in year 0 to –0.4 in year 5) since the patenting process takes years to take effect fully. This suggests that the gap between the branches with different levels of NCA enforceability will likely widen over time. In addition, Figure 4 presents a scatter plot of NCA enforceability and the average number of patents per inventor across states, using the whole sample of inventors working at public firms in 2014. The plot confirms a very weak correlation between these two in general, as most observations fall within the confidence intervals.

Furthermore, I demonstrate that people's productivity is not associated with NCA enforceability before the merger announcement, and firms do not allocate inventors' productivity according to NCA Enforceability as follows. I estimate the following during the period before the merger announcement (i.e., within the window of –5 $\sim$ –1),

$$\text{Productivity}_{ijst} = \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{ijst}. \quad (6)$$

NCA Enforceability$_{st}$ is the degree of NCA Enforceability one year prior to the announcement. Target firm fixed effects allow us to compare how each firm allocates its employees based on their productivity in relation to NCA enforceability. Table 5 shows no significant relationship exists between NCA Enforceability and productivity during the pre-merger period. This further confirms that firms do not allocate their inventors based on NCA Enforceability, indicating that NCAs impact departures and productivity only when there is a shock to the employer-employee match quality in the presence of mergers.

Moreover, this result also implies that the acquirer retains poorly matched employees as they enforce NCAs. If target firms had allocated more productive employees in the non-compete branch, the results from the previous section (Section 2.1) might suggest that the acquirer simply let go of unproductive workers in branches with looser NCA enforceability. However, since productivity is not correlated with NCA enforceability during the pre-merger period, this finding suggests that the results in Table 2 are not simply due to the acquirer terminating unproductive employees in states with looser NCA enforceability. Instead, it implies that M&As are effective shocks to the employer-employee match quality: People whose match quality has declined in the branch where NCAs are not enforced can choose to leave, whereas the same group of people in branch with NCA enforcement cannot do so.

3 Mechanisms

The previous section characterizes how mergers create a mismatch between the acquirer and employees from target firms, reducing their productivity and entrepreneurial activity. This section explores the drivers of this mismatch.

3.1 Firm-Specific Human Capital

Becker (1962) proposes there are two types of human capital: firm-specific and general human capital. The impact of NCAs can be mitigated if human capital is more firm-specific, as employees prefer to stay with their original employers where their human capital is established. For instance, in firms where employees actively collaborate as a team, they may be less inclined to leave, making NCAs less relevant. To put it differently, the departure of team members can have a ripple effect, determining the match quality of those who remain. Even for those who stay, whether by choice or due to factors like match quality, the dynamics within their teams can be significantly disrupted if key collaborators leave after a merger. Employees who would otherwise be expected to perform well may experience a decline in match quality and, thus, in productivity, due to the loss of critical team members.

I use three measures to test how the firm-specificity of human capital affects mismatch. First, I use the intensity of firm-specific training. The idea is that firm-specific training fosters firm-specific and team-based human capital. To empirically measure the magnitude of firm-specific training, I use the length of the *in-plant* training as a proxy. O*NET provides several training periods under Specific Vocational Preparation (SVP) by occupation level, providing details on the different types of training and experiences required for a typical worker to achieve average performance in a specific job. I focus on in-plant training, which refers to long-term, hands-on training provided by employers[7]. This training represents the employer's investment in human capital, enabling employees to develop firm-specific skills. Using this training period as the measure, I estimate the following triple-difference regression,

$$
\begin{aligned}
y_{ijst} = {} & \alpha_i + \alpha_j + \alpha_t + \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\
& + \beta_2 \cdot (\text{NCA Enforceability}_{st} \times \text{Firm-Specific Training}_{it} \times \text{Post}_t) \\
& + \beta_3 \cdot (\text{Firm-Specific Training}_{it} \times \text{Post}_t) \\
& + \beta_4 \cdot (\text{NCA Enforceability}_{st} \times \text{Firm-Specific Training}_{it}) + \varepsilon_{ijst},
\end{aligned} \tag{7}
$$

where I use a dummy variable for departure and the number of patents as dependent variables. The first two columns from Table 6 Panel A presents the results. The coefficient on the triple difference regression term (β_2), is significantly positive for both departure and productivity. This indicates the impact of NCAs is smaller if employees have gone through intensive firm-specific training and thus, possess more firm-specific human capital. In the appendix, I use two other measures for productivity, citations, and market value of patents, and the results remain robust.

The second measure I use to analyze how firm-specific human capital can influence match quality following mergers is the likelihood of collaborators leaving by computing their average NCA enforceability. I identify coauthors of patents within the same firm during the five years

[7] The classification they use is 1. Short demonstration only, 2. Anything beyond a short demonstration up to and including 1 month, 3. Over 1 month up to and including 3 months, 4. Over 3 months up to and including 6 months, 5. Over 6 months up to and including 1 year, 6. Over 1 year up to and including 2 years, 7. Over 2 years up to and including 4 years, 8. Over 4 years up to and including 10 years, and 9. Over 10 years

before the merger announcement and calculate the average NCA enforceability score for these coauthors one year prior to the merger. I use each patent's number of citations as a weight when computing the average, giving more weight to coauthors with whom they collaborated on more valuable patents[8]. I estimate the following triple difference regression,

$$\begin{aligned} y_{ijst} = \alpha_i + \alpha_j + \alpha_t &+ \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\ &+ \beta_2 \cdot \big(\text{NCA Enforceability}_{st} \times \text{Coauthors' NCA}_{ijst} \times \text{Post}_t\big) \\ &+ \beta_3 \cdot \big(\text{Coauthors' NCA}_{ijst} \times \text{Post}_t\big) + \varepsilon_{ijst}. \end{aligned} \tag{8}$$

Columns 3 and 4 from Table 6 Panel A presents the results. The coefficient on the triple interaction term, β_2, is significantly positive, which indicates the negative impact of NCA on productivity is mitigated. This suggests that people are less likely to suffer from mismatches following mergers if their team members are more likely to stay.

The last measure I use for firm-specific human capital is organization capital, following Eisfeldt and Papanikolaou (2013). Organization capital refers to a production factor tied to the firm's key talent, with its efficiency being uniquely firm-specific. Therefore, firms investing heavily in their organization capital tend to develop more firm-specific human capital. I use the perpetual inventory method, based on sales, general, and administrative expenses, to compute the stock of organization capital and scale it by book assets, following the approach of Eisfeldt and Papanikolaou (2013) and estimate the following triple-difference regression,

$$\begin{aligned} y_{ijst} = \alpha_i + \alpha_j + \alpha_t &+ \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\ &+ \beta_2 \cdot \big(\text{NCA Enforceability}_{st} \times \text{Organization Capital}_{ijst} \times \text{Post}_t\big) \\ &+ \beta_3 \cdot \big(\text{Organization Capital}_{ijst} \times \text{Post}_t\big) + \varepsilon_{ijst}. \end{aligned} \tag{9}$$

Columns 1 and 2 from Table 6 Panel B show that the coefficient on the triple-difference term is significantly positive when regressing the number of patents, indicating a substantial effect on productivity, while the coefficient for departures is directionally consistent but not statistically significant. This further confirms that the impact of NCAs on productivity is less pronounced when employees have more firm-specific human capital. Overall, across all three measures, the results robustly suggest that the impact of NCAs is mitigated once employees' human capital is more firm-specific.

[8] The results remain consistent when computing the equal-weighted average.

3.2 Level of Human Capital

Is the impact of NCAs more pronounced among individuals with more human capital? I find that the effects of NCAs are more pronounced among employees with higher levels of human capital, as their general skills make them more likely to leave if not restricted by NCAs. Table 4 already shows the impact of NCAs on productivity is bigger for individuals who were more productive before the merger. This suggests that individuals with more human capital suffer more severely from the mismatch caused by NCAs following mergers.

To investigate this further, I use two alternative measures to capture the level of human capital: social skills and routine task intensity. Deming (2017) finds that individuals with strong social skills who perform non-routine tasks are significantly more rewarded than their peers. Leveraging occupation-level social skills and routine task intensity measures from Deming (2017) and Acemoglu and Autor (2011)[9], I estimate the following triple-difference regression,

$$\begin{aligned} y_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\ + \beta_2 \cdot \left(\text{NCA Enforceability}_{st} \times X_{ijst} \times \text{Post}_t\right) \\ + \beta_3 \cdot \left(X_{ijst} \times \text{Post}_t\right) + \varepsilon_{ijst}, \qquad X \in \{\text{Social}, \text{Routine}\}. \end{aligned} \tag{10}$$

Table 6 Panel C documents that the impact of NCAs on productivity is significantly more pronounced for employees with higher social skills who perform non-routine tasks. The coefficients on the triple difference terms are significant when regressing the number of patents, although there is no difference in the departure rates. These results indicate that employees with higher human capital are disproportionately impacted by NCAs, experiencing a more severe productivity decline.

4 Robustness and Additional Implications

4.1 Robustness Checks

In this subsection, I conduct three robustness checks. I employ a placebo test with failed mergers, examine whether the treatment is exogenous, and show patenting decisions are not correlated with NCA enforceability.

[9] For the social skill measure, I compute the average score of four components from the O*NET "social skill" module: (1) coordination, (2) negotiation, (3) persuasion, and (4) social perceptiveness. For the routine task intensity measure, I calculate the sum of the standardized routine manual and routine cognitive scores, then subtract the standardized non-routine manual (interpersonal and physical) and non-routine cognitive (analytical and interpersonal) scores, as generated by the code from Acemoglu and Autor (2011). All data are provided by O*NET.

4.1.1 Placebo Test

I employ a placebo test to examine whether the productivity results are driven by other factors related to M&A announcements or NCAs aside from mismatch. I test whether the effect observed in the previous subsection can also be found in a scenario where no effect should logically exist. To do so, I use failed mergers as a control scenario. I re-estimate Equation (3) using a sample of individuals who worked at target firms where the merger was announced but never completed.

Table 7 shows no differences in departure rates across branches with different levels of NCA enforcement. That is, the impact of NCAs on departures only exists among employees from complete mergers. The table also shows the adverse effects of NCAs on productivity only exist among complete mergers. In fact, the coefficient estimates are positive, if at all. This indicates that when a merger is not completed, employees' match quality with their original employer stays the same. Thus, their productivity does not decline compared to colleagues without NCA enforcement. These results strongly suggest that the observed productivity decline in the main analysis is indeed attributable to the completed mergers and not merely to other confounding factors.

4.1.2 Alternative Control Group

The main difference-in-difference analysis (Table 3) demonstrates employees from the branch with NCA enforcement experience a productivity decline relative to those from the branch without enforcement within the same firm post-merger. This subsection explores an alternative control group by comparing employees from failed mergers. Employees from failed mergers will likely share similar characteristics with those in the treated group, making them a suitable control group. Additionally, I restrict the sample to individuals who stayed with the target firm throughout the entire sample period to ensure my estimates can clearly capture the impact of locking in employees. Using a sample of both completed and incomplete mergers, I estimate the following triple difference regression,

$$\begin{aligned}\text{Productivity}_{ijkst} = {} & \alpha_i + \alpha_k + \alpha_t + \beta_1 \cdot \big(\text{Complete}_j \times \text{Post}_t\big) \\ & + \beta_2 \cdot \big(\text{Complete}_j \times \text{NCA Enforceability}_{ijkst} \times \text{Post}_t\big) \\ & + \beta_3 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\ & + \beta_4 \cdot \big(\text{Complete}_j \times \text{NCA Enforceability}_{st}\big) + \varepsilon_{ijkst},\end{aligned} \tag{11}$$

where Complete_j is a dummy variable equal to 1 if the target firm j is successfully acquired by the acquirer k. Acquirer fixed effects (α_k) allow us to compare the productivity of employees from two firms: one that the acquirer successfully acquired and the other that was not. The sample consists of individuals who remained with the acquirer for up to five years after the merger announcement.

In cases where individuals in the sample worked for both acquired and non-acquired target firms, I classified them based on completed mergers and retained only relevant observations.

Table 8 presents the results. A difference-in-difference setting where I use $\text{Complete}_j \times \text{Post}_t$ as the regressor in columns (1), (3), and (5) shows the merger itself does not seem to affect the productivity. However, the triple difference regression in columns (2), (4), and (6) show that locking-in employees does indeed decline employees' productivity. The coefficients on the triple difference term (β_2) are significantly negative, except when using the market value to measure productivity, although the point estimate is still negative. This result implies that individuals who are forced to stay with the acquirer after the merger due to non-compete agreements produce fewer and less valuable patents than those not acquired but still bound by NCAs. Their counterfactual productivity could be much higher, and they do not reach their full potential with the current acquirer.

4.1.3 Patenting Ideas

Previous literature has claimed that stricter non-compete agreements lead to a decline in patents, as firms are less concerned about losing their ideas due to the spread of knowledge when employees move to competitors. Thus, one might argue that less patenting may not necessarily be associated with lower productivity; firms may simply find less need to patent ideas in areas with stricter non-compete agreements.

My immediate response to this critique is that I am estimating a difference-in-difference regression. I am conditioning on the same NCA Enforceability regime and using the change of the employer due to M&As as the shock, rather than changes in NCA Enforceability. Hence, it is unlikely that people would suddenly stop filing patents in states with stricter NC agreements compared to those with looser agreements after the merger announcement. Additionally, if firms were choosing whether to patent their ideas based on NCA Enforceability, the patents filed in stricter NC areas should be much more valuable, as firms would only tend to patent their most critical ideas. However, Table 3 has already shown that the decline in productivity is not limited to the number of patents but also extends to the value of patents, as measured by citations and market value.

On top of that, I find firms' patenting decisions do not depend on local NCA enforceability. I estimate the following using the sample of patents written by the inventors in my sample,

$$\text{No. Patents Coauthored within One State}_{jst} = \alpha_{jt} + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{jst}. \tag{12}$$

$\text{No. Patents Coauthored within One State}_{jst}$ is the total number of patents with more than one coauthor, all of whom are from the same state s in year t. NCA Enforceability$_{st}$ is the degree of non-compete agreement's enforceability in state s and year t. Suppose firms are more likely to patent their ideas in states where non-compete agreements are less enforced. In that case, we expect the coefficient on NCA Enforceability$_{st}$ to be significantly negative. Table 9 presents this is not the case. The coefficient is insignificant in both columns, with and without firm $\times$ year fixed effects (α_{jt}). In fact, the R^2 is close to 0 in column 1, which indicates NCA Enforceability does not explain the tendency to patent at all. The fact that firm $\times$ year fixed effects increase the R^2 from 0 to 0.42 in column (2) suggests that patenting decisions are made at the corporate level rather than the branch level. Collectively, it is difficult to conclude that firms' policies could have influenced the main causality from the previous subsection.

4.2 Additional Implications

This section explores additional implications, including why acquiring firms are willing to retain target firm employees even when they become less productive, the impact of NCAs on entrepreneurship, how teams are disrupted after a merger, and the relationship between acquirers' post-merger outcomes and NCAs.

4.2.1 Why Acquirers Retain Mismatched Employees

In this subsection, I demonstrate why acquiring firms retain mismatched employees, even when these employees could be significantly more productive elsewhere. At first glance, this may seem counterintuitive. However, an employee's enhanced productivity at another firm provides no direct benefit to the acquiring firm. One could imagine a scenario in which an employee, recognizing their higher productivity potential elsewhere, attempts to pay an upfront transfer fee to leave. Clearly, contracting frictions exist, but I take these frictions as given, as such transactions rarely occur in practice.

Furthermore, there are several reasons why acquiring firms may choose to retain mismatched employees in high-NCA states. First, firms may retain mismatched employees to avoid strengthening competitors. If these employees leave, they may secure new positions–potentially at competing firms–where they can be significantly more productive. This, in turn, would strengthen competitors and ultimately harm the acquiring firm. Consistent with this hypothesis, I find that the effects of NCAs are more pronounced when the target firm operates in a highly competitive industry, where I use Herfindahl-Hirschman Index (HHI) of the target firm one year prior to the announcement as a proxy for product market competitiveness. Table 10 column (1) and (2) show the effects of NCAs on departure rates are smaller once the target firm operates in the industry with higher HHI. This finding aligns with the main findings in Cunningham, Ederer, and Ma (2021), where they argue that firms may acquire others to eliminate potential competitors, forcing them to abandon target firm projects and, in effect, remain inactive.

Next, acquiring firms may seek to expand their operations into new locations. Harford, Piotrowski, and Qian (2023) argue that whether an acquiring firm retains or closes a specific branch of the target firm depends on its existing presence in that location. Specifically, if the target firm has a branch in a state where the acquiring firm does not, the acquirer is more likely to retain that branch to facilitate expansion. Acquiring firms may enforce NCAs to retain target firm employees in locations where they do not have an existing branch. I test this hypothesis by examining whether the effects of NCAs are weaker when there is geographic overlap between the acquiring and target firms in a given state. Table 10 column (3) and (4) show the effects of NCAs are indeed weaker once there is a geographic overlap.

Lastly, in high-NCA states, replacing mismatched employees is more difficult, as employees from other firms are also likely bound by NCAs. Consequently, acquiring firms are more inclined to retain mismatched employees, given the challenges of hiring or poaching talent from other firms. I test this hypothesis by examining how the effects of NCAs vary with labor market competition for a given occupation in each state. Specifically, if demand for a certain occupation is high in a particular location, labor market competition makes it more difficult for firms to recruit talent from other firms. In such cases, the effects of NCAs should be stronger. I find that this is indeed the case, using the Herfindahl-Hirschman Index (HHI) of a given occupation in the labor market, measured with LinkedIn data. Table 10 column (3) and (4) show the effects of NCAs are weaker when employer concentration is higher.

4.2.2 NCAs and Entrepreneurship

Since stricter NCAs limit employee departures, they can also effectively suppress entrepreneurship. Past literature has documented that employee departures spur entrepreneurship, particularly in industries where employees are key assets (Gompers, Lerner, and Scharfstein (2005); Babina (2020)). Once a firm is acquired, employees whose match quality has declined may choose to leave and start their own businesses. This entrepreneurial activity can be a natural response to dissatisfaction, where individuals seek new opportunities outside the constraints of their current employment. However, strict NCAs can suppress this form of entrepreneurship by restricting employees' ability to leave and pursue their own ventures, thereby stifling innovation and reducing the overall dynamism in the market.

To study how NCAs deter entrepreneurship, I estimate the following difference-in-difference regression in a similar fashion to Equation (2),

$$\text{Entrepreneurial Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{ijst}, \tag{13}$$

where Entrepreneurial Departure$_{ijst}$ is 1 if an individual i from target firm j joins a new firm as a founding team member in year t and 0 otherwise. I define a founding team member as someone with a job title that includes *Founder*, *Co-founder*, and *President*. Table 11 presents the results. The table clearly shows the coefficients on the difference-in-difference term (β) are all significantly negative, even when including Target Firm $\times$ Year fixed effects in column (2). Moving from states with no non-compete agreements to those with the strictest enforcement would result in approximately 200 fewer startups.

I quantify the economy-wide loss of entrepreneurship due to NCAs as follows,

$$\text{Loss of Entrepreneurship} \equiv \sum_i \left(\hat{\beta}(\text{Entrepreneurship}) \times \text{NCA Enforceability}_{st}\right), \tag{14}$$

where $\hat{\beta}$(Entrepreneurial Departure) is the coefficient estimate from Equation (13). Based on Table 11, the economy would have approximately 2,700 new firms annually if NCAs were fully unenforceable. This represents a significant reduction compared to the current annual mean of 4,600 entrepreneurship departures from the announcement year through year 5. The high number of reductions could be due to the spillover effects of entrepreneurship, where NCAs not only deter individuals from starting their own businesses but also indirectly affect others who might otherwise be encouraged to pursue entrepreneurial ventures (Giannetti and Simonov (2009); Nanda and Sorensen (2010); Sorensen and Fassioto (2011); Guiso, Pistaferri, and Schivardi (2021)). Overall, these results imply that non-compete agreements hinder the entrepreneurial activities of former employees, likely by restricting their ability to leave and use their industry-specific knowledge to launch new ventures.

4.2.3 Teams' Disruption and Weak Synergies

Although the previous results indicate that losing a collaborator leads to a decline in productivity, this issue could be mitigated if acquired employees can replace those collaborators with employees from the acquirer. This could be beneficial if there is a strong synergy with the acquirer and they can form new, productive collaborations. To explore this potential synergy, I analyze whether there is active collaboration between the two groups by examining how employees work together post-merger.

Table 12 studies whether employees from the target firm coauthor patents with employees from the acquirers after the merger. Based on individuals from target firms who were granted patents one year prior to the announcement, I track their patents for up to 10 years following the completion of the merger. The reason for restricting the sample to those granted patents one year prior to the announcement is to identify active inventors. If I were to include every inventor and compute the ratio of those who coauthor patents with employees from the acquirer, I might underestimate the ratios, as this could include individuals who do not frequently produce patents. Then, I compute the ratio of coauthored patents to the total patents written by these employees, as well as the ratio of individuals who coauthor patents with employees from the acquirer. The table illustrates that only a tiny proportion of patents and authors from the target firm collaborate with employees from the acquirer. Specifically, out of 6,358 total patents, only 405, or about 6%, are coauthored with the acquirer. Similarly, out of 296 total authors, only 26, or approximately 8.8%, have coauthored patents with employees from the acquiring firm. This data suggests that very few individuals from the target firm engage in collaborative patenting with the acquirer post-merger.

One possible critique of the analysis could be that patents involving both employees from the target firm and the acquirer might represent larger, more significant projects, which could inherently be more valuable, even if fewer in numbers. However, Figure 7 illustrates this is not the case. Again, I track patents for up to 10 years following the completion of the merger among active inventors. The figure compares the value of patents that are coauthored with the acquirer to those that are not coauthored – solely written among employees from the target firms – showing that the latter group is much more valuable. Specifically, coauthored patents have an average of 5.798 citations, while patents not coauthored with the acquirer average 14.781 citations. Regarding market value, coauthored patents are valued at $10.269 million, whereas non-coauthored patents are valued at $11.365 million. This comparison clearly shows that not only do very few employees coauthor patents with employees from the acquirer, but coauthored patents also have significantly lower scientific and market values, indicating weak synergies between the acquirer and the target firm's employees.

4.2.4 Post-Merger Outcomes

Lastly, I explore how NCA enforceability affects the acquirer's post-merger outcomes. In cases where an acquirer targets multiple firms within the same year, I compute the value-weighted average of the NCA enforceability scores for each target firm, using the number of employees at each firm as weights. I regress 5-year changes in various accounting variables as well as innovation outputs measured by patent activities on the NCA enforceability scores. All dependent variables are industry-adjusted by subtracting industry (NAICS 4-digit) median.

Table 13 presents the results. Panel A regresses 5-year sales growth, 5-year changes in profitability measured by return on assets (ROA), operating profitability (OP), and the R&D expense to sales ratio. The results indicate that acquirers with high bargaining power experience greater sales growth and improved profitability post-merger, as evidenced by higher NCA enforceability. However, no significant increase in R&D expenses is associated with higher NCA enforceability. This may suggest that stronger NCAs help protect existing business operations and profitability, but do not necessarily encourage additional investment in innovation or research and development.

To further confirm that higher NCA enforceability is not associated with innovation outputs, Panel B presents regressions on the 5-year changes in the number of patents, citations, and market value of patents. The results suggest stricter NCA enforceability is not associated with higher innovation output post-merger. This indicates that acquirers do not necessarily engage in more innovative activities, even when they use their bargaining power to retain more employees from target firms . This finding is consistent with the main results from Section 2, where I document that although more employees stay in the branch where NCAs are enforced, they experience a decline in productivity as the acquirer also retains employees whose match quality has decreased. This finding also suggests that higher retention rates of target employees help improve the acquirers' sales and profitability, but not necessarily because they have become more innovative through synergies. Instead, the increase in sales growth and profitability is more likely due to greater market power.

5 Conclusion

This paper characterizes the causal impact of non-compete agreements (NCAs) on employee departure decisions and productivity in the context of mergers and acquisitions (M&As). By leveraging a unique combination of datasets, worker-level data from LinkedIn Revelio and USPTO PatentsView, I provide a comprehensive analysis of how NCAs influence various facets of employment and innovation. The findings indicate that the acquiring firms can retain target firm employees by enforcing NCAs. However, NCAs significantly reduce the productivity of individuals who are locked in by these agreements. I show that employees who remain with a firm due to NCAs tend to have lower patent output and reduced innovation quality, as measured by citations and market value. This decline in productivity is more pronounced among employees with higher pre-merger productivity, underscoring how NCAs can lock in highly skilled but poorly matched talent, thereby diminishing their potential contributions. Beyond the decline in productivity, NCAs suppress entrepreneurship, as employees bound by these agreements are less likely to leave and start their own ventures.

These results call for a re-examination of NCAs, particularly in light of their broader economic implications for innovation, entrepreneurship, and labor mobility. This discussion has become especially timely with the Federal Trade Commission's (FTC) recent attempt to ban NCAs nationwide in 2024, ultimately overturned by a court decision. This ruling has reignited debate over the role of NCAs in the modern economy, raising important questions about balancing firm interests with worker mobility and innovation.

Future research could explore the long-term impacts of NCAs on employees' career development and innovation activities. For instance, investigating whether employees subject to NCAs experience slower career progression or are less likely to initiate their own businesses later in their careers would provide deeper insights into the enduring effects of these agreements. Additionally, we could explore why acquirers retain employees who are no longer a good fit for the company. For example, are acquirers concerned about losing valuable knowledge to competitors, or are they reluctant to lay off workers who could become productive with competitors? Answers to these questions can underscore the importance of M&As in limiting employees' choices, especially as the issue of monopsony in the labor market, where there are fewer employers in the labor market, becomes more pronounced. As M&As reduce the number of available options for job seekers, the impact of NCAs could exacerbate this problem, further restricting employee mobility and career opportunities. Understanding these dynamics is crucial for addressing the growing concerns about labor market competition and the potential adverse effects on innovation and entrepreneurship.

References

Acemoglu, D. and D. Autor (2011). Skills, tasks and technologies: Implications for emplooyment and earnings. *Handbook of Labor Economics 4*(B), 1043–1171.

Acharya, V. V., R. P. Baghai, and K. V. Subramanian (2014). Wrongful discharge laws and innovations. *Review of Financial Studies 27*(B1), 301–346.

Agrawal, A., J. F. Jaffe, and G. N. Mandelker (1992). The post-merger performance of acquiring firms: A re-examination of an anomaly. *Journal of Finance 47*(4), 1605–1621.

Akcigit, U. and S. T. Ates (2023). What happened to U.S. business dynamism? *Journal of Political Economy 131*(8), 2059–2124.

Asquity, P. (1983). Merger bids, uncertainty, and stock holder returns. *Journal of Financial Economics 11*, 51–83.

Babina, T. (2020). Destructive creation at work: How financial distress spurs entrepreneurship. *Review of Financial Studies 33*(9), 4061–4101.

Balasubramanian, N., J. W. Chang, M. Sakakibara, J. Sivadasan, and E. Starr (2022). Locked in? The enforceability of covenants not to compete and the careers of high-tech workers. *Journal of Human Resources 57*(S), 349–396.

Becker, G. S. (1962). Investment in human capital: A teoretical analysis. *Journal of Political Economy 70*(5), 9–49.

Bishara, N. D. (2011). Fifty ways to leave your employer: Relative enforcement of covenants not to compete, trends, and implications for employee mobility policy. *University of Pennsylvania Journal of Business Law 13*, 751–795.

Colvin, A. J. S. and H. Shierholz (2019). Noncompete agreements: Ubiquitous, harmful to wages and to competition, and part of a growing trend of employers requiring workers to sign away their rights. *Economic Policy Institute*.

Conti, R. (2014). Do non-competition agreements lead firms to pursue risky r&d projects? *Strategic Management Journal 35*, 1230–1248.

Cunningham, C., F. Ederer, and S. Ma (2021). Killer acquisitions. *Journal of Political Economy 129*(3), 649–702.

Deming, D. J. (2017). The growing importance of social skills in the labor market. *Quarterly Journal of Economics 132*(4), 1593–1640.

Eisfeldt, A. and D. Papanikolaou (2013). Organization capital and the cross-section of expected returns. *Journal of Finance 68*(4), 1365–1406.

FTC (2024). FTC announces rule banning noncompetes. https://www.ftc.gov/news-events/news/press-releases/2024/04/ftc-announces-rule-banning-noncompetes.

Garmaise, M. J. (2011). Ties that truly bind: Noncompetition agreements, executive compensation, and firm investment. *Journal of Law, Economics, and Organization 27*(2), 376–425.

Giannetti, M. and A. Simonov (2009). Social interactions and entrepreneurial activity. *Journal of Eonomics and Management Strategy 18*(3), 135–191.

Gilson, R. (1999). The legal infrastructure of high technology industrial districts: Silicon Valley, Route 128, and covenants not to compete. *New York University Law Review 74*, 575–629.

Gompers, P., J. Lerner, and D. Scharfstein (2005). Entrepreneurial spawning: Public corporations and the genesis of new ventures, 1986 to 1999. *Journal of Finance 60*(2), 577–614.

Guiso, L., L. Pistaferri, and F. Schivardi (2021). Learning entrepreeneurship from other entrepreneurs? *Journal of Labor Economics 39*(1), 135–191.

Harford, J., S. Piotrowski, and Y. Qian (2023). Geographic overlap, agglomeration externalities and post-merger restructuring. *Working Paper*.

Hellmann, T. (2007). When do employees become entrepreneurs? *Management Science 53*(6), 919–933.

Jeffers, J. (2024). The impact of restricting labor mobility on corporate investment and entrepreneurship. *Review of Financial Studies 1*(4), 1–44.

Johnson, M. S., K. Lavetti, and M. Lipsitz (2024). The labor market effects of legal restrictions on worker mobility. *Journal of Political Economy*.

Johnson, M. S., M. Lipsitz, and A. Pei (2023). Innovation and the enforceability of noncompete agreements. *Working Paper*.

Jovanovic, B. (1979). Firm-specific capital and turnover. *Journal of Political Economy 87*(6), 1246–1260.

Kamepalli, S. K., R. G. Rajan, and L. Zingales (2021). Kill zone. *Working Paper*.

Kaplan, S. N. and P. Stromberg (2003). Financial contracting theory meets the real world: An empirical analysis of venture capital contracts. *Review of Economic Studies 70*(2), 281–315.

Kim, D. J. (2022). Startup acquisitions, relocation, and employee entrepreneurship. *Strategic Management Journal 43*(11), 2189–2216.

Kim, D. J. (2023). Startup acquisitions as a hiring strategy: Turnover differences between acquired and regular hires. *Strategy Science 9*(2), 97–228.

Kini, O., R. Williams, and S. Yin (2021). CEO noncompete agreements, job risk and compensation. *Review of Financial Studies 34*(10), 4701–4744.

Kogan, L., D. Papanikolaou, A. Seru, and N. Stoffman (2017). Technological innovation, resource allocation, and growth. *Quarterly Journal of Economics 132*(2), 665–712.

Kolev, J., A. Haughey, F. Murray, and S. Stern (2023). Of academics and creative destruction: Startup advantage in the process of innovation. *Working Paper*.

Lavetti, K., C. Simon, and W. D. White (2020). The impacts of restricting mobility of skilled service workers: Evidence from physicians. *Journal of Human Resources 55*(3), 1025–1067.

Leonard, B. (2001). Recruiting from the competition. *HR Magazine 46*(2), 78–85.

Lipsitz, M. and E. Starr (2021). Low-wage workers and the enforceability. *Management Science 68*(1), 143–170.

Marx, M. (2011). The firm strikes back: Non-compete agreements and the mobility of technical professionals. *American Sociological Review 76*(5), 641–807.

Marx, M., D. Strumsky, and L. Fleming (2009). Mobility, skills, and the michigan non-compete experiment. *Management Science 55*(6), 875–889.

Michaels, D. (2024). Judge tosses FTC ban on noncompete agreements. *Wall Street Journal*.

Moller, S. B., F. P. Schlingemann, and R. M. Stulz (2003). Do shareholders of acquiring firms gain from acquisitions? *NBER Working Paper*.

Nanda, R. and J. B. Sorensen (2010). Workplace peers and entrepreneurship. *Management Science 56*(27), 1116–1126.

Ng, W. and T. E. Stuart (2022). Acquired employees versus hired employees: Retained or turned over? *Strategic Management Journal 43*(5), 1025–1045.

Ouimet, P. and R. Zarutskie (2020). Acquiring labor. *Quarterly Journal of Finance 10*(3), 1–38.

Reinmuth, K. and E. Rockall (2024). Innovation through labor mobility: Evidence from noncompete agreements. *Working Paper*.

Samila, S. and O. Sorenson (2011). Noncompete covenants: Incentives to innovate or impediments to growth. *Management Science 57*(3), 425–438.

Saxenian, A. (1996). Regional advantage: Culture and competition in Silicon Valley and Route 128. *Harvard Press*.

Siegel, D. S. and K. L. Simons (2010). Assessing the effects of mergers and acquisitions on firm performance, plant productivity, and workers: New evidence from matched employer-employee data. *Strategic Management Journal 31*(8), 903–916.

Sorensen, J. B. and M. A. Fassioto (2011). Organizations as fonts of entrepreneurship. *Organization Science 22*(5), 1322–1331.

Starr, E., N. Balasubramanian, and M. Sakakibara (2018). Screening spinouts? How noncompete enforceability affects the creation, growth, and survival of new firms. *Management Science 64*(2), 552–572.

Starr, E. P., J. J. Prescott, and N. D. Bishara (2021). Noncompete agreements in the U.S. labor force. *Journal of Law and Economics 64*(1), 53–84.

Stuart, T. E. and O. Sorenson (2003). Liquidity events and the geographic distribution of entrepreneurial activity. *Administrative Science Quarterly 48*(2), 175–201.

Timoneda, J. C. (2021). Estimating group fixed effects in panel data with a binary dependent variable: How the lpm outperforms logistic regression in rare events data. *Social Science Research 93*.

Younge, K. A. and M. Marx (2016). The value of employee retention: Evidence from a natural experiment. *Management Science 25*(3), 652–677.

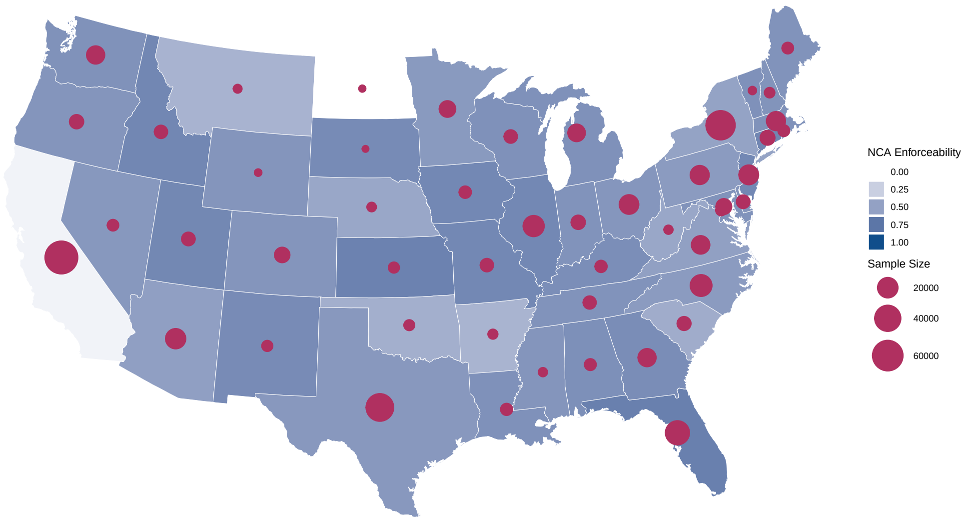

Figure 2: **NCA Enforceability and Sample Size Across U.S. States.** This figure plots the enforceability of non-compete agreements (NCAs) across U.S. states as of 2014, along with the sample sizes of target firm employees in each state.

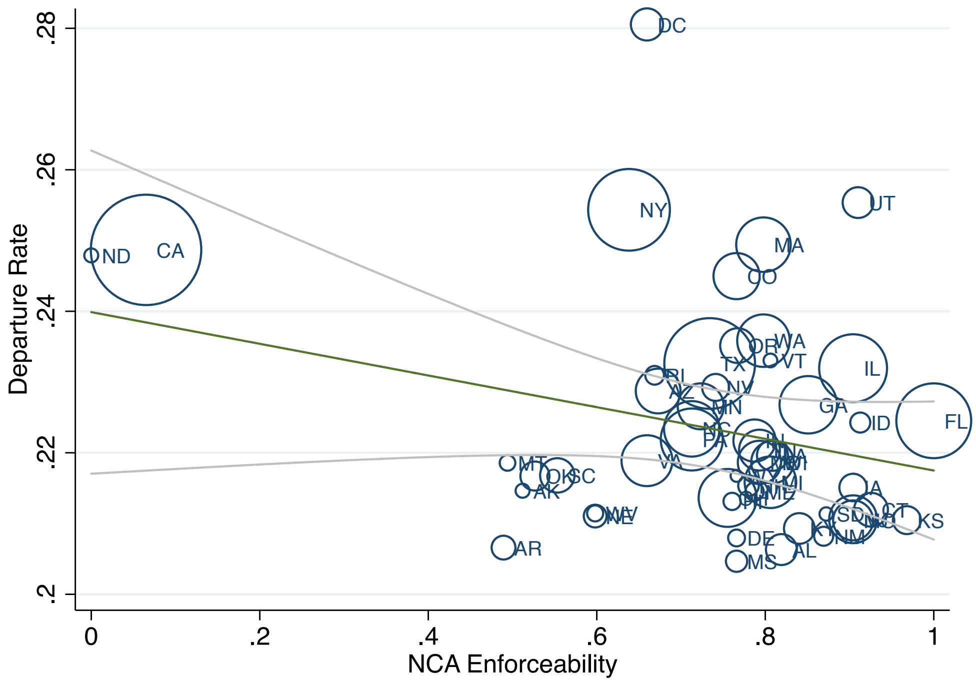

Figure 3: **NCA Enforceability and Departure Rate Across U.S. States.** This figure presents a scatter plot of non-compete agreement (NCA) enforceability and the average departure rates across U.S. states in 2014. I also plot the fitted regression line and 99% confidence intervals. Bubble sizes represent the number of LinkedIn users in each state.

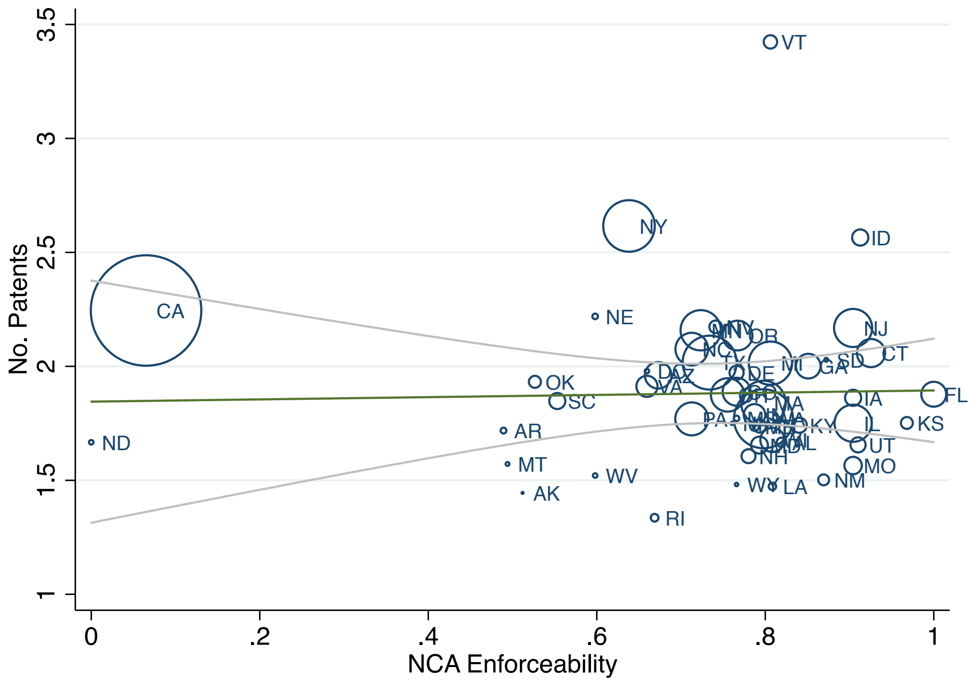

Figure 4: **NCA Enforceability and Productivity Across U.S. States.** This figure presents a scatter plot of non-compete agreement (NCA) enforceability and the average number of patents per inventor across U.S. states in 2014. I also plot the fitted regression line and 99% confidence intervals. Bubble sizes represent the number of inventors in each state.

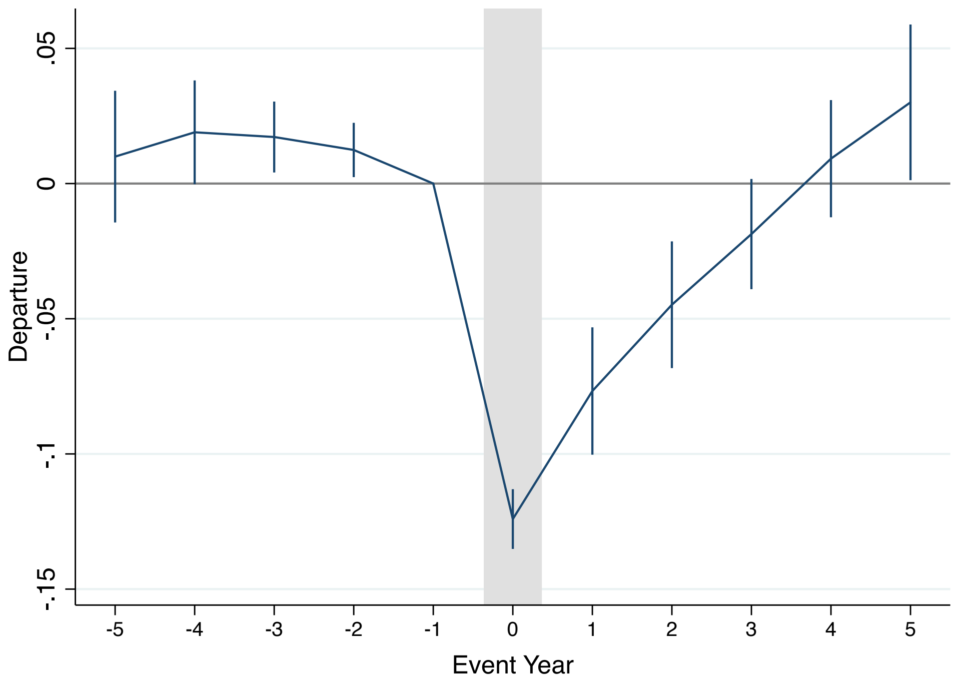

Figure 5: **Event Study Plot: Departure and Non-compete Agreements.** The figure plots the coefficients, β_k, as long as their 95% confidence intervals, from the following regression:

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \sum_{k=-5}^{5} \beta_k \cdot (\text{NCA Enforceability}_{st} \times \tau_k) + \varepsilon_{ijst},$$

where Departure_{ijst} is 1 if an individual i who is from target firm j joins a new firm in year t and 0 otherwise. NCA $\text{Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement. Confidence intervals are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

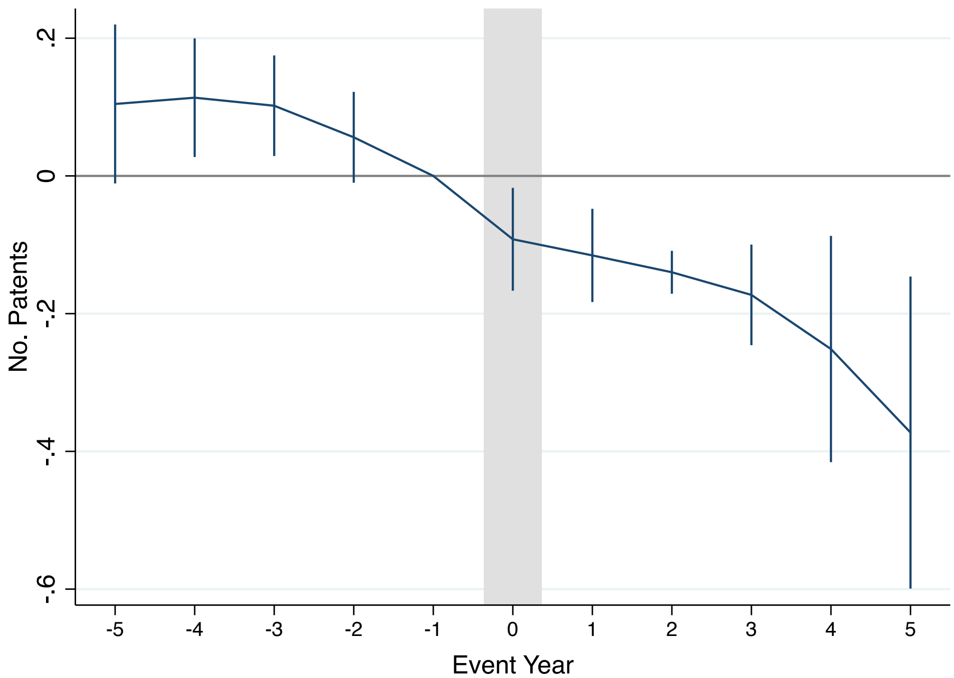

Figure 6: **Event Study Plot: Productivity and Non-compete Agreements.** The figure plots the coefficients, β_k, as long as their 95% confidence intervals, from the following regression:

$$\text{Productivity}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \sum_{k=-5}^{5} \beta_k \cdot (\text{NCA Enforceability}_{st} \times \tau_k) + \varepsilon_{ijst},$$

where $\text{Productivity}_{ijst}$ is the number of patents granted to i in year t and NCA $\text{Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement. Confidence intervals are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

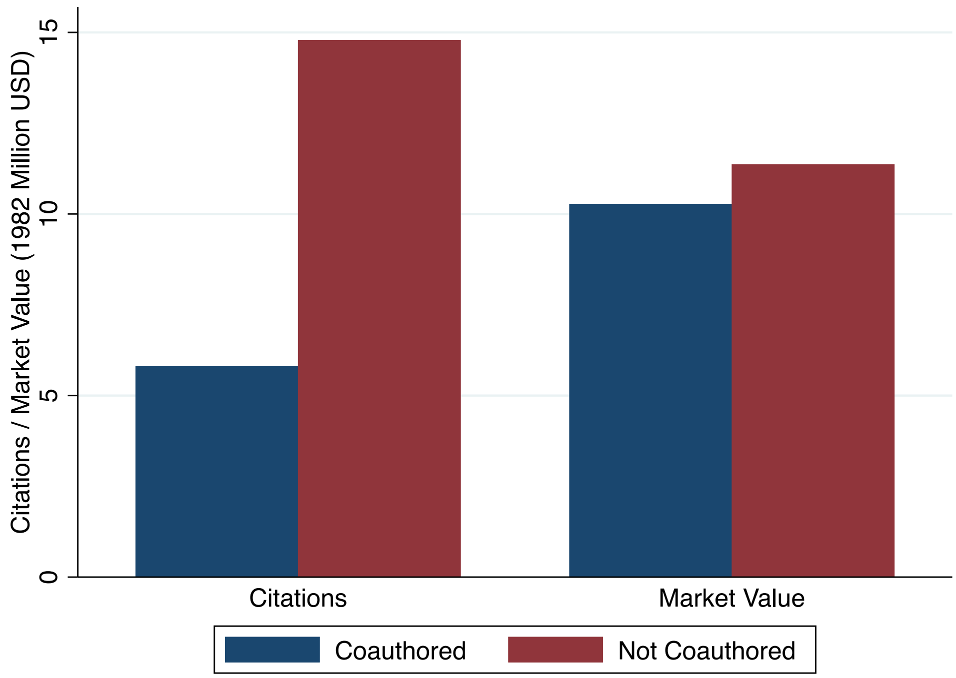

Figure 7: **Weak Synergies: The Value of Patents Coauthored with the Acquirer.** The figure compares the value of patents coauthored with the acquirer (*Coauthored*) to those written solely by employees from the target firms (*Not Coauthored*) after the merger. Estimates are based on individuals from target firms who were granted patents one year prior to the announcement, where I track their patent submissions up to 10 years following the completion of the merger.

Table 1: **Summary Statistics**

This table presents the number of target firm employees in my sample along with their key characteristics.

	LinkedIn	LinkedIn + Patent
Number of Individuals	513,847	7,652
Number of Mergers	301	105
Average Age	35	35
Bachelor (Master) or Higher	98% (40%)	99.5% (63%)
Male	58%	87%

Table 2: **Departure and Non-Compete Agreements Around Mergers**

This table estimates the following difference-in-difference regression,

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst},$$

where Departure_{ijst} is 1 if an individual i who is from target firm j joins a new firm in year t and 0 otherwise. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	
	(1)	(2)
NCA Enforceability × Post	–0.111***	–0.110***
	(–11.304)	(–11.258)
Observations	3,555,203	3,068,041
R^2	0.252	0.253
Individual FE	Yes	Yes
Target Firm FE	Yes	Yes
Year FE	Yes	Yes
MSA FE	No	Yes
Job Title FE	No	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: **Productivity and Non-Compete Agreements: Diff-in-Diff**

This table estimates the following difference-in-difference regression,

$$\text{Productivity}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst},$$

where $\text{Productivity}_{ijst}$ is an individual i's productivity measured by the number or value of patents granted. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	No. Patents		Citations		Market Value	
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability × Post	–0.130***	–0.127***	–2.267***	–2.225***	–4.170***	–4.079***
	(–5.852)	(–6.355)	(–4.058)	(–4.231)	(–3.986)	(–4.494)
Observations	66,116	66,116	66,116	66,116	66,116	66,116
R^2	0.583	0.584	0.413	0.413	0.275	0.276
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	No	Yes	No	Yes	No	Yes
Job Title FE	No	Yes	No	Yes	No	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: **Productive Employees Becoming Less Productive**

This table studies the heterogeneous impact of non-compete agreements on productivity. I estimate the following triple difference regression,

$$\begin{aligned}\text{Productivity}_{ijst} &= \alpha_i + \alpha_j + \alpha_t + \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) \\ &\quad + \beta_2 \cdot (\text{NCA Enforceability}_{st} \times \text{Pre-Merger Productivity}_i \times \text{Post}_t) + \gamma \cdot X_{ijt} + \varepsilon_{ijst},\end{aligned}$$

where $\text{Productivity}_{ijst}$ is an individual i's productivity measured by the number or value of patents granted. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. $\text{Pre-Merger Productivity}_i$ is an average productivity of individual i before the merger announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	No. Patents	Citations	Market Value
	(1)	(2)	(3)
NCA Enforceability × Post	–0.128***	–2.781	–2.083
	(–6.513)	(–1.647)	(–1.559)
NCA × Pre-Merger Productivity × Post	0.037	–0.253***	–0.695***
	(0.736)	(–3.450)	(–2.959)
Pre-Merger Productivity × Post	–0.127***	0.068	0.041
	(–3.663)	(0.947)	(0.236)
Observations	66,077	66,077	66,077
Individual FE	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Job Title	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: **Impact of NCAs During Pre-Merger Period**

This table estimates the following regression before the merger announcement,

$$\text{Productivity}_{ijst} = \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{ijst}$$

where $\text{Productivity}_{ijst}$ is an individual i's productivity measured by the number or value of patents granted. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement. Estimates are based on 5-year before the merger announcement ($-5 \sim --1$). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	No. Patents	Citations	Market Value
	(1)	(2)	(3)
NCA Enforceability	–0.341	–19.292	–2.219
	(–1.323)	(–1.125)	(–0.322)
Observations	29,537	29,537	29,537
Target Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Job Title FE	Yes	Yes	Yes

t statistics in parentheses

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

Table 6: **Mechanisms**

This table studies how collaborators' departure affects productivity. I estimate the following difference-in-difference regression

$$y_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta_1 \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \beta_2 \cdot (\text{NCA Enforceability}_{st} \times X_{ijst} \times \text{Post}_t) + \varepsilon_{ijst},$$

where I use two dependent variables, a dummy variable for departure (*Departure*) and number of patents granted each year (*No. Patents*). NCA Enforceability$_{st}$ is the degree of non-compete agreements' enforceability one year prior to the merger announcement and Post$_t$ is equal to 1 from the announcement. firm-specific Training is the length of a training period through organized classroom study provided by an employer. It varies by occupation code and is sourced from O*NET. Coauthors' NCA Enforceability is the average of coauthors' NCA Enforceability one year prior to the merger announcement. Organization capital is defined as the ratio of organization capital to book asset one year prior to the announcement following Eisfeldt and Papanikolaou (2013) and Δ Org. Capital is the target firm's organization capital-to-asset ratio less that of the acquirer's. I measure the level of human capital using occupation-level social skills (Deming (2017)) and the extent to which their tasks are routine (Acemoglu and Autor (2011)). Estimates are based on people who have patents with more than one author and 11-year window around the merger announcement (–5 ∼ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year. Fixed effects include individual, target firm, year, MSA, and job title.

Panel A. Firm-Specific Human Capital

	Departure	No. Patents	Departure	No. Patents
	(1)	(2)	(3)	(4)
NCA Enforceability × Post	–0.157***	–0.459***	–0.109***	–0.256***
	(–5.672)	(–4.744)	(–9.648)	(–9.784)
NCA × Firm Specific Training × Post	0.046***	0.060***		
	(4.052)	(3.232)		
Firm Specific Training × Post	–0.025***	0.031***		
	(–4.543)	(6.529)		
NCA × Firm Specific Training	0.013**	–0.093***		
	(2.758)	(–4.028)		
NCA × Coauthors' NCA × Post			0.114***	0.556***
			(13.032)	(4.603)
Coauthors' NCA × Post			0.059***	0.179**
			(18.554)	(2.317)
Observations	56,837	56,837	66,116	66,116
R^2	0.240	0.593	0.236	0.586
Fixed Effects	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Continued on the next page.

Panel B. Organization Capital

	Departure	No. Patents
	(1)	(2)
NCA Enforceability × Post	–0.091***	–0.228***
	(–4.370)	(–3.374)
NCA × Target's Org. Capital × Post	0.016	0.172***
	(0.458)	(3.213)
Target's Org. Capital × Post	–0.005	0.047*
	(–0.411)	(1.768)
Observations	65,657	65,657
R^2	0.230	0.585
Fixed Effects	Yes	Yes

Panel C. Level of Human Capital

	Departure	No. Patents	Departure	No. Patents
	(1)	(2)	(3)	(4)
NCA Enforceability × Post	–0.077***	–0.112***	–0.071***	–0.155***
	(–8.751)	(–9.238)	(–6.562)	(–13.996)
NCA × Social × Post	–0.007	–0.068**		
	(–0.765)	(–2.207)		
Social × Post	0.021***	0.037		
	(5.070)	(1.601)		
NCA × Routine × Post			–0.003	0.016**
			(–0.539)	(2.470)
Routine × Post			–0.001	–0.024***
			(–0.243)	(–2.984)
Observations	59,341	59,341	41,176	41,176
R^2	0.236	0.592	0.245	0.634
Fixed Effects	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: **Placebo Test: Failed Mergers**

This table re-estimates the following difference-in-difference regression as in Table 3 using mergers that are not completed,

$$y_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst},$$

where the dependent variable is either a dummy variable for departure or productivity, measured by the number of patents granted. NCA Enforceability$_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post$_t$ is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	No. Patents	Citations	Market Value
	(1)	(2)	(3)	(4)
NC Enforceability × Post	-0.016	0.094*	2.495**	1.479
	(-0.726)	(1.997)	(2.778)	(0.859)
Observations	68,732	68,732	68,732	68,732
R^2	0.227	0.478	0.347	0.348
Individual FE	Yes	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Job Title FE	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: **Productivity Decline After Mergers**

This table estimates the following triple difference regression using data from both completed and incomplete mergers,

$$\begin{aligned}\text{Productivity}_{ijkst} &= \alpha_i + \alpha_k + \alpha_t + \beta_1 \cdot \left(\text{Complete}_j \times \text{Post}_t\right) \\ &\quad + \beta_2 \cdot \left(\text{Complete}_j \times \text{NCA Enforceability}_{ijkst} \times \text{Post}_t\right) + \gamma \cdot X_{ikjt} + \varepsilon_{ijkst},\end{aligned}$$

where $\text{Productivity}_{ijkst}$ is an individual i's productivity measured by the number or value of patents granted. Complete_j is a dummy variable equal to 1 if the target firm j is successfully acquired by the acquirer k. $\text{NCA Enforceability}_{ijkst}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	No. Patents		Citations		Market Value	
	(1)	(2)	(3)	(4)	(5)	(6)
Complete × Post	–0.079*	0.082**	–2.136	1.401	0.716	1.969
	(–1.778)	(2.239)	(–1.003)	(1.034)	(0.200)	(0.621)
Complete × NCA × Post		–0.396***		–9.170**		–1.978
		(–3.440)		(–2.602)		(–1.302)
NCA × Post		0.211***		5.412**		–0.202
		(3.361)		(2.143)		(–0.110)
Complete × NCA		0.785***		–0.124		–15.092**
		(3.520)		(–0.015)		(–2.182)
Observations	126,993	125,403	126,993	125,403	126,993	125,403
R^2	0.552	0.553	0.406	0.407	0.303	0.304
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Acquirer FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes	Yes	Yes
Job Title FE	Yes	Yes	Yes	Yes	Yes	Yes

t statistics in parentheses

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

Table 9: **Patenting Ideas and Non-Compete Agreements**

This table examines whether firms' patenting policies depend on local NCA enforceability. I estimate the following regression,

$$\text{No. Patents Coauthored within One State}_{jst} = \alpha_{j(s)t} + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{jst}$$

where No. Patents Coauthored within One State$_{jst}$ is the total number of patents with more than one coauthor, all of whom are from the same state s in year t. NCA Enforceability$_{st}$ is the degree of non-compete agreement's enforceability in state s and year t. Estimates are based on target firms from my main sample. t-statistics shown in the parentheses are based on double clustered standard errors by the target firm and year.

	No. Patents	
	(1)	(2)
NCA Enforceability	–1.009	0.772
	(–0.343)	(0.080)
Observations	3,299	3,299
R^2	0.000	0.420
Firm × Year FE	No	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: **Why Locking-in Mismatched Employees**

This table studies why acquiring firms lock-in mismatched employees in states with high NCA enforceability. *Product Market Concentration* is Herfindahl-Hirschman Index (HHI) of the target firm one year before the announcement. *Geographic Overlap* is a dummy variable equals to 1 if an acquiring firm has the branch in a given state. *Employer Concentration* is Herfindahl-Hirschman Index (HHI) of a given occupation in the labor market. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure					
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability × Post	-0.113***	-0.112***	-0.154***	-0.154***	-0.160***	-0.159***
	(-12.060)	(-12.035)	(-24.025)	(-24.061)	(-12.663)	(-12.968)
NCA × Product Market Concentration × Post	3.037**	2.955***				
	(2.799)	(2.933)				
Product Market Concentration × Post	-2.948***	-2.893***				
	(-3.932)	(-4.192)				
NCA × Geographic Overlap × Post			0.026**	0.027**		
			(2.461)	(2.273)		
Geographic Overlap × Post			0.005	0.006		
			(0.874)	(0.923)		
NCA × Employer Concentration × Post					0.071***	0.072***
					(3.975)	(3.794)
Employer Concentration × Post					0.032	0.030
					(1.242)	(1.222)
NCA × Employer Concentration					-0.059	-0.064
					(-1.634)	(-1.573)
Observations	3,477,909	3,006,520	2,363,283	2,363,283	2,730,300	2,355,605
R^2	0.249	0.251	0.307	0.308	0.308	0.309
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
MSA FE	No	Yes	No	Yes	No	Yes
Job Title FE	No	Yes	No	Yes	No	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: **Entrepreneurial Departures**

This table studies the impact of non-compete agreements on the entrepreneurship following mergers. I estimate the following difference-in-difference regression,

$$\text{Entrepreneurial Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability}_{st} + \varepsilon_{ijst},$$

where Entrepreneurial Departure$_{ijst}$ is 1 if an individual i from target firm j joins a new firm as a founding team member in year t and 0 otherwise. NCA Enforceability$_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post$_t$ is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on double clustered standard errors by the target firm and year.

	Entrepreneurial Departure (%)	
	(1)	(2)
NCA Enforceability $\times$ Post	–0.897**	–0.893**
	(–2.757)	(–2.752)
Observations	3,484,590	3,484,496
R^2	0.185	0.186
User FE	Yes	Yes
Target Firm FE	Yes	Yes
Year FE	Yes	Yes
MSA FE	No	Yes
Job Title FE	No	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: **Weak Synergies Between Target and Acquirer**

This table studies whether employees from the target firm coauthor patents with employees from the acquirers after the merger. Based on individuals from target firms who were granted patents one year prior to the announcement, I track their patent submissions up to 10 years following the completion of the merger. Then, I compute the ratio of coauthored patents to the total patents written by these employees, as well as the ratio of individuals who coauthor patents with employees from the acquirer.

	Coauthored	Total	Ratio
No. Patents	405	6358	0.060
No. Authors	26	296	0.088

Table 13: **Post-Merger Outcomes**

This table regresses the post-merger outcomes of the acquirers on the value-weighted average of the target firm's NCA enforceability. To compute this measure, I first count the number of employees at each target firm one year prior to the merger, categorized by state. I then calculate the value-weighted average of the NCA enforceability scores using the number of employees in each state as weights. In cases where an acquirer targets multiple firms in a given year, I compute the value-weighted average of the NCA enforceability scores for each target firm using the number of employees at each firm as weights. Panel A presents regressions of accounting variables, including 5-year sales growth and 5-year changes in profitability, measured by return on assets (ROA) and operating profitability (OP), as well as the R&D expense to sales ratio. Panel B focuses on innovation output measures, specifically 5-year changes in the number of patents, citations, and the market value of patents. For all dependent variables, I adjust for industry effects by subtracting the median values within the industry, defined by the 4-digit NAICS code. t-statistics shown in the parentheses are based on double clustered standard errors by the industry (NAICS 4-digit) and year.

Panel A. Accounting Variables

	Sales Growth	Δ ROA	Δ OP	Δ R&D/Sale	Δ Capex/Sale
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability	1.624**	0.169**	0.123***	-0.298	-0.064
	(2.140)	(2.514)	(2.873)	(-1.124)	(-1.171)
Observations	125	125	125	125	125
R^2	0.028	0.041	0.062	0.004	0.007

Panel B. Innovation Output

	Δ No. Patents	Δ Citations	Δ Market Value
	(1)	(2)	(3)
NCA Enforceability	12.874	–1663.817	1766.717
	(0.280)	(–1.217)	(0.274)
Observations	125	125	125
R^2	0.000	0.004	0.000

Appendix

For

Acquiring Human Capital: Do Non-Competes Help?

A.1 Non-Compete Agreements and Labor Market

A potential critique of the findings on non-compete agreement (NCA) enforceability could be that the observed effects are confounded by a correlation between NCA enforceability and other labor market-related variables. This section demonstrates that NCA enforceability is not correlated with a set of labor market variables other than NCAs. I examine two sets of variables: (1) wrongful discharge laws and (2) variables from the Job Openings and Labor Turnover Survey. Wrongful discharge laws protect employees from being terminated without just cause, and their enforcement varies significantly across states. If states with stricter NCA enforceability also have strong wrongful discharge protections, then the observed effects attributed to NCAs might be confounded with the effects of these discharge laws. This could mean that the reduced employee mobility and productivity declines observed in the study could partly be due to the presence of robust wrongful discharge laws rather than the NCAs alone.

For instance, if states with high NCA enforceability also make it difficult to terminate employees, the acquirer might retain workers who are less productive or less suitable for their roles, not solely due to NCAs, but because terminating these employees could lead to costly legal disputes under wrongful discharge laws. As a result, the effects attributed to NCAs could be overstated if the analysis does not adequately control for the influence of wrongful discharge laws. To address this critique, future research should carefully disentangle the effects of NCAs from other employment protections, such as wrongful discharge laws, to ensure that the findings accurately reflect the specific impact of NCAs on employee mobility and productivity.

Thus, I test whether NCA enforceability is correlated with wrongful discharge laws using data on three types of wrongful discharge laws from Acharya, Baghai, and Subramanian (2014). Acharya, Baghai, and Subramanian (2014) construct a panel dataset of wrongful discharge laws by compiling data on the adoption of three distinct exceptions to the employment-at-will doctrine across U.S. states from 1970 to 1999. The three types of exceptions are: Good Faith, Implied Contract, and Public Policy. The Good Faith exception provides the strongest protection for employees, preventing employers from terminating employees in bad faith, such as to avoid paying benefits. The Implied Contract exception limits employers' ability to terminate employees without cause if an implied contract, often derived from an employer?s actions or statements, exists. The Public Policy exception protects employees from being terminated for reasons that would violate public policy, such as refusing to engage in illegal activities. The dataset was created by systematically coding each state's adoption of these laws annually, using legal sources such as state court decisions and statutes.

Using this state-level panel data, I estimate the following,

$$\text{NCA Enforceability}_{st} = \alpha + \beta \cdot \text{Wrongful Discharge Laws}_{st} + \varepsilon_{it}, \tag{A.1}$$

where Wrongful Discharge Laws_{st} is a set of three different types of wrongful discharge laws, *Good Faith*, *Implied Contract*, and *Public Policy*. *Good faith*, a dummy that takes a value of one if a state has adopted a good-faith exception to the employment-at-will doctrine in a given year and is zero otherwise. *Implied Contract* and *Public policy* are defined analogously. Table A.10 Column (1) - (3) shows that there is no significant correlation between NCA enforceability and any of the three types of wrongful discharge laws. This implies that the enforceability of non-compete agreements operates independently of the presence of wrongful discharge protections, thereby reinforcing the main causal conclusions presented in my results.

Next, I regress NCA enforceability on three variables from the Job Openings and Labor Turnover Survey: (1) the rate of hires, defined as all additions to the payroll during the month; (2) the rate of job openings, defined as all positions open (not filled) on the last business day of the month, provided a specific position exists, work is available, and the job could start within 30 days; and (3) the rate of layoffs, defined as involuntary separations initiated by the employer. The rate is defined as the number of occurrences (e.g., hires, job openings, or layoffs) divided by total employment. Table A.10 Column (4) suggests NCA enforceability is not significantly correlated with any of these variables. This result implies the main effects are entirely driven by NCA enforceability and not by other labor market variables.

A.2 Where Do Employees Go Once They Leave?

This section describes where employees move after leaving following the M&A announcements. Table A.11 the changes in firm characteristics between the acquiring firm and the firm employees switched to after the merger announcements. The table suggests that target firm employees tend to move to smaller, younger, and growth-oriented companies. This aligns with the idea that individuals whose match quality has diminished prefer to work in smaller, more agile environments. Table A.12 further examines whether this departure behavior is associated with NCA enforceability. The table shows there is no strong relation between these two.

As described in Section 1, specify two dimensions, one of which is geographic limitations. NCAs often restrict individuals from moving within the same state. Therefore, I test whether, conditional on people's first departures, they are more likely to switch to different industries or states if they are subject to NCAs. Table A.13 shows that this is indeed the case: people are more likely to switch to a different state if they are in states where NCAs are more strictly enforced. This result might introduce an upward bias in my main estimates from Table 2 if individuals were frequently willing to move to different states to avoid the restrictions imposed by NCAs. However, I find that it is very rare for people to move to different states. Across the entire sample of LinkedIn users, only 2% of the observations involve people switching to different states. Thus, it is very unlikely that my main estimates contain an upward bias that would significantly affect my conclusions.

A.3 Supplementary Figures and Tables

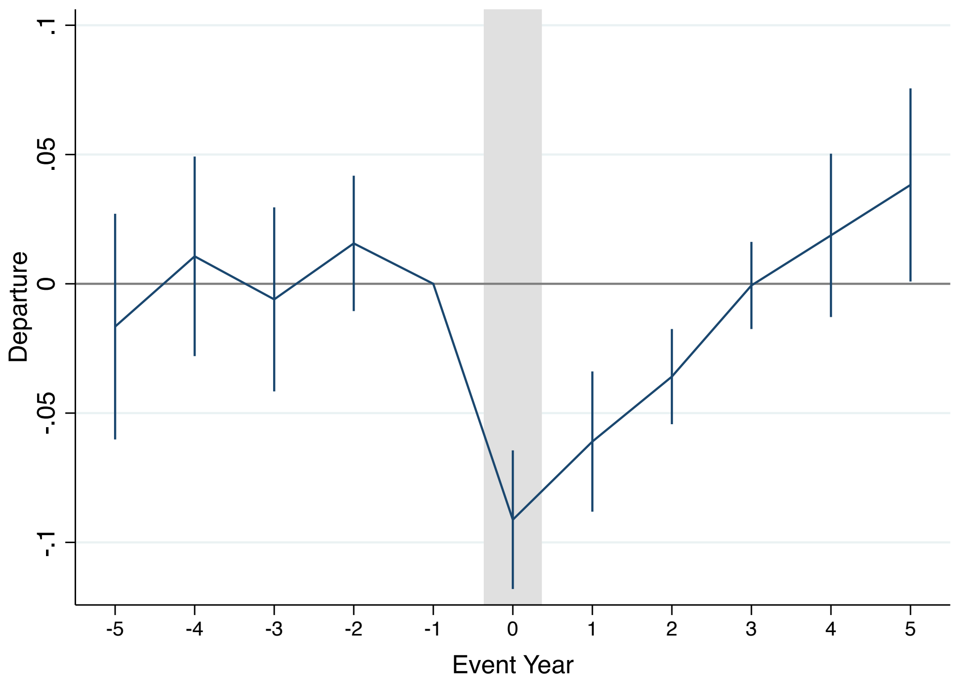

Figure A.1: **Event Study Plot: Departure and Non-compete Agreements (Inventors).** The figure repeats the same analysis in Figure 5 using the inventor sample. I plot the coefficients, β_k, as long as their 95% confidence intervals, from the following regression:

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \sum_{k=-5}^{5} \beta_k \cdot (\text{NCA Enforceability}_{st} \times \tau_k) + \varepsilon_{ijst},$$

where Departure_{ijst} is 1 if an individual i from target firm j joins a new firm in year t and 0 otherwise. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement. Confidence intervals are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

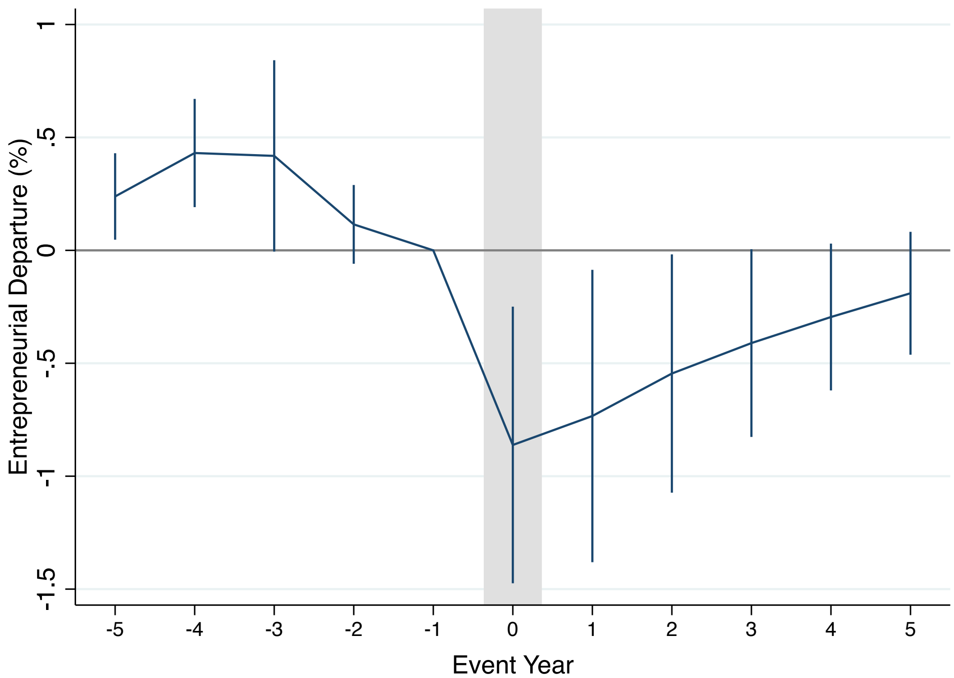

Figure A.2: **Event Study Plot: Entrepreneurship and Non-compete Agreements.** The figure plots the coefficients, β_k, as long as their 95% confidence intervals, from the following regression:

$$\text{Entrepreneurial Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \sum_{k=-5}^{5} \beta_k \cdot (\text{NCA Enforceability}_{st} \times \tau_k) + \varepsilon_{ijst},$$

where Entrepreneurial Departure_{ijst} is 1 if an individual i who is from target firm j joins a new firm as a founding member in year t and 0 otherwise. NCA $\text{Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement. Confidence intervals are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

Table A.1: Enforceability of Non-Compete Agreements (Bishara (2011))

This table is Table C.1 from Johnson, Lavetti, and Lipsitz (2024), which tabulates seven quantifiable dimensions of non-compete agreements' enforceability and their respective weights used to compute the composite score (*NCA Enforceability*), first introduced in Bishara (2011).

Criteria #	**Question**	**Criteria**	**Weight**
Q1	Is there a state statute that governs the enforceability of covenants not to compete?	10 = Yes, favors strong enforcement 5 = No, either case neutral on enforceability 0 = No	10
Q2	What is an employer's protectable interest and how is that defined?	10 = Broadly defined protectable interest 5 = Balanced approach to protectable interest 0 = Strictly defined, limiting the protectable interest	10
Q3	What must the plaintiff be able to show to prove enforceability of an enforced covenant not to compete?	10 = Weak burden of proof on plaintiff (employee) 5 = Balanced burden of proof on plaintiff 0 = Strong burden of proof on plaintiff	5
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship constitute sufficient consideration to support the covenant?	10 = Yes, signing at inception always sufficient 5 = Sometimes sufficient 0 = Never sufficient consideration to support CNC	5
Q3b/c	Will a change in the terms and conditions of employment require the covenant to compete to be supported by additional consideration?	10 = Continued employment always sufficient 5 = Minor change in terms sufficient to support CNC 0 = Neither continued employment nor minor change in terms sufficient to support CNC	5
Q4	If the restrictions in the covenant not to compete become overbroad or otherwise unreasonable, will the courts 'blue pencil' the covenant to make the covenant enforceable?	10 = Judicial modification allowed, broad interpretation of terms allowed 5 = Judicial modification allowed, balanced enforceability 0 = Blue pencil or modification not allowed	10
Q8	If the employer terminates the employment relationship, is the covenant enforceable?	10 = Enforceable if employer terminates relationship 5 = Enforceable only if terminated for cause 0 = Not enforceable if employer terminates	10

Table A.2: Inventors' Job Title

This table presents the list of job titles (2-digit O*NET occupation codes) for the inventors in my sample. *No. Observations* refers to the number of inventor-year observations.

2-digit O*NET Code	Job Ttitle	No. Observations
11	Management	8,275
13	Business and Financial Operations	6,447
15	Computer and Mathematical	14,050
17	Architecture and Engineering	36,150
19	Life, Physical, and Social Science	5,697
21	Community and Social Service	46
23	Legal	257
25	Educational Instruction and Library	563
27	Arts, Design, Entertainment, Sports, and Media	2,203
29	Healthcare Practioners and Technical	283
31	Healthcare Support	14
33	Protective Service	52
35	Food Preparation and Serving Related	13
37	Building and Groups Cleaning and Maintenance	30
39	Personal Care and Service	394
41	Sales and Related	1,171
43	Office and Administrative Support	676
45	Farming, Fishing, and Forestry	155
47	Construction and Extraction	919
49	Installation, Maintenance, and Repair	554
51	Production	968
53	Transportation and Material Moving	150

Table A.3: **Departure and Non-Compete Agreements: Additional Fixed Effects**

This table repeats the same anlysis in Table 2 with different set of fixed effects: Target Firm × MSA and Target Firm × Job Title. I estimate the following difference-in-difference regression,

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst},$$

where Departure_{ijst} is 1 if an individual i from target firm j joins a new firm in year t and 0 otherwise. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure		
	(1)	(2)	(3)
NC Enforceability × Post	–0.109***	–0.111***	–0.110***
	(–11.192)	(–11.229)	(–11.178)
Observations	3,069,521	3,553,003	3,068,041
R^2	0.255	0.254	0.257
User FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Target Firm × MSA FE	Yes	No	Yes
Target Firm × Job Title FE	No	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: **Departure and Non-Compete Agreements: Logit Regression**

This table repeats the analysis in Table 2 but using a binary logit model for the departure. *All* refers to the sample of individuals from LinkedIn not restricted to inventors and *Inventors* refers to the sample of inventors. z-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	
	(1)	(2)
NCA Enforceability × Post	–1.260***	–1.230***
	(–8.620)	(–4.619)
Observations	1,797,126	36,888
R^2	0.252	0.253
Sample	All	Inventors
Individual FE	Yes	Yes
Target Firm FE	Yes	Yes
Year FE	Yes	Yes
MSA FE	Yes	Yes
Job Title FE	Yes	Yes

z statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: **Acquisition Premium**

This table regresses acquisition premium on the employee-weighted average of the target firm's NCA enforceability. To compute this measure, I take the average of NCA Enforceabilty based on employees one year prior to the announcement and estimate the following,

$$\text{Acquisition Premium}_{jt} = \alpha_j + \alpha_t + \beta \cdot \left(\frac{\sum_i \text{NCA Enforceability}_{ijt}}{N_{jt}} \right) + \varepsilon_{jt},$$

where Acquisition Premium is defined as the difference between the transaction value and the market capitalization of the target firm at the month-end prior to the announcement. N_{jt} is the number of employees from target firms one year prior to the announcement. The right-hand-side variable is standardized to have mean 0 and standard deviation of 1. t-statistics shown in the parentheses are based on double clustered standard errors by the industry (NAICS 2-digit) and year.

	Acquisition Premium	
	Total	Per Employee
	(1)	(2)
NCA Enforceability	549.835***	28.947***
	(3.043)	(2.980)
Observations	144	144
R^2	0.048	0.076
Industry FE	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: **Departure and Non-Compete Agreements: Inventors**

This table repeats the same analysis in Table 2 using the inventor sample. I estimate the following difference-in-difference regression,

$$\text{Departure}_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst},$$

where Departure_{ijst} is 1 if an individual i from target firm j joins a new firm in year t and 0 otherwise. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	
	(1)	(2)
NC Enforceability × Post	-0.080***	-0.081***
	(-6.594)	(-6.631)
Observations	66,098	66,098
R^2	0.225	0.233
User FE	Yes	Yes
Target Firm FE	Yes	Yes
Year FE	Yes	Yes
MSA FE	No	Yes
Job Title FE	No	Yes

t statistics in parentheses

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

Table A.7: **Productivity and Non-Compete Agreements: Additional Fixed Effects**

This table repeats the same anlysis in Table 2 and Table 3 using the inventor sample with different set of effects: Target Firm × MSA and Target Firm × Job Title. I estimate the following difference-in-difference regression,

$$y_{ijst} = \alpha_i + \alpha_j + \alpha_t + \beta \cdot (\text{NCA Enforceability}_{st} \times \text{Post}_t) + \varepsilon_{ijst}.$$

I use two types of dependent variables, departure and productivity: Departure_{ijst} is 1 if an individual i from target firm j joins a new firm in year t and 0 otherwise and $\text{Productivity}_{ijst}$ is an individual i's productivity measured by the number or value of patents granted. $\text{NCA Enforceability}_{st}$ is the degree of non-compete agreement's enforceability one year prior to the merger announcement and Post_t is equal to 1 from the announcement. Estimates are based on 11-year window around the merger announcement (–5 $\sim$ 5). t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	Productivity		
		No. Patents	Citations	Market Value
	(1)	(2)	(3)	(4)
NC Enforceability × Post	–0.076***	–0.125***	–2.180***	–4.247***
	(–6.071)	(–7.317)	(–4.267)	(–4.312)
Observations	66,116	66,116	66,116	66,116
R^2	0.253	0.587	0.417	0.280
Individual FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Target Firm × MSA FE	Yes	Yes	Yes	Yes
Target Firm × Job Title FE	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.8: **Productivity and Non-Compete Agreements: Active Inventors**

This table repeats the same analysis in Table 3 using the sample of active inventors. Active inventors are defined as the ones who were granted patents one-year prior to the merger announcement. t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	No. Patents	Citations	Market Value
	(1)	(2)	(3)
NC Enforceability $\times$ Post	-0.592***	-9.434**	-12.606***
	(-4.080)	(-2.803)	(-4.929)
Observations	6,986	6,986	6,986
Individual FE	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes
Job Title	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.9: **Mechanism: Firm-Specific Human Capital with Controls**

This table repeats the analysis in Table 6 Panel A with additional controls from O*NET Specific Vocational Preparation (SVP). *Required Level of Education* is the level of vocational education required and *On-the-job Training* is the training period under the instruction of a qualified worker. t-statistics shown in the parentheses are based on triple clustered standard errors by the target firm, industry (2-digit NAICS code) and year.

	Departure	Productivity		
		No. Patents	Citations	Market Value
	(1)	(2)	(3)	(4)
NC Enforceability × Post	-0.157***	-0.459***	-9.671***	-11.364***
	(-5.646)	(-4.548)	(-4.360)	(-7.809)
NC × Firm Specific Training × Post	0.046***	0.061**	1.868*	2.516***
	(4.131)	(2.855)	(2.080)	(3.118)
Firm SpecificTraining × Post	-0.026***	0.030***	0.101	-0.544
	(-4.691)	(8.319)	(0.415)	(-0.831)
NC ×Firm Specific Training	-0.008	-0.136***	-0.454	-2.851
	(-1.328)	(-3.176)	(-0.433)	(-0.807)
Required Level of Education	0.004	0.013	0.877***	1.001
	(1.132)	(0.579)	(3.057)	(0.983)
On-the-job Training	0.032***	0.063	-1.918	0.051
	(4.707)	(1.513)	(-1.258)	(0.025)
Observations	56,837	56,837	56,837	56,837
R^2	0.240	0.593	0.436	0.287
Individual FE	Yes	Yes	Yes	Yes
Target Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
MSA FE	Yes	Yes	Yes	Yes
Job Title FE	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.10: **Non-Compete Agreements and Labor Market**

This table estimates the following regression,

$$\text{NCA Enforceability}_{st} = \alpha + \beta \cdot X_{st} + \varepsilon_{it},$$

where X is a set of other labor market related variables that vary at the state-level. I use three different types of wrongful discharge laws, *Good Faith*, *Implied Contract*, and *Public Policy*. *Good faith*, a dummy that takes a value of one if a state has adopted a good-faith exception to the employment-at-will doctrine in a given year and is zero otherwise. *Implied Contract* and *Public policy* are defined analogously, and these data are from Acharya, Baghai, and Subramanian (2014). *Hires*, *Openings*, and *Layoffs* are the rate of hires, job openings, and involuntary layoffs, respectively from Job Openings and Labor Turnover Survey. The rate is defined as the number of occurrences (e.g., hires, job openings, or layoffs) divided by total employment. t-statistics shown in the parentheses are based on double clustered standard errors by state and year.

	NCA Enforceability			
	(1)	(2)	(3)	(4)
Good Faith	-0.063			
	(-0.993)			
Implied Contract		-0.015		
		(-0.300)		
Public Policy			-0.027	
			(-0.535)	
Hires				0.011
				(0.426)
Openings				-0.065
				(-1.743)
Layoffs				-0.042
				(-1.453)
Observations	650	650	650	714
R^2	0.027	0.004	0.006	0.045
Year FE	Yes	Yes	Yes	Yes

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.11: **Where Do They Go?**

This table presents the changes in firm characteristics between the new firm that individuals join and the old firm they leave after their first departure following the merger announcement.

	Mean	Median
Δ Book Asset	–21012.23	–8999
Δ Age	–3.18	–8
Δ Market-to-book	1.68	0.15
Δ Profitability	–0.01	–0.00

Table A.12: **Employee Transitions and Non-Compete Agreements**

This table examines the relationship between non-compete agreements and employee transitions. I estimate the following,

$$\Delta y_{ijt} = \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability} + \varepsilon_{ijt},$$

where Δy_{ijt} are changes in firm characteristics between the new firm that individuals join and the old firm they leave after their first departure following the merger announcement. t-statistics shown in the parentheses are based on double clustered standard errors by the target firm and year.

	Δ Asset	Δ Age	Δ MB	To Private
	(1)	(2)	(3)	(4)
NCA Enforceability	–20732.719	1.105*	–0.136	–0.033**
	(–1.465)	(2.024)	(–0.898)	(–2.256)
Observations	74376	10244	73203	231346
R^2	0.433	0.192	0.162	0.018

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: **Employee Transitions to Different Industries or States**

This table examines whether individuals switching to different industries or states after leaving is associated with non-compete agreements. I estimate the following,

$$\text{New Industry/State}_{ijt} = \alpha_j + \alpha_t + \beta \cdot \text{NCA Enforceability} + \varepsilon_{ijt},$$

where New Industry/State$_{ijt}$ is a dummy variable equals to 1 if an individual i switches to a new industry or moves to a new state and 0 otherwise. t-statistics shown in the parentheses are based on double clustered standard errors by the target firm and year.

	Industry	State
	(1)	(2)
NCA Enforceability	-0.003	0.062***
	(-0.286)	(9.606)
Observations	231346	231346
R^2	0.035	0.020

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$