The Labor Market Effects of Legal Restrictions on Worker Mobility*

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Abstract

We analyze how the legal enforceability of Noncompete Agreements (NCAs) affects labor markets. Using newly-constructed panel data, we find that higher NCA enforceability diminishes workers' earnings and job mobility, with larger effects among workers most likely to sign NCAs. These effects are far-reaching: examining local labor markets that cross state borders reveals that enforceability affects workers' earnings in different legal jurisdictions. Revisiting a classic model of wage-setting, we find that—in contrast to prior evidence—workers facing high enforceability are unable to leverage tight labor markets to increase their wage. Finally, higher NCA enforceability exacerbates gender and racial wage gaps.

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1 Introduction

There is growing consensus that the US labor market has failed to produce economic gains for the majority of workers in recent years. Average real hourly wages have changed little over four decades,[1] and the share of income accruing to labor declined from 65 percent in the late 1940s to 63 percent in 2000, before accelerating downward to 58 percent in 2016.[2] Various forces have been posited to underlie these trends, including the decline of labor unions, the rise of superstar firms (Autor et al., 2017), and the rise of domestic outsourcing (Weil, 2014; Goldschmidt and Schmieder, 2017).

Another potential explanation that has received increasing scrutiny is firms' use of postemployment restrictions, the most salient of which are noncompete agreements (NCAs). NCAs contractually limit a worker's ability to enter into a professional position in competition with his or her employer in the event of a job separation. NCAs are common: Starr et al. (2021) find that at least 15, and as high as 46, percent of workers in 2016 were bound by NCAs, whereas Colvin and Shierholz (2019) found this range to be between 28 and 47 percent in 2019.[3] NCAs may hinder wage growth by limiting workers' ability to seek higher-paying jobs, by suppressing their ability to negotiate higher wages at current jobs, or by decreasing labor market churn. At the same time, NCAs might increase incentives for firms to invest in training, knowledge creation, and other portable assets (Rubin and Shedd, 1981) that could increase their workers' productivity and earnings.

The extent to which NCAs are legally enforceable is determined by state law. Despite growing momentum from policy makers at state and national levels[4] to amend the enforceability of NCAs, there remains an incomplete understanding of the labor market effects of NCAs. At least two factors have limited existing research. One factor is a lack of comprehensive panel data on NCA enforceability. Researchers have, to date, relied largely on either cross-sectional measures of states' enforceability or case studies of a small number of states with law changes. This approach has drawbacks: cross-sectional variation in enforceability might be correlated with other unobserved

[1]Desilver, Drew, "For Most U.S. Workers, Real Wages Have Barely Budged in Decades," *Pew Research Center*, August 7, 2018.

[2]President's Council of Economic Advisors Issue Brief "Labor Market Monopsony: Trends, Consequences, and Policy Responses" October 2016.

[3]Specifically, Starr et al. (2021) find that 15% of workers were bound by NCAs, and another 29.7% were unsure if they were bound by NCAs.

[4]The Workforce Mobility Act of 2018 (US Senate Bill 2782, introduced by Chris Murphy) states "No employer shall enter into, enforce, or threaten to enforce a covenant not to compete with any employee of such employer" (`https://www.congress.gov/bill/115th-congress/senate-bill/2782/text?r=6.`). The Freedom to Compete Act of 2019 (US Senate Bill 124, introduced by Marco Rubio) has similar language (`https://www.congress.gov/bill/116th-congress/senate-bill/124/all-info`).

differences across states, cross-sectional measures based on a single year can introduce measurement error if NCA laws change over time, and a small sample of law changes may not generalize to the population. A second limitation to existing research is that identifying the mechanisms through which NCAs affect labor markets has proven elusive. Without a clear understanding of *why* NCA enforceability affects workers, it is difficult to translate empirical evidence into specific policy recommendations.

We present new evidence on the effect of NCA enforceability on workers' wages and job mobility. First, we construct a new panel dataset to use *within-state* changes in NCA laws to identify the overall labor market effects of NCA enforceability. Second, we illustrate one mechanism through which NCA enforceability—by increasing the costs of mobility—affects earnings. Finally, informed by prior evidence of differences in bargaining power and wage-setting across worker demographics, we show that the earnings effect of NCA enforceability exhibits economically meaningful heterogeneity across demographic groups, contributing a new insight to the determinants of wage inequality in the United States.

To identify the effects of NCA enforceability, we create a new dataset with annual measures of NCA enforceability for each of the 50 US states and the District of Columbia from 1991 to 2014. These data include both judicial and legislative decisions that change state-level NCA enforceability, coded to match the criteria developed by legal scholars to quantify enforceability. The vast majority of these law changes (91.4%) occur due to judicial decisions via court rulings. An important component of the judicial process is *stare decisis*, or the doctrine of precedent. A consequence is that judges are more constrained than legislators in allowing economic or political trends to affect decisions, a fact that is useful for our research design. We combine our enforceability dataset with earnings and mobility outcomes from the Current Population Survey, the Job to Job Flows dataset, and the Quarterly Workforce Indicators dataset, all from the US Census Bureau.

We find that increases in NCA enforceability decrease workers' earnings and mobility. Moving from the 10^{th} to 90^{th} percentile in enforceability is associated with a 3-4% decrease in the average worker's earnings. The earnings effects are almost entirely driven by declines in implied hourly wages. The effect is even stronger among occupations, industries, and demographic groups in which NCAs are used more frequently (according to Starr et al. (2021)). We also find that NCA enforceability reduces worker mobility, particularly when NCAs are used more frequently. A back of the envelope calculation using an out-of-sample extrapolation implies that rendering NCAs unenforceable nationwide would increase average earnings among *all* workers by 3.3% to 13.9%. The midpoint of this interval (8.6%) is roughly equal to the estimated effects of very large increases in employer consolidation on affected workers' wages (Prager and Schmitt, 2019); it is also approximately equal to the estimated wage premium that accrues to workers who enter occupations with government-mandated licensing, and roughly half the size of the wage premium associated with membership in a labor union.

To interpret these estimates, we must confront the fact that only a fraction of workers (between 15–47%) actually sign NCAs. However, the wage effects of NCA enforceability plausibly extend beyond this subset of workers. Similar to how labor unions have been shown to reduce wage inequality even among workers not themselves unionized (Fortin et al., 2021) due to the union "threat effect", the use of NCAs might create negative externalities on the wages of other workers by reducing labor market churn, thinning labor markets, or increasing recruitment costs (Starr et al., 2019). We show that such externalities exist and are economically meaningful. NCA enforceability laws generate spillover effects within local labor markets on workers in different legal jurisdictions. Focusing on local labor markets that are divided by a state border, we show that a change in NCA enforceability in one state indirectly affects the earnings and mobility of workers located in an adjoining state. This finding suggests that the "treatment" of NCA enforceability affects a larger population than the relatively small share of workers bound by NCAs, and the magnitudes suggest that spillovers account for a non-trivial share of the overall wage effects of enforceability.

To investigate the mechanisms behind these earnings effects, we consider the implications of how NCAs make worker mobility costly. We first show that strict NCA enforceability attenuates the returns to tenure. We then posit that stricter enforceability hinders workers' ability to leverage improvements in labor market conditions to negotiate wage increases. We revisit a longstanding theory that wages are determined under a model of implicit contracts between workers and employers. Beginning with the influential paper of Beaudry and DiNardo (1991), this literature has consistently found that workers' wages rise when their outside option improves: a worker's current wage is more strongly affected by the minimum unemployment rate over the course of her job spell than by the initial unemployment rate at the beginning of the spell. This finding implies that the cost of job mobility for workers is low. Because (enforceable) NCAs by construction raise the costs of job mobility, however, it is plausible that this relationship is dependent on states' NCA policies.

Using more recent CPS data, we show that the result from Beaudry and DiNardo (1991) still holds *on average*, but it flips in states with strongly enforceable NCA policies. In these states, the minimum unemployment rate has essentially no effect on a worker's current wage, and the unemployment rate at the start of the job spell has a much stronger effect (consistent with contracts negotiated under costly mobility). In contrast, in states with weakly enforceable NCA policies, the effect of the minimum unemployment rate over a job spell on the current wage is even more pronounced, even conditioning on the initial unemployment rate (consistent with very costless mobility). These findings imply that strict NCA enforceability erodes workers' ability to leverage tight labor markets to achieve higher earnings, and is consistent with the hypothesis that NCAs "undermine workers' prospects for moving up the income ladder" (Krueger, 2017).

Finally, we document economically meaningful heterogeneity in the earnings effect of NCA enforceability across demographic groups. NCAs might restrict outside options more for women than for men, due for example to gender differences in willingness to commute (Le Barbanchon et al., 2019), and differences in men and women's willingness to negotiate (Bertrand, 2011) could imply that women are less willing to violate the terms of their NCA than are men. Similar evidence has been found for non-white workers relative to white workers. More broadly, to the extent that enforceable NCAs decrease the competitiveness of labor markets, they may endow firms with monopsony power to price discriminate among their workers (Robinson, 1933). Consistent with this evidence, we find that stricter NCA enforceability reduces earnings for female and for non-white workers by twice as much as for white male workers. Neither of these effects are explained by differences in occupations, industries or education across groups. Using a standard wage decomposition, our estimates imply that the 90-10 differential in NCA enforceability accounts for 3.6-9.1% of the earnings gaps between white men and other demographic groups.

Our findings contribute to a growing literature on the effects of NCA enforceability. Recent studies using cross-sectional variation have estimated that greater NCA enforceability reduces workers' earnings (Starr, 2019; Starr et al., 2019), though others have found opposing evidence in some high-skilled labor markets such as doctors (Lavetti et al., 2018) and CEOs (Kini et al., 2019). A more consistent finding in this literature is that NCA enforceability reduces mobility (Marx et al., 2009; Garmaise, 2011; Starr et al., 2019). Other papers have used cross-sectional variation to test how enforceability moderates the employment effects the minimum wage (Johnson and Lipsitz, 2019) or studied specific law changes to analyze effects of enforceability on subgroups of workers, like knowledge workers (Marx et al., 2015, 2009), managers (Garmaise, 2011), and hourly workers (Lipsitz and Starr, 2021). Our paper is the first to examine labor market effects using a comprehensive set of all NCA law changes between 1991 and 2014, and, to the best of our knowledge, the first study to empirically demonstrate one way that NCA policies fundamentally change how workers and employers bargain over wages.

Our findings also contribute to several other literatures. First, a growing literature investigates the effects of employer power in labor markets. Many studies have found evidence consistent with local employer concentration affecting wages (for example, Azar et al. (2017), Benmelech et al. (2018), Prager and Schmitt (2019)), and other studies find other ways that employers have monopsony power (Dube et al., 2018). Our results imply that NCA enforceability is a means through which employers wield market power over workers, even in the absence of explicit changes in employer concentration. By diminishing workers' outside options, (enforceable) NCAs skew power dynamics and give employers effective market power over the workers that they have hired, a finding that complements other work showing the importance of outside options on wages (Caldwell and Danieli, 2018). Second, our findings provide new insight to a longstanding debate in law and economics regarding freedom of contracting (see, e.g., Bernstein (2008) for an overview). Advocates of the freedom of contract argue that the ability to freely enter into contracts increases economic efficiency, as contracts that decrease economic efficiency would leave one or both actors worse off, and would therefore not be signed in the first place. Our findings of substantial externalities of NCA enforceability reveals a shortcoming in this argument to justify the allowance of NCAs from an efficiency standpoint.[5] Finally, our work complements a literature analyzing the effects of NCA enforceability on outcomes outside of the labor market, such as corporate investment (Jeffers, 2018), entrepreneurship (Marx, 2018), and knowledge spillovers from patents (Belenzon and Schankerman, 2013).

2 Conceptual Framework

To develop a conceptual framework to guide our empirical analysis of how NCA enforceability affects wages, we must first be explicit about two things.

First, it is useful to define what NCAs *do*. NCAs, by construction, limit workers' mobility in a specific way. While the exact terms of an NCA are contract-specific, a central reason that an employer has a worker sign an NCA is to prevent her from moving jobs to a competing firm. The definition of "competing" depends on the nature of production. In an industry in which client lists are essential to production, an NCA might dictate that the worker cannot depart for another employer in the same industry and within a specified geographic radius (e.g. within 25 miles or the same state); in an industry in which trade secrets are essential for firms to retain a

[5]In this regard, we complement the findings from Starr et al. (2019), who illustrate externalities of NCA *use* in a different approach.

competitive edge, the NCA might dictate that the worker cannot depart for another employer in the same industry anywhere in the country. No matter the specifics, the NCA is intended to limit the worker's outside options.

Second, we must be explicit that our focus is on effects of NCA *enforceability*, not NCA *use*. Though we argue below that NCA enforceability likely affects wages beyond the relatively small group of workers that sign NCAs, our analysis presumes that changes in enforceability will alter the *intensity* of NCA use. This could occur on both the extensive and intensive margin. On the extensive margin, changes in enforceability could change the share of workers bound by NCAs. We do not observe panel data on individual-level NCA use, so we cannot directly test whether this is the case. However, in the cross-section, states with higher NCA enforceability have a larger share of physicians (Lavetti et al., 2018), CEOs (Kini et al., 2019), managers (Shi, 2020), and hair stylists (Johnson and Lipsitz, 2019) that sign NCAs.[6] On the intensive margin-, enforceability could alter the effect of signing an NCA on those who have already signed one. Indeed, though NCAs are used in states that render them unenforceable (e.g. Starr et al. (2021)), employers are in a better position to leverage a worker's NCA when the enforceability regime is more strict.[7]

Having articulated what NCAs are intended to do, and what we are measuring by focusing on enforceability, we turn to the focus of this section: how we expect NCA enforceability to affect wages. For this purpose, we embed NCAs into the job search model of the labor market developed in Bagger et al. (2014). This model decomposes workers' wage growth over the course of their careers into contributions of human capital accumulation and job search, the latter including both "between-job" (moving from lower-paying to higher-paying firms) and "within-job" (using competing offers to negotiate for pay increases from their current employer) wage growth. This model provides a natural framework for our purpose, as its focus on the role of human capital accumulation versus job search highlights two competing channels through which NCAs could affect wages, which we elaborate on below.

In the remainder of this section, we provide economic intuition for multiple channels through which NCAs and earnings might interact. In Appendix A, we formalize this intuition by extending the model in Bagger et al. (2014) to embed NCAs into a search model of the labor market.

We organize our framework by considering:

[6]This evidence is not unanimous, however: Starr et al. (2021) find essentially no difference in NCA use by states' enforceability in a representative sample of US workers.

