Do Non-Compete Agreements Help or Hurt Workers? Evidence from the NLSY97

Bhargav Gopal *   Xiangru Li†   Luke Rawling ‡ §

April 15, 2025

Abstract

Non-compete agreements are provisions within employment contracts that prevent workers from joining competing firms. They are widespread in the US workforce, with 15% of workers having such clauses in their contracts at a given point in time. Despite their prevalence, there is limited research on the incentives for workers and firms to use non-compete agreements, and the causal effects of these agreements on worker outcomes. We show theoretically that non-compete agreements shift the nature of allocative inefficiency – reducing inefficient quits but increasing inefficient retention – while mitigating the canonical hold-up problem. The model predicts that (i) non-compete agreements are more likely to be used in industries with higher rates of job mobility and (ii) non-compete signers have longer job tenures, higher wages, and receive more firm-provided investment than similar workers without such agreements. Using panel data from the NLSY97 and a difference in difference research design, we estimate the causal impact of signing a non-compete agreement on various labor market outcomes. We find that non-compete agreements raise job tenures by 6% and wages by 9% within one year, with these effects persisting at least six years. Consistent with the theory, we observe that non-compete signers are concentrated in industries with higher rates of job mobility, though we do not find evidence indicating that signing a non-compete agreement raises observed measures of employer-provided training.

*Corresponding Author: Smith School of Business at Queen's University. Email: b.gopal@queensu.ca

†Department of Economics, Columbia University. Email: xl3147@columbia.edu

‡Department of Economics, Queen's University. Email: luke.rawling@queensu.ca

§We thank Michael Best, Sandra Black, Bentley MacLeod, Justin McCrary, Suresh Naidu, and Bernard Salanie for guidance, Ziyan Rui, Wenqi Xie, Bailey Kraus, Michael Duarte, and Odeya Leikin for research assistance, and Evan Starr for helpful comments. Gopal acknowledges funding from the Social Sciences and Humanities Research Council of Canada Grant Number 430-2024-00035.

1 Introduction

The assumption that labor markets are perfectly competitive has been increasingly questioned in recent decades (e.g. Card 2022). One factor that can limit competition is the prevalence of non-compete agreements — contractual provisions that restrict workers from joining competing firms after leaving their current employer. The impact of non-compete agreements on labor markets remains contentious. Proponents argue that non-compete agreements increase worker retention and encourage firms to invest in industry-specific training, potentially benefiting workers through higher wages in the long run. Critics argue that non-compete agreements create mobility frictions, reducing workers' bargaining power and preventing them from transitioning to firms where they would be more productive.[1]

Despite their widespread use — 15% of U.S. workers were bound by non-compete agreements in 2017 — there is limited causal evidence on the effects of signing a non-compete agreement on long-run individual labor market outcomes. Most empirical studies focus on the effects of non-compete regulation rather than the direct worker-level effects of signing a non-compete agreement (e.g. Johnson, Lavetti, and Lipsitz 2023; Lipsitz and Starr 2022; Jeffers 2023; Kini, Williams, and Yin 2021; Starr 2019; Balasubramanian et al. 2022). More limited research leveraging micro-data on the usage of non-compete agreements has focused on their effects on particular subpopulations such as physicians (Lavetti, Simon, and White 2020), or on the descriptive relationships between signing the agreement and various worker characteristics (Rothstein and Starr 2022; Shi 2023; Starr, Prescott, and Bishara 2021). While these studies provide valuable insights into how regulation of non-compete agreements affects labor markets and the types of workers who sign non-compete agreements, they do not address the fundamental question of whether signing a non-compete helps or harms workers, and for which types of workers. Understanding these worker-level effects is crucial for evaluating whether non-compete agreements create long-term benefits for labor, such as a more skilled workforce and higher earnings, or primarily serve to restrict labor mobility and suppress wages.

Theoretical research has provided important insights into how non-compete agreements affect investment incentives and labor market efficiency, but significant gaps remain. Prior models have explored how non-compete agreements encourage firms to invest in general training (e.g. Meccheri 2009; Posner, Triantis, and Triantis 2004) and influence the efficient matching between workers and firms (e.g. Shi 2023; Gottfries and Jarosch 2023). However, these theories do not fully explain why workers would voluntarily agree to sign non-compete agreements, despite their restrictive

[1] Critics also argue that non-compete agreements deter business formation by making it difficult for startups to hire skilled workers (e.g., Aghion and Bolton 1987, Jeffers 2023). Additionally, some firms impose non-compete agreements on workers after they have already accepted job offers or without their full awareness, which may allow firms to retain workers at lower wages (Starr, Prescott, and Bishara 2021).

nature. If non-compete agreements are detrimental to workers, as critics suggest, why do they remain so common? A complete framework must incorporate both firm and worker incentives, explicitly modeling the conditions under which signing a non-compete agreement is in the interest of both parties.

Our model integrates these two theoretical perspectives — investment incentives and labor market matching — into a unified framework. The first strand of literature examines how contracts influence employer investment incentives, particularly when investments are general and non-contractible (e.g., Grossman and Hart 1986; Che and Hausch 1999; Acemoglu and Pischke 1999; Meccheri 2009; MacLeod and Malcomson 1993).[2] The second focuses on how contracts shape labor market matching efficiency and mobility (e.g., Shi 2023; Gottfries and Jarosch 2023; Pakes and Nitzan 1983). By combining these two perspectives in a single theoretical framework, we formalize the trade-offs arising from using a non-compete agreement in the employment relationship. We show that non-compete agreements reduce inefficient quits, increase inefficient retention (job lock), and encourage firms to provide non-contractible industry-specific training.[3] Profit-maximizing firms offer a contract with a non-compete agreement when the gains arising from more skilled labor and lower turnover exceed investment costs and the higher wages required to compensate workers for restricted mobility. Risk-neutral workers accept contracts with non-compete agreements when expected lifetime compensation from signing the agreement exceeds that from not signing the agreement. The model predicts that non-compete agreements should be most prevalent in industries with high turnover rates or where firms make substantial investments in industry-specific human capital. At the individual level, the model generates testable predictions that (i) workers who sign non-compete agreements should have longer job tenures, higher wages, and greater employer-sponsored training, but (ii) mobility restrictions may prevent workers from accessing higher-paying external opportunities.

A key assumption in our model is that contract renegotiation is infeasible due to private information and high transaction costs, following Hashimoto (1981). Prior work has shown that, in theory, firms could release workers from non-compete agreements in exchange for buyout payments, restoring efficient ex-post matching in a Coasean world (Coase 1960; Shi 2023; Posner, Triantis, and Triantis 2004). Our model departs from this idealized setting by assuming that workers hold private information about their outside options, which they cannot credibly communicate to firms. This feature prevents efficient renegotiation, leading to persistent allocative inefficiencies, consis-

[2]When investments are non-contractible, variations in investment by one party cannot be measured or priced by the courts. The modelling choice to make investment non-contractible follows a long literature studying how contracts may be designed to encourage investment and resolve the hold-up problem (i.e. Grossman and Hart 1986; Che and Hausch 1999).

[3]Other contract theory models have examined the interplay between investment incentives and matching efficiency in various contexts (e.g. Hellmann and Thiele 2017; MacLeod and Malcomson 1993), but none have explicitly analyzed this trade-off in the specific setting of non-compete agreements.

tent with the call by Hart and Moore (2007) to model ex-post inefficiencies arising from various market frictions.[4] Unlike Shi (2023), who focuses on how non-compete agreements can generate excessive rent extraction via buyouts, we focus on a distinct inefficiency: the outright prevention of efficient matches when workers cannot be released from binding non-compete agreements. At the same time, our model shows that non-compete agreements help resolve the hold-up problem by encouraging firms to invest in industry-specific skills. These theoretical results generate the policy implication that blanket bans on non-compete agreements are unlikely to be socially efficient. Although such bans may increase short-term labor mobility and matching efficiency, they risk reducing firms' incentives to invest in worker training, leading to a less-skilled workforce over time.

We assess the causal impact of signing a non-compete agreement on various labor market outcomes using data from the National Longitudinal Survey of Youth (NLSY97) and a difference-in-differences research design, thereby allowing us to test the model's predictions. The NLSY97 is a nationally representative longitudinal survey that tracks a cohort of individuals who were teenagers in 1997. By 2017, when the survey first includes a question on non-compete status, the sample is between the ages of 32 and 38 — an ideal period for studying labor market outcomes as respondents are in their prime working years. Of the 5,236 individuals who reported their non-compete status in the 2017 questionnaire, approximately 15% responded affirmatively to having a non-compete agreement in their contract and more than 90% are "Very Confident" in the accuracy of the response.[5] The NLSY97 includes a wide range of outcome variables, allowing us to assess the effects of non-compete agreements on key variables featured in the theoretical model — wages, job mobility, and training — but also on broader aspects of job quality, such as job tasks, job satisfaction, and working hours. The dataset also provides detailed worker characteristics, enabling us to examine heterogeneity across several dimensions, including race, gender, education, and cognitive ability.

An advantage of the NLSY97 is that it allows us to track individual workers over time and across jobs, enabling the use of panel data research methods to estimate the causal impact of signing a non-compete agreement on career trajectories. Unlike prior research using this dataset which has primarily examined non-compete usage in cross-section (Rothstein and Starr 2022), we follow

[4] To further elaborate, in our model, the firm cannot verify the terms of the worker's outside offer since it is private information to the worker. As such, the worker cannot command a higher wage by claiming to have a superior outside offer because the firm will assume such claims are inflated. If the parties do not have the ability to renegotiate the initial contract, there may be inefficient separations as well as inefficient lack of job separation.

[5] NLSY97 respondents appear confident in reporting their non-compete status, which contrasts with findings from Cowgill, Freiberg, and Starr (2024). In a field experiment with job applicants for full-time positions at a large U.S. financial services company, they document that many workers fail to notice or recall non-compete clauses in their contracts, particularly when the clause is not made salient at the time of signing. Their results suggest that some workers unknowingly accept non-compete agreements.

workers across survey waves. Using the panel component of the NLSY97 is critical for several reasons. First, it allows us to use individual fixed effects to account for time-invariant unobserved heterogeneity across workers who do and do not sign non-compete agreements, improving causal identification. Second, it enables us to examine how non-compete agreements affect wage growth, rather than cross-sectional wage differences. Third, it allows us to track the effects of signing a non-compete agreement even if a worker later changes jobs, ensuring that we capture the longer-run impacts of the agreement on labor market outcomes.

Estimating the causal effects of signing a non-compete agreement is challenging because workers who sign these agreements differ systematically from those who do not. Signing a non-compete agreement is often coincident with job mobility, which itself is linked to wage increases (i.e. Topel and Ward 1992). Furthermore, in the cross-section, we observe that non-compete signers have characteristics that are associated with higher wages, which complements findings from prior research (i.e. Rothstein and Starr 2022; Starr, Prescott, and Bishara 2021). They earn 21% higher wages in the cross-section and are 10 percentage points more likely to hold a bachelor's degree. They score 5.4 percentile points higher on the ASVAB test, suggesting stronger cognitive ability. NC signers are 8 percentage points more likely to have negotiated their job offer and are more frequently engaged in math-intensive (+10 p.p.), supervisory (+6 p.p.), and problem-solving roles (+11 p.p.). Consistent with the idea that NC signers tend to have higher-wage, higher-skilled jobs, the cross-sectional wage premium of 21% declines as we add control variables for detailed worker characteristics, falling to 8%.

To overcome these challenges, we leverage the fact that different individuals sign non-compete agreements at different points in time and compare their individual labor market outcomes to a control group of workers who never sign a non-compete agreement during the sample period but start jobs in the same year. This approach ensures that we are comparing the trajectories of new job holders (in a given year) with and without a non-compete agreement rather than workers with fundamentally different labor market experiences. We construct our dataset using NLSY97 data from 2013 to 2021, defining a cohort as a group of individuals who start a new job in a given year. In a given cohort, treated workers are those who begin a job with a non-compete agreement in that year, while control workers are those who start a job in the same year but never sign a non-compete agreement over the entire sample period.[6] This setup ensures that for each "experiment," we are

[6] Since the NLSY97 only begins tracking non-compete status in 2017, we assume that if an individual reports signing a non-compete agreement in 2017, they had it from the beginning of their job tenure. This assumption allows us to estimate the longer-term effects of signing a non-compete agreement, even though the dataset does not capture non-compete agreements from the job's start date. As a sensitivity check, we re-estimate our results using only the 2017, 2019, and 2021 cohorts—workers who started new jobs in those survey waves. For these workers, we directly observe their non-compete agreement status at the time of hiring, eliminating the need for any assumptions about when the agreement was signed. The results from this restricted sample closely align with our main findings, reinforcing the validity of our long-term estimates. If an individual signs multiple non-compete agreements over the sample period,

comparing newly hired workers with and without non-compete agreements, reducing concerns about selection bias.

Following prior work on treatment effects with staggered adoption, we estimate the parameters of a stacked difference-in-differences model, aggregating across cohorts to estimate the average treatment effect of signing a non-compete agreement (Cengiz et al. 2019; Johnson, Lavetti, and Lipsitz 2023; Gormley and Matsa 2011). By stacking these cohorts together, we construct a series of clean difference-in-differences comparisons, avoiding the issues that arise in standard two-way fixed effects models with staggered treatment timing (Goodman-Bacon 2021; Callaway and Sant'Anna 2021; Sun and Abraham 2021). We are confident in the validity of our causal estimates because we do not observe pre-trends for wages, job mobility, or other key outcomes in the event-study design, indicating that workers who go on to sign non-compete agreements are not experiencing systematically different trends before signing.

Our primary empirical finding is that signing a non-compete agreement leads to a statistically significant, immediate and persistent increase in wages. Our stacked event-study estimates indicate that signing a non-compete agreement raises wages by 9.4% within one year, a magnitude comparable to the returns to an additional year of education (e.g. Card 1999). This wage premium persists for at least six years, though we observe that it declines by approximately 1% per year, consistent with non-compete agreements lowering wage growth over time.[7] Notably, the estimated causal wage effect from the quasi-experimental research design (9.4%) is similar in magnitude to the cross-sectional wage premium with controls (8%), suggesting that our cross-sectional wage results are not likely subject to substantial omitted variable bias.

We also observe that non-compete agreements lower job mobility, as predicted by our theoretical model. On average, non-compete agreements increase job tenure by 0.3 years (approximately 6% of the average tenure in the 2017 cross-section). We also find that non-compete agreements deter job mobility when we use alternative measures. For example, we find that non-compete agreements reduce rates of job to job transitions by 3.6 percentage points, which represents a 12% reduction relative to average rates between the 2017 and 2019 survey years. While we view these point estimates as substantial, we also note that many workers who sign non-compete agreements do eventually change employers, albeit at lower rates than non-signers.[8] Despite theoretical predictions that non-compete agreements encourage firm-provided training, we find no significant effects on measures of employer-provided training. Similarly, we find no significant effects on job

the individual is only included in the treated cohort corresponding to the earliest recorded use of the agreement.

[7]The fact that career earnings remain higher for non-compete signers but that the wage gap narrows over time aligns with the descriptive statistics reported in Shi (2023).

[8]For example, the share of workers remaining in the same job after 4 years of starting employment is 0.63 for signers and 0.59 for non-signers. These descriptive statistics are consistent with the fact that enforceable non-compete agreements do not deter all job mobility, but rather to competing firms within the same industry in a pre-defined geographic radius.

satisfaction, working hours, or the nature of job tasks.

We examine heterogeneity across several dimensions, including education, gender, race, cognitive skills, and whether contract terms were negotiated. The positive wage effects of non-compete agreements are relatively stable across worker subgroups, ranging from a low of 7.3% for Black and Hispanic workers to a high of 12.4% for above-median wage workers (as defined by 2011 wages). In contrast, tenure effects vary more substantially, ranging from an effect statistically indistinguishable from zero for workers with below-median cognitive skills to slightly above 0.5 years for college-educated workers and those with above-median cognitive skills. While we do not find that non-compete agreements affect measured outcomes besides wages and mobility in the main sample, we do uncover important heterogeneity across worker subgroups. Workers with at most a high-school degree who sign a non-compete agreement work 1.9 more hours per week and are 4.5 percentage points more likely to report job dissatisfaction. Interestingly, we find that workers who negotiated the terms of their employment contract are 5 percentage points more likely to report job dissatisfaction from signing a non-compete agreement.

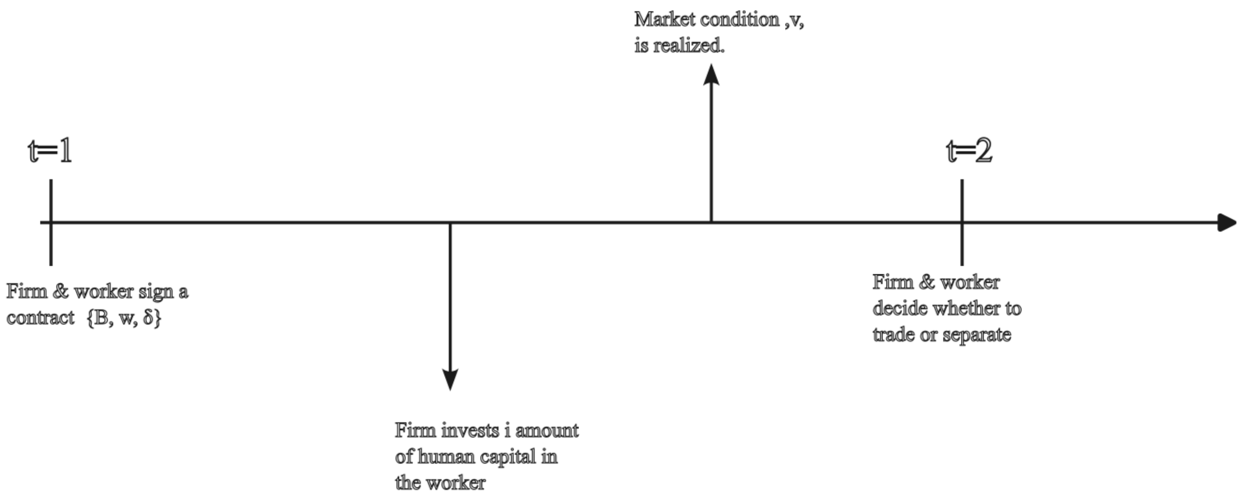
The immediate positive wage effects for non-compete signers, along with lower wage growth, likely represent a compensating wage differential, with firms offering higher upfront wages to offset restrictions on future job mobility. Non-compete agreements are concentrated in high-turnover industries, consistent with the idea that firms use them to protect investments in workers. However, the lack of significant employer-provided training effects contradicts the model's prediction that firms should invest more in worker skills. Despite this observation, non-compete signers continue to earn higher wages even six years after signing, suggesting that factors beyond formal training contribute to these wage gains. Workers with non-compete agreements may develop stronger relationships with managers, receive mentoring, or gain valuable social connections at work, all of which influence career trajectories (e.g. Cullen and Perez-Truglia 2023). Our findings highlight an important distinction between the direct effects of signing a non-compete agreement and the broader effects of stronger non-compete enforcement. While signing a non-compete agreement raises career wages, research to date has shown that stricter enforcement of non-compete agreements generally reduces wages, with an exception for executives (e.g. Johnson, Lavetti, and Lipsitz 2023; Lipsitz and Starr 2022; Garmaise 2011; Kini, Williams, and Yin 2021). This divergence underscores the importance of analyzing the effects of signing a non-compete agreement separately from the regulatory environment that governs their enforcement.

The paper proceeds as follows. Section 2 lays out the theoretical framework. Section 3 discusses data sources and Section 4 examines the effect of signing a non-compete agreement on various labor market outcomes. Section 5 concludes.

2 Theoretical Framework

2.1 The Model

Figure 1: Timeline of the Model



Our model features two periods. In the first period ("ex-ante"), a single worker W and firm F choose a contract, which consists of a wage w and may include a non-compete agreement $\delta \in \{0,1\}$.[9] The non-compete agreement prevents W from moving to poaching firms θ within the same industry as F. Between the first and second period, F sinks non-contractible industry-specific investments i with associated cost $i^2/2$ that raise W's productivity within F by r, should trade between the parties occur. At the time the investment is made, it is uncertain how much F's investment raises W's productivity if separation occurs. At the beginning of period 2, overall market conditions $v \sim logNormal(\mu, \sigma^2)$ are revealed and a poaching firm $\theta \in \{0,1\}$ makes an offer to W. With probability q, the poaching firm is in the same industry as the original firm ($\theta = 1$) and values the worker at $v + \rho \times i$. With remaining probability, the poaching firm is outside of the original firm's industry ($\theta = 0$) and values the worker at v.[10] Observe that F's investment raises W's productivity among poaching firms only when $\theta = 1$. We assume the labor market is competitive ex-post, so the poaching firm makes an offer equal to its valuation.[11] As in Hashimoto (1981), we

[9]The parties also have the option of making side payments to each other at the contracting stage. Let B denote the side-payment made by W to F at the contracting stage. We assume that $B \geq 0$. This assumption can be justified on several grounds. One reason is that it is unrealistic for workers to post bonds to employers (i.e. Baker, Gibbons, and Murphy 1994). Another is that the firm may be unable to commit to a rising wage profile or deferred benefits. Note that the wage may depend on whether the non-compete agreement is included in the contract.

[10]Note that if $q = 1$ and $\rho = r$, the investment is completely general.

[11]A similar assumption is made in Spier and Whinston (1995), pg 186-188

further assume that the poaching firm's offer is private information to the worker. After the worker receives his outside offer, the parties can trade at the contractual terms or separate.[12] To simplify matters, we suppose that the poaching firm is always an industry competitor ($q = 1$).

The incentives for parties to use a non-compete agreement will depend on a comparison between the internal return on investment r and the external return on investment ρ.[13] In our model, r and ρ can take arbitrary values, so we do not constrain firm-provided investment to be purely specific or industry-specific. We choose to be agnostic about the exact values of these parameters since they may differ across industries. For example, the internal return on investment may be large relative to the external return on investment among individuals in the Education, Health, and Social Services industry, where average job tenures are long. In contrast, the external returns on investment may be large relative to the internal return on investment in Professional and Related Services, where job-hopping between firms in the same industry is more common.[14]

The model approximates the ideal of a perfectly-competitive labor market in all but one exception – the worker has private information on the outside option which cannot credibly be communicated to the incumbent firm. Otherwise, we highlight that the poaching firm makes an offer equal to its valuation of the worker. This assumption can be justified in a setting where there are many homogeneous firms who each simultaneously bid for the worker's services.[15] In addition, the firm must offer a contract that meets or exceeds the utility value of the worker's outside option in order for the worker to accept. In perfectly competitive labor markets, this value equals the worker's utility from accepting a job at one of many other homogeneous firms.

First, consider what happens if the parties do not include a non-compete agreement in the contract. At the investment stage, the firm chooses investment to equate marginal cost and expected marginal benefits. It earns a return of r when trade occurs, but does not earn a private return when separation occurs even though such investment would raise W's productivity by ρ. Anticipating this, F underinvests, which is the well-known hold-up problem.

One solution to the hold-up problem is for W and F to write a contract that reduces the chance that job separation occurs (i.e. Autor 2003; MacLeod and Malcomson 1993). A binding non-compete agreement fits this bill, as W is less likely to quit when $\delta = 1$ than when $\delta = 0$, holding all else equal. The non-compete agreement thus encourages F to invest more but prevents W from

[12]Since trade is voluntary, W can quit or F can fire. The firm's payoff from trade (net of investment) is $ri - w$, while the worker's is w. The firm's payoff from separation is 0. The worker's payoff from separation is $\bar{w} = v + \big(\theta(1-\delta)\big)\rho \times i$. F fires the worker if $w > ri$, and W quits if $w < \bar{w}$. Since workers are not allowed to make transfer payments to the firm in the initial period, in equilibrium, the wage will always be less than the value of the worker's output.

[13]When investment is purely specific, $\rho = 0$, and when investment is purely industry-specific, $r = \rho$.

[14]As of January 2022, the median job tenure for workers in Professional and Business Services is 3.4 years, while that for Education and Social Services is 4 years: https://www.bls.gov/news.release/pdf/tenure.pdf

[15]The poaching firm and the incumbent firm may place different weights on the worker's human capital, which contributes to different valuations for the worker's services (e.g. Lazear 2009).

joining the industry competitor, even when such separation is socially efficient.[16]

2.2 Benchmark Outcomes

To more formally describe how firms under-invest relative to the socially optimal quantity without a non-compete agreement, we first characterize the planner's allocation. The social planner invests and allocates workers to firms efficiently. In the final period, the social planner executes trade between the worker-firm match if the social surplus from trade exceeds that of separation. This condition occurs when $S^T \geq S^{NT}$, where $S^T = ri_s - c(i_s)$ and $S^{NT} = v - c(i_s) + \rho i_s$. Thus it is efficient to trade if $v \leq (r-\rho)i_s$. Denote $p_s := \Phi(\frac{ln((r-\rho)i_s)-\mu}{\sigma})$ as the probability that trading is efficient from the perspective of the initial period. The efficient investment level is solved by the following equation:

$$i_s^* = \text{argmax } E(S) = -c(i_s) + (ri_s)\cdot p_s + [v+\rho i_s]\cdot(1-p_s) \tag{1}$$

Proposition 1: If $r < \rho$, separation is always efficient and the efficient investment level is $i_s^* = \rho$. If $r > \rho$, the efficient investment level is $i_s^* = rp_s + \rho(1-p_s)$. The probability of separation is not equal to 0, and the efficient investment level increases with r and ρ.

Proof: If $r < \rho$, $P\big(v \leq (r-\rho)i_s\big) = 0$, so separation is always efficient. Solving $i_s^* = \text{argmax } E(S) = -c(i_s) + v + \rho i_s$ we have $i_s^* = \rho$. The efficient level of investment increases with ρ. See the appendix for case when $r > \rho$.

Corollary 1: As investment becomes more specific, the planner's probability of separation declines.

Proof: $\partial p_s / \partial (r-\rho) > 0$.

When the external return on investment is larger than the internal return on investment, poaching firms always have a higher valuation than the incumbent firm. As a result, separation is always efficient ex-post. On the other hand, when the internal return on investment is larger than the external return on investment, trading is not necessarily efficient ex-post. When market conditions are sufficiently strong, the planner assigns the worker to the poaching firm; otherwise, the planner maintains the match between the incumbent firm and worker.

2.2.1 A Simple Example

We use a simple example with exogenous investments to illustrate how non-compete agreements create "job-lock", or inefficient lack of job separation. We still use the same setup described

[16]This occurs when $ri \geq w \geq v$ and $\rho \times i > 0$

in Subsection 2.1, but we assume that the investment level, i, is a fixed parameter. Hence the only actions are for the firm to offer a contract in period 1 and for the parties to make separation decisions in period 2. For a given wage, we compare ex-post separation decisions when non-compete agreements are and are not used.

When the worker and firm use a non-compete agreement, the worker will remain with the firm so long as the wage is less than or equal to the outside option, or when $w \leq v$. The region where trade will occur is given by the shaded region in Figure 2. The scenarios where trade occurs in a competitive equilibrium differs from the scenarios where trade occurs under an efficient allocation between workers and firms. To see this point, Figure 2 also visualizes the efficient trading rule presented in Section 2.2. When $r > \rho$, trade is efficient whenever $v \leq (r-\rho)i$, depicted by the region below the dashed line and above the x-axis. When $\rho > r$, trading is never efficient, as the poaching firm always has a higher valuation than the incumbent firm. We observe that two types of (allocative) inefficiencies occur with a non-compete agreement. First, there are inefficient separations: there are scenarios where trade is efficient but separation occurs. This case is represented by the region below the dashed line and above the shaded area. Second, there are cases where separation is efficient but where it does not occur. This case is depicted by i) The shaded region when $r-\rho < 0$ and ii) the shaded region above the dashed line when $r-\rho > 0$.

Under the assumptions of our model, all efficient separations are realized when a non-compete agreement is excluded from the contract. When the worker's outside option exceeds the firm's valuation, that is when $v+\rho \cdot i > ri$, the worker always quits. To see why this is the case, observe that the firm will never offer a contract where it earns negative profits ex-post.[17] Hence $w \leq ri < v+\rho \cdot i$, so the worker will quit when separation is efficient. However, for any given wage, inefficient quits are more likely to occur without a non-compete agreement than with a non-compete agreement.[18]

This example illustrates that inefficient separation decisions occur both with and without a non-compete agreement. Since the initial wage cannot be renegotiated ex-post, there are instances where the worker finds it profitable to quit even though the incumbent firm has the highest valuation. There are inefficient quits without a non-compete agreement, but all efficient separations are achieved. The reason is that the wage never exceeds the incumbent firm's valuation, so the worker will quit when a poaching firm has the highest valuation. In the presence of a non-compete agreement, there are scenarios where efficient job separation is not realized, as the agreement may block the worker from moving to a poaching firm with a higher valuation. However, there are fewer inefficient quits with a non-compete agreement than without a non-compete agreement.

[17]This conclusion is a consequence of our assumption that workers cannot make transfer payments to firms in the initial period (i.e. $B \geq 0$)

[18]Observe that $Pr(v > w | v \leq (r-\rho)i_s) < Pr(v+\rho \cdot i > w | v \leq (r-\rho)i_s)$

Figure 2: Separation Decisions with a Non-Compete Agreement

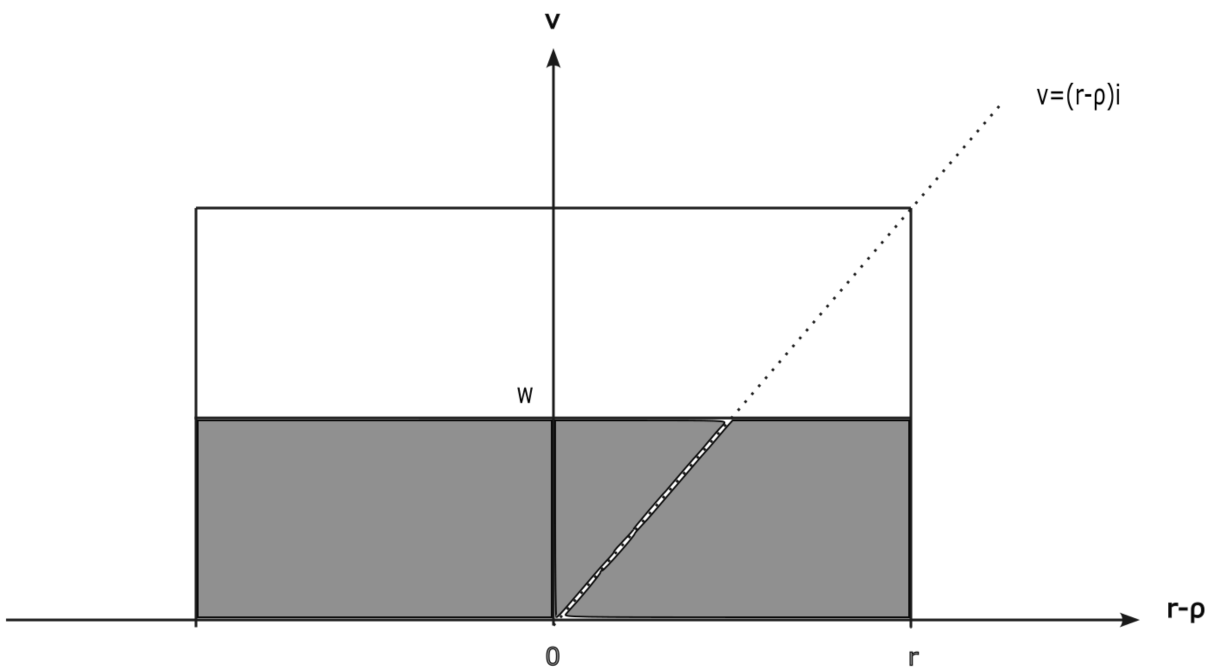
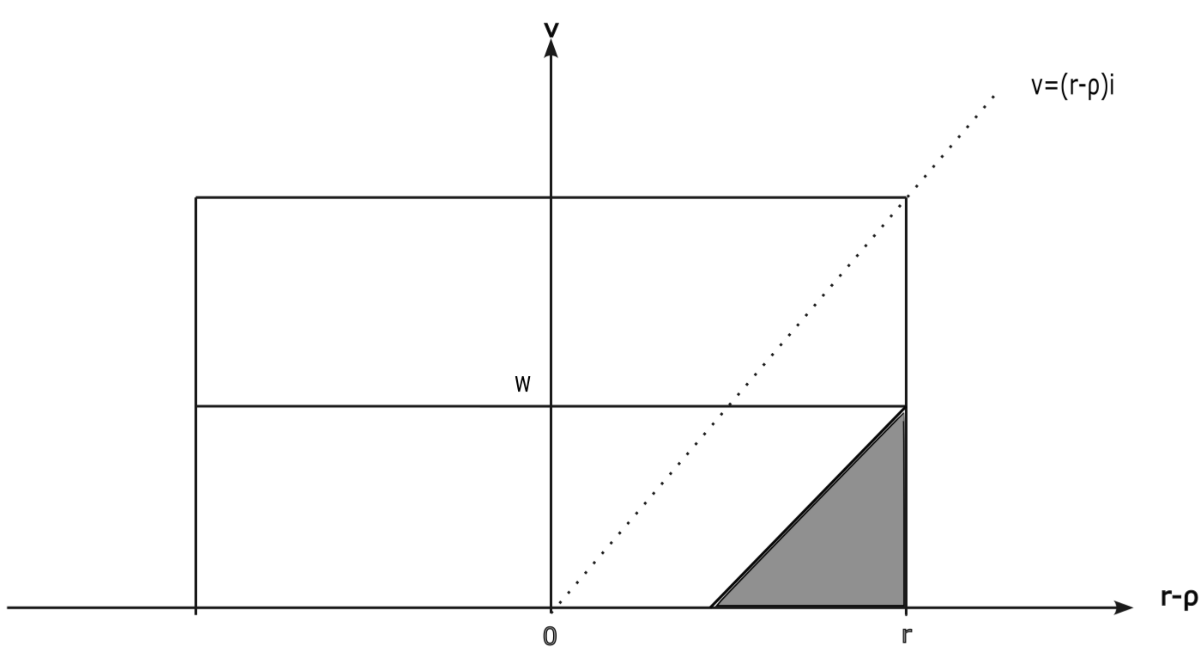


Figure 3: Separation Decisions without a Non-Compete Agreement



2.3 General Results

We solve for the Subgame Perfect Nash Equilibrium. At $t = 2$, the worker quits if the outside option is higher than the wage they receive in the last period. The outside option is v if a non-compete is signed and $v + \rho i$ if a non-compete is not signed:

$$W : \text{Stay} \iff w_\delta \geq v + \rho i_\delta (1 - \delta) \tag{2}$$

Since v is not known before investment, we denote $p_\delta = \Phi(\frac{\ln(w_\delta - \rho i_\delta(1-\delta)) - \mu}{\sigma})$ as the ex-ante probability of the match persisting. Between $t = 1$ and when market conditions are revealed, the firm makes human capital investments. We can solve for the firm's profit maximizing level of investment (conditional on the parameters of the contract) with the following equation.

$$\widetilde{i_\delta} = \text{argmax } E(\pi_\delta^F) = -c(i_\delta) - B_\delta + p_\delta(ri_\delta - w_\delta) \tag{3}$$

In the first period, the firm offers a contract that specifies a wage, bonus, and may contain a non-compete agreement. It makes this choice taking into account the participation constraints of the worker and firm, as well as by anticipating decisions later in the game. Since the firm offers the contract, the worker's participation constraint is binding, so that $E(\pi^W) = \mu_0$.[19]

$$w_\delta^*, B_\delta^* = \text{argmax } E(\pi_\delta^F) \text{ s.t. } E(\pi_\delta^F) \geq 0,\ E(\pi_\delta^W) = \mu_0, \text{ and } B_\delta \geq 0 \tag{4}$$

Proposition 2: When a non-compete agreement is not signed, the firm will under-invest relative to the socially optimal quantity ($i_0^* \leq i_s^*$). When $r \geq \rho$, there is always inefficient separation ($0 < p_0^* \leq p_s^* < 1$). When $\rho > r$, there is efficient turnover without a non-compete agreement.
Proof: See Appendix.
Proposition 3: There is more employer-provided investment ($i_0^* \leq i_1^*$), higher cross-sectional wages ($w_0^* \leq w_1^*$) and less turnover ($0 < p_0^* \leq p_1^* < 1$) with a non-compete agreement than without a non-compete agreement.
Proof: See Appendix.
Proposition 4: When $r < \rho$, a non-compete agreement is always used. When $r > \rho$, it is ambiguous whether the parties use a non-compete agreement.
Proof: See Appendix.

[19] Once we solve for the wage and bonus levels, we can plug into $\tilde{i_\delta}$ to arrive at i_δ^*.

2.4 Discussion

Propositions 1-4 constitute our main theoretical results and provide a framework to guide our empirical analysis. First, we characterize the efficient allocation, which consists of an investment level and an ex-post mapping between workers and firms. We show that the efficient level of investment increases as we raise the return on investment. Next, we show that absent a non-compete agreement, there is under-investment relative to the socially optimal quantity. This well-known hold-up problem occurs in our context because there are inefficient separations without a non-compete agreement. Our main theoretical result is that a non-compete agreement lowers turnover and raises firm-provided investment, thereby mitigating the hold-up problem. If the external returns on investment are large relative to the internal returns on investment, the probability of a job separation is large without a non-compete agreement. The firm thus has an incentive to use a non-compete agreement to raise investment and profits. In the remaining cases, the firm uses a non-compete agreement if the gains from a higher expected return on investment outweigh the costs from higher wages.

In our model, non-compete signers earn higher wages because firms must compensate workers for their reduced option value of job search. We show in Appendix Section A.4 that workers receive compensation for signing a non-compete agreement in the form of higher wages rather than an up-front bonus. All else equal, firms prefer to provide compensation with higher wages, since higher wages reduce the probability of a job separation.[20]

Our empirical results do suggest that non-compete signers earn higher cross-sectional wages, which is consistent with our theoretical prediction.[21] However, the predicted effect of non-compete agreements on wages may be negative under modified modelling assumptions. In our Monopsony Appendix, we extend our baseline model so that a single firm offers a contract to hetereogenous workers with varying reservation utilities. As in Hashimoto (1981), these reservation utilities are private information to workers. We show that when the firm uses a non-compete agreement, employment is less than socially efficient and that wages may be lower than without a non-compete agreement.[22]

[20] Both higher wages and up-front bonus payments raise the worker's utility, but up-front bonus payments do not affect the worker's decision to quit or stay ex-post.

[21] In our fully saturated model that includes controls for tenure, age, gender, potential experience, and industry, we find that non-compete agreements are associated with 16% higher wages in the 2017 cross-section and 20% higher wages in the 2019 cross-section.

[22] By lowering wages, the monopsonistic firm attracts fewer workers but earns more profits per worker. Without a non-compete agreement, the firm has employment less than the socially efficient level and earns positive (ex-post) profits per worker (Proposition A1). A non-compete agreement may result in even lower wages and employment than without a non-compete agreement if the gains in profit-per-worker exceed the losses in employment. However, whether this occurs depend on the exact parameters of our theoretical model.

3 Empirical Set-up

3.1 Data and Descriptive Relationships

We use data from the National Longitudinal Survey of Youth 1997 (NLSY97) to understand the characteristics of non-compete signers and analyze the effects of such agreements on worker outcomes, including wages, job mobility, and employer-provided investment. This data set is a nationally representative panel that tracks the outcomes of individuals aged 12-16 in 1997. The survey runs annually from 1997-2011, and then biannually from 2011-2021. The survey includes information on the workers' employment history, including hourly wages for each job held, as well as detailed information on worker demographics and job information.

Importantly, the NLSY97 starts measuring whether non-compete agreements are used within employment contracts starting in 2017, when survey respondents are between ages 32-36. In 2017, all working respondents are asked whether they currently have a non-compete agreement. In the following survey years, all individuals who obtain *new* jobs are asked about their non-compete status. We assume throughout that non-compete status is fixed for the duration of the employment relationship.[23]

Throughout the analysis, we focus on the 2013-2021 time period and individuals who sign non-compete agreements after 2013, allowing us to estimate the impact of NC's for up to 6 years. We focus on employed workers and remove observations with real hourly wages below 3 or above 200, following Deming (2017).[24] When individuals hold multiple jobs in a survey year, we restrict attention to their primary job which we define as the current or most recent employer as of the interview date. If multiple jobs are current, the main job is the one with the longest tenure.

Using the 2017 cross section, we find that 14% of workers report having a non-compete agreement in their contract.[25] More than 90% of affirmative respondents reported being "Very Confident" in their answer. There is substantial heterogeneity in non-compete usage across the 17 (two-digit) industries considered. Table A2 shows that among industries with more than 100 respondents, non-compete agreements are most commonly used in Professional and Related Services (26%) and least commonly used in Public Administration (8%). Figure A1 further shows that NCs are used more frequently in high mobility industries, consistent with our theoretical predictions.

We are interested in the relationships between non-compete agreements and various labor market outcomes. We observe the employment history of each worker, including job identifiers and

[23] As support for this assumption, Starr, Prescott, and Bishara (2021) conducts a large survey that asks about the timing of non-compete agreements and find that in the vast majority of cases a non-compete agreement is signed prior to or immediately after starting the job, with only 2.2% associated with promotions or raises.

[24] We construct real hourly wages by deflating nominal hourly pay by annual CPI indices from BLS, setting 2017 as our base year.

[25] This statistic is relatively stable across survey years.

Table 1: Respondent Characteristics by Non-Compete Status in 2017

	NC	no NC	Difference	P Value	N: NC	N: No NC
Job Mobility						
Tenure (Yrs)	5.24	5.11	0.12	0.50	699	4185
1(Main Job Separation btwn 2017 and 2019)	0.33	0.37	-0.04	0.04	705	4263
1(Main Job Mobility btwn 2017 and 2019)	0.28	0.31	-0.04	0.05	705	4263
1(Within-Industry Job Mobility btwn 2017 and 2019)	0.10	0.12	-0.02	0.08	686	4176
Wages and Wage Growth						
Log(Starting Wage)	2.94	2.76	0.19	0.00	705	4263
Log(Wage in 2017)	3.21	3.00	0.21	0.00	705	4263
$Log(Wage_{2017}) - Log(Wage_{2015})$	0.13	0.12	0.02	0.22	628	3778
$Log(Wage_{2019}) - Log(Wage_{2017})$	0.11	0.10	0.01	0.56	632	3753
Demographics						
Age	35.03	34.96	0.07	0.25	705	4263
1(Male)	0.58	0.50	0.08	0.00	705	4263
1(High School Degree or Higher)	0.89	0.86	0.03	0.01	699	4224
1(Bachelors Degree or Higher)	0.52	0.42	0.10	0.00	699	4224
ASVAB Percentile	57.50	52.06	5.44	0.00	582	3473
1(Black)	0.14	0.16	-0.02	0.13	705	4263
1(Hispanic)	0.11	0.13	-0.01	0.33	705	4263
Wage Bargaining and Negotiation						
1(Possible to Keep Previous Job)	0.46	0.45	0.01	0.74	304	1848
1(Negotiate Job Offer)	0.40	0.33	0.08	0.02	249	1454
Training						
1(Received Some Training)	0.09	0.11	-0.02	0.12	705	4263
1(Received Training Run by Employer)	0.01	0.03	-0.01	0.03	705	4263
1(Received On-Site Training by Non-Employer)	0.01	0.01	0.00	0.64	705	4263
1(Employer Paid for Training)	0.06	0.08	-0.02	0.08	705	4263
1(Employer Paid for Mandatory Training)	0.03	0.04	-0.01	0.26	705	4263
1(Employer Paid for Voluntary Training)	0.03	0.04	-0.01	0.16	705	4263
Job Tasks						
1(Use Math Skills Frequently)	0.37	0.27	0.10	0.00	661	3808
1(Supervise Frequently)	0.37	0.31	0.06	0.00	662	3802
1(Problem Solve Frequently)	0.85	0.74	0.11	0.00	661	3807
Other Firm Characteristics						
1(Dislike Job)	0.05	0.06	-0.01	0.57	645	3792
1(Unionized Worker)	0.11	0.16	-0.05	0.00	636	3743
Firm Size	986.28	1134.72	-148.43	0.65	595	3377

Note: The sample includes respondents with valid NC status for the main employer in 2017. All wage variables are measured in terms of real dollars earned per hour. Respondents earning real wages below 3 dollars or above 200 dollars are dropped. The training variables capture whether the respondent received training under any employer in 2017. Means weighted by nationally representative sample weights and p-values from a two-sided t-test are reported. Sample sizes vary due to missing values of the outcome variable. For details on variable definitions, refer to Table A1 hourly wages at each job, allowing us to assess the impact of signing an NC on wages, wage growth, and job mobility. We measure job tenure in years and job mobility as an indicator variable for whether an individual changed main employers between survey years.[26] The NLSY97 also asks a variety of questions about formal employer-provided training programs. As a default, we report statistics pertaining to whether an individual was involved in a formal training program, but also consider whether this training was paid for and/or provided by the employer.

In Table 1 we report summary statistics for the 2017 cross-section, comparing workers with and without non-compete agreements. Consistent with our theory, we observe that non-compete signers have higher wages, earning 21 log points more. They also have slightly longer job tenures and 4pp lower job mobility rates between 2017 and 2019. Since non-compete agreements typically restrict within-industry job mobility, we also compare mobility rates for workers who transition to new jobs in the same industry. NC signers have 2pp (17%) lower within-industry mobility.

Two interesting facts about job mobility stand out. First, within-industry mobility only accounts for about one-third of all job mobility for both groups.[27] The frequent occurrence of between-industry job transitions suggests that NC's may be less restrictive than one might think, especially for younger workers who tend to be more mobile between industries (Kambourov and Manovskii (2008)). Second, the fact that between-industry mobility is lower for NC signers suggests these lower transition rates may be partially voluntary (e.g., due to higher wage compensation) since this type of mobility should theoretically not be restricted by non-compete agreements.

Despite higher wages, there is no significant relationship between signing an NC and wage growth or formal employer training in the cross-section. However, we note that firm investment in human capital is a broader notion than formal training programs and may not be fully captured by our training variables. Indeed, in 2017 only about 11% of workers report having formal training in their current job.[28]

There are also substantial differences in the types of workers that have non-compete agreements. Non-compete signers are much more likely to be male and less likely to be Black or Hispanic. They also have characteristics positively associated with higher wages, with 52% having a bachelors degree or higher (relative to 42% for non-compete signers) and higher ASVAB test scores, which we use as a proxy for cognitive ability.[29] Workers that sign non-compete agreements

[26] As a robustness check, we also consider alternative measures, such as within-industry job mobility and redefining job mobility to exclude jobs retained as secondary employers.

[27] This statistic is consistent with Parent (2000) who also finds, using NLSY79, that about two-thirds of job changes are between one-digit industries. The frequency of between-industry job mobility is also documented in, for example, Neal (1999) and Kambourov and Manovskii (2008).

[28] In contrast to the firm-level investment measures in Shi (2023), we measure training at the individual-level. Our sample of workers is also approximately 10 years younger than her sample of executives (mean age of 35 in our study versus mean age of 45 in Shi (2023)). Nevertheless, we arrive at similar differences in job tenure among those with and without non-compete agreements (0.12 years in our sample versus 0.10 years among the sample of executives).

[29] The ASVAB is a standardized test on science, math and language skills.

are more likely to perform tasks requiring mathematical skills, leadership, and problem solving, less likely to be unionized, and more likely to negotiate over wages. Interestingly, we find no significant differences in terms of job satisfaction or firm size.

In Table 2 we assess whether these differences are driven by observable worker or job characteristics rather than NCs themselves by estimating the following equation via Ordinary Least Squares

$$Y_i = \beta_0 + \beta_1 * NC_i + \beta_2 * X_i + \varepsilon_i \tag{5}$$

where Y_i is the outcome of interest for worker i, X_i is a vector of observable characteristics for the worker and their current job, and β_1 is the relationship between Y_i and NC usage. We consider dependent variables log wages in 2017, log wage growth, job tenure, indicators for whether the worker changed jobs between 2017 and 2019, whether the worker received formal training, and whether the employer paid for that training. We report results with no controls, "basic" controls which includes sex, education, tenures, and potential experience, and "advanced" controls which further adds ASVAB test score percentiles, firm size, and industry and occupation fixed effects.

The estimated cross-sectional wage premium for signing a non-compete agreement declines as we add control variables, falling from 21.1 to 8.1 log points, which implies that differences in wages are partly attributable to the fact that, on average, workers who sign NCs have characteristics that are positively associated with wages. The relationship between non-compete agreements and the other dependent variables is largely insensitive to the inclusion of observable covariates, still finding that NC signers have slightly lower job mobility rates and insignificant differences in terms of wage growth, formal training, and job tenure. In Table A3 we report results based on the 2019 cross-section and find they are qualitatively similar.

Since our goal is to understand the effects of NC's on wages and job mobility, we consider log wages and job tenure as our main outcome variables throughout the analysis. We prefer to use job tenure as our main metric for identifying the effects of NC's on job mobility since it does not require defining what constitutes a job transition. As robustness we explore other measures of mobility and find similar results.

3.2 Empirical Strategy

Even in the fully saturated model of Equation (5) we cannot rule out the possibility that there are omitted variables correlated with NC usage and labor market outcomes of interest. For example, if NC signers have higher unobserved ability, then our estimated wage coefficients from Equation (5) will be biased upwards. The key challenge is estimating the counterfactual wage and mobility trajectories that would have been experienced by an individual in the absence of an NC.

To isolate the causal effects of signing an NC, we adopt a stacked DiD research design where

Table 2: Estimated Effects of Non-Compete Agreements using the 2017 Cross-Section

Panel 1: Wages and Wage Growth

Dependent Variables:	Log(Wage)			Wage Growth		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	0.211***	0.144***	0.081***	0.009	0.007	0.004
	(0.027)	(0.022)	(0.024)	(0.014)	(0.014)	(0.018)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	3.04	3.04	3.04	0.088	0.088	0.088
Fit statistics						
Observations	4,968	4,836	3,141	4,968	4,836	3,141
R^2	0.017	0.296	0.456	0.0001	0.0005	0.026

Panel 2: Training

Dependent Variables:	1(Any Training)			1(Emp Paid for Training)		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	-0.019	-0.025*	-0.017	-0.018*	-0.026**	-0.022
	(0.013)	(0.013)	(0.017)	(0.010)	(0.011)	(0.014)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	0.112	0.112	0.112	0.071	0.071	0.071
Fit statistics						
Observations	4,968	4,836	3,141	4,968	4,836	3,141
R^2	0.0005	0.007	0.048	0.0006	0.012	0.063

Panel 3: Job Mobility

Dependent Variables:	Tenure (Yrs)			1(Job Mobility 2017-2019)		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	0.125	0.002	-0.267	-0.036*	-0.026	-0.032
	(0.197)	(0.198)	(0.234)	(0.020)	(0.020)	(0.024)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	5.08	5.08	5.08	0.304	0.304	0.304
Fit statistics						
Observations	4,884	4,836	3,141	4,968	4,917	3,177
R^2	9.34×10^{-5}	0.017	0.083	0.0008	0.011	0.039

Notes: Standard errors are heteroskedasticity-robust. The sample restricts to individuals who report NC status and have real wages between 3 and 200 in 2017. Basic controls include sex, education, tenure, and potential experience. Advanced controls further add industry and occupation fixed effects, ASVAB percentile, and firm size. Tenure controls not included in the job mobility panel. All regressions are weighted so as to be nationally representative. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

for each survey year c we construct a "clean" dataset containing only those who we observe first signing a non-compete agreement in year c (the treatment group) and an associated c-specific control group. A challenge to constructing a suitable control group is that we do not observe within-job variation in the treatment variable. This implies that any worker who moves from $NC_{i,t-1} = 0$ to $NC_{i,t} = 1$ (and vice versa) is necessarily a job mover in our data, so that our treatment is perfectly correlated with job mobility.[30] Due to the well-known fact that job mobility is associated with changes in labor market outcomes (such as wage growth), the estimated coefficients from equation (6) will be picking up both the effects of NCs and the effects of job mobility. Our procedure allows us to address this concern by flexibly defining the control group for each cohort, allowing us to compare movers to movers. Specifically, for each cohort c, both the treated and the control group consist of workers who transitioned to a new job with known NC status between year c and the previous survey year. The treated are those who we observe first signing an NC in year c, and the control group are the job movers in year c who we never observe signing an NC over the sample period (the never-treated).

We focus on years 2013-2021 and cohorts $c \in \{2015, 2017, 2019, 2021\}$ to understand the effects of non-compete agreements over a reasonable time horizon without pushing our assumption that NC status is invariant over the employment relationship too far.[31] We then "stack" the data for each cohort c and estimate

$$Y_{itc} = \alpha_{ic} + \lambda_{tc} + \sum_{k \in \{-6,-4,0,2,4,6\}} \beta^k d_{i,t-k,c} + \varepsilon_{itc} \tag{6}$$

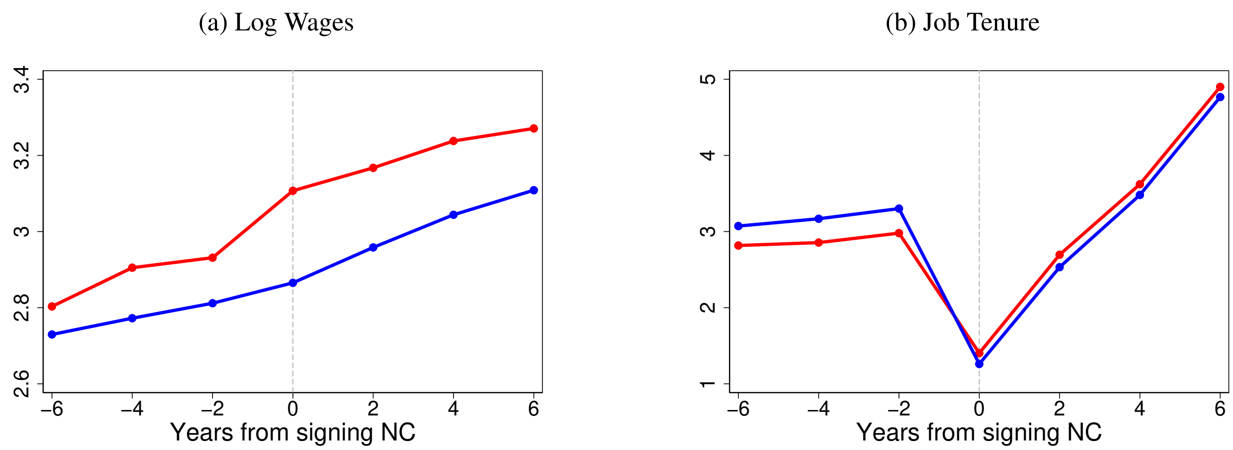
where Y_{itc} is the outcome variable of interest and d_{itc} is an event indicator equal to 1 for all $t \geq c$ and workers i who we first observe signing a non-compete agreement at time $t = c$.

Our approach has a number of advantages. First, the inclusion of cohort-specific individual (α_{ic}) and time (λ_{tc}) fixed effects ensures that workers who were first treated in other years are never implicitly used as the control group, bypassing the "bad comparison problem" of the standard TWFE estimator (Baker, Larcker, and Wang 2022). Second, while other difference-in-difference approaches also restrict to the never-treated (e.g., Callaway and Sant'Anna (2021)), our approach permits the additional flexibility needed to restrict the control group to job movers, allowing us

[30] This problem is not simply due to the structure of the NLSY97 (which does not ask about NC status across survey years within the same job), but due to the standard timing of NC signage. NC's are most often signed upon starting a new job (Starr, Prescott, and Bishara (2021)), so we would expect very little within-job variation in the treatment variable and a high correlation with job mobility in either case.

[31] We note that NC status is unobserved for jobs ending prior to 2017. This implies that treated workers in cohort 2015 are necessarily job stayers between 2015 and 2017. We impose the same restriction for the control group. Unobserved pre-2017 NC status also implies that some of the jobs held prior to year c for both the treatment and control group may have had a non-compete agreement. We confirm our results are not sensitive to restricting to later cohorts (that are unaffected by this issue), giving confidence that this issue is not of consequence (see section 4.3).

Figure 4: Means Relative to Event Time



Note: Figure reports means of log wages and job tenure aggregated over all cohorts in the stacked data. See text for data construction. Red lines correspond to NC signers (treatment group) and blue lines to non-NC signers (control group).

to isolate the effect of NC signage from job mobility. Finally, the inclusion of individual-cohort fixed effects also addresses the concern that those who sign non-compete agreements have better time-invariant unobservable characteristics by focusing on within-worker changes. Similar stacked research designs have been used in a number of contexts, including firm responses to liability risk (Gormley and Matsa (2011)), the effects of minimum wage increases (Cengiz et al. 2019), and the effects of state-level changes to NC enforceability (Johnson, Lavetti, and Lipsitz (2023)), among others.

For ease of presentation, and to generate more precise estimates, we also estimate the effects of NC's aggregated over post-treatment time periods by replacing the dynamic indicator variables $d_{i,t-k,c}$ in equation (6) with a single treatment indicator for the post-treatment period

$$Y_{itc} = \alpha_{ic} + \lambda_{tc} + \beta^{Agg} d^{Agg}_{i,t,c} + \varepsilon_{itc} \tag{7}$$

where $d^{Agg}_{i,t,c}$ equals one for treated individuals i in years $t \geq c$ and β^{Agg} is our coefficient of interest. In all of our estimates we cluster our standard errors at the individual level.

Equations (6) and (7) form our main empirical specifications. Here, we have two main identifying assumptions. First, for each cohort c, NC signers and those in their cohort-specific control group (the set of individuals who never sign an NC between 2013 and 2021 and who also move jobs in year c) have similar trends in potential outcomes. We test this assumption by inspecting the pre-trends in the event study and find they are not significantly different from zero. We also plot the means of our main outcome variables and find that these groups have similar pre-trends (Figure 4). Secondly, we assume that the non-compete dummy variable d_{itc} is uncorrelated with the error term. Under these assumptions, the coefficients $\hat{\beta}^k, k \geq 0$ represent the causal effect of signing a non-compete agreement.

In Section 4.3 we explore a number of robustness exercises, such as controlling for proxies of firm productivity, restricting to more recent cohorts, and alternative empirical specifications, such as the standard two-way fixed effects estimator or using the later-treated as the control group. We leave the details of these exercises and their results to 4.3, but note here that our results are highly robust in each case.

4 Results

4.1 The Effect of Signing an NC on Labor Market Outcomes

We present our main results in figure 5. Panel (a) plots the dynamic wage effects following equation (6), finding that NC's raise wages sharply at the time of signing but that this effects declines monotonically with time. At the time of signing, wages are 9.9 log points higher relative to the control group but only 4.7 log points higher 6 years later. This result indicates that while NC signers enjoy an upfront wage premium, their annual wage growth is about 1 log point lower. Remarkably, this statistic is almost identical to Shi (2023)'s descriptive wage patterns for executives.

Panel (b) likewise reports our dynamics estimates for job tenure. Consistent with NC's reducing job mobility, we find that NC's increase mean job tenure and that this effect accumulates over time. Six years later NC signers have job tenure about 0.5 years (6 months) longer.

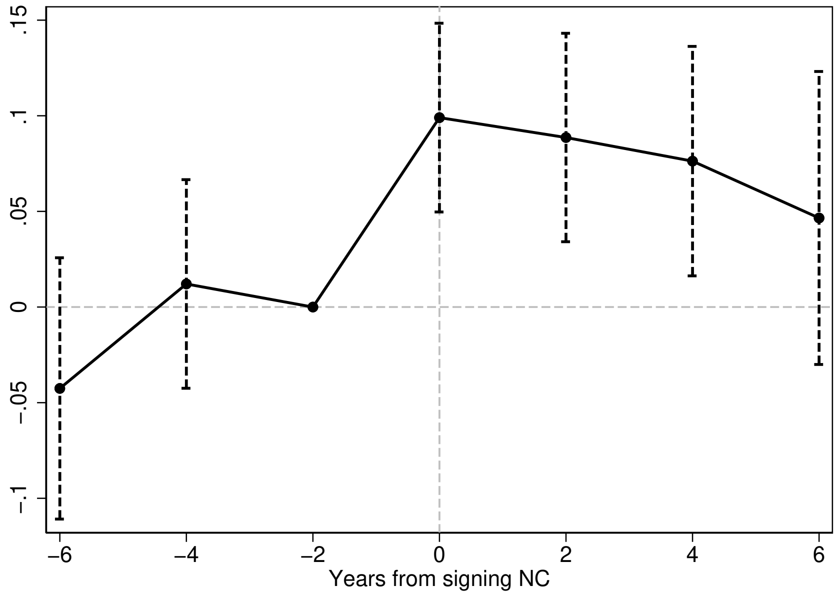
In Table 3 we report aggregated estimates from equation (7) for log wages, job tenure, and an indicator for job mobility. Consistent with our theory and Figure 5 we find that non-compete agreements increase wages (by 9.4 log points), increase job tenure (by about 0.29 years or 3.4 months) and decrease the bi-annual rate at which workers change main employers (by 3.6 percentage points). All of these estimates are both statistically and economically significant.

To interpret the magnitude of these estimates, note that a wage increase of 9.4 log points is almost the same expected wage growth that the average worker would expect to receive over two years (see Table 2) or from one additional year of schooling (Card (1999)). In terms of mobility, NC's increase tenure by about 6% and decrease the instances of bi-annual changes in main employer by about 12% relative to the 2017 average. Surprisingly, we find no effect on the rate at which workers move to another employer within the same industry, though we note that the sample size of treated within-industry job movers is quite small.

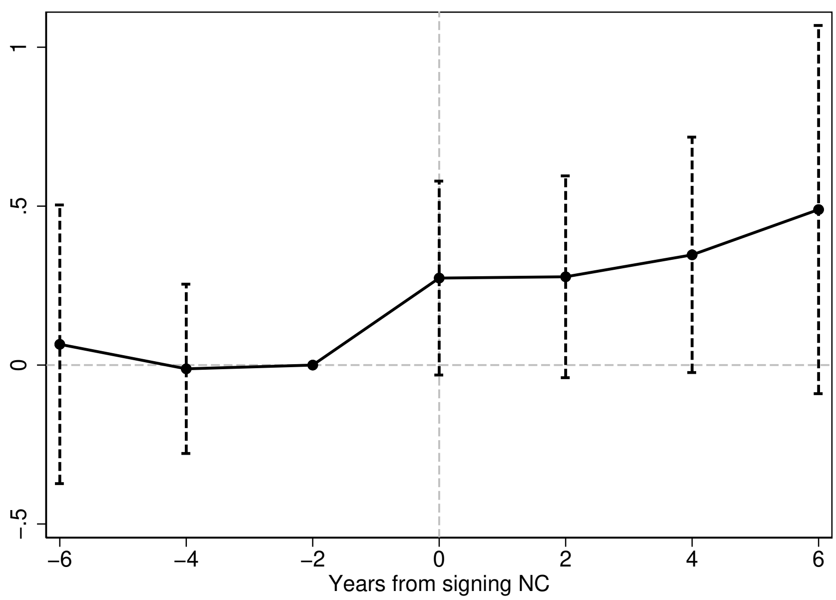
Having established that NC's raise wages and reduce job mobility, the next panel explores whether NC's cause other changes to a workers labour market situation, including hours worked, an indicator for self-reported job dissatisfaction, an indicator for formal training, and an indicator

Figure 5: The Dynamic Effects of Signing an NC

(a) Log Wages



(b) Job Tenure



Note: Estimates are from stacked difference-in-differences estimation (equation (6) in the text) over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers who never held a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

Table 3: The Aggregate Effects of Signing an NC

(a) Wages and Mobility

	(1) Log Wages	(2) Tenure	(3) Change Main Emp.	(4) Change Main Emp., Within Ind.
Treat × Post	0.094***	0.287*	-0.036*	0.006
	(0.022)	(0.126)	(0.016)	(0.018)
Observations	22394	22040	21614	22004
Dependent Variable Mean	2.888	2.692	0.458	0.165
Unique Treated Workers	682	680	681	679
Unique Control Workers	3300	3263	3296	3285
R^2	0.770	0.588	0.598	0.388

(b) Other Outcomes

	(1) Hours/Wk	(2) Job Dissat.	(3) Training	(4) Employer-Paid Training
Treat × Post	1.025	0.026	-0.023	-0.022
	(0.624)	(0.016)	(0.017)	(0.015)
Observations	22159	17686	22394	22394
Dependent Variable Mean	37.950	0.072	0.130	0.078
Unique Treated Workers	682	636	682	682
Unique Control Workers	3293	3039	3300	3300
R^2	0.511	0.401	0.459	0.455

Notes: This table reports estimates from stacked difference-in-differences estimation, aggregated over post-treatment years, over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers who never held a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. Standard errors are clustered by worker and reported in parenthesis. Significance codes: ***: 0.01, **: 0.05, *: 0.1.

for whether that training was paid for by the employer. We find that NC signers work about 1 more hour per week and are more likely to report being dissatisfied with their job. However, we note that these results are not statistically significant at the 10% level.

Somewhat surprisingly, we also find that non-compete agreements have no impact on the incidence of employer-provided training. This result runs contrary to the theoretical expectation that non-compete agreements mitigate the hold-up problem, thereby *increasing* employer-provided investments. There are two ways to rationalize this result. It could be the case that firms use NC's as a way to avoid outside competition without investing in worker productivity. But it could equally be the case that non-compete agreements do raise employer provided investments, just in ways that are difficult to measure. For example, firms may invest in their workers through informal on-the-job training where workers learn and develop skills through the tasks they are assigned and interactions with their team. Since investment in human capital is unobserved, it is not clear to what extent this variable accurately captures the total investments firms are making in their workers. The fact that less than 10% of workers report receiving any kind of formal training in 2017 (see Table 2) suggests this may not be a holistic measure of firm investment. Moreover, formal training is reported as an indicator (extensive margin), which abstracts from any notion of the intensity of training (intensive margin).

Since measuring firm investment directly is potentially difficult, our theory suggests an alternative: That one can infer the effects of NC's through observed wage dynamics. If firms do not respond by investing in worker human capital, one should observe a sharp increase in wages followed by a sharp decline in wage growth. If firms do invest in human capital and share some of those rents with the worker, then the NC signers wage trajectory will be less negative and perhaps even flat or increasing relative to those without an NC.

4.2 Heterogeneity by Worker Demographics

A policy relevant question is whether these average effects are experienced equally by all workers. For example, one may be concerned that while NC's raise wages for workers on average, they harm certain subgroups. This is especially relevant in light of the discussion and enactment of banning non-compete agreements for low-skill or low-wage worker (e.g., Lipsitz and Starr (2022)).[32]

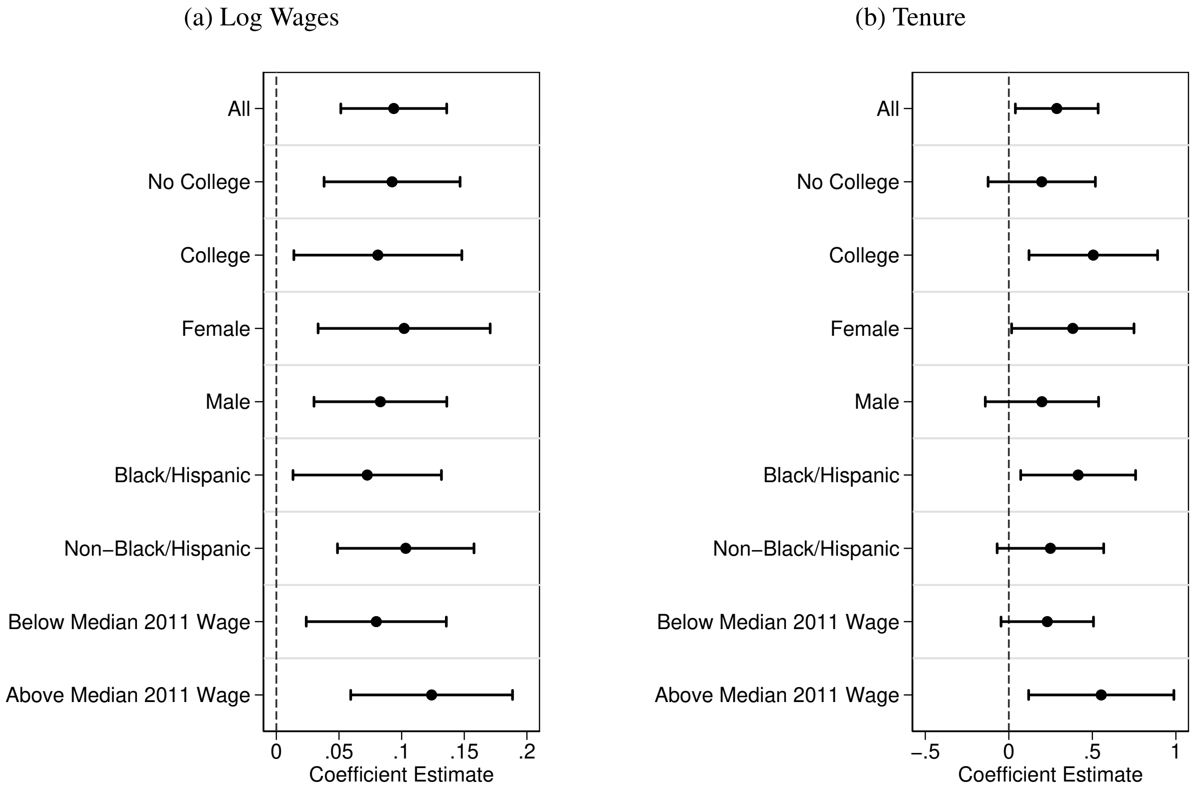
Figure 6 reports aggregated estimates for different subgroups of workers, grouping workers by education, sex and race. In light of the emphasis in the political sphere surrounding NC's and low-wage workers, we also group workers by whether they had above/below median wages in 2011 (just prior to our sample window).

The estimated wage effects are remarkably stable across subgroups, ranging from 7.3 log points (Black/Hispanic) to 12.4 log points (workers with above median pre-sample wages). While the estimated tenure effects are more pronounced for certain subgroups, most notably comparing by education or pre-sample wages, these tables give little evidence that non-compete agreements have systematically adverse implications for certain subgroups.

However, these aggregate wage effects hide important dynamics. In Figure 7 we run our dynamic event study by worker subgroup and find that, while NC's lead to higher wage *levels* for all types of workers over the first 6 years, there are systematic differences in the relative wage *trajectories* across subgroups. For example, male workers tend to have a larger up front wage premium than females, but much lower subsequent wage growth. More concerning is that we find strong negative wage growth effects for workers with no college, low pre-sample wages, or who are black/hispanic. In contrast, college-educated, high-wage and non-black/hispanic workers have

[32] Articles in the popular press also often claim that non-compete agreements are exploitative for low-income workers. See for example this article about non-compete agreements in the fast-food industry: https://www.nytimes.com/2014/10/15/upshot/when-the-guy-making-your-sandwich-has-a-noncompete-clause.html

Figure 6: The Effect of Signing an NC: Heterogeneity Across Worker Groups



Note: Coefficient estimates are from stacked difference-in-differences estimation, aggregated over post-treatment years, over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers who never held a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

flat (or even increasing) wage profiles.

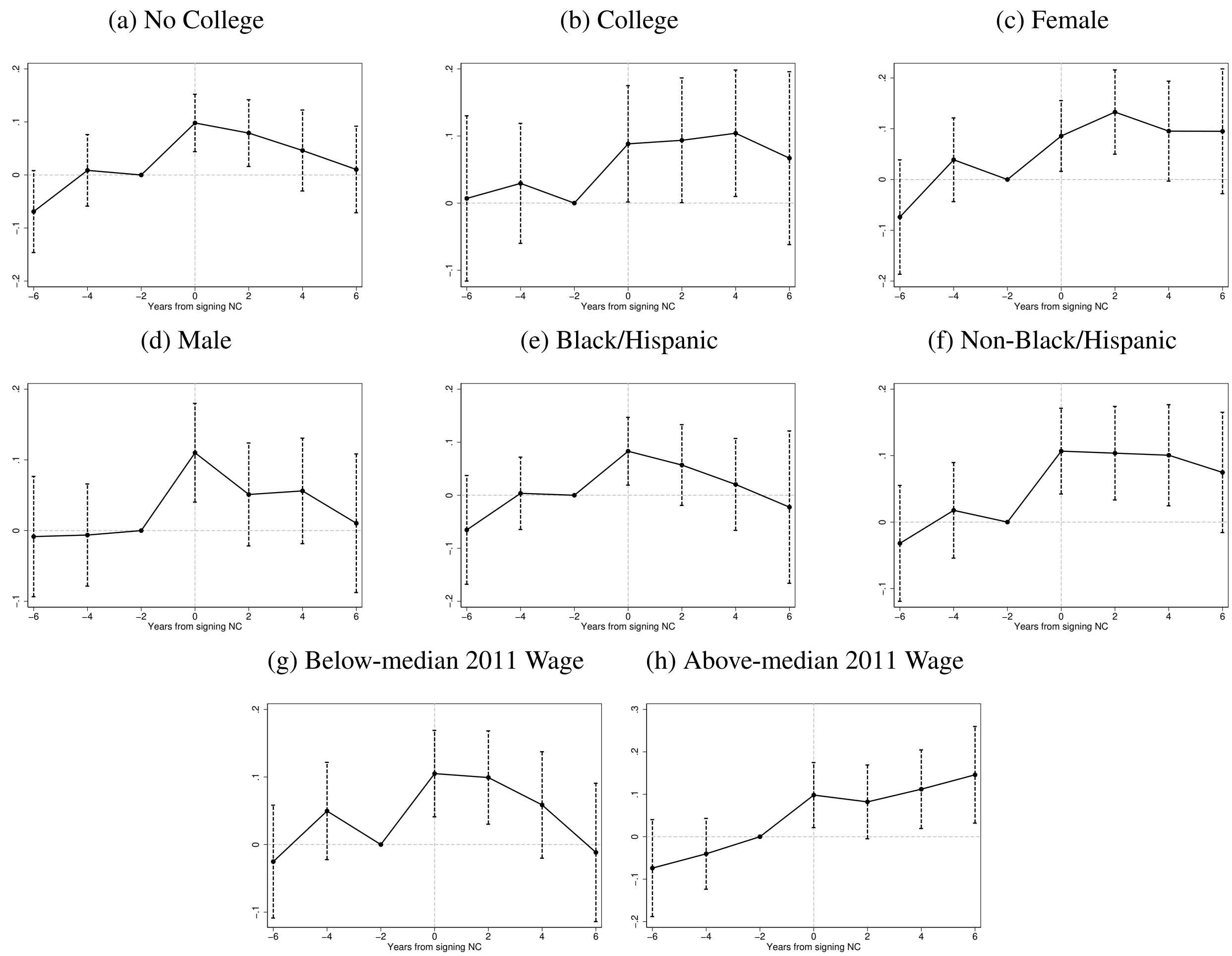
4.3 Robustness

4.3.1 Job productivity as an omitted variable

Our main identifying assumption is that the error term in equations 6 and 7 are uncorrelated with the treatment dummy. One concern with this assumption is that, while we may be controlling for systematic differences in NC usage across *workers*, we are not doing so across *jobs*. NC usage could be correlated with firm productivity, introducing an omitted variable problem.

While our worker-level data prevents us from controlling for firm productivity directly, we re-estimate our aggregate effects from Table 3 sequentially controlling for industry, occupation, and firm size decile fixed effects. The industry and occupation fixed effects control for the possibility that non-compete agreements are used more intensively in certain high-paying sectors. Firm size is

Figure 7: The Dynamic Wage Effects of Signing an NC by subgroup



Note: Estimates are from stacked difference-in-differences estimation (equation (6) in the text) over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers who never held a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

strongly correlated with firm productivity in standard labor market models (e.g., Manning (2013)), and we confirm it is strongly correlated with wages in the NLSY97 data (see Figure A2). We plot the results in Figure A3, where the black circle markers in Figure A3 report our baseline estimates, while the next three lines report estimates sequentially adding industry, occupation, and then finally firm size decile fixed effects.

We find that controlling for these detailed job-level characteristics hardly changes our results. The first panel reports our wage estimates. We find that our estimated wage effect falls only slightly, from 9.4 to 8.5 log points in the most saturated model. The fact that our results hardly change even with this rich set of controls gives confidence that the potential omitted variable problem (of NC's being correlated with other job-level characteristics that positively effect wages) is not driving our wage estimates.

In the second and third panel we see that our job mobility estimates are also robust to controlling for these job-level characteristics. The tenure effect only changes from 0.29 to 0.24 years and the effect on the rate at which workers change main employers changes from -3.6 to -4.1 percentage points. Overall, we conclude that our baseline estimates are unlikely to be driven by unobserved job-level characteristics.

4.3.2 Data limitations on NC usage

Another possible objection is that our assumptions on the timing of NC signage is invalid. As discussed in Section 3.1, since we only observe NC status starting in 2017, we assume that the NC was signed at the beginning of the employment relationship, imposing that NC status does not change with job tenure. A potential violation of this would be if non-compete clauses were tied to promotions or raises within the same job. Starr, Prescott, and Bishara (2021) find that this happens very infrequently, with almost all non-compete agreements being signed at the beginning of the employment relationship.

A related data limitation is the fact that, as discussed in Section 3.2, we do not observe NC status in the pre-period for cohorts 2015 and 2017. To address this issue formally, we sequentially remove cohorts 2015 and 2017 from the sample and observe how our estimates change. Specifically, we re-calculate our aggregate estimates (1) using only cohorts 2017-2021, and (2) using only cohorts 2019-2021. In the latter case, restricting to these years and cohorts means we only use observations for which we observe NC status before and after the job move, providing more transparent estimates. The results from this exercise are given in Figure A4. Reassuringly, we find that our wage and job mobility rate estimates are not significantly different and if anything increase as we remove cohorts 2015 and 2017. Similarly, the point estimates for tenure are nearly identical. This exercise demonstrates that our results are not driven by measurement error in NC status and also gives confidence that our results are quite stable across cohorts.

Finally, non-compete agreements are often bundled with other employment restrictions, such as non-disclosure agreements (NDAs) (Balasubramanian, Starr, and Yamaguchi (2024)). To the extent this is true in our data, we acknowledge that our estimates identify the causal effects of all of these restrictions simultaneously rather than NCs in isolation.

4.3.3 Alternative estimation methods: Later-treated and TWFE

Even though we control for individual fixed effects, one may still have concerns about selection. For example, workers who sign NC's may have higher returns on employer-provided investment, and this might not be fully captured by an additively separable fixed effect. To address this, we re-estimate equation 6, this time defining the control group for each cohort c as job-movers at time c who do not have an NC at time c but who do sign an NC in a later survey year $t > c$ (later-treated job-movers). In this comparison, we compare NC signers to NC signers, exploiting only variation in the timing of signage. We plot the results in Figure A5.

Relative to our baseline estimates (Figure 7), we observe a larger initial wage effect but stronger negative wage growth in the post-period. Given that the later-treated are necessarily workers who move into NC contracts in the post-period, we would expect them to have larger wage increases and therefore the sharper negative wage growth in this alternative set-up in unsurprising. Although the wage effects after time zero are difficult to interpret, the fact that the initial wage effect is even stronger in this set-up gives assurance that our main estimates are not being driven by some fundamental difference between those who do and do not sign non-compete agreements.

Finally, for completeness we present estimates from the standard two-way fixed effect estimator (TWFE). Specifically, we estimate

$$w_{it} = \alpha_i + \lambda_t + \beta^{Agg} d_{it}^{Agg} + \varepsilon_{it} \tag{8}$$

where w_{it} is log wages for individual i in year t and d_{it}^{Agg} is an indicator variable equal to one beginning in the first year the worker signs an NC, and zero otherwise. The main differences between equation 8 and equation 7 is that here the control group is effectively any worker not currently holding an NC, as opposed to restricting to the never-treated job movers.

We report these estimates for the entire sample and across subgroups and find nearly identical results (see Figure A6). The aggregate wage effect of signing an NC is 9.5 log points (relative to 9.4 in our baseline case, see Table 3) and there are similar patterns across subgroups. Specifically, as in our baseline estimates worker with a college degree, who are non-black/hispanic or who had above-median wages in 2011 appear to experience larger wage gains from signing and NC than their counterparts.

5 Conclusion

Economists have long been interested in the factors that promote human capital development. Schooling is often considered as an important determinant of an individual's productivity, but there are many skills that can only be learned on the job. The market for employer-provided training, however, suffers a well-known failure: employers do not have an incentive to provide transferable skills if they later need to compensate workers for their increased productivity (e.g. Becker 1962; Acemoglu and Pischke 1999). In this paper, we develop a model illustrating how non-compete agreements can be used to address this market failure, at the risk of generating ex-post allocative inefficiencies. We explicitly characterize the conditions under which workers will sign a non-compete agreement, an important omission in the theoretical literature to date.

While previous empirical research has examined the causal effects of non-compete regulation, we study the causal effects of signing a non-compete agreement on various labor market outcomes. As expected, we find that signing a non-compete agreement lowers job mobility, raising job tenures by 6% and lowering rates of job-to-job transitions by 12%. In contrast to the negative wage effects of stricter enforcement of non-compete agreements, we show that signing a non-compete agreement raises wages by 9% within one year and that these positive effects persist up to six years after the agreement is signed. Workers across socio-economic backgrounds gain higher wages from signing non-compete agreements, consistent with our theoretical framework that non-compete agreements are only used if they are mutually beneficial to workers and firms. Our findings highlight an important distinction between signing a non-compete agreement and broader enforcement policies, similar to the well-studied union literature where the effects of Right-to-Work laws differ from the effects of joining a union.

Overall, we find that signing a non-compete agreement leads to higher career earnings, aligning with both a compensating wage differential and returns to greater human capital accumulation. However, despite theoretical predictions that non-compete agreements should encourage employer investment in worker training, we find no evidence of increased formal training. This observation raises the question of whether non-compete signers become more productive through less observable channels, such as receiving more mentoring or building stronger relationships with managers. Understanding these mechanisms could provide deeper insight into how non-compete agreements influence long-term worker productivity and career trajectories.

A Appendix

A.1 Proposition 1

When $r > \rho$, we solve for the efficient investment level as follows:
$i_s^* = \text{argmax } E(S) = -c(i_s) + p_s \cdot ri_s + (1-p_s)\rho i_s + (1-p_s) \cdot E(v \mid v \geq (r-\rho)i_s)$
We know that $(1-p_s)E(v \mid v \geq (r-\rho)i_s) = \int_{(r-\rho)i_s}^{\infty} \frac{1}{\sigma\sqrt{2\pi}} exp \frac{-(lnt-\mu)^2}{2\sigma^2} dt$
Denote $v(i_s) = (r-\rho)i_s$.
Assume $\phi(\frac{\ln v(i_s)-\mu}{\sigma}) \approx 0, \phi(ln(v(i_s))) \approx 0$. Take the FOC with respect to i:

$$-i_s + rp_s + \frac{\partial p_s}{\partial i_s} ri_s + (1-p_s)\rho - \frac{\partial p_s}{\partial i_s}\rho i_s - (r-\rho)\phi(ln(v(i_s))) = 0$$
$$-i_s + rp_s + \frac{r}{\sigma}\phi(\frac{\ln v(i_s)-\mu}{\sigma}) + (1-p_s)\rho - \frac{\rho}{\sigma}\phi(\frac{\ln v(i_s)-\mu}{\sigma}) - (r-\rho)\phi(ln(v(i_s))) = 0$$
$$rp_s + (1-p_s)\rho \approx i_s^*$$

$$\frac{\partial i_s^*}{\partial r} = (r-\rho)\frac{\partial p_s}{\partial r} + p_s$$
$$\frac{\partial p_s}{\partial r} = \phi(\frac{\ln v(i_s)-\mu}{\sigma}) * \frac{1}{(r-\rho)\sigma}$$
$$\phi(\frac{\ln v(i_s^*)-\mu}{\sigma}) \approx 0$$
$$\implies \frac{\partial i_s^*}{\partial r} \approx p_s > 0$$

Likewise,

$$\frac{\partial i_s^*}{\partial \rho} = (r-\rho)\frac{\partial p_s}{\partial \rho} + (1-p_s)$$
$$\implies \frac{\partial i_s^*}{\partial \rho} \approx 1 - p_s > 0$$

A.2 Proposition 2

By solving for the value that optimizes Equation 3, we arrive at $\widetilde{i_0} = \widetilde{p_0}r + \frac{\partial\widetilde{p_0}}{\partial\widetilde{i_0}}(r\widetilde{i_0} - w_0)$, where we denote $\widetilde{p_0} = \Phi(\frac{\ln(w_0-\rho\widetilde{i_0})-\mu}{\sigma}) \times 1(w_0 \geq \rho\widetilde{i_0})$. Since $\frac{\partial\widetilde{p_0}}{\partial\widetilde{i_0}} \leq 0$, $\widetilde{i_0} \leq \widetilde{p_0}r = \Phi(\frac{\ln(w_0-\rho\widetilde{i_0})-\mu}{\sigma})r$. Meanwhile, the firm is earning a non-negative profit, resulting in $w_0 < ri_0^*$. Since Φ is a strictly increasing function, we have $i_0^* < r\Phi(\frac{\ln(w_0-\rho i_0^*)-\mu}{\sigma}) < r\Phi(\frac{\ln(r-\rho)i_0^*)-\mu}{\sigma})$. From Section 2.2, we know the socially efficient investment level is $i_s^* = rp_s + \rho(1-p_s)$, which means that $i_s^* > rp_s = r\Phi(\frac{\ln(r-\rho)i_s^*)-\mu}{\sigma})$. Consider the fixed point of the function $f(x) = r\Phi(\frac{\ln(r-\rho)x)-\mu}{\sigma})$. The previous inequalities imply that $i_0^* < x < i_s^*$, which proves our case.

Now, we turn to prove $p_0^* < p_s$. If a non-compete agreement is not signed, the worker will not quit if and only if $v \leq w_0 - \rho i_0^*$. For the firm to earn profits, the wage must be less than output: $w_0 < ri_0$. As a result, trade will occur when $v \leq (r-\rho)i_0^*$. For the planner, it is efficient to trade if $v \leq (r-\rho)i_s^*$. Since we have proven that $i_0^* < i_s^*$, it now follows that $p_0^* < p_s^*$.

A.3 Proposition 3

We will prove that $w_1 \geq w_0$, which will imply that $i_1^* \geq i_0^*$ and $p_1 \geq p_0$. First, as a corollary, we will prove that all compensation will be in the form of the wage and none will be in the form of the bonus. I.e. $B_1 = B_0 = 0$.

When a non-compete is not signed, we solve for equilibrium wage and investment first. From Equation 3, we can arrive at $\widetilde{i_0} = \widetilde{p_0}r + \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}(r\widetilde{i_0} - w_0)$. Now since $\frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}} = -\rho \cdot \frac{1}{(w_0 - \rho\widetilde{i_0})\sigma} \cdot \phi(\frac{\ln(w_0-\rho\widetilde{i_0})-\mu}{\sigma}) < 0$, we know $\widetilde{i_0} < \widetilde{p_0}r$. Now onto the contracting stage, given that the worker's expected payoff binds at μ_0, we find the wage that optimizes firm profit:

$$
\begin{aligned}
E(\pi_0^W) &= B_0 + \widetilde{p_0}w_0 + (1-\widetilde{p_0})E[v|v \geq w_0 - \rho\widetilde{i_0}] + (1-\widetilde{p_0})\rho\widetilde{i_0} = \mu_0 \\
E(\pi_0^F) &= -c(\widetilde{i_0}) - B_0 + \widetilde{p_0}(r\widetilde{i_0} - w_0) \\
\implies E(\pi_0^F) &= -c(\widetilde{i_0}) + \widetilde{p_0}r\widetilde{i_0} - \mu_0 + \int_{w_0-\rho\widetilde{i_0}}^{\infty} \phi(ln(t))\,dt + (1-\widetilde{p_0})\rho\widetilde{i_0}
\end{aligned}
$$

Now we take derivative $\frac{\partial E(\pi_0^F)}{\partial w_0}$. Note that $\widetilde{i_0}$ is a function of w_0 and $\widetilde{p_0}$ is a function of $\widetilde{i_0}$ and w_0.

$$
\frac{dE(\pi_0^F)}{dw_0} = -\widetilde{i_0} \cdot \frac{d\widetilde{i_0}}{dw_0} + \frac{d\widetilde{p_0}}{dw_0} \cdot r\widetilde{i_0} + \widetilde{p_0}r\frac{d\widetilde{i_0}}{dw_0} - \phi(ln(w_0 - \rho\widetilde{i_0})) + (1-\widetilde{p_0})\rho\frac{d\widetilde{i_0}}{dw_0} - \frac{d\widetilde{p_0}}{dw_0}\rho\widetilde{i_0}
$$

$$
\text{where } \frac{d\widetilde{p_0}}{dw_0} = \frac{\partial \widetilde{p_0}}{\partial w_0} + \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}} \cdot \frac{d\widetilde{i_0}}{dw_0}
$$

Assuming $\phi(ln(w_0 - \rho\widetilde{i_0})) \approx 0$,

$$
\frac{dE(\pi_0^F)}{dw_0} = \overbrace{(-\widetilde{i_0} + \widetilde{p_0}r + (1-\widetilde{p_0})\rho)}^{>0 \text{ as } \widetilde{i_0}<\widetilde{p_0}r} \cdot \frac{d\widetilde{i_0}}{dw_0} + \frac{d\widetilde{p_0}}{dw_0} \cdot \overbrace{(r\widetilde{i_0} - \rho\widetilde{i_0})}^{>0 \text{ as } r>\rho}
$$

To determine the sign of $\frac{dE(\pi_0^F)}{dw_0}$, we need to know the signs of $\frac{d\widetilde{i_0}}{dw_0}$ and $\frac{d\widetilde{p_0}}{dw_0}$. To determine how investment responds to the wage, take the total derivative of the following: $\widetilde{i_0} = \widetilde{p_0}r + \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}(r\widetilde{i_0} - w_0)$

$$
\begin{aligned}
\frac{d\widetilde{i_0}}{dw_0} &= \frac{d\widetilde{p_0}}{dw_0}r + \overbrace{\frac{\partial^2 \widetilde{p_0}}{\partial \widetilde{i_0}\partial w_0}}^{\approx 0}(r\widetilde{i_0} - w_0) + \overbrace{\frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}}^{<0}(r\frac{d\widetilde{i_0}}{dw_0} - 1) \\
\frac{d\widetilde{i_0}}{dw_0} &= r(\frac{\partial \widetilde{p_0}}{\partial w_0} + \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}} \cdot \frac{d\widetilde{i_0}}{dw_0}) - \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}
\end{aligned}
$$

$$
\implies \overbrace{(1 - r\overbrace{\frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}}^{<0})}^{>0} \cdot \frac{d\widetilde{i_0}}{dw_0} = r\overbrace{\frac{\partial \widetilde{p_0}}{\partial w_0}}^{>0} - \overbrace{\frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}}}^{<0}
$$

This implies that $\frac{d\widetilde{i_0}}{dw_0} > 0$. Assume $\frac{\partial \widetilde{p_0}}{\partial w_0} >> \frac{\partial \widetilde{p_0}}{\partial \widetilde{i_0}} \cdot \frac{d\widetilde{i_0}}{dw_0}$, so that $\frac{d\widetilde{p_0}}{dw_0} > 0$. Thus $\frac{dE(\pi_0^F)}{dw_0} > 0$ for all w_0 that satisfies the utility constraint. Given this, the profit is maximized at the higher bound for w_0, which happens when we have $B_0 = 0$.[33]

The equilibrium w_0^* satisfies the following equation:

$$p_0^* w_0^* + (1-p_0^*)E[v|v \geq w_0^* - \rho i_0^*] + (1-p_0^*)\rho i_0^* = \mu_0$$

With this, we can also solve for the investment i_0^*. Now that we have the first period wage and optimal investment solved implicitly, we can see how i_0^* changes with ρ. Taking the total derivative of the function above with respect to ρ, we have

$$\frac{dp_0^*}{d\rho} w_0^* + p_0^* \cdot \frac{dw_0^*}{d\rho} - \phi(ln(w_0^* - \rho i_0^*))i_0^* - \frac{dp_0^*}{d\rho}\rho i_0^* + (1-p_0^*)i_0^* + (1-p_0^*)\rho \frac{di_0^*}{d\rho} = 0$$
$$\implies (1-p_0^*)[i_0^* + \frac{\partial i_0^*}{\partial \rho}\rho] + p_0^* \frac{\partial w_0^*}{\partial \rho} \approx 0 \tag{9}$$

Now to learn the sign for $\frac{di_0^*}{d\rho}$ and $\frac{dw_0^*}{d\rho}$, we can turn to the optimal investment function $i_0^* = p_0^* r + \frac{\partial p_0^*}{\partial i_0^*}(ri_0^* - w_0^*)$ Taking the total derivative of the function with respect to the variable ρ, we have

$$\frac{di_0^*}{d\rho} = r \cdot (\frac{\partial p_0^*}{\partial w_0^*} + \frac{\partial p_0^*}{\partial i_0^*} \cdot \frac{di_0^*}{dw_0^*}) + \overbrace{\frac{\partial^2 p_0^*}{\partial i_0^* \partial \rho}}^{\approx 0}(i_0^* - w_0^*) + \frac{\partial p_0^*}{\partial i_0^*}(r\frac{di_0^*}{d\rho} - \frac{dw_0^*}{d\rho})$$
$$\implies \overbrace{(1 - 2\overbrace{\frac{\partial p_0^*}{\partial i_0^*}}^{<0})}^{>0}\frac{di_0^*}{d\rho} = \overbrace{(r \cdot \overbrace{\frac{\partial p_0^*}{\partial w_0^*}}^{>0} - \overbrace{\frac{\partial p_0^*}{\partial i_0^*}}^{<0})}^{>0} \cdot \frac{dw_0^*}{d\rho}$$

From the optimal investment equation, we can see that $\frac{di_0^*}{d\rho}$ and $\frac{dw_0^*}{d\rho}$ have the same sign. Thus we can denote $\frac{di_0^*}{d\rho} = k\frac{dw_0^*}{d\rho}$ with $k > 0$.
Plugging into Equation 7, we have

$$\left(\overbrace{(1-p_0^*)\rho + p_0^* k}^{>0}\right)\frac{di_0^*}{d\rho} \approx -(1-p_0^*)i_0^* < 0$$

We have successfully proven that $\frac{di_0^*}{d\rho} < 0$, which implies that $i_0^* < i_1^*, \forall \rho > 0$.

Likewise, when $\rho = 0$, we have $w_0^* = w_1^*$. And since $\frac{di_0^*}{d\rho} = \overbrace{k}^{>0}\overbrace{\frac{dw_0^*}{d\rho}}^{<0} < 0$, we also have $w_0^* < w_1^*$.

Lastly, with $\delta = 1$, trading occurs when $v \leq w_1^*$, and with $\delta = 0$, trading occurs when $v \leq w_0^* - \rho i_0^*$. From $w_1^* > w_0^*$, we have $w_0^* - \rho i_0^* < w_1^*$. Thus we have $p_0^* < p_1^*$, meaning there is a higher probability of separation when a non-compete agreement is not signed.

[33]This occurs because $\frac{\partial E(\pi_0^F)}{\partial w_0} > 0$.

A.4 Proposition 4

When $\rho > r$, we have already shown that all contracts without non-compete agreements generate zero or negative profits, so will not be used. We just need to show that there exists a contract with a non-compete agreement that generates positive profits. Such an illustration will prove that a non-compete agreement will be used.

From Equation 3, we can arrive at $\widetilde{i_1} = \widetilde{p_1}r$, where we denote $\widetilde{p_1} = \Phi(\frac{\ln w_1 - \mu}{\sigma})$. Now onto the contracting stage, given that the worker's expected payoff binds at μ_0, we find the wage that optimizes firm profit:

$$E(\pi_1^W) = B_1 + \widetilde{p_1}w_1 + (1-\widetilde{p_1})E[v|v \geq w_1] = \mu_0$$
$$E(\pi_1^F) = -c(\widetilde{i_1}) - B_1 + \widetilde{p_1}(r\widetilde{i_1} - w_1)$$
$$\implies w_1^* = \text{argmax } E(\pi_1^F) = -c(\widetilde{i_1}) + \widetilde{p_1}r\widetilde{i_1} - \mu_0 + (1-\widetilde{p_1})E[v|v \geq w_1] \text{ s.t. } B_1 \geq 0, E(\pi_1^F) \geq 0$$

Solving for the optimization problem using Karush–Kuhn–Tucker conditions, we know the profit is maximized at the higher bound for w_1, which happens when we have $B_1 = 0$ binding. The equilibrium w_1^* satisfies the following equation.[34]

$$p_1^* w_1^* + (1-p_1^*)E[v|v \geq w_1^*] = \mu_0$$

Plugging this value into the equation for $\tilde{i_1}$, we solve for $i_1^* = p_1^* r$. As long as $E(\pi_1^F(w_1^*)) \geq 0$, there will be a contract that includes a non-compete agreement. Thus when $r < \rho$, a non-compete agreement will be used.

When $r > \rho$, it is ambiguous whether the firm chooses to include a non-compete agreement in the contract. Observe that $\frac{\partial E(\pi_0^F)}{\partial \rho} = \frac{\partial p_0^*}{\partial \rho}(ri_0^* - w_0^*(\rho)) - p_0^* \frac{\partial w_0^*}{\partial \rho}$. The first term is negative while the second term is also negative, so the sign of the overall expression is ambiguous.

A Monopsonistic Model

We make several changes to develop a monopsonistic model that may better reflect the dynamics with non-compete agreements for low wage workers. First and foremost, we assume that there is a measure 1 of workers on the unit interval $[0,1]$ with different initial outside options, instead of one worker with a fixed outside option. Workers' first period outside options, denoted as μ_0^j, with j indicating different workers, are distributed log normally with mean η and variance σ^2. The firm can only choose one wage, which will determine the level of employment, in order to maximize its expected profit. We additionally assume that investment i is exogenous to simplify the model.

First, we solve the social planner's problem. The social planner in this case determines the efficient level of employment. In the last period, it is efficient to trade if and only if $v \leq (r-\rho)i$. Denote the probability that trading is efficient as p_s. In the first stage, the planner matches workers to the monopsonistic firm only if the expected surplus from the match is greater than the worker's ex-ante outside option. We denote the efficient employment level as $q_s = P(E(S) \geq \mu_0^j)$, where

$$E(S) = -c(i) + p_s ri + (1-p_s)\rho i + (1-p_s) \cdot E(v \mid v \geq (r-\rho)i_s)$$

Now we solve the firm's decisions. In the last period, the worker will not quit so long as $w_\delta \geq v + \rho i(1-\delta)$. We denote the probability of trade as p_δ, which does not depend on the identity of the worker. Now in the first period, the firm chooses the wage to maximize its profit. Given a wage w_δ, we denote the proportion of workers agreeing to the contract as $q_\delta = P(E(\pi_\delta^W) \geq \mu_0^j)$. Thus, the firm chooses the wage

[34] It is assumed there is a single solution to the equation below.

that maximizes the following equation:

$$E(\pi_\delta^F) = q_\delta[-c(i) + p_\delta(ri - w_\delta)]$$

Proposition A1: Employment without a non-compete agreement is less than the socially efficient level ($q_0 \leq q_s$).
Proof. Given that $q_0 = P(E(\pi_0^W) \geq \mu_0^j)$ and $q_s = P(E(S) \geq \mu_0^j)$, we only need to show that $E(\pi_0^W) \leq E(S)$ in order to prove $q_0 \leq q_s$.

$$E(\pi_0^W) = p_0^* \cdot w_0^* + (1 - p_0^*)\rho i + (1 - p_0) \cdot E(v \mid v \geq (w_0^* - \rho i)$$

In any contract, we know $w_0^* \leq ri$, since the firm must earn non-negative profits ex-post. Thus $w_0 - \rho i \leq (r - \rho)i$, which means $p_0^* \leq p_s^*$. In addition, the firm chooses wages so that it earns non-negative profits ex-post: $-c(i) + p_0(ri - w_0) \geq 0$.
Now we can compare $E(S)$ with $E(\pi_0^W)$.

$$\begin{aligned}
E(S) - E(\pi_0^W) &= -c(i) + p_s ri - p_0 w_0 - (p_s - p_0)\rho i - \int_{w_0^* - \rho i}^{(r-\rho)i} \phi(ln(t))\, dt \\
&= \overbrace{-c(i) + p_0(ri - w_0)}^{\geq 0} + (p_s - p_0)(r - \rho)i - (p_s - p_0)E(v \mid v \in [w_0^* - \rho i, (r - \rho)i]) \\
&\geq \overbrace{(p_s - p_0)}^{\geq 0}[(r - \rho)i - \overbrace{E(v \mid v \in [w_0^* - \rho i, (r - \rho)i])}^{\leq (r-\rho)i}] \\
&\geq 0
\end{aligned}$$

This is the proof of the well-known result that employment under a monopsony is less than socially efficient.

Proposition A2: The effect of non-compete agreements on wages is theoretically ambiguous.
Proof. When $\delta = 0$, take FOC we have

$$\frac{dq_0^*}{dw_0^*} \cdot [-c(i) + p_0^*(ri - w_0^*)] + q_0^* \cdot [\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*] = 0 \tag{10}$$

Now to compare the wage with and without a non-compete, like in Proposition 3, we only need to check $\frac{dw_0^*}{d\rho}$. From the equation above, we can take total derivative in terms of q and assuming that $\frac{d^2 q_0^*}{dw_0^* d\rho} \approx 0$ we get

$$\frac{dq_0^*}{dw_0^*} \cdot [\frac{dp_0^*}{d\rho} \cdot (ri - w_0^*) - p_0^* \cdot \frac{dw_0^*}{d\rho}] + \frac{dq_0^*}{d\rho} \cdot [\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*] + q_0^* \cdot [-\frac{dp_0^*}{dw_0^*} \cdot \frac{dw_0^*}{d\rho} - \frac{dp_0^*}{d\rho}] = 0$$

$$\text{where } \frac{dp_0^*}{d\rho} = \frac{\partial p_0^*}{\partial \rho} + \frac{dp_0^*}{dw_0^*} \cdot \frac{dw_0^*}{d\rho} \text{ and } \frac{dq_0^*}{d\rho} = \frac{dq_0^*}{dw_0^*} \cdot \frac{dw_0^*}{d\rho} + \frac{\partial q_0^*}{\partial \rho}$$

$$\implies 2[\frac{dq_0^*}{dw_0^*} \cdot (\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*) - q_0^* \cdot \frac{dp_0^*}{dw_0^*})]\frac{dw_0^*}{d\rho} = \frac{\partial p_0^*}{\partial \rho} \cdot [q_0^* - \frac{dq_0^*}{dw_0^*} \cdot (ri - w_0^*)] - \frac{\partial q_0^*}{\partial \rho}[\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*] \tag{11}$$

From equation 1 above, since we have

$$\overbrace{\frac{dq_0^*}{dw_0^*} \cdot [-c(i) + p_0^*(ri - w_0^*)]}^{\geq 0} + \overbrace{q_0^*}^{\geq 0} \cdot [\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*] = 0 \tag{12}$$

$$\implies \frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^* \leq 0$$

Thus from equation 2 we have

$$\overbrace{2[\frac{dq_0^*}{dw_0^*} \cdot (\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*) - q_0^* \cdot \frac{dp_0^*}{dw_0^*})]}^{\leq 0} \frac{dw_0^*}{d\rho} = \overbrace{\frac{\partial p_0^*}{\partial \rho}}^{<0} \cdot [q_0^* - \frac{dq_0^*}{dw_0^*} \cdot (ri - w_0^*)] - \frac{\partial q_0^*}{\partial \rho} \overbrace{[\frac{dp_0^*}{dw_0^*}(ri - w_0^*) - p_0^*]}^{\leq 0} \tag{13}$$

It is unclear whether w_0^* increases with ρ or not without knowing more about the distributions of q_0 and p_0. If $\frac{dw_0^*}{d\rho} > 0$, then we know that the wage is higher without a non-compete agreement.

References

Acemoglu, Daron and Jörn-Steffen Pischke (1999). "The structure of wages and investment in general training." *Journal of political economy* 107.3, 539–572.

Aghion, Philippe and Patrick Bolton (1987). "Contracts as a Barrier to Entry." *The American economic review*, 388–401.

Autor, David H. (2003). "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing." *Journal of Labor Economics* 21.1, 1–42.

Baker, Andrew C., David F. Larcker, and Charles C.Y. Wang (2022). "How much should we trust staggered difference-in-differences estimates?" *Journal of Financial Economics* 144.2, 370–395.

Baker, George, Robert Gibbons, and Kevin J. Murphy (1994). "Subjective Performance Measures in Optimal Incentive Contracts." *The Quarterly Journal of Economics* 109.4, 1125–1156.

Balasubramanian, Natarajan, Evan Starr, and Shotaro Yamaguchi (2024). "Employment restrictions on resource transferability and value appropriation from employees." *Strategic Management Journal* 45.12, 2519–2547.

Balasubramanian, Natarajan et al. (2022). "Locked In? The Enforceability of Covenants Not to Compete and the Careers of High-Tech Workers." *Journal of Human Resources* 57 (S), S349–S396.

Becker, Gary S. (1962). "Investment in Human Capital: A Theoretical Analysis." *Journal of Political Economy* 70.5, 9–49.

Callaway, Brantly and Pedro H.C. Sant'Anna (2021). "Difference-in-Differences with multiple time periods." *Journal of Econometrics* 225.2, 200–230.

Card, David (1999). "The causal effect of education on earnings." *Handbook of labor economics* 3, 1801–1863.

— (2022). "Who Set Your Wage?" *American Economic Review* 112.4, 1075–1090.

Cengiz, Doruk et al. (2019). "The Effect of Minimum Wages on Low-Wage Jobs*." *The Quarterly Journal of Economics* 134.3, 1405–1454.

Che, Yeon-Koo and Donald B Hausch (1999). "Cooperative investments and the value of contracting." *American Economic Review* 89.1, 125–147.

Coase, R. H. (1960). "The Problem of Social Cost." *The Journal of Law & Economics* 3, 1–44.

Cowgill, Bo, Brandon Freiberg, and Evan Starr (2024). "Clause and Effect: Theory and Field Experimental Evidence on Noncompete Clauses."

Cullen, Zoe and Ricardo Perez-Truglia (2023). “The old boys’ club: Schmoozing and the gender gap.” *American Economic Review* 113.7, 1703–1740.
Deming, David J. (2017). “The Growing Importance of Social Skills in the Labor Market*.” *The Quarterly Journal of Economics* 132.4, 1593–1640.
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.
Goodman-Bacon, Andrew (2021). “Difference-in-differences with variation in treatment timing.” *Journal of Econometrics* 225.2, 254–277.
Gormley, Todd A. and David A. Matsa (2011). “Growing Out of Trouble? Corporate Responses to Liability Risk.” *Review of Financial Studies* 24.8, 2781–2821.
Gottfries, Axel and Gregor Jarosch (2023). “Dynamic Monopsony with Large Firms and Noncompetes.” w31965. National Bureau of Economic Research, w31965.
Grossman, Sanford J. and Oliver D. Hart (1986). “The Costs and Benefits of Ownership: A Theory of Vertical and Lateral Integration.” *Journal of Political Economy* 94.4, 691–719.
Hart, Oliver and John Moore (2007). “Incomplete Contracts and Ownership: Some New Thoughts.” *American Economic Review* 97.2, 182–186.
Hashimoto, Masanori (1981). “Firm-specific human capital as a shared investment.” *The American Economic Review* 71.3, 475–482.
Hellmann, Thomas and Veikko Thiele (2017). “Partner Uncertainty and the Dynamic Boundary of the Firm.” *American Economic Journal: Microeconomics* 9.4, 277–302.
Jeffers, Jessica S (2023). “The Impact of Restricting Labor Mobility on Corporate Investment and Entrepreneurship.” *The Review of Financial Studies* 37.1. Ed. by Manju Puri, 1–44.
Johnson, Matthew S., Kurt J. Lavetti, and Michael Lipsitz (2023). “The Labor Market Effects of Legal Restrictions on Worker Mobility.”
Kambourov, Gueorgui and Iourii Manovskii (2008). “RISING OCCUPATIONAL AND INDUSTRY MOBILITY IN THE UNITED STATES: 1968–97*.” *International Economic Review* 49.1, 41–79.
Kini, Omesh, Ryan Williams, and Sirui Yin (2021). “CEO Noncompete Agreements, Job Risk, and Compensation.” *The Review of Financial Studies* 34.10. Ed. by David Denis, 4701–4744.
Lavetti, Kurt, Carol Simon, and William D. White (2020). “The Impacts of Restricting Mobility of Skilled Service Workers: Evidence from Physicians.” *Journal of Human Resources* 55.3, 1025–1067.
Lazear, Edward P. (2009). “Firm-Specific Human Capital: A Skill-Weights Approach.” *Journal of Political Economy* 117.5, 914–940.
Lipsitz, Michael and Evan Starr (2022). “Low-Wage Workers and the Enforceability of Noncompete Agreements.” *Management Science* 68.1, 143–170.
MacLeod, W Bentley and James M Malcomson (1993). “Investments, holdup, and the form of market contracts.” *The American Economic Review*, 811–837.
Manning, Alan (2013). *Monopsony in Motion: Imperfect Competition in Labor Markets*. Princeton University Press.
Meccheri, Nicola (2009). “A note on noncompetes, bargaining and training by firms.” *Economics Letters* 102.3, 198–200.
Neal, Derek (1999). “The Complexity of Job Mobility among Young Men.” *Journal of Labor Economics* 17.2, 237–261.

Pakes, Ariel and Shmuel Nitzan (1983). "Optimum contracts for research personnel, research employment, and the establishment of rival enterprises." *Journal of labor economics* 1.4, 345–365.

Parent, Daniel (2000). "Industry-specific capital and the wage profile: Evidence from the national longitudinal survey of youth and the panel study of income dynamics." *Journal of Labor Economics* 18.2, 306–323.

Posner, Eric A, George G Triantis, and Alexander J Triantis (2004). "Investing in human capital: The efficiency of covenants not to compete." *U Chicago Law & Economics, Olin Working Paper* 137, 01–08.

Rothstein, Donna and Evan Starr (2022). "Noncompete agreements, bargaining, and wages." *Monthly Labor Review*.

Shi, Liyan (2023). "Optimal regulation of noncompete contracts." *Econometrica* 91.2, 425–463.

Spier, Kathryn E and Michael D Whinston (1995). "On the efficiency of privately stipulated damages for breach of contract: entry barriers, reliance, and renegotiation." *The RAND Journal of Economics*, 180–202.

Starr, Evan (2019). "Consider This: Training, Wages, and the Enforceability of Covenants Not to Compete." *ILR Review* 72.4, 783–817.

Starr, Evan P., J.J. Prescott, and Norman D. Bishara (2021). "Noncompete Agreements in the US Labor Force." *The Journal of Law and Economics* 64.1, 53–84.

Sun, Liyang and Sarah Abraham (2021). "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics* 225.2, 175–199.

Topel, R. H. and M. P. Ward (1992). "Job Mobility and the Careers of Young Men." *The Quarterly Journal of Economics* 107.2, 439–479.

Table A1: Variables Dictionary

Variable	Definition
Job Mobility	
Tenure (Yrs)	Time in years working at a job
1(Main Job Separation btwn 2017 and 2019)	Worker transitions to a new main job, becomes unemployed, or exits the labor force
1(Main Job Mobility btwn 2017 and 2019)	Worker transitions to a new main job
1(Within-Industry Job Mobility btwn 2017 and 2019)	Worker transitions to a new main job within the same industry
Wages and Wage Growth	
Log(Starting Wage)	Log of starting wage for 2017 main job
Log(Wage in 2017)	Log of wage in 2017 for main job
$Log(Wage_{2017}) - Log(Wage_{2015})$	Difference between log of wage in 2017 and log of wage in 2015 for 2017 main job
$Log(Wage_{2019}) - Log(Wage_{2017})$	Difference between log of wage in 2019 and log of wage in 2017 for 2017 main job
Demographics	
Age	Computed as the survey year minus birth year
1(Male)	The respondent is male
1(High School Degree or Higher)	The respondent has attended at least 12 years of school
1(Bachelors Degree or Higher)	The respondent has attended at least 16 years of school
ASVAB Percentile	Percentile achieved on ASVAB test
1(Black)	The respondent is Black
1(Hispanic)	The respondent is Hispanic
Wage Bargaining and Negotiation	
1(Possible to Keep Previous Job)	The respondent was able to keep previous job when offered their main job
1(Negotiate Job Offer)	The respondent negotiated their main job offer
Training	
1(Received Some Training)	Received training in a survey year
1(Received Training Run by Employer)	Received training ran by employer in a survey year
1(Received On-Site Training by Non-Employer)	Received training on-site by non-employer in a survey year
1(Employer Paid for Training)	Employer paid for training in a survey year
1(Employer Paid for Mandatory Training)	Employer paid for mandatory training in a survey year
1(Employer Paid for Voluntary Training)	Employer paid for voluntary training in a survey year
Job Tasks	
1(Use Math Skills Frequently)	Respondent claims to use math at least once a week at main job
1(Supervise Frequently)	Respondent claims to supervise more than half the time at main job
1(Problem Solve Frequently)	Respondent claims to problem solve at least once a week at main job
Other Firm Characteristics	
1(Dislike Job)	Respondent claim to 'Dislike it somewhat' or 'Dislike it very much' when asked about main job
1(Unionized Worker)	Respondent's contract was negotiated by a union or employee association for main job
Firm Size	Number of employees at respondent's main job

Table A2: Confidence in Non-Compete Status by Industry

Industry	NC Confidence: Very Confident	NC Confidence: Somewhat Confident	NC Confidence: Not Confident	Total	Share Very Confident
AGRICULTURE, FORESTRY AND FISHERIES	33	0	1	34	0.97
CONSTRUCTION	293	14	4	311	0.94
OTHER SERVICES	152	9	2	163	0.93
TRANSPORTATION AND WAREHOUSING	211	12	7	230	0.92
EDUCATIONAL, HEALTH, AND SOCIAL SERVICES	1169	93	8	1270	0.92
ACS SPECIAL CODES	191	16	1	208	0.92
UTILITIES	29	3	0	32	0.91
INFORMATION AND COMMUNICATION	86	8	0	94	0.91
FINANCE, INSURANCE, AND REAL ESTATE	312	29	3	344	0.91
ENTERTAINMENT, ACCOMODATIONS, AND FOOD SERVICES	422	38	3	463	0.91
PUBLIC ADMINISTRATION	232	21	1	254	0.91
MANUFACTURING	412	42	4	458	0.90
MINING	24	3	0	27	0.89
WHOLESALE TRADE	104	12	1	117	0.89
RETAIL TRADE	458	49	5	512	0.89
PROFESSIONAL AND RELATED SERVICES	560	64	7	631	0.89
TOTAL	4688	413	47	5148	0.91

Note:
The sample consists of NLSY97 respondents who report non-compete, non-compete confidence, and industry status in their 2017 main job. Rows are organized by share “Very Confident” in response to the non-compete confidence question. Active duty military respondents are dropped.

Table A3: Estimated Effects of NCs using the 2019 Cross-Section

Panel 1: Wages and Wage Growth

Dependent Variables:	Log(Wage)			Wage Growth		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	0.305***	0.220***	0.145***	0.005	0.006	0.028
	(0.051)	(0.044)	(0.052)	(0.033)	(0.033)	(0.043)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	3.14	3.14	3.14	0.111	0.111	0.111
Fit statistics						
Observations	1,638	1,585	762	1,638	1,585	762
R^2	0.034	0.302	0.574	2.96×10^{-5}	0.004	0.060

Panel 2: Training

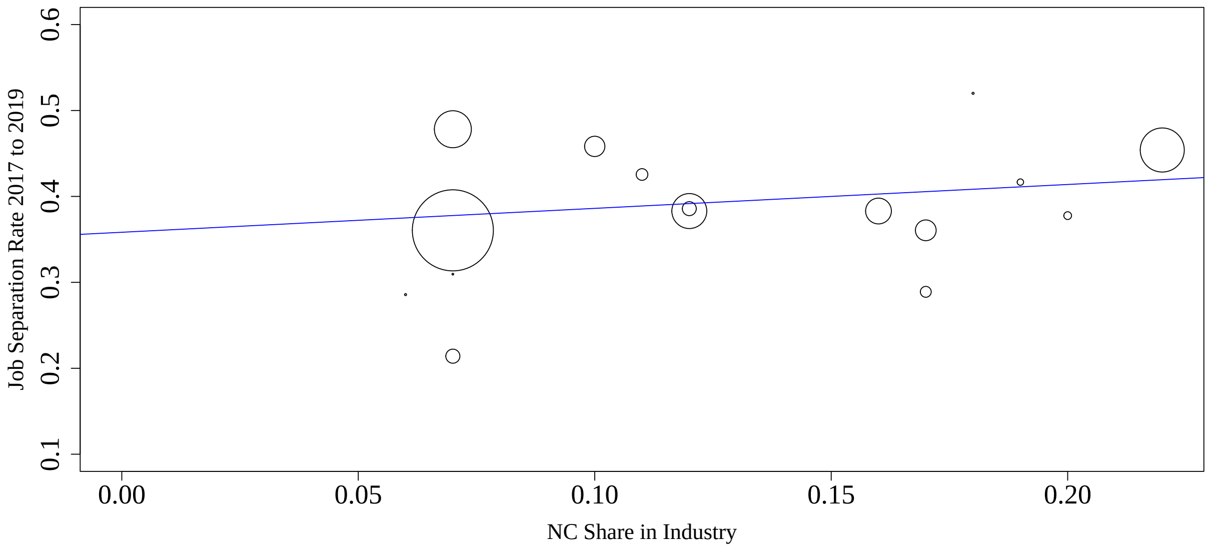
Dependent Variables:	1(Any Training)			1(Emp Paid for Training)		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	0.011	-0.002	-0.028	0.015	-0.002	-0.031
	(0.029)	(0.029)	(0.043)	(0.024)	(0.024)	(0.038)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	0.120	0.120	0.120	0.078	0.078	0.078
Fit statistics						
Observations	1,638	1,585	762	1,638	1,585	762
R^2	0.0001	0.007	0.108	0.0004	0.020	0.139

Panel 3: Job Mobility

Dependent Variables:	Tenure (Yrs)			1(Main Job Mobility btwn 2019 and 2021)		
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
1(NC)	0.256	0.237	0.116	-0.051	-0.050	-0.064
	(0.184)	(0.187)	(0.226)	(0.036)	(0.037)	(0.052)
Controls	None	Basic	Advanced	None	Basic	Advanced
Weighted Dependent Variable Mean	5.64	5.64	5.64	0.249	0.249	0.249
Fit statistics						
Observations	1,616	1,585	762	1,638	1,607	771
R^2	0.003	0.008	0.098	0.001	0.010	0.068

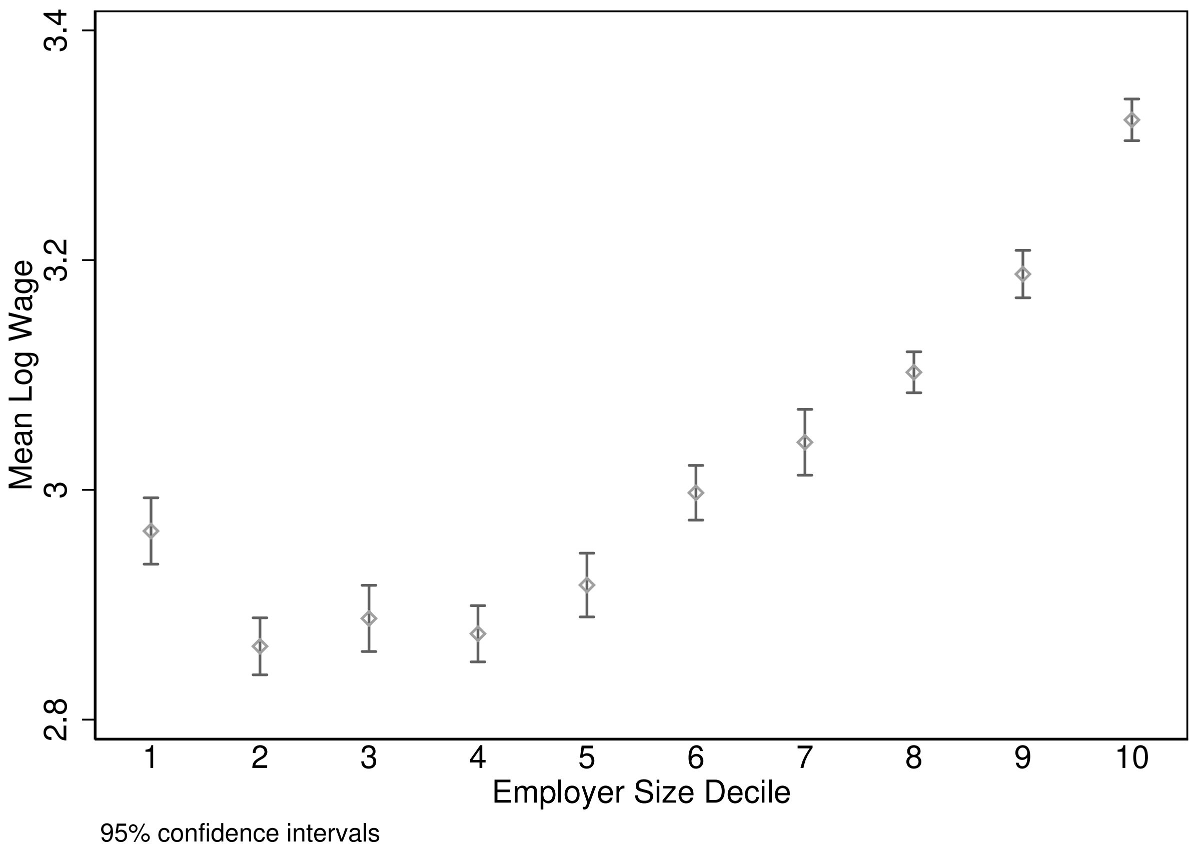
Notes: Standard errors are heteroskedasticity-robust. The sample restricts to individuals who report NC status and have real wages between 3 and 200 in 2019. Basic controls include sex, education, tenure, and potential experience. Advanced controls further add industry and occupation fixed effects, ASVAB percentile, and firm size. All regressions are weighted so as to be nationally representative. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Figure A1: Job Mobility vs Non-Compete Usage by Industry



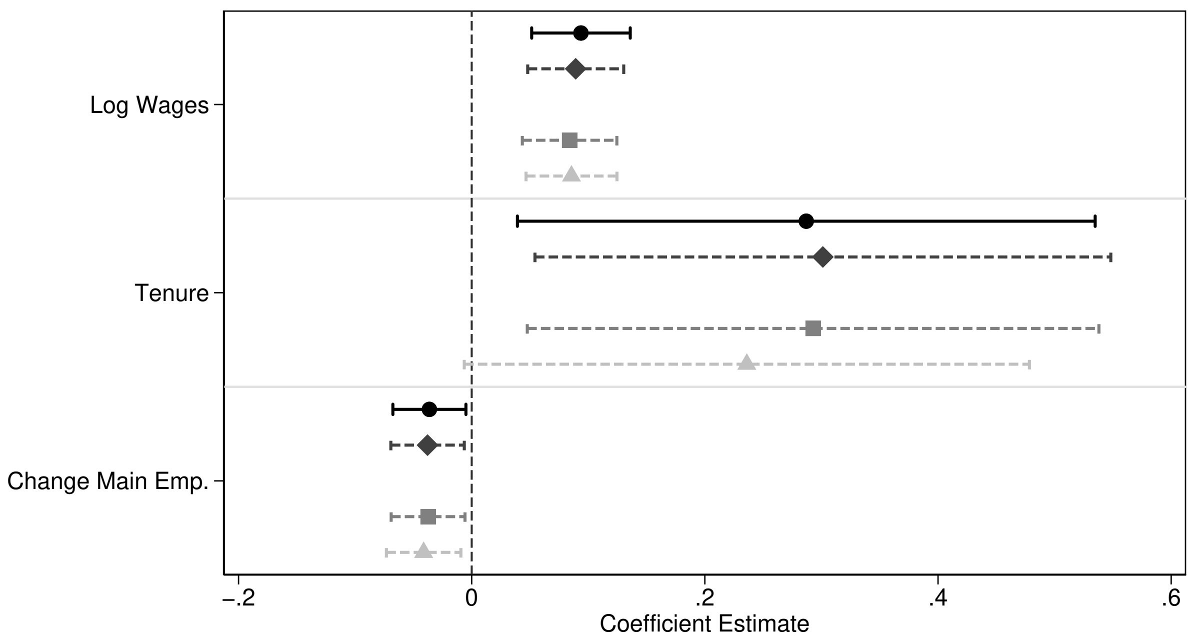
Note: The figure presents the rate of job separations in each industry between 2017 and 2019 versus non−compete usage by industry in 2017. The size of the circles are proportional to industry size and the line of best fit is weighted by industry size. The intercept is 0.36 and the slope is 0.28

Figure A2: Relationship Between Log Wages and Firm Size



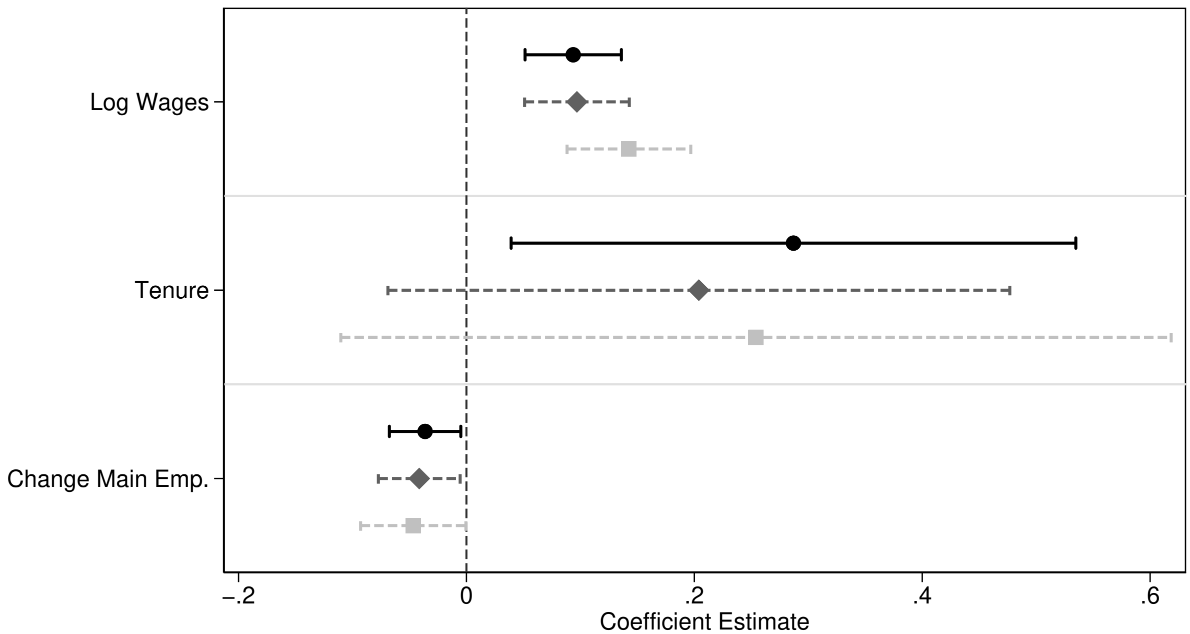
Note: Based on 2017 cross-section.

Figure A3: The Effect of Signing a Non-Compete Agreement: Robustness to Firm-Level Covariates



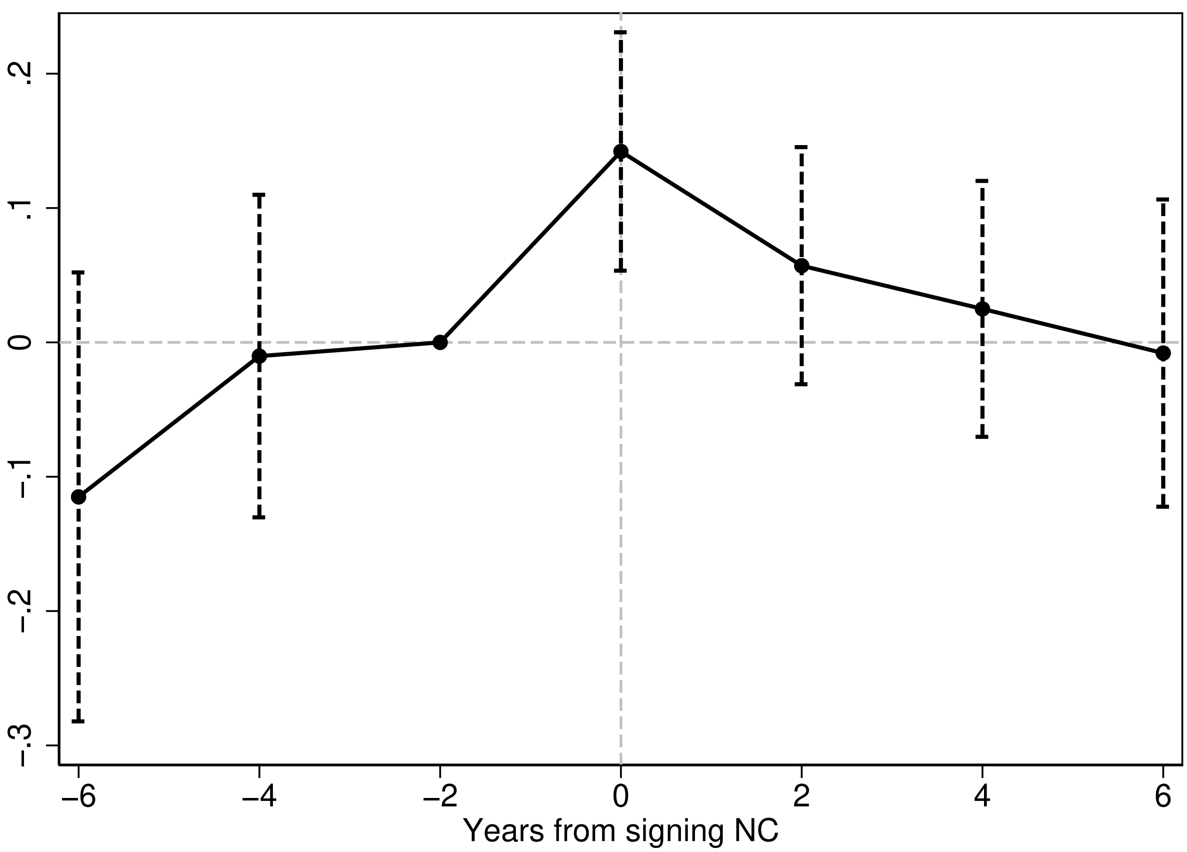
Note: Coefficient estimates are from stacked difference-in-differences estimation, aggregated over post-treatment years, over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers whom we do not observe holding a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. The black circle markers report baseline estimates from the main text. The gray diamond markers with dashed lines report estimates controlling for industry fixed effects. The square markers further add occupation fixed effects, and the triangle markers further add firm size decile fixed effects. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

Figure A4: The Effect of Signing a Non-Compete Agreement: Robustness to Different Samples



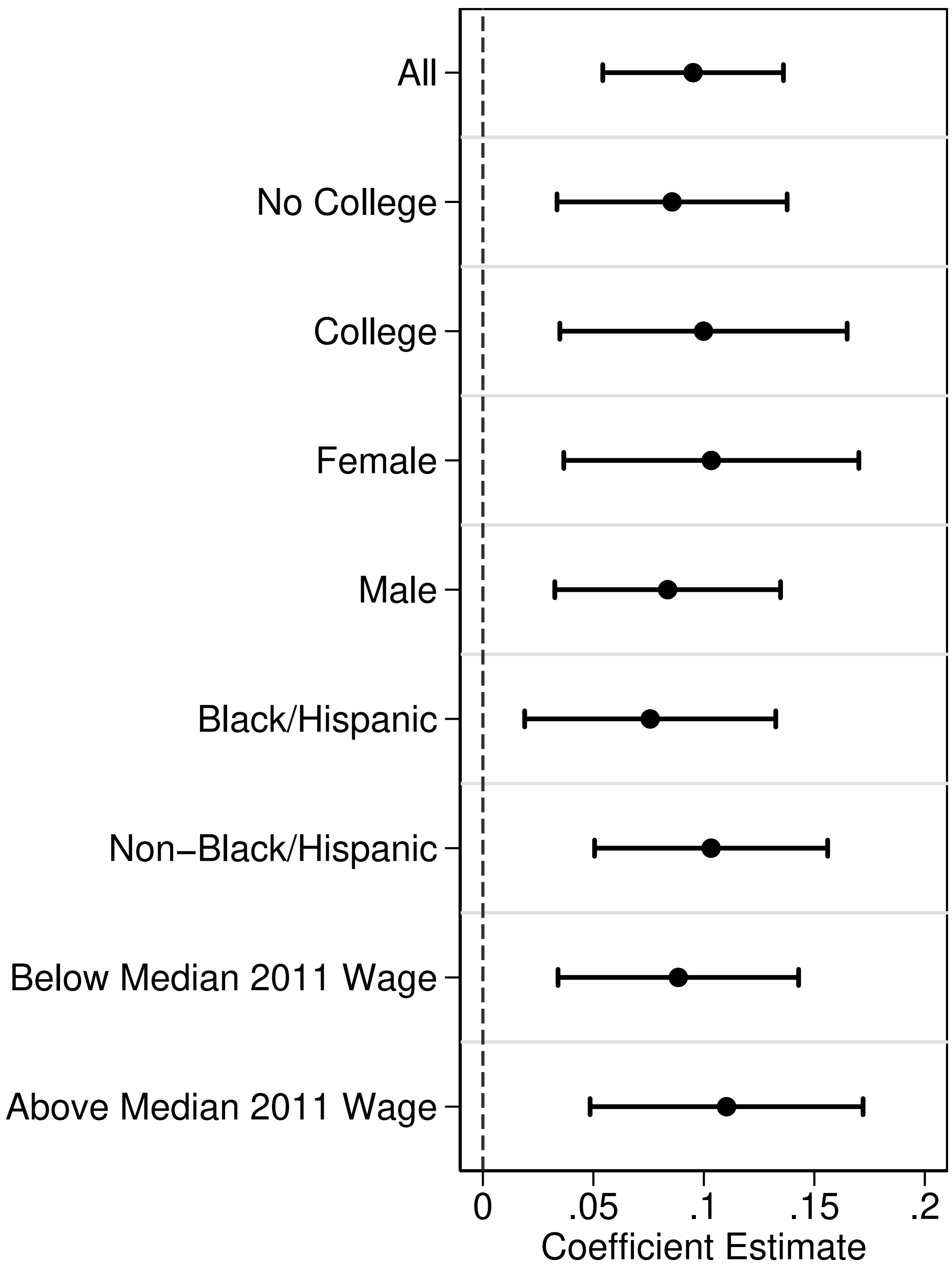
Note: Coefficient estimates are from stacked difference-in-differences estimation, aggregated over post-treatment years, over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers whom we do not observe holding a NC during the event window and who also changed jobs between year c and the preceding survey year. Job mobility is defined as changing main employers between the current and preceding survey year. The black circle markers report baseline estimates from the main text. The gray diamond markers with dashed lines report coefficient estimates when we restrict attention to years 2015-2021 and cohorts $\{2017, 2019, 2021\}$. The square markers further restrict attention to years 2017-2021 and cohorts $\{2019, 2021\}$. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

Figure A5: The Effect of Signing a Non-Compete Agreement on Wages: Later-treated as Control Group



Note: Coefficient estimates are from stacked difference-in-differences estimation, aggregated over post-treatment years, over a bi-annual sample period of 2013-2021 and using cohorts $c \in \{2015, 2017, 2019, 2021\}$. The treatment group for cohort c are those who we observe first signing an NC in year c. The control group consists of workers who (a) changed jobs between year c and the preceding survey year, (b) do not hold an NC in year c, and (c) sign an NC at some $t > c$ (the later-treated job movers). Standard errors are clustered by worker and confidence intervals are reported at the 95% level.

Figure A6: Wage Effects of Signing an NC: Two-way Fixed Effects (TWFE) Model



Note: Coefficient estimates are from the two-way fixed effect (TWFE) model given in equation 8 over a bi-annual sample period of 2013-2021. Standard errors are clustered by worker and confidence intervals are reported at the 95% level.