[7]Note that this argument holds even if a worker is not fully informed about the enforceability of the NCA she has signed. As long as employers *are* informed, and there is some probability that workers can learn, then employers will know the NCA has less bite in expectation when it is not legally enforceable.

1. *Direct* effects of NCA enforceability on earnings of workers that sign NCAs
2. *Indirect* (spillover) effects of NCA enforceability on earnings of other workers
3. *Differential* effects of NCA enforceability across different groups of workers

2.1 Direct Effects on Workers that Sign NCAs

To the extent that enforceability affects the incidence of NCA use, the direct effect of signing an (enforceable) NCA on wages reflects multiple competing channels.

Human Capital Accumulation

A common justification for NCAs is that they increase employers' incentives to make productivity-enhancing investments in their employees. Employers might be reluctant to invest in "transferable" assets, such as general human capital or client lists, that an employee could take with her in the event of a departure, since the employer is unlikely to recoup the full value of the investment (Grossman and Hart, 1986; Williamson, 1975). By preventing an employee from departing to a competitor or founding her own competing firm, an NCA can alleviate this investment "hold-up." NCAs would then clearly benefit firms by increasing quasi-rents associated with higher investment and productivity. As long as a portion of these quasi-rents are passed through to employees, then NCAs—by inducing firm-sponsored investment—would translate to higher wages.[8]

In Appendix A, we model the human capital effects of NCAs by extending Bagger et al. (2014) to incorporate faster human capital accumulation for workers who sign NCAs. All else equal, faster human capital accumulation drives faster wage growth for workers with NCAs, contributing to Proposition A.2, which presents conditions under which wage growth is faster with or without an NCA.

Reducing Workers' Outside Options

Workers' outside options have a large effect on their wage (Caldwell and Harmon, 2019). An NCA, by construction, limits a worker's options for future job mobility. Workers bound by enforceable NCAs are unable to move to competing firms (or, at least, face higher costs of doing so). This reduction in expected *realized* mobility reduces workers' expected future wage trajectory, in so much as it prevents them from accepting jobs at higher-paying firms (Haltiwanger et al., 2018) and prolongs exposure to potential negative match-specific wage shocks (Liu, 2019).

[8]The extent of such pass-through of quasi-rents to wages is likely to depend on many factors, such as the ease with which a worker can be replaced (Kline et al., 2019).

Similarly, because an NCA reduces a worker's *threat* of departure, it reduces her ability to leverage that threat to get a pay increase from her current employer. This reduced threat point likely meaningfully affects a worker's wage trajectory: Bagger et al. (2014) find that the wage effects of such *within-job* search dominate the wage effects of *between-job* search.

Moreover, the wage penalty associated with an inability to leverage competing offers is likely to be particularly pronounced under certain labor market conditions. Workers that begin job spells during a period in which the labor market is weak can leverage subsequent improvements in labor market conditions as long as their costs of mobility are low: this is because a worker cannot commit to a wage contract that reflects a weak outside option at the time of hire if her outside option suddenly improves (Beaudry and DiNardo, 1991). Because an NCA raises a worker's cost of mobility, it would leave her unable to bargain for a pay increase commensurate with the strength of the labor market.

In Appendix A, we extend Bagger et al. (2014) by limiting the offer arrival rate for employed workers with an NCA, which decreases wage growth for those workers.[9] This modification contributes to Proposition A.2, which describes when NCAs increase or decrease earnings: by limiting workers' ability to bargain for earnings increases by leveraging outside offers, and their ability to accept new, higher-paying jobs, (enforceable) NCAs reduce the wage gains that accrue through job search.

Compensating Differentials

A forward-looking and well-informed worker, recognizing that signing an NCA would reduce the future value of job search, would require an initial compensating wage differential to accept one. In a frictionless model, competition drives the size of the compensating differential to the marginal worker's aversion to accepting an NCA, which may reflect a combination of expected financial returns to job search and worker preferences. In a model with search frictions, a worker receiving a job offer with an NCA will accept it if the total value of the job offer is greater than the value of remaining unemployed; in this case the size of the compensating differential would depend on parameters such as the arrival rate and value of unemployment. Any of these models would predict that workers with NCAs would have lower wage *growth*; a subset would predict potentially higher initial *levels* of compensation.

A prerequisite for the existence of a compensating differential is that workers must be aware that they are signing an NCA at the time of initial negotiation. There is

[9] Another way to represent how NCAs reduce the value of job search would be to impose a cost (e.g., a buy-out payment) on workers for moving to a new firm. As discussed in Appendix A, both approaches generate the same qualitative prediction.

evidence that this condition is not always met: firms frequently ask workers to sign NCAs *after* accepting their job (Marx, 2011; Starr et al., 2021), which renders workers unable to demand a compensating differential for accepting an NCA. Furthermore, workers must fully anticipate the costs of future decreased mobility to bargain for a compensating differential, which might not be the case if workers are myopic or have very high discount rates (Greenwald, 1986).

Under the assumption that workers recognize when they have signed an NCA, and understand its costs, a compensating differential can arise in the model of Bagger et al. (2014). In that model, workers' pay when entering a new match is set to guarantee them a proportion, β, of future match-specific rents. Since an NCA reduces their future value stream (by limiting their gains from job search), the initial wage will increase to guarantee the same proportion of match-specific rents accrues to the worker.[10]

2.2 Indirect (Spillover) Effects on Other Workers

The use of (enforceable) NCAs by some firms could affect the wage not just of the workers that sign them, but also have spillover effects on other workers in the same labor market.[11] Such spillover effects may arise for several reasons.

First, the prevalence of NCAs in the population might affect offer arrival rates to workers searching for jobs. One reason that this might happen is that NCAs thin labor markets: when a firm hires a worker with an (enforceable) NCA, the worker becomes effectively removed from the the pool of potential hires for other firms. The firm also, to an extent, becomes removed from the pool of searching firms; this is because the firm has a worker who a) it might have already made a costly investment in, and b) is unable to leverage outside options to negotiate for wage increases—both of which make that worker more valuable moving forward than hiring a new worker. If the worker has been paid a compensating differential, that cost is likely sunk, as well. In thinner labor markets, workers and firms match less often, which drives down equilibrium wages (Bleakley and Lin, 2012; Gan and Li, 2016).

Another reason that NCA enforceability could reduce arrival rates for job seekers is that NCA use by some firms can increase recruitment costs for all firms (Starr et al.,

[10] We note that, while the Bagger et al. (2014) easily accommodates an NCA initial compensating differential for the reduced value of job search, it does *not* accommodate one for workers' increased growth rate of human capital development. In other words, in this model, if worker A has a faster projected rate of human capital development than worker B, but the two workers are otherwise identical, both workers will have identical starting wages. See the paragraph immediately following Equation 6 in Bagger et al. (2014) for discussion.

[11] Our discussion in this section draws heavily from Starr et al. (2019).

2019). Firms are unlikely to be able to directly observe whether a job applicant is currently bound by an NCA, which would (in expectation) slow down the recruiting process and decrease the value of posting vacancies (Starr et al., 2019). More generally, NCA use might reduce overall labor market dynamism, which can decrease wages by slowing workers' ability to find higher-paying employers (Haltiwanger et al., 2018) and be decreasing the offer arrival rate (Bagger et al., 2014).

Relatedly, NCA use could decrease the number of searching firms, increasing local firms' market power (in, e.g., a Cournot sense). Enforceable NCAs decrease entrepreneurship (Starr et al., 2018) and new firm entry (Jeffers, 2018). These effects would increase local labor market concentration, which a wide literature has shown depresses wages (e.g. Azar et al. (2017); Arnold (2019); Jarosch et al. (2019)).[12]

As we describe in Appendix A.4, we formalize this spillover effect by extending Bagger et al. (2014) to assume that the job offer arrival rate depends positively on market thickness. NCAs, by causing labor markets to thin, decrease arrival rates to other workers, decreasing the steady-state distribution of wages for workers. Proposition A.4 describes how this spillover effect affects initial wages, and the ensuing discussion describes how it affects wage growth.

2.3 Differential Effects of NCA Enforceability Across Groups of Workers

NCA enforceability has the potential to contribute to gender and racial earnings gaps. Although there is no clear evidence that NCAs are used more frequently among men and women (Starr et al., 2021), NCA enforceability may still differentially affect wages by gender for several reasons.

First, NCAs may have a stronger deterrent or "chilling" effect for women than for men. Marx (2018) finds that strict NCA enforceability disproportionately decreases entrepreneurship among women, meaning that men might be more willing (and able) to violate NCAs. Second, NCAs may reduce female workers' outside options more than male workers'. Men tend to be more willing than women to commute far distances for their job (Le Barbanchon et al., 2019). As a result, the geographic breadth of an NCA might be less restrictive on average for men than women. Third, any potential wage *gains* from NCAs might accrue more to men than to women. To the extent that NCAs create positive quasi-rents for employers, there is evidence that

[12] It is also possible that enforceable NCAs could have *positive* spillover effects on wages: if NCAs reduce competition in the goods markets by limiting the supply of entrepreneurs who are potential entrants in those markets, workers' marginal revenue product of labor would be higher because goods market prices are higher, potentially increasing wages.

firms share rents to a greater extent with male workers than with female workers (Black and Strahan, 2001; Card et al., 2015; Kline et al., 2019). Fourth, strict NCA enforceability might give firms more power to discriminate between male and female workers by reducing labor market competitiveness (Robinson, 1933; Black and Brainerd, 2004; Barth and Dale-Olsen, 2009; Hernandez et al., 2018).

For these same four reasons, strict NCA enforceability could depress the earnings of racial minorities more than for white workers.[13]

2.4 Takeaways

The framework presented in this section organizes the empirical analyses we report in Sections 4, 5, 6, and 7. The qualitative takeaways of the model are:

1. The overall effect of NCA enforceability on earnings is ambiguous.
2. Enforceability could have spillover effects on wages of workers not bound by NCAs, and the effect is likely negative.
3. By reducing workers' ability to threaten to change jobs, NCAs reduce workers' ability to secure wage gains, particularly in tight labor markets.
4. The negative earnings effects of NCA enforceability are likely more pronounced for women and racial minorities.

Before reporting these results, we describe the institutional background of NCA enforceability, how we quantify states' history of enforceability, and the data sources we use.

3 Data

3.1 State-Level NCA Enforceability Measures

The cornerstone of our paper is a state-level panel dataset with annual measures of states' NCA enforceability. As documented by Bishara (2010), NCA laws vary along

[13] There is anecdotal evidence that NCAs are more implicitly binding for black workers than their white co-workers. An illustrative example is a suit brought forward by plaintiff Tracy Miller, an African American worker employed by Illinois Central Railroad. After receiving an employment offer from a competitor, Miller was told by his employer that he could not take it because he had signed an NCA. The plaintiff alleged that the same NCA went unenforced multiple times when several of his white co-workers accepted employment with other industry competitors, in clear violation of NCAs they had also signed. More information available at `https://www.bsjfirm.com` (accessed July 2019).

seven quantifiable dimensions across states and over time (see Table C.1 for a list of the dimensions). For example, one dimension (Q3a) indicates the extent to which employers are legally required to compensate workers that sign NCAs at the beginning of a job spell. Another dimension (Q8) reflects whether the NCA is enforceable when the employer terminates the employee who signed the NCA (as opposed to a voluntary separation).

Our dataset contains values representing the stringency of the law on each of these seven legal dimensions for every state between 1991 and 2014. This dataset builds from Bishara (2010), who quantified how each state's law treated each of these seven dimensions on a scale from 0 (completely unenforceable) to 10 (easily enforceable) in the years 1991 and 2009. We created the panel version by first replicating the cross-sectional scores from Bishara (2010) in 1991 and 2009 using the same primary sources: a series of legal texts titled "Covenants Not to Compete: A State by State Survey," updated annually by Malsberger. We used detailed notes and decision rules provided by Bishara (2010) to ensure our approach to quantify enforceability followed that of Bishara (2010). After replicating the cross-sectional scores, we filled in the timing of all intervening changes using the same quantification methodology, and extended the data through 2014. Our approach mirrors that of Hausman and Lavetti (2017), who created an analagous dataset for NCA enforceability specific to physicians from 1991–2009. Using the seven dimensions of enforceability, we construct a composite *NCA Enforceability Score* for each state-year from 1991-2014.[14] These data have never previously been used to study the general labor-market effects of NCA laws.

Differences in how states interpret these dimensions lead to substantial differences in the *NCA Enforceability Score* across states. At the extreme ends of this spectrum, Florida Statute 542.335 explicitly allows the use of NCAs as long as a legitimate business interest is being protected, the agreement is in writing, and the agreement is reasonable in time, area, and line of business.[15] The law allows for a large variety of protectable interests (such as trade secrets, training, and client relationships), permits the beginning of employment or continued employment to act as "consideration" (i.e., compensation) for an NCA, allows the courts to modify NCAs to make them enforceable, and renders NCAs enforceable even when an employer terminates an employee. At the other end of the spectrum, North Dakota Century Code 9-08-

[14]Following Bishara (2010), for questions in states where no legal precedent exists, we mark the value as missing. The composite index is a weighted average of scores on each of the seven legal dimensions. When the score for a question is missing, we omit it from the calculation of that weighted average, as in Bishara (2010). Out of 8,568 year-state-question observations (24 years, 51 states, 7 questions), a total of 900 (10.5%) are missing.

[15]Florida Statute 542.335. Full text available at `http://www.leg.state.fl.us/statutes/index.cfm?App_mode=Display_Statute&URL=0500-0599/0542/Sections/0542.335.html`

06 explicitly bans all NCAs in employment contracts.[16] Quantifying these statutes, Florida has the highest NCA Enforceability Score during our time period (which we normalize to 1), and North Dakota has the lowest score (which we normalize to 0).

Furthermore, law changes have led to sizable changes in the NCA Enforceability Score *within* states over time. Consider, for example, a state Superior Court case in Pennsylvania: Insulation Corporation of America v. Brobston (1995). The case concerned an employee of an insulation sales company who had signed an NCA. After being terminated for poor performance, he was hired by a competitor of his original employer, in alleged violation of the NCA. While the NCA in question was ultimately not enforced, the court's decision set new precedent that NCAs may generally be enforced following employer termination: "...the circumstances under which the employment relationship is terminated are an important factor to consider in assessing... the reasonableness of enforcing the restrictive covenant."[17] This case resulted in the component of the NCA Enforceability Score specific to treatment following employer termination (Q8) to change from 4 (out of 10) to 7 in Pennsylvania; the resulting change in Pennsylvania's overall NCA Enforceability Score was equal to roughly a third of a standard deviation in the distribution across our sample period.

Table 1 summarizes differences in levels of NCA enforceability across the country and within states over time, between 1991 and 2014. There are 82 within-state NCA law changes over our sample period, and these are dispersed roughly evenly across the Northeast, Midwest, South, and West regions. The average law change results in a change in the magnitude of the NCA Enforceability Score that is about 7% of the average score over this period, and the within-state standard deviation in enforceability is equal to roughly 17% of the overall standard deviation. Our analyses rely on these within-state changes in enforceability.

Figure 1 shows the timing of NCA law change events. Changes were relatively evenly dispersed throughout the study time period. There are a few more enforceability increases than decreases, though both are well-represented. Figure 2 shows the sample-weighted mean NCA Enforceability Score across states over the sample period. NCA enforceability has been generally flat or increasing over time, with an especially steep increase during the mid to late 1990s.

[16]North Dakota Century Code 9-08-06. Full text available at `https://www.legis.nd.gov/cencode/t09c08.pdf`

[17]Insulation Corp. of America v. Brobston, 667 A.2d 729, 446 Pa. Superior Ct. 520, 446 Pa. Super. 520 (Super. Ct. 1995).

3.1.1 Testing the Exogeneity of NCA Law Changes

Our ability to use within-state changes in NCA enforceability to identify its causal effect on earnings and mobility would be compromised if legal changes to NCA enforceability were correlated with states' underlying political, labor, or business characteristics that may also impact earnings growth. For instance, changes to enforceability could potentially be spurred by strong labor unions on the one hand, mobilized business interests on the other, or a general change in the business climate.

Ex ante, we expect this concern to be minimal. The majority of law changes in our sample are due to judicial decisions. In most cases, these decisions are initiated due to a legal case that is idiosyncratic to a particular occupation, industry, or employment relationship; however, the consequences of these decisions affect the state's labor law much more broadly. Relative to legislators, judges are less influenced by stakeholder pressure that could sway their decision-making because of the doctrine of *stare decisis*.[18]

To examine this possibility, we test whether states' political, social and economic characteristics predict NCA law changes. We use a variety of data sources. These include the University of Kentucky Center for Poverty Research's National Welfare Data (University of Kentucky Center for Poverty Research, 2018) on population, workers compensation beneficiaries, an indicator for whether the state governor is a member of Democratic party, the share of state house and senate representatives (respectively) in the Democratic party, minimum wage, and the number of Medicaid beneficiaries. We also use the database constructed in Caughey and Warshaw (2018) to obtain measures of policy liberalism (liberalism in the state as reflected by government policy) and mass liberalism (liberalism in the state as reflected by responses of individuals to policy questions), both of which are measured separately on social and economic dimensions. From this dataset we also obtain the percentage of voters who identify as Democrats. For more details on the construction of these measures, see Caughey and Warshaw (2018). Next, we gather data on the ideologies of state legislatures from McCarty and Shor (2015), including the State House and State Senate ideology scores, in aggregate as well as separately by Democrats and Republicans. Finally, we include data on union membership from Hirsch and Macpherson (2019).

Table 2 presents the results from a regression in which the dependent variable is a state's annual NCA enforceability, and the independent variables are each of the 20 characteristics noted above (lagged by one year), as well as state and Census division by year fixed effects (we use these same fixed effects in our subsequent analysis). Out of 20 variables, the vast majority have coefficients that are both economically and

[18]For a discussion of stare decisis, see Knight and Epstein (1996).

statistically insignificant. Only two of these 20 variables are statistically significant at the 10% level (the minimum wage and the State Senate Democrats ideology score), none are significant at the 5% level, and a joint F test on the statistical significance of these predictors is insignificant at the 10% level ($p = 0.184$).[19] Furthermore, the partial R^2 of the model, after residualizing on division by year and state fixed effects, is 0.113, meaning that these predictors collectively explain only 11% of the variance in within-state changes to NCA policy. Thus, these results provide supportive evidence that NCA law changes are indeed exogenous to underlying economic, political, or social trends.

To complement the evidence in Table 2, we further assess the concern that NCA law changes might not be exogenous in our subsequent analysis. We use an event study analysis in Section 4.2.2 to check for pre-trends in the outcome variable, and we show that our results are qualitatively robust to controlling for all the economic and political controls used in this section.

3.2 Data on Earnings and Mobility

We gather data on earnings, employment, mobility, and other labor market outcomes from four sources: the Current Population Survey (CPS) Annual Social and Economic Supplement, the Job-to-Job Mobility dataset, the Quarterly Workforce Indicators (QWI) dataset, and the CPS Occupational Mobility and Job Tenure Supplement (JTS). We describe each of these datasets, and how they fit into our analysis, in turn.

First, we gather individual-level data on earnings and employment from the CPS ASEC (otherwise known as the March Supplement).[20] The ASEC is a CPS supplement collected each March that contains information about the wage and salary income of respondents. The CPS also includes respondents' demographic and geographic information.[21] We restrict the ASEC sample to include individuals who reported having worked for a private-sector employer (not self-employed) in the year prior to being surveyed. We include the years 1991 to 2014, restrict to individuals who were between the ages of 18 and 64 at the time they were surveyed, and remove

[19]It is not surprising that two out of twenty predictors are statistically significant. The probability of finding two or more significant predictors (at the 10% level) out of twenty, conditional on each of the predictors having zero true effect and each being independent (which is surely not true in practice, but provides an adequate benchmark) is approximately 0.88 ($1 - 0.90^{20}$).

[20]Sarah Flood, Miriam King, Renae Rodgers, Steven Ruggles, and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 6.0 [dataset]. Minneapolis, MN: IPUMS, 2018. `https://doi.org/10.18128/D030.V6.0`

[21]While the ASEC is relatively small compared with, for example, the American Communities Survey (ACS), its existence precedes our earliest data on NCA enforceability (whereas the ACS does not). We are therefore able to leverage all changes in NCA enforceability from 1991-2014. Our results are quite similar if we instead use the ACS.

observations for which earnings or hours variables have been topcoded. The resulting ASEC dataset contains approximately 1.5 million observations, 1.2 million of which represent full-time workers. We deflate earnings and wages in the ASEC using the Consumer Price Index. We match NCA enforceability measures by state and year.

Our second dataset is the Job-to-Job Flows (J2J) dataset from the U.S. Census Bureau, which we use to examine the effect of enforceability on job mobility. Derived from the Longitudinal-Employer Household Dynamics dataset,[22] these data contain aggregate job flows between cells defined by combinations of age, sex, quarter, origin job state, destination job state, origin employer industry, and destination employer industry. We aggregate these data to the level of the state-industry-year, and we create three measures of job mobility that could potentially be affected by NCA enforceability: (1) the *total rate* of job-to-job separations per worker, (2) the share of job-to-job separations in which the separating worker's destination job is in a different state than his or her origin job, and (3) the share of job-to-job separations in which the separating worker's destination job is in a different industry than his or her origin job.

Third, we use the Quarterly Workforce Indicators (QWI) dataset from the Census Bureau. Like the J2J, the QWI is a public use file that aggregates data from the LEHD, and it contains data on earnings, as well as numbers of hires and separations, at the county-quarter level for the near-universe of private workers, stratified by sex and age group. We use the QWI both to complement the CPS in our estimation of the earnings effects of NCA enforceability, and also to investigate spillovers from enforceability. One drawback with the QWI for our purposes is that the QWI is not a balanced panel over our sample period, as some states did not begin reporting the necessary data until the late 1990s or later. For this reason, we are left with only 64 legal changes (instead of the universe of 82 legal changes) when using the QWI.

Fourth, in our investigation of the mechanism underlying the relationship between enforceability and earnings, we use data from the CPS Occupational Mobility and Job Tenure Supplement (JTS) over the years 1996 to 2014. The JTS is conducted biannually in either January or February. Among other things, it includes questions about the respondent's history of employment, such as "How long have you been working [for your present employer]?"[23] We use responses to this question to calculate the year that the worker began his or her job spell, which allows us to match

[22]U.S. Census Bureau. (2019). Job-to-Job Flows Data (2000-2019). Washington, DC: U.S. Census Bureau, Longitudinal-Employer Household Dynamics Program, accessed on April 7, 2020 at https://lehd.ces.census.gov/data/#j2j. Version R2019Q1.

[23]Note that "for your present employer" may alternatively be "for company name from basic CPS/as a self-employed person/at your main job." See http://www.nber.org/cps/cpsjan2016.pdf.

individuals to the enforceability score at the time of hire. Our outcome variable of interest is weekly earnings, and we use additional variables as controls. We merge in annual national unemployment rates between 1947 and 2014 from the Bureau of Labor Statistics website for the analysis, which we describe in Section 6.

4 The Effect of NCA Enforceability on Workers' Earnings and Mobility

In this section, we examine the effect of NCA enforceability on earnings and mobility. We then consider whether these effects are more pronounced among workers who are most likely to have signed an NCA, and we then show that our estimates are stable to numerous robustness checks and sensitivity analyses.

4.1 Main Results on Earnings

We use a difference-in-difference design to estimate the effects of NCA enforceability on earnings, leveraging intra-state variation in NCA enforceability over time. Our basic regression model is

$$Y_{ist} = \alpha + \beta * Enforceability_{st} + X_{it}\gamma + \rho_s + \delta_{d(s)t} + \varepsilon_{ist}, \tag{1}$$

where Y_{ist} is the outcome of interest, $Enforceability_{st}$ is a state's annual composite NCA enforceability score across the 7 dimensions described in Section 3, X_{it} is a vector of individual-level controls, ρ_s is a fixed effect for each state, and $\delta_{d(s)t}$ is a fixed effect for each Census division by year.[24] The coefficient of interest, β, is identified from changes in earnings in states that change their NCA enforceability, relative to other states in the same Census division over the same period. Standard errors are clustered by state. A key identifying assumption is $E(Enforceability_{st}\varepsilon_{ist}|\rho_s, \delta_{d(s)t}) = 0$: conditional on state and division-year effects, changes in enforceability are uncorrelated with the error term. The evidence in Section 3.1.1 supports this assumption.

Results are reported in Table 3. Columns 1-4 use data from the ASEC, restricted to full-time workers between the ages of 18 and 64 who reported working for wage and salary income at a private employer the prior year.[25] The coefficient in Column

[24]There are 9 Census divisions that partition the United States. We include division-year fixed effects to account for potential time-varying shocks to different areas of the country. The estimated effect of *Enforceability* on earnings, corresponding to Column 1 of Table 3, is roughly 50 percent larger (-0.177, $p < .01$) if we use year fixed effects in lieu of Division by year fixed effects.

[25]All results are very similar if we include part-time workers.

1 suggests that going from NCA enforceability of 0 (completely unenforceable) to 1 (the strictest enforceability observed in our sample) leads to an 11.0 percent decline in earnings ($exp(-.117)-1, p = .002$). Adding fixed effects for broad occupation codes in Column 2 diminishes the point estimate slightly but improves its precision ($p < .001$). To get a sense of the magnitude of this estimate, the 10^{th} and 90^{th} percentiles of *Enforceability* observed in our sample are 0.55 and 0.9, respectively. The estimates thus imply that moving from the 10^{th} to the 90^{th} percentile in *Enforceability* leads to a 3.5 percent average decline in annual earnings ($exp(-.101 * .35 - 1 = 0.035)$).

A negative effect of *Enforceability* on annual earnings could reflect either a decline in hours worked or a decline in workers' implied hourly wage. In Column 3, the dependent variable is instead the log of a worker's reported weekly hours:[26] while the point estimate is negative, it is relatively small and statistically insignificant ($p = 0.16$). In Column 4 the dependent variable is the individual's implied log hourly wage (calculated as annual earnings divided by fifty-two times usual weekly hours). The estimated coefficient is identical to the coefficient on annual earnings.

Finally, in Column 5, we corroborate the estimates in Columns 1–4 that used the CPS ASEC sample by using data from the QWI. We run essentially the same regression specification as Column 1, except that we are able to include fixed effects for each county (rather than state)[27] and each Division-Year-Quarter (rather than Division-Year). We weight the regression by county-level employment. The estimate is very similar in magnitude to that in Column 1 and highly statistically significant.

It is instructive to benchmark our results against the estimated wage effects of other labor market characteristics or institutions. One particularly instructive comparison is the effect of explicit employer concentration on wages: Prager and Schmitt (2019) find that large changes in employer concentration, caused by local hospital mergers, caused a 6.5 percent decline in wages among the most affected workers. As two comparable institutions, the household income premium associated with membership in a labor union is an estimated 15-20 log points (Farber et al., 2018); the income premium for workers in an occupation that requires a government-issued occupational license is estimated to be 7.5% Gittleman et al. (2018).[28] To derive a comparable effect of NCA enforceability, we can extrapolate our estimates to consider what would happen to earnings under a national policy that rendered all NCAs unenforceable. We generate predicted earnings for each individual in the 2014 ASEC sample using

[26]We include part time workers in this regression to avoid selecting the sample based on the dependent variable.

[27]The estimate is essentially unchanged if we instead use state fixed effects.

[28]Estimates of the wage premium associated with occupational licensing vary widely: for example, Redbird (2017) finds no wage premium using a 30-year comprehensive panel of licensing laws

coefficients from Column 1 of Table 3, for two different levels of NCA score: first, the NCA score observed in 2014 in that individual's state, and second, at the lowest observed NCA enforceability level (0). These predictions imply that average earnings among *all* workers would likely increase by 3.3% to 13.9% nationally if NCAs were made unenforceable.[29] The midpoint of this interval (8.6%) is similar to the effect of a large change in employer concentration, roughly one half the household premium from labor union membership, and comparable to the premium attained by workers in occupations with government-mandated licenses.

Figure 3 visually illustrates the relationship between annual earnings and NCA enforceability using binned scatterplots. Each graph plots earnings and NCA enforceability, net of state and census division by year effects. Panel (a) includes no additional controls, and panel (b) includes the additional controls used in Column 2 of Table 3 (1-digit occupation codes and individual-level demographic controls). Both figures clearly depict a strongly negative, roughly linear relationship between enforceability and earnings, corroborating the regression estimates.

Our NCA Enforceability Score pools seven dimension of NCA enforceability, but these dimensions might differ in their earnings effects. In Table C.2, we reestimate the effect of changes in NCA law on earnings in a specification analogous to Column 1 of Table 3, but focusing on each individual component of the composite NCA score separately. The first seven rows represent separate regressions identical to Equation 1, except that $Enforceability_{st}$ is replaced with each respective element of the NCA score described in Table C.1.[30] With two exceptions (which are both insignificant at the 10% level), the effect of each score is negative, and is significant at the 5% level for two out of seven components, and at the 10% level for one additional component. The dimensions yielding the greatest negative earnings effect are those requiring consideration, both at the outset of employment (Q3a) and after employment has already begun(Q3bc), consistent with evidence in Starr (2019). The existence of a state

[29]Specifically, let X_i be the vector of the values of all variables (including fixed effects), except for NCA enforceability score, that are present in the regression in Column 1 of Table 3 for each individual, i, in 2014. Let γ be the vector of respective coefficients estimated in the same regression, and let β_{Low} and β_{High} be the bounds of the 95% confidence interval for the coefficient on $Enforceability_i$, the NCA Enforceability Score for individual i's state of residence in 2014. Then, if $\hat{Y}_{i,1,j} = \gamma X_i + \beta_j Enforceability_i$ represents predicted earnings for individual i for $j \in \{Low, High\}$, and $\hat{Y}_{i,2} = \gamma X_i$ represents predicted earnings for individual i when $Enforceability_i = 0$, we report the averages of $[\hat{Y}_{i,2} - \hat{Y}_{i,1,j}]/\hat{Y}_{i,1,j}$.

[30]Estimating a model with each component of the score separately likely introduces some omitted variable bias, as elements of the score are correlated with each other. However, including all individual components of the score in the same regression causes the sample size to shrink significantly due to missingness in some of the components (where missingness indicates that the question has not been legally settled). That model, however, generates coefficients qualitatively similar to those shown in Table C.2.

statute (Q1) has a negative but insignificant earnings effect. This lack of an effect for Q1 is perhaps not surprising: some states that do not have explicit statutes regarding NCA enforceability (e.g., Kansas and Connecticut) nonetheless enforce NCAs more readily than many other states. Given this ambiguity of the Q1 dimension, in the final row of Table C.2 we replace $Enforceability_{st}$ with a modified version of the NCA Enforceability Score that omits the component related to existence of a state statute (Q1). The resulting coefficient is, if anything, stronger than that estimated in Table 3. Thus, no single dimension drives our results, and the dimensions with the largest effects are consistent with what one might expect based on theory and on prior results.

4.2 Assessing the Causal Interpretation and Robustness of the Estimated Earnings Effect

We conduct three distinct tests to assess the causal interpretation of our results, which we describe in turn below.

4.2.1 Heterogeneous Earnings Effects Based on Prevalence of NCA Use

The results in Table 3 imply that stricter NCA enforceability leads to lower earnings for the average worker. This relationship should be stronger in settings in which NCAs are used more often; in the limiting case, if NCAs are never used for a certain group of workers, we should expect no effect of NCAs on earnings for those workers (unless spillover effects are sufficiently large).

In this section, we examine heterogeneity in the effect of enforceability by prevalence of NCA use. This exercise serves two useful purposes. First, it serves as a test of the robustness of the results reported in Section 4.1. If we find that enforceability has larger earnings effects among groups less likely to be bound by NCAs, it might raise questions about the research design. Second, this exercise allows policymakers to assess the impact that changes in NCA enforceability will have on the earnings of groups more likely to be exposed to NCAs.

While we do not observe whether individual workers have or have not signed an NCA, Starr et al. (2021) report several sources of heterogeneity in NCA use by worker characteristics. We focus on three sources: workers' education, occupation, and industry. First, Starr et al. (2021) find that workers with a Bachelor's degree or higher are significantly more likely to sign NCAs than workers without a college degree. Second, Starr et al. (2021) find heterogeneity in use across 22 occupation categories and 19 industry categories. We use the occupation and industry in which an individual reports working to the CPS to classify workers as working in *High or Low NCA Use Occupations* and *High or Low NCA Use Industries*.[31] We replicate our main difference-in-difference specification, Equation 1, except that we now add an interaction term of *Enforceability* with an indicator for *College Educated Worker*, *High NCA Use Occupation*, or *High NCA Use Industry* (as well as an indicator for the respective main effects).

Table 4 reports these heterogeneity estimates. Column 1 reports the baseline average effect on earnings, corresponding to Column 1 in Table 3. Column 2 includes an interaction of NCA Enforceability Score with an indicator for whether a worker has a college degree (*College Educated Worker*). The main effect on *NCA Enforceability Score* is close to zero and statistically insignificant, implying that enforceability has little to no effect on earnings for non-college educated workers. On the other hand, the interaction term $(-0.143, p < .01)$ implies that enforceability has a much stronger effect on earnings of college-educated workers. The sum of the main effect on *NCA Enforceability Score* and the interaction effect implies that going from the 10^{th} to 90^{th} percentile of enforceability leads to a 6.0% decrease in earnings for college-educated workers $(exp((-0.035 + -0.143) * 0.35) - 1 = -0.06, p < .01)$, an earnings effect that is 70 percent larger than the earnings effect for the whole population implied by Column 2 of Table 3.

Column 3 reports heterogeneity by occupational use of NCAs. The estimates imply that going from the 10^{th} to 90^{th} percentile of enforceability leads to a 4.9% decrease in earnings in high-use occupations $(exp((-0.083 + -0.061) * 0.35) - 1 = -0.049, p < .01)$; the effect for low-use occupations is roughly half as large $(p = .02)$, and the difference is statistically significant $(p < .01)$. Finally, Column 4 reports heterogeneity by industries' use of NCAs. Going from the 10^{th} to 90^{th} percentile of enforceability leads to a 5.5% decrease in earnings in high-use industries $(p < .01)$; the effect for low-use industries is roughly 60% as large $(p < .01)$, and the difference is statistically significant $(p < .01)$.

In Column 5, we simultaneously estimate the heterogeneous impacts of NCA enforceability along these three categories. The coefficients on the interactions of NCA Score with *High Use Occupation* and *High Use Industry* attenuate, but remain neg-

[31] We define Low NCA Use Occupations as Farm, Fish and Forestry; Legal Occupations; Grounds Maintenance; Food Preparation and Serving; Construction; Extraction; Transport and Materials Moving; Office Support; and Community and Social Services, and High NCA Use Occupations as all others. Low NCA Use Industries are Agriculture and Hunting; Accommodation and Food Services; Arts, Entertainment, and Recreation; Construction; Real Estate; Transportation and Warehousing; Retail Trade; Other Services; and Management of Companies. These occupations and industries represent those with NCA use below or above the national average, according to Figures 5 and 6 in Starr et al. (2021).

ative and significant. The interaction of NCA Score with *College Educated* changes little and remains statistically significant.[32]

4.2.2 Distributed Lag Estimates on Earnings

Two concerns are common with difference-in-difference designs. The first is evaluating the plausibility of the assumption that treatment and control groups would counterfactually follow common trends in the absence of a law change in the treated state. In our context, this assumption might be violated if, for example, business or labor advocacy organizations change lobbying efforts that influence both earnings and judges' decision-making. Such effects would constitute a form of reverse causality. A second concern is imbalance in treatment timing. Our regression design leverages changes in NCA laws that occurred in different states in different years (and that were of different magnitudes). This variation in treatment timing can give differential weight to states depending on the distribution of event times within the sample; this weighting could cause the interpretation of our estimates to differ from that of an average treatment effect (Goodman-Bacon, 2018).

To address these concerns, we complement our difference-in-difference estimates with a distributed lag model, which allows us to assess the dynamic effects of an NCA law change in the years immediately before and after the change takes place. A distributed lag model is similar to an event study model: Schmidheiny and Siegloch (2020) show that a distributed lag model with leads and lags is in fact numerically identical to an event study model with binned endpoints. For our setting this model is better suited because it accounts for different magnitudes of NCA law changes, unlike the standard event study model that uses an indicator variable for treatment.

We estimate the distributed lag regression in first differences, similar to the approach used by Fuest et al. (2018).[33] We collapse the CPS data to the state-demographic group-year level and estimate the following model:

$$\ln w_{s,g,t} - \ln w_{s,g,t-1} = \sum_{k=-4}^{k=5} \beta_k [Enforceability_{s,t-k} - Enforceability_{s,t-k-1}] + \delta_{d(s),t} + \varepsilon_{s,g,t}.$$

where $\ln w_{s,g,t}$ is the log of the average earnings in state s, demographic group g, and

[32]Since college-educated workers tend to get paid more than those without a college degree, this stability of the *College Educated* estimate is consistent with the evidence in Starr et al. (2021) that NCA use is increasing in workers' annual earnings.

[33]Our setting is similar to that in Fuest et al. (2018), who estimate the effects of corporate tax changes on wages. They consider tax changes across municipalities that occur at staggered times, can occur multiple times in one municipality over the panel, and are of different magnitudes, all of which is also true in our setting.

year t. We define demographic groups as individuals with the same values of the following variables: white/nonwhite, college/non-college, male/non-male, and age category.[34] Because this is a first difference model, time-invariant characteristics are differenced out and thus not included as controls. We weight the regression by the number of ASEC observations in each s, g, t cell, and we cluster standard errors by state.

For comparison we also estimate a distributed lag model using the QWI sample, which is based on the universe of jobs in the U.S.. In this specification the unit of observation is a county $c(s)$, demographic group g (defined as combinations of sex and age), and quarter $q(t)$.[35]

As illustrated by Schmidheiny and Siegloch (2020), because the distributed lag model measures treatment effect changes, to obtain event study estimates we calculate the cumulative sum of the distributed lag coefficients away from the normalized year, $j = -1$.

We report the results from these models in Figure 4, for the ASEC sample (Panel A) and the QWI sample (Panel B). Both figures depict two noteworthy features. First, there is little evidence of a pre-trend in earnings using either sample, supporting the assumption (and the evidence in Section 3.1.1) that NCA law changes were largely exogenous to underlying economic trends. Second, earnings begin to decline in the first year following the law change, and the effects grow in magnitude in each of the following three years. The decline in earnings becomes statistically significant by year two in the QWI sample, and by year three in the CPS sample.[36] The two samples also yield similar estimates of the earnings decline, with overlapping confidence intervals in each of the post-event years.

While Figure 4 provides strong support to the validity of our baseline difference-

[34]This grouping includes some, but not all, of the control variables we include in our baseline regression specification reported in Column 1 of Table 3. We omit married/non-married, Hispanic/non-Hispanic, and metro area indicators to avoid creating overwhelmingly small groups, which could be prone to outliers. Our estimates are stable to alternative choices of which variables to include. We create four arbitrary age categories of: under 30, 30–39, 40–49, 50 and older. Our results are not sensitive to using alternative partitions to construct these categories.

[35]For simplicity, we use data from the third quarter of each year. The results are essentially unchanged if we keep a different quarter, or if we create annual averages. The specific distributed lag model we estimate using QWI data is:

$$\ln w_{c(s),g,t} - \ln w_{c(s),g,t-1} = \sum_{k=-4}^{k=5} \beta_k [Enforceability_{s,t-k} - Enforceability_{s,t-k-1}] + \delta_{d(s),t} + \varepsilon_{c(s),g,t}.$$

We weight observations by cell employment and cluster standard errors by state.

[36]The delay in effects could be due to delays in knowledge about law changes, frictions in adjusting contracting terms, grandfathering of contractual provisions, or other factors.

in-difference design, there are other methodological choices available to researchers to check for pre-trends and examine dynamic effects. In Appendix B, we discuss a variety of alternative approaches we considered. First, we estimate a "stacked" event study model, following Cengiz et al. (2019), where we compare the changes in earnings in the three years before and after an NCA law change in the state experiencing the law change to other states in its same Census division that did not experience a change over the same period. Second, we estimate a similar event study, but only consider "clean" event windows in a balanced panel and use a continuous treatment measure. Third, we use a long-panel event study, which avoids the complications of staggered law changes and multiple law changes within-states over the time period. Each of these approaches corroborates both the direction and magnitude of our baseline results, indicating that our results are not sensitive to particular choices in our specification.

4.2.3 Addressing Other Threats to Identification

In Section 3.1.1, we provided evidence that economic, social, and political factors do not collectively predict changes in NCA enforceability, and this argument was corroborated by the lack of pre-trends in the event study graph just shown. Still, we can ensure that coincidental changes in these factors are not driving our estimated effect of enforceability. We assess the robustness of our estimates to this concern in Table C.3. We replicate the structure of Table 3, but we include additional controls for each of the predictors included in Table 2.[37] While there are minor changes in the magnitudes of estimates, the qualitative conclusions are all unchanged, supporting the causal interpretation of the *Enforceability* coefficient in our regressions.

Focusing on the institutions underlying our identifying variation, we argued above that judicial decisions, which make up the vast majority of NCA law changes, are less prone to endogeneity than are statutory changes from legislative action. However, there is some evidence that judges' decision-making can be swayed by external forces like business interests, particularly for judges that are elected rather than appointed (Katz, 2018). To ensure that our results are not driven by confounding influences on elected judges we obtained data on how judges are selected across states from Bannon (2018). We recreate our main analyses a) excluding the 6 states that have partisan judicial elections (i.e., judges are selected via election and the judge's political party is listed on the ballot) and b) excluding the 21 states in which judges are elected

[37]We omit the ideology variables gathered by McCarty and Shor (2015), which were only calculated since 1993. Inclusion of those variables (which limits the sample period) does not substantively change the estimates.

(whether or not the elections are partisan), in Tables C.4 and C.5, respectively. If anything, our point estimates are *larger* in magnitude with these restricted samples (they become more imprecise in the latter table, which is to be expected since we are eliminating over 40% of the states in our sample). Since judicial elections are a key mechanism through which political or economic preferences of voters might affect judicial decisions, this evidence provides further reassurance against this potential form of endogeneity.

4.3 Effects of Enforceability on Job Mobility

While the main focus of our analysis is the earnings effect of NCA enforceability, we also estimate its effect on worker mobility. This analysis is useful because it serves as validation that our variation in enforceability is capturing what NCAs are designed to do—restrict workers' mobility.

Table 5 presents estimates based on job mobility data from the J2J dataset. We explore three measures of job mobility, each measured within state-year-quarter-sex-age group-industry cells: the overall job-to-job separation rate,[38] the share of job-to-job flows across state lines, and the share of job-to-job flows across two-digit NAICS industries. For each of these measures, we estimate the overall effect of NCA enforceability, as well as the differential effect for *High NCA Use Industries*, which we defined in Section 4.2.1.

In Column 1 we estimate the effect of the origin state NCA enforceability score on the overall job-to-job separation rate and find a small and statistically insignificant effect. However, in Column 2 we interact NCA enforceability with an indicator for whether the origin job was in a high NCA use industry, and find that NCA enforceability substantially reduces job-to-job separations in high use industries. The coefficient on *High NCA Use Ind* $\times$ *NCA Score* is negative (-0.199) and highly significant ($p < .01$). The estimate implies that moving from the 10^{th} to the 90^{th} percentile of NCA enforceability decreases the rate of job-to-job separations by 6.0% in high use industries.

In Columns 3 and 4 we test for effects on the share of job-to-job transitions that occur across state borders. In high NCA use industries, stricter enforceability increases the geographic distance associated with job changes. Moving from the 10^{th} to the 90^{th} percentile of NCA enforceability increases the share of job changes that

[38] We define the overall job-to-job separation rate as the number of new hires in a cell with no nonemployment spell or a short nonemployment spell, divided by the total employment in that cell. At first glance it might seem more appropriate to name this measure job-to-job *hiring* rate, but it nonetheless represents all of the separation events that resulted in a job-to-job transition.

cross state lines by 0.6%. This estimate suggests that greater NCA enforceability forces workers bound by NCAs to move further to escape restrictions imposed by NCAs, which typically include a geographic component. In the case of labor markets that are defined by industries rather than geography, escaping an NCA may require changing industries. In Columns 5 and 6 our dependent variable is the share of job-to-job transitions in which a worker switches industries. The coefficient on *High NCA Use Ind* $\times$ *NCA Score* is negative, but it is small and not statistically significant.

This evidence reveals that NCA enforceability has meaningful effects on both the level and direction of workers' job mobility, and it illustrates that our measures of NCA enforceability capture actual changes to the *effective* use of NCAs. The results also motivate our investigation into one mechanism through which NCA enforceability affects earnings, which we describe in Section 6.

5 Spillover Effects of NCA Enforceability

If NCA enforceability only affects wages by shifting the incidence of NCA use, then our results thus far could be considered "intent-to-treat" effects, in which the "treatment" is signing an (enforceable) NCA. In this case, one could scale our estimates by the share of workers that sign an NCA to recover local average treatment effects of signing an NCA. However, in Section 2, we described theoretical reasons why NCA enforceability could have indirect, or spillover effects on the wages of a broader group of workers, meaning the earnings effects of NCA enforceability might not be limited only to those workers signing NCAs. In this section we show that such spillover effects are present and economically meaningful.

We examine whether legal changes to NCA enforceability in a "donor" state affect workers who share a local labor market with that state but work in a different state. Consider the St. Louis metro area, which includes counties in Missouri but also several counties across the state border in Illinois. If Illinois experiences an NCA law change, does it affect the earnings of workers employed on the Missouri side of the St. Louis metro area? And vice versa if Missouri experiences a law change?

We measure local labor markets as commuting zones, which are clusters of counties that have strong commuting ties and have been used in many prior studies as measures of local labor markets (e.g.,Autor et al. (2013)). We identify commuting zones that straddle state borders: these commuting zones are local labor markets that include business establishments in two states and are therefore subject to two different NCA enforcement regimes (as well as changes therein). We remove 8 commuting zones that contain counties in more than 2 states (to ensure clarity of measurement for

Donor State NCA Score, the variable measuring enforceability in the cross-border state). These restrictions leave us with a set of 137 commuting zones. In our main analysis, we focus on the 545 counties in these commuting zones that themselves lie directly on state borders; with this restriction, we avoid counties such as Los Angeles County, which shares a commuting zone with counties in Arizona but is nearly 200 miles driving distance from anywhere in Arizona.

We employ data from the QWI, which, as described in Section 3, includes quarterly earnings and employment flows at the county level, separated by various firm characteristics and worker demographics. Each observation in the dataset represents a unique year, quarter, county, sex, and age group cell.

To test for spillovers, we use an analog of the difference-in-difference model corresponding to Equation 1 to estimate the impact of a change in NCA enforceability across a state border, among workers employed in a commuting zone that straddles the state border. The outcome variable is the log of average quarterly earnings within each cell for all private sector employees. We estimate the model:

$$\begin{aligned} Y_{ctga} = \phi_0 + \phi_1 * Enforce_{ct} + \phi_2 * BorderEnforce_{ct} \\ + \phi_3 * Female_g + \psi_a + \zeta_c + \Omega_{d(c)t} + \varepsilon_{ctga}, \end{aligned} \tag{2}$$

where c indexes county, t indexes year-quarter, g indexes sex, a indexes age group, and $d(c)$ indexes the Census division in which county c is located. $\Omega_{d(c)t}$ is a Census division by year-quarter fixed effect. The primary coefficient of interest is ϕ_2, which is an estimate of the spillover effect on workers in county c of enforceability in the state that borders the commuting zone in which county c is located. ϕ_1 estimates the direct effect of enforceability in a worker's own state, analogous to our estimates thus far. We cluster standard errors two ways by state and commuting zone.

We report results in Table 6. Column 1 verifies that the direct relationship between (own) state NCA scores and earnings holds in this restricted sample. The coefficient on *Own State NCA Score* is -0.178, and statistically significant ($p < .01$). This magnitude is slightly larger than the main estimates reported in Table 3 (though the difference between the estimates is not significant). Column 2 includes the *Donor State NCA Score*. In this model the direct effect of *Own State NCA Score* increases slightly to -0.207, $p = .003$, while the coefficient on *Donor State NCA Score* reveals evidence of meaningful spillover effects: the coefficient is negative and economically and statistically significant (-0.181, $p = .021$).

The spillover effect is large in magnitude, but not implausibly so. One may be tempted to compare the magnitudes of the 'donor state' and 'own state' coefficients, but this comparison is difficult to interpret. The main effect of own-state enforceability represents a weighted average of the direct effect of enforceability on workers that sign NCAs and indirect effects on workers that do not sign NCAs. As described in Section 2, the direct wage effect on NCA-signers includes positive components, for example caused by human capital investments or compensating differentials, as well as the prevailing negative component.[39] In contrast, enforceability only affects wages in border counties of neighboring states via the indirect effect, and is thus unlikely to include any positive component. In the next section we conduct several tests to evaluate the reliability and clarify the interpretation of these spillover estimates.

5.1 Assessing the Interpretation of Spillover Estimates

We conduct three tests to corroborate the interpretation of these spillover estimates of NCA enforceability across state borders. First, we test whether the magnitude of spillover effects varies in proportion to the relative sizes of the labor forces on each side of a bisected commuting zone. Second, we estimate heterogeneity in the magnitude of spillover effects by distance from state borders. Finally, we consider whether alternative mechanisms can explain our spillover results.

We first examine heterogeneity in spillover effects among border counties. Intuitively, in a commuting zone bisected by a state border, the magnitude of a spillover effect from a 'donor state's' law change should be smaller if the donor state comprises a small share of total employment in the commuting zone. Conversely, if the 'donor state' is the primary location of employers in the commuting zone, any change in labor market conditions resulting from an NCA law change in the donor state should create greater leverage on labor market conditions across the border in the neighboring state.

Column 3 of Table 6 shows our estimates of this heterogeneity. Along with their main effects, we include interactions of the 'own state' and 'donor state' NCA Scores with the share of the commuting zone labor force that is employed on the 'own state' side of the border. Since the unit of observation in this regression is at the county-demographic group-quarter level, we calculate these shares at the demographic group (age-sex combinations) level.[40] The results show that spillover effects are heterogeneous in a manner consistent with the logic above. The main effect of *Donor State*

[39] Indeed, Starr et al. (2021) find that workers who sign an NCA earn 7 percent *higher* wages than observationally similar workers who do not (though this relationship could be due to selection on unobservables).

[40] We also include the main effect of this ratio but do not report its coefficient in the table.

NCA Score, representing the spillover effect in a county that comprises zero percent of its commuting zone's employment, is negative (-.210, $p < .01$). However, the spillover effect is substantially smaller in counties that contribute a large share of employment in their commuting zone. In the extreme case in which a county contains 100% of commuting zone employment, the estimated spillover effect is -0.047 (-0.047 = -0.210 + 0.163*100%) and statistically insignificant ($p = .511$).[41]

Our main estimates of spillover effects consider wages in adjacent pairs of counties bisected by state borders. In Table C.7 we present estimates from models that test whether the magnitude of spillovers change with distance to the state border. We present three supplemental estimates from samples that include (1) interior counties that are neither in commuting zones that straddle state borders nor on state borders; (2) the subset of these interior counties that lie at least 50 miles from any state border; and (3) the subset that lie at least 100 miles from a border. We assign to each county a 'Donor State NCA Score' that corresponds to the state geographically closest to that county.[42] Reassuringly, the point estimate on *Donor State NCA Score* decays progressively in each of these three subsamples, with coefficients -0.084, -0.058, and -0.011, respectively.[43] None of the coefficients are statistically significant.

As a third test, we examine whether spillover effects of NCA enforceability could be driven by alternative mechanisms that we have not considered. In Section 2.2, we discussed several reasons why strict NCA enforceability could generate the negative externalities on earnings documented in this section, including by thinning labor markets or giving firms wage-setting power. Other explanations are possible. For example, workers may decide to find a job across state lines if their own state increases

[41] Unlike the analysis with the QWI dataset that we reported in Table 3 and Figure 4, we leave the regressions in Table 6 unweighted. We do this for two reasons. First, we weight the prior QWI analysis by employment to estimate an average treatment effect for the US population; because the sample in Table 6 is limited to border counties, weighting serves no such purpose. Second, spillover effects (as we show) are likely to be more pronounced in counties with a small share of employment. Therefore, an estimate that weights observations by employment may reveal little to no impact of Donor State NCA Score. We report a weighted version of Table 6 in Table C.6, which indeed shows an attenuated average effect. However, Column 3 reveals that the heterogeneity based on employment shares in the CZ in Column 3 persists in the weighted specification, as expected.

[42] Specifically, we calculate the distance between county centroids. If the centroid of a county in a different state is less than m miles from the centroid of the focal county, we exclude that focal county from the relevant regression. We assign Donor state NCA scores by finding the county in a different state whose centroid is closest to the focal county's centroid, and using that donor state's NCA score. Note that this approach to assign Donor state NCA scores is slightly different from the approach used in the results reported in Table 6, where we assigned the cross-border state's NCA score to be a focal county's Donor score. These two approaches to assign Donor Score are often identical, but they diverge in a handful of cases; this discrepancy drives the slight divergence in estimates of wage effect of the *Donor State Score* reported in Table C.7 and Table 6.

[43] At the same time, however, the point estimate on *Own State NCA Score* reveals that the direct effect of own-state NCA score remains stable across these various geographic restrictions.

NCA enforceability. Such behavior would cause an outward shift in labor supply in border states, causing the market-clearing wage to decline. We find no evidence, however, that such worker behavior can explain the spillover effects on earnings. In Table C.8, we present estimates of the spillover effects of enforceability on workers' *mobility*. The structure mimics Table 6, except that our dependent variables are the log quarterly number of hires and separations from QWI in Columns 1 to 3 and 4 to 6, respectively. Across all six columns, enforceability in a worker's *own* state has a negative effect—of roughly the same magnitude—on hires and separations, corroborating the mobility results we found in Section 4.3 using the J2J dataset. The spillover effects (reported in Columns 2 and 5) are imprecisely estimated, though they are negative and of a magnitude that is 40-50 percent as large as the direct effect.[44] Thus, there is no evidence that workers move across state lines in response to an NCA law change in their own state; if anything, these estimates suggest that strict NCA enforceability *reduces* cross-border mobility.

Collectively, these results on earnings and mobility provide evidence that NCA enforceability reduces earnings and labor market churn, even across state borders. Though we cannot observe which workers sign NCAs, these results suggest that NCA use has external effects on workers and firms that do not use them, consistent with the theoretical considerations discussed in Section 2.2.

5.2 Interpreting Enforceability Effects in the Presence of Spillovers

Our results thus far have illustrated that NCA enforceability has meaningful effects on workers' earnings, and this effect extends across legal jurisdictions within labor markets. A natural related question is what effect *signing* an NCA has on a worker's earnings. We do not answer this question for several reasons.

First, to our knowledge there does not exist a large-scale, representative panel dataset on individual NCA use. Notably, it is hard enough to measure cross-sectional use of NCAs because many workers do not know whether they have signed an NCA: Starr et al. (2021) found that 29.7% of workers did not know whether their current job contract included an NCA. However, even if a panel dataset existed that perfectly captured NCA use existed, it would not necessarily be useful for answering the earnings effect of signing an NCA. The decision by workers and firms to use NCAs is likely to be correlated with many unobserved worker and firm characteristics, such

[44] Additionally, Columns 3 and 6 document an identical pattern of heterogeneity to that observed on earnings: an NCA law change in a donor state has a larger effect on mobility in a focal county among counties comprising a small portion of the commuting zone's total employment, compared to counties comprising a large share.

as intangible capital and opportunities for investments, causing endogenous selection into employment contracts with NCAs (Starr et al., 2021). This endogeneity makes it challenging to separately estimate the effects of enforceability on the earnings of workers that sign NCAs and on those that do not sign NCAs.

More importantly, our finding that NCA enforceability has meaningful spillover effects within local labor markets implies that NCA enforceability affects both NCA signers directly and non-signers indirectly. Therefore one cannot consider our estimates as intent-to-treat effects of signing an NCA, and re-scale to local average treatment effects (LATEs) using data on NCA use. In this same spirit, many studies in economics that have conducted individually randomized controlled trials report main estimates in the form of intent-to-treat estimates when spillover effects are important. For example, Miguel and Kremer (2004) study an RCTs that randomly allocated deworming medication to students in Kenya. The authors show that estimates from individual-level randomization are biased because these estimates miss the effects of externalities across participants. In their recent re-analysis of the RCT, the authors explain that they "focus on intention-to-treat estimates...because previous research shows that untreated individuals within treatment communities experienced gains (Miguel and Kremer 2004), complicating estimation of treatment effects on the treated (TOT)" (Hamory et al., 2020). Our evidence of large spillover effects of NCA enforcement onto workers who are not bound by the policy change suggests that the same type of externalities exist in our setting.

Moreover, the intention-to-treat estimate of NCA enforceability is the most policy-relevant estimand. From the perspective of policymakers, the set of available choices consists of enforceability policies. The endogenous responses to those policies by workers and firms are largely beyond the control of policymakers. Such endogenous responses are also irrelevant to the primary policy question of how enforceability maps to labor market outcomes; our intention-to-treat estimates directly answer this question.

6 Why Does NCA Enforceability Reduce Earnings? The Role of Costly Mobility

In Section 2, we discussed channels through which enforceable NCAs, by restricting workers' mobility, could reduce earnings. We conduct two tests to examine how such costly mobility affects wages. First, strict NCA enforceability reduces the returns to tenure. Second, and in part explaining the lower returns to tenure, strict enforceability diminishes workers' ability to take advantage of favorable labor markets to increase their wage over the course of their job tenure.

6.1 NCA Enforceability and the Returns to Tenure

Workers' wages rise as their tenure at a job increases (Shi, 2009). Our framework in Section 2 notes two contrasting ways that NCA enforceability could affect these returns to tenure. First, if NCAs spur more rapid human capital accumulation, wages may rise more quickly with tenure under stricter NCA enforceability. Second, since NCAs (by construction) limit a worker's outside options, wages may rise *less* quickly with tenure under stricter enforceability.[45] Unless these two effects cancel each other out, we would expect differential returns to tenure under different NCA enforceability regimes.

The direction of this effect is straightforward to ascertain with data on workers' tenure and earnings, which are fortunately contained in the CPS JTS. Limiting the data to full-time, private-sector workers in the years 1996-2014 (as CPS JTS data has only been collected since 1996), we estimated the model:

$$\ln w(i,t) = \omega_1 X_{i,t} + \omega_2 Tenure_{i,t} + \omega_3 Enf_{\tau(i),s(i)} + \omega_4 Tenure_{i,t} * Enf_{\tau(i),s(i)} + \varepsilon_{i,t}, \quad (3)$$

where $w(i,t)$ is the wage of individual i at time t, $X_{i,t}$ is a vector of individual level characteristics (race, Hispanic status, sex, marital status, age, age squared, tenure, tenure squared, education, and industry dummies), $Tenure_{i,t}$ is worker i's tenure at time t, and $Enf_{\tau(i),s(i)}$ is the NCA enforceability score in state $s(i)$, the state in which worker i works, at time $\tau(i)$, the year in which the worker began their job spell. The model ultimately estimates returns to tenure, allowing the rate of return to vary by NCA enforceability at the beginning of a worker's job spell (the most likely time the worker would have signed an NCA). We also estimate a version in which tenure is quadratic, and enforceability is interacted with tenure and tenure squared, to allow for nonlinear returns to tenure.

We report results in Table 7. Column 1 shows that wages increase in job tenure, consistent with many prior studies; the inclusion of a quadratic term in tenure in Column 2 demonstrates that this relationship is concave. Column 3 shows that returns to tenure are diminishing in NCA enforceability. When enforceability is strict, workers' earnings increase less with tenure than when enforceability is lax. This implies that

[45] This second effect could be present even for workers that have not signed an NCA. The evidence of spillovers in Section 5 implies that strict enforceability reduces labor market dynamism (and thus the arrival rates of outside offers) for a broader set of workers than those bound by an NCA.

the diminished ability of workers to get earnings increases by threatening to leave their job when enforceability is strict is greater than the effect of increased human capital accumulation.

We note that, while this analysis is consistent with the overall negative wage effects reported in Section 4.1, it suffers from a major shortcoming: tenure is likely endogenous to NCA enforceability. We are thus using an outcome (tenure) as an explanatory variable, which might bias the estimated effects of enforceability. We therefore view this analysis as suggestive; next we conduct a more formal test of why enforceability reduces the returns to tenure specifically, and wage growth more broadly.

6.2 NCA Enforceability Reduces Workers' Ability to Leverage Tight Labor Markets

The results in Table 7 are consistent with a specific way that (enforceable) NCAs reduce wages: namely, by reducing workers' threat of departure, and their ability to leverage this threat for pay increases. Indeed, Bagger et al. (2014), whose model underlies our conceptual framework in Section 2, find that outside offers that workers accrue through job search are important for within-job wage growth, and in fact account for a meaningful share of overall wage growth. It is not in the scope of this paper to apply the Bagger et al. (2014) model in the context of the U.S. and estimate the extent to which NCA enforceability shuts down this channel. Rather, we embed NCA enforceability in a different classic model of wage setting that yields similar implications for how the ease of mobility affects wages.

A longstanding theory in labor economics is that wages are not determined in a spot market, but rather by "implicit contracts" in which firms insure workers against declines in their wage. The influential paper by Beaudry and DiNardo (1991) (hereafter, BDN) theorized that wages will behave differently depending on whether or not workers' mobility across jobs is costly. If mobility is costly—that is, it is difficult for workers to find another job once they have begun a job spell—then labor market conditions at the time a worker begins her spell will determine her wage for the duration of her spell. If labor market conditions improve, the worker's costly mobility means she cannot take advantage of new job opportunities. Alternatively, if mobility is costless, a worker cannot commit to a contract if her outside option subsequently improves; because the worker can threaten to quit, improvements in labor market conditions induce employers to raise wages. Therefore, costless mobility implies that the best labor market conditions over the course of a worker's job spell will be correlated with her current wage.

BDN develop a simple empirical method to test between these models. They find strong evidence consistent with a model of implicit contracts with costless mobility: the effect of the most favorable labor market conditions over a worker's job spell exceeds and washes out any effect of the contemporaneous condition (predicted by a spot market) or the condition at the time of hire (predicted by an implicit contracts model with costly mobility). This result has been replicated numerous times with different datasets and time periods (e.g., Schmieder and Von Wachter, 2010).

However, by construction, NCAs make mobility costly. NCA enforceability may thus change the nature of implicit contracts. When NCAs are more enforceable, workers cannot leverage improvements in their outside option during a job spell, and their wage will be determined in much larger part by the initial labor market condition than in states where NCAs are less enforceable. This result would explain one reason why strict NCA enforceability attenuates the returns to tenure (Table 7).

We begin by replicating BDN. Using the CPS JTS, and limiting our analysis to full-time, private sector workers, for the years 1996-2014 (compared to BDN, who used the years 1976 to 1984).[46] We estimate the model:

$$\ln w(i, t + j, t) = \Omega_1 X_{i,t+j} + \Omega_2 C(t, j) + \varepsilon_{i,t+j}, \tag{4}$$

where $w(i, t + j, t)$ is the wage of individual i at time $t + j$ who began her job spell at time t. $C(t, j)$ is a vector of unemployment rates which, depending on the model, include *Initial UR* (the unemployment rate at the beginning of the individual's job spell) and/or *Minimum UR* (the lowest unemployment rate between the beginning of the job spell and the time of measurement of the wage). Following BDN, we use annual national unemployment rates from the Bureau of Labor Statistics. $X_{i,t+j}$ is a vector of individual level characteristics. Again following BDN, in $X_{i,t+j}$ we include race, Hispanic status, sex, marital status, age, age squared, tenure, tenure squared, education, and industry dummies. We depart from the BDN specification in three minor ways. First, we do not include Metropolitan Statistical Area (MSA) fixed effects: doing so decreases our sample size by approximately 25% (due to individuals whose MSA has been omitted from public use extracts of CPS supplements). In their stead, we use dummy variables for metropolitan area status (as used in Equation 1). Second, we include Census division by year fixed effects to harmonize with the main estimates of the effects of NCA enforceability. Third, we do not consider the contemporaneous unemployment rate, which would be collinear with Division by

[46] We omit years prior to 1996 due to a lack of data availability: though BDN use CPS data collected prior to 1996, the dataset we employ (the CPS JTS) has only been collected since 1996.

year fixed effects. Each of these adjustments ultimately has little bearing on our estimates.[47]

We report these results in Table 8. Columns 1–3 replicate the BDN main results for our sample period. In Column 1 we include only the unemployment rate at time of hire (*Initial UR*): our estimated coefficient has a smaller magnitude than that estimated in BDN (ours: -0.008; BDN: -0.030), but it is negative and highly statistically significant ($p < .01$). Column 2 uses, instead, the minimum unemployment rate over the course of the worker's job spell (*Minimum UR*). Similar to BDN, we find a negative and statistically significant effect. Column 3 mimics the main finding of BDN: including both *Initial UR* and *Minimum UR* attenuates the coefficient on *Initial UR* close to zero but leaves the coefficient on *Minimum UR* negative and highly significant ($p < .01$). In other words, on average, wages are consistent with a model of implicit contracts with costless mobility—just as Beaudry and DiNardo (1991) and the subsequent literature have found.

To test the hypothesis that NCA enforceability shifts the labor market from an implicit contracts model with costless mobility to one with costly mobility, we estimate the model:

$$\ln w(i,t+j,t,s) = \Omega_1 X_{i,t+j} + \Omega_2 C(t,j) + \Omega_3 Enf_{t,s} + \Omega_4 C(t,j) * Enf_{t,s} + \varepsilon_{i,t+j}, \quad (5)$$

where $Enf_{t,s}$ is the NCA enforceability score in state s at time t, the beginning of the worker's job spell. This model allows the effect of labor market conditions to vary with the strength of NCA enforceability at the time the worker was hired. If NCA enforceability affects the cost of mobility in an implicit contracts environment, we expect two effects. First, we expect the coefficient on $Enf_{t,s} \times$ *Minimum UR* to be positive, indicating that employees have *less* ability to leverage favorable labor markets over the course of their job spell when NCA enforceability is high. Second, we expect the coefficient on $Enf_{t,s} \times$ *Initial UR* to be *negative*, indicating that wages are *more* responsive to labor market conditions at the time of hire when NCA enforceability is high.

We report the results in Columns 4 and 5. Column 4 mirrors Column 3, but includes an additional control: NCA enforceability at the employee's time of hire ($Enf_{t,s}$). Encouragingly, the coefficients on *Initial UR* and *Minimum UR* do not

[47]Inclusion of MSA fixed effects (unreported) has little effect on our estimates. Our estimates are also robust to excluding Census division by year fixed effects, and to using state-level unemployment rates in lieu of national unemployment rates, which allows us to include contemporaneous unemployment rates in our regressions (since they are not collinear with division-year fixed effects). We choose to use national rates to follow BDN, and also because state-level unemployment rates could in theory be an outcome of NCA enforceability policies.

change, indicating that NCA enforceability is not acting as a de facto proxy for one of the unemployment rates.[48]

In Column 5, we include the interactions demonstrating the change in the cost of mobility. First, consider the main effects of *Initial UR* and *Minimum UR*, which indicate the effect of initial and most favorable labor market conditions, respectively, for a state with the lowest NCA enforceability. These coefficients mirror, and amplify, the findings from Beaudry and DiNardo (1991): a higher initial unemployment rate for a worker in a low-enforcing state does not reduce her wage today—if anything it leads to a *higher* wage—whereas the main effect of *Minimum UR* indicates that a worker's wage today is strongly responsive to her most favorable labor market condition over her tenure. In other words, wages in a state with low NCA enforceability are *even more* aligned with an implicit contracts model of costless mobility than the overall population.

Next, consider the two interaction terms, indicating the differential effects of these conditions for a worker in the highest enforcing state. The coefficient on $Enf_{t,s} \times$ *Initial UR* (-0.017; $p < .05$) shows that a higher unemployment rate at time of hire affects the current wage much more negatively when NCAs are more enforceable. The coefficient on the other interaction term, $Enf_{t,s} \times$ *Minimum UR* (0.020; $p < .05$), shows that the most favorable labor market condition over job tenure has a much more muted effect on the current wage for workers in states with higher enforceability. Combining the main effect on *Minimum UR* with this interaction term reveals that the most favorable labor market condition over the course of tenure has essentially no effect on the wage of a worker in a state with the highest observed enforceability ($-0.028 + 0.020 = -0.008, p = .20$).

These results reveal one mechanism through which NCA enforceability—by increasing the costs of worker mobility—affects earnings. When NCAs are strictly enforced, individuals are less able to increase their earnings as their outside option improves over the course of their job tenure. An important implication of this result is that NCA enforceability can affect earnings even if enforceability does not directly affect a worker's *realized* job mobility: by shutting down a worker's *threat* of mo-

[48] Caution should be taken when interpreting the coefficient on *Initial NCA Score* in Column 4, which is smaller in magnitude than in our prior results and statistically insignificant. This specification includes controls for tenure and tenure squared: these are important controls in the BDN framework but may bias the magnitude of the coefficient on *Initial NCA Score* towards zero. This is because tenure may be affected by NCA laws, especially given our prior results that NCA enforceability impedes worker mobility. Omitting tenure and tenure squared as controls in the regression in Column 4 slightly increases the coefficient on *Initial NCA Score* to -0.054, though it is not statistically significant. Excluding the tenure controls does not meaningfully affect the magnitude or significance of the coefficients of interest in subsequent regressions.

bility, NCAs reduce workers' bargaining power. Prior evidence has highlighted how important this threat of mobility is for wage growth: Bagger et al. (2014) show that the wage gains from job search *within* job spells dominates the gains from search *across* job spells. In other words, enforceable NCAs change the terms that govern how workers and employers bargain over wages.

7 NCA Enforceability Reduces Earnings More for Women and Racial Minorities

In Section 2.3, we discussed reasons why the earnings effect of NCA enforceability would be unevenly distributed across demographic groups, and in particular be more pronounced for women and racial minorities. Motivated by this discussion, we investigate whether the earnings effect of NCA enforceability is heterogeneous on the basis of sex and race.

Figure 5 displays results from two regressions that add demographic group indicators, alone and interacted with NCA Score, to the regression reported in Column 1 of Table 3. We make two additional modifications: first, we remove the restriction that workers must be working full-time to avoid selecting the sample on an outcome that is known to differ across men and women.[49] Second, whereas before we simply controlled for whether a respondent is white or not, and male or female, we include the more detailed demographic categories presented in the figure. The displayed coefficients, which are on the interaction of the relevant group indicator with the *NCA Enforceability Score*, represents the impact of NCA enforceability on the earnings of individuals in that group. We report coefficients from two models: our "main estimate" that makes no further modifications, and one that additionally includes interactions of the *NCA Enforceability Score* with dummies for college-educated, high use occupation, and high use industry.

First, consider the coefficients from the "main estimate" model. The effect of NCA enforceability on earnings is negative and significant for each demographic group, or close to significant in the case of white men. However, the effect is much more negative for all female groups (White Female, Black Female, Other Female) and for Black Men than it is for White Men. The coefficient for white men is statistically significantly different from the coefficient for every other demographic group.

These estimated differences might be difficult to interpret if sex or race are correlated with education or occupational choice, which Section 4.2.1 showed moderates

[49]The results do not meaningfully change if we reimpose the full-time restriction.

the effect of NCA enforceability on earnings. To address this concern, the second set of estimates depicted in the figure additionally controls for the interactions of the *NCA Enforceability Score* with dummies for college-educated, high use occupation, and high use industry.[50] While the estimates do attenuate somewhat, they remain negative and mostly statistically significant. Furthermore, the earnings effect of NCA enforceability remains statistically significantly different for nonwhite women and black men when compared with white men, though the difference for white women loses statistical significance ($p = 0.137$).

These results suggest that NCA enforceability not only reduces earnings *on average*, but it also exacerbates existing disparities across demographic groups. This point is illustrated two ways. First, the coefficients in Column 2 of Table C.9 imply that moving from the 10^{th} to 90^{th} percentile of the NCA Score distribution (NCA score = 0.55 and 0.9, respectively) would decrease average earnings of white men by approximately 3.2%, vs. decreases ranging from 3.7% to 7.7% for the other demographic groups. Together with the estimates in Column 1, these results imply that if a state that enforces NCAs at the 90^{th} percentile of the distribution were to switch to enforcing NCAs at the 10^{th} percentile of the distribution, the earnings gap between white men and each other demographic group would close by 3.6% for nonblack, nonwhite men, 4.6% for black women, 5.6% for white women, 8.7% for black men, and 9.1% for nonblack, nonwhite women.

The evidence provided in this section shows that, in addition to affecting average earnings across workers in the US workforce, strict NCA enforceability specifically harms workers who have historically faced disadvantages in the labor market. Thus, limiting the enforceability of NCAs would not only likely raise earnings on average, but also help close racial and gender wage gaps.

8 Conclusion

We estimate the impact of NCA enforceability on workers' earnings, and investigate the mechanisms underlying this relationship. Using newly-assembled panel data on state-level NCA enforceability, we show that stricter NCA enforceability leads to a decline in workers' earnings and mobility. The earnings effect is greater for workers more likely to be bound by NCAs, and greater for females and racial minorities. We also find that the earnings effect of NCA enforceability spills over across legal jurisdictions, illustrating that NCA enforceability has far-reaching consequences on

[50]For a full accounting of the two regressions depicted in the figure, as well as regressions which control separately for each additional control, see Table C.9.

labor market outcomes, with effects that likely extend far beyond the subset of workers that actually sign NCAs.

Furthermore, we identify and find evidence of one mechanism underlying the relationship between earnings and NCA enforceability: stricter NCA enforceability undermines workers' ability to negotiate for pay increases when labor market conditions improve. This finding suggests that making NCAs enforceable fundamentally changes the way that workers and employers negotiate wages. Rather than setting wages consistent with a model of implicit contracts and *costless* mobility of workers (which a long literature has found to be the case), wages under strict NCA enforceability are instead consistent with a model of implicit contracts with *costly* mobility. This finding is not just important for academic reasons: given that on-the-job wage growth accounts for a meaningful share of workers' earnings growth over their career, our findings imply that NCA enforceability shuts down a primary way that workers can otherwise negotiate for higher pay over their job tenure.

Our findings suggest that NCA enforceability may have contributed to the macro-level decline of wages as a share of economic output over past decades, from 65 percent in the late 1940s, to 63 percent in 2000, to 58 percent in 2016.[51] A realistic implication of our paper is that productivity gains translate less into wage gains when NCAs are more enforceable, which would lead strict NCA enforceability to reduce the labor share of income. In fact, we find suggestive evidence in support of this claim. Regressing the labor share of income at the state-year level on NCA enforceability, state effects, and census division by year effects, we estimate that increasing NCA enforceability from the 10th percentile to the 90th percentile is associated with a 2.3 percentage point (p-value 0.016) decline in the labor share.[52] A 2.3 percentage point difference in labor shares represents roughly one-third of the cumulative change in labor shares over the past 80 years in the US. Even if the overall intertemporal change in NCA enforceability has been modest (though we do not have data back to the 1940s, Figure 2 indicates the average NCA score increased by roughly 3 percentage points during our sample period of 1991–2014), this macro-level finding suggests at a minimum that NCA policies play a first-order role in understanding variation in the

[51] President's Council of Economic Advisors Issue Brief "Labor Market Monopsony: Trends, Consequences, and Policy Responses" October 2016.

[52] In particular, we gather annual data on the labor share of income at the state level from the Bureau of Labor Statistics from 2007 to 2018. We estimate

$$LS_{st} = \alpha + \beta * Enforceability_{st} + \rho_s + \delta_{d(s)t} + \varepsilon_{st},$$

where LS_{st} is the labor share of income in state s at time t (the other variables are described in Section 4.1). We cluster standard errors at the state level. The estimated coefficient is -0.067 (against a sample mean of 0.584), with a standard error of 0.027.

average labor share.

Our results also inform a longstanding debate regarding freedom of contract. An argument frequently cited in this debate is that workers would not sign NCAs if they were made worse off by doing so. However, at the *market* level, our findings imply that freedom to contract harms workers. This relationship arises due to the negative externalities from NCA use, which we find to be economically meaningful. This relationship could also suggest that there frictions in the labor market that mean that NCAs do not always enhance efficiency among firms that use them.

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9 Tables and Figures

Table 1: Descriptive Statistics on NCA Law Changes, 1991-2014

Region	Northeast	Midwest	South	West	Total
Average NCA Score	0.75	0.79	0.77	0.41	0.69
Standard Deviation of NCA Score	0.09	0.12	0.12	0.34	0.24
Maximum NCA Score	0.91	0.97	1.00	0.94	1.00
Minimum NCA Score	0.63	0.00	0.50	0.08	0.00
Number of Law Changes	21	20	25	16	82
Number of States in Region	9	12	17	13	51
Number of NCA Score Increases	13	15	15	9	52
Number of NCA Score Decreases	8	5	10	7	30
Average Magnitude Positive NCA Score Change	0.04	0.05	0.08	0.05	0.05
Maximum Positive NCA Score Change	0.15	0.11	0.24	0.17	0.24
Average Magnitude Negative NCA Score Change	-0.05	-0.04	-0.04	-0.03	-0.04
Maximum Negative NCA Score Change	-0.06	-0.06	-0.17	-0.09	-0.17
Between-State Standard Deviation	0.08	0.25	0.11	0.21	0.18
Within-State Standard Deviation	0.03	0.03	0.04	0.03	0.03

Notes: Statistics in the table represent data from 1991–2014, and the unit of observation is a state-year. The minimum and maximum of the NCA Score are normalized to 0 and 1, respectively.

Figure 1: Timing of NCA law changes from 1991 through 2014

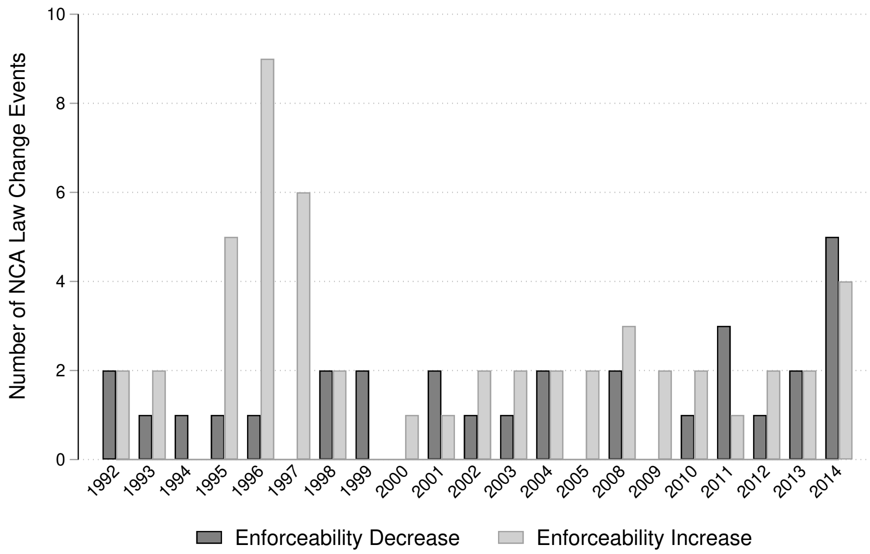
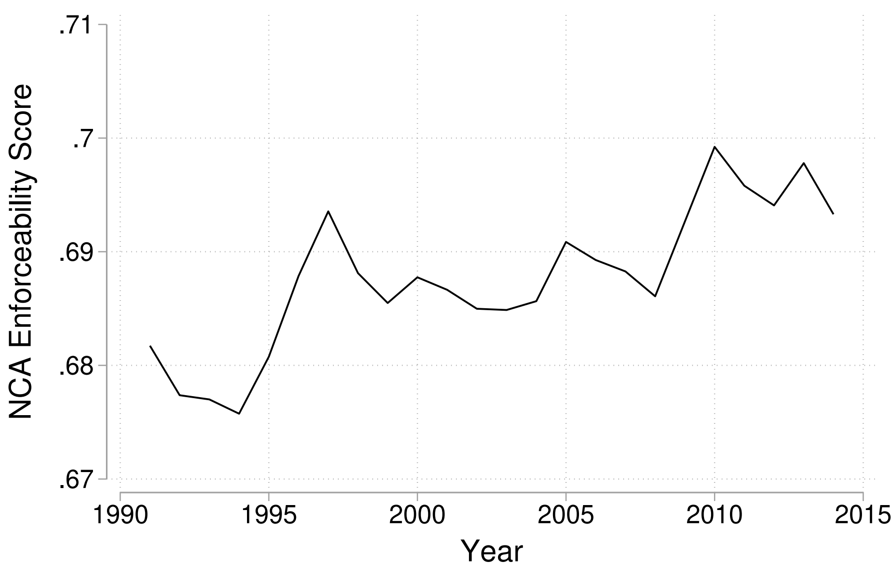


Figure 2: Average NCA Enforceability Score from 1991 to 2014



Notes: The series in this figure represents the population-weighted average NCA Score in the US in each year.

Table 2: Can Economic and Political Factors Explain Changes in NCA Enforceability?

Dependent Variable:	NCA Enforceability	
Population (100,000s)	–0.00	(0.00)
Unemployment Rate	0.00	(0.00)
Number of Workers Compensation Beneficiaries	–0.00	(0.00)
Democratic Party Governor	–0.01	(0.00)
% of State House from Democratic Party	0.01	(0.07)
% of State Senate from Democratic Party	0.04	(0.03)
State Minimum Wage	–0.01*	(0.01)
Number of Medicaid Beneficiaries (100,000s)	–0.00	(0.00)
Social Policy Liberalism Score	–0.00	(0.02)
Economic Policy Liberalism Score	–0.02	(0.01)
Social Mass Liberalism Score	–0.00	(0.02)
Economic Mass Liberalism Score	0.03	(0.04)
Democratic Party ID Count	–0.09	(0.31)
State House Ideology Score	–0.00	(0.01)
State Senate Ideology Score	0.00	(0.01)
House Democrats Ideology Score	–0.04	(0.04)
House Republicans Ideology Score	0.04	(0.05)
Senate Democrats Ideology Score	–0.03*	(0.02)
Senate Republicans Ideology Score	–0.00	(0.02)
Union Membership	–0.00	(0.00)
N	829	
R^2	0.113	
F-Test p-Value	0.184	

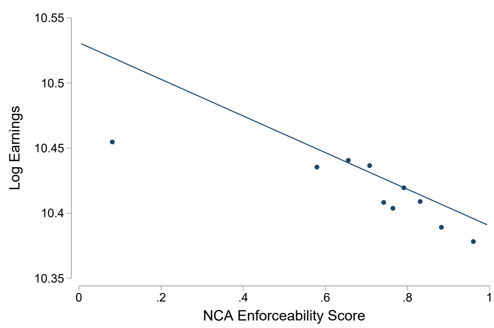
Notes: Models also include state and year fixed effects. Reported R^2 calculated after residualizing on state and year fixed effects. Standard errors reported in parentheses are clustered by state.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of NCA Enforceability on Earnings

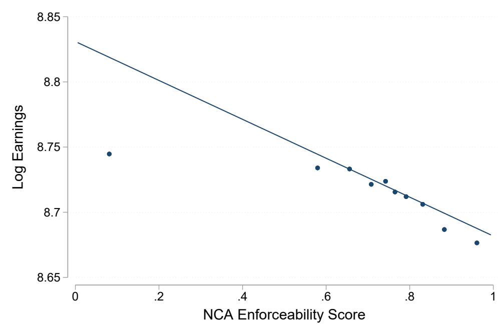
	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.117***	-0.101***	-0.025	-0.101***	-0.135***
	(0.036)	(0.027)	(0.017)	(0.027)	(0.031)
Observations	1216726	1216726	1545874	1216726	3548388
R^2	0.275	0.357	0.132	0.346	0.942
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

ASEC samples use years from 1991-2014 and include individuals between ages 18-64 who reported working for wage and salary income at a private employer. All ASEC regressions include controls for male, white, Hispanic, age, age squared, whether the individual did not complete college, and indicators for the metropolitan city center status of where the individual lives. Column (5) includes controls for male, age group, and county fixed effects. The dependent variable in Column (4), log hourly wage, is calculated as the log of total annual wage and salary income last year divided by (usual weekly hours last year times 52). Columns (1), (2), and (4) include full-time workers only, while Column (3) includes part-time workers to avoid selection on the dependent variable.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Figure 3: The Relationship between NCA Enforceability and Earnings: Binned Scatterplots



(a) All state-years with no controls.



(b) All state-years with 1-digit occupation code and demographic controls.

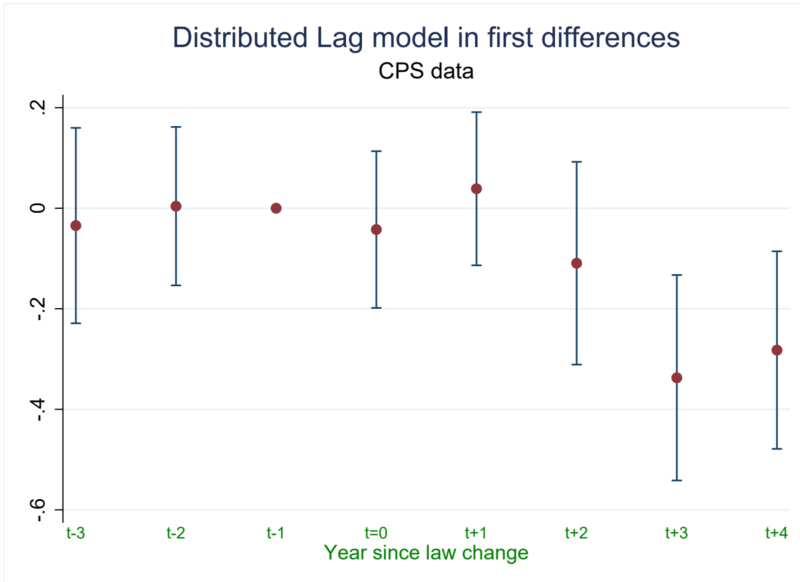
Each figure represents a binned scatterplot that relates an individual's log annual earnings to the NCA Enforceability score in his or her state that year. In each graph, both variables are residualized on state and Census division by year fixed effects. In panel (b), the variables are further residualized on broad occupation class fixed effects, age and age-squared, and indicators for white, Hispanic, male, not having completed college, and metro area status.

Table 4: Heterogeneous Effects of NCA Enforceability on Earnings by Education, Occupation, and Industry

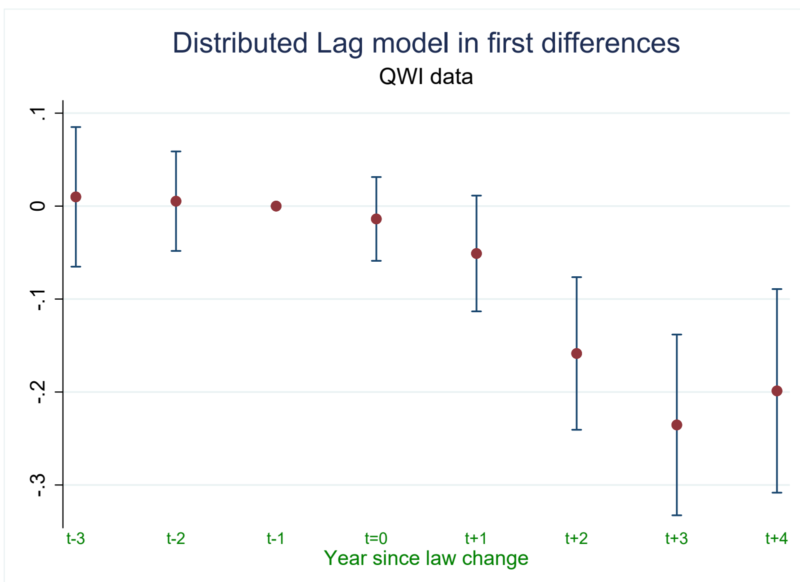
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.117***	-0.035	-0.083**	-0.092***	-0.025
	(0.036)	(0.039)	(0.034)	(0.034)	(0.036)
College Educated Worker	0.415***	0.514***	0.376***	0.391***	0.445***
	(0.013)	(0.021)	(0.012)	(0.010)	(0.015)
College Educated Worker × NCA Score		-0.143***			-0.122***
		(0.032)			(0.023)
High NCA Use Occ			0.256***		0.194***
			(0.008)		(0.005)
High NCA Use Occ × NCA Score			-0.061***		-0.015*
			(0.014)		(0.008)
High NCA Use Ind				0.270***	0.220***
				(0.008)	(0.007)
High NCA Use Ind × NCA Score				-0.068***	-0.037***
				(0.013)	(0.010)
Observations	1216726	1216726	1216726	1216726	1216726
R^2	0.275	0.275	0.290	0.292	0.304

The sample in all columns is the CPS ASEC from 1991-2014 and includes individuals between ages 18-64 who reported working for wage and salary income at a private employer the prior year. All regressions include fixed effects for state, fixed effects for Census region by year, fixed effects for broad occupational class, and individual controls for male, white, Hispanic, age, age squared, whether the individual did not complete college, and indicators for the metropolitan city center status of where the individual lives. In Columns (3) and (4), High NCA Use Occupations are occupations with NCA use greater than the national average, as tabulated by Starr et al. (2021).
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Figure 4: Distributed Lag Estimates of the Effect of NCA Enforceability Changes on Log Hourly Earnings



(a) CPS ASEC Sample.



(b) QWI Sample

Each panel plots distributed lag model estimates of the dynamic effects of an NCA law change, using the CPS sample (Panel A) and QWI sample (Panel B). See Section 4.2.2 for the regression equations. The coefficients represent the effect of an NCA law change that occurred j years ago ($j \in \{-4, 5\}$) on log earnings. The coefficient representing one year prior to law change is normalized to zero.The dependent variable is the yearly change in the log average earnings in a state-group (or county-group in Panel B). In both panels, the underlying regressions also include division-year fixed effects, and standard errors are clustered by state.

Table 5: The Effects of NCA Enforceability on Job Mobility

	J2J Separation Rate		Share J2J Across State		Share J2J Across Industry	
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Score	-0.006	0.062	0.009	0.003	-0.004	-0.005
	(0.064)	(0.065)	(0.025)	(0.025)	(0.012)	(0.012)
High NCA Use Ind		-0.199***		0.017**		0.004
× NCA Score		(0.065)		(0.008)		(0.013)
Observations	677272	677272	659380	659380	668807	668807
R^2	0.866	0.867	0.602	0.602	0.667	0.667
Mean Dep Var	1.12	1.12	0.16	0.16	0.60	0.60

The sample is the J2J from 1991-2014. An observation is a state-sex-age group-quarter-industry cell. All regressions include controls for sex, age group, and industry, as well as division by year by quarter and state fixed effects.
Regressions are weighted by employment, and standard errors are clustered by state. ***P<.01, **P<.05, *P<.1

Table 6: The External Effects of NCA Enforceability on Earnings

	(1)	(2)	(3)
Own State NCA Score	-0.178***	-0.207***	-0.184***
	(0.057)	(0.066)	(0.068)
Donor State NCA Score		-0.181**	-0.210**
		(0.076)	(0.079)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.124
			(0.151)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.163***
			(0.054)
Observations	615097	615097	613679
R^2	0.898	0.898	0.901

The dependent variable is log earnings. The sample is the QWI from 1991-2014 restricted to counties directly on state borders in commuting zones that straddle a state border. An observation is a county-sex-age group-quarter. All regressions include controls for sex, age group, as well as division by year by quarter and county fixed effects. Own Cty Emp/CZ Emp is the ratio of sex- and age-group-specific employment in own county divided by sex- and age-group-specific employment in the entire commuting zone.
Standard errors are clustered by own state in Column (1), and two-way clustered by own state and commuting zone in columns (2) and (3). ***P<.01, **P<.05, *P<.1

Table 7: NCA Enforceability Reduces the Returns to Tenure

	(1)	(2)	(3)	(4)
Tenure (years)	0.01026***	0.02042***	0.01169***	0.02368***
	(0.00028)	(0.00074)	(0.00060)	(0.00103)
Tenure squared		-0.00037***		-0.00048***
		(0.00002)		(0.00004)
Initial NCA Score			-0.04044	0.00189
			(0.07797)	(0.07571)
Initial NCA Score x Tenure			-0.00208**	-0.00470***
			(0.00086)	(0.00168)
Initial NCA Score x Tenure Squared				0.00015**
				(0.00006)
No. Obs.	76350	76350	76350	76350
R^2	0.361	0.363	0.361	0.363

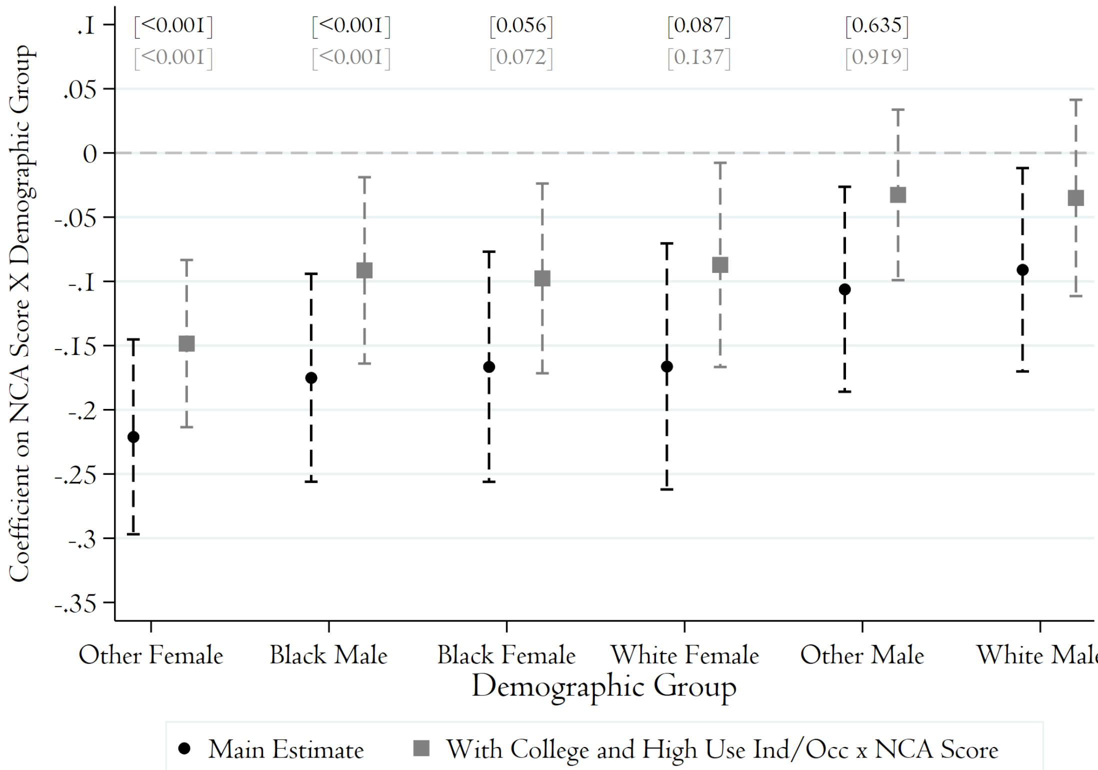
The dependent variable is log weekly earnings. All regressions include state, Census division by year, and industry fixed effects, as well as controls for a quadratic in age and indicators for high school or less, black, Hispanic, married, union member, metro center status, and female.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table 8: NCA Enforceability Changes How Workers and Employers Negotiate Implicit Contracts

	Log Earnings				
	(1)	(2)	(3)	(4)	(5)
Initial UR	-0.008***		-0.002	-0.002	0.010**
	(0.002)		(0.003)	(0.003)	(0.005)
Minimum UR		-0.017***	-0.014***	-0.014***	-0.028***
		(0.003)	(0.005)	(0.005)	(0.006)
Initial NCA Score				0.007	-0.019
				(0.068)	(0.082)
Init. NCA Score × Init. UR					-0.017**
					(0.006)
Init. NCA Score × Min. UR					0.020**
					(0.009)
No. Obs.	76350	76350	76350	76350	76350
R^2	0.364	0.364	0.364	0.364	0.364

The dependent variable is log weekly earnings. All regressions include state, Census division by year, and industry fixed effects, as well as controls for quadratics in age and tenure, and indicators for high school or less, black, Hispanic, married, union member, metro center status, and female.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Figure 5: Heterogeneous Effects of NCA Enforceability on Earnings by Race and Sex



The figure depicts coefficients from two regressions of earnings on NCA Score, interacted with demographic groups. The first regression builds on Column 1 of Table 3, adding indicators for each demographic group, as well as interactions of those indicators with NCA Score (the coefficients on which are depicted in the figure, along with 90% confidence intervals). The second regression adds controls for college education, high use occupation, and high use industry, as well as each interacted with NCA Score. The values in brackets report p-values for the *difference* between each coefficient and the coefficient for white males, with the main estimate above and the estimate including the extra controls below.

A Formalization of Theory

This appendix considers an augmentation of the model of Bagger et al. (2014). Bagger et al. (2014)'s baseline model of workers' wage growth over their career uses a search and matching framework with human capital accumulation and on-the-job search. We consider a modification in which some workers sign NCAs with a firm, preventing their job mobility while employed by that firm. We consider channels linking wages and NCAs posited in Section 2, and derive conditions under which those channels would lead to the expected relationships in the model.

A.1 Summary of Bagger et al. (2014)

First, we introduce and summarize the model of Bagger et al. (2014). In that model, unemployed and employed workers match with prospective employers at rates λ_0 and λ_1, respectively. Workers produce according to their human capital: a worker with human capital level h_t produces, in log terms, $y_t = p + h_t$, where p is the productivity of the firm, drawn from exogenous distribution $F(p)$. Workers are paid according to a piece rate: their wage is (again, in log terms) $w_t = r + p + h_t$, where $R = e^r \leq 1$ is the piece rate. The logged piece rate, r, is actually negative, meaning that it represents the amount of productivity that is "returned" to the employer. When exponentiated, the piece rate, R, therefore represents the *share* of productivity that is "returned" to the employer.

When unemployed workers match with a new employer, their wages are determined by setting the piece rate such that the worker receives a share, β, of the value of their match above and beyond the value of unemployment, which is assumed to be the value of matching with the least productive firm type, p_{min}. Employed workers who contact new employers may leave their current job (if the new employer is able to offer more attractive contract terms) or may leverage an outside offer to receive a wage increase (if the incumbent employer is able to offer more attractive contract terms), in either case receiving a share, β, of the match-specific rents above and beyond their relevant threat point. Workers also exogenously separate from their employers at rate $\delta \in [0, 1]$ (and immediately rematch at rate $\kappa \in [0, 1]$), and leave the labor force altogether at exogenous rate $\mu \in [0, 1]$. The discount rate is ρ.

We selected this model as a baseline due to the harmony between the drivers of wage growth in the model and the channels through which NCAs could affect wages that we discussed in Section 2. In the baseline model, workers' wage growth occurs because of growth in their human capital, h_t , and their ability to search for higher-paying jobs. These two mechanisms for wage growth match well to potential roles for

NCAs. First, NCAs are typically justified as a solution to a hold-up problem, where firms are not willing to invest in workers' human capital (e.g., training, imparting trade secrets, client lists, etc.) for fear that the worker will depart the firm and therefore deny the firm its return on investment. Therefore, an NCA in this model should cause h_t to grow at a greater rate, as the firm is more willing to invest in the worker. Second, NCAs prevent workers from changing jobs or threatening to change jobs, meaning that workers will not be able to increase wages by moving to a dominant firm, or by leveraging an outside offer to increase their wages at their current firm. The tradeoff between these two competing mechanisms will partially determine the difference in the rates of wage growth with and without an NCA for the worker.

A.2 Modifications to Bagger et al. (2014)

We operationalize NCAs in the model by assuming that workers exogenously sign enforceable NCAs with probability γ when they commence their first employment relationship (in other words, at the beginning of a worker's career, they randomly become an NCA worker or a non-NCA worker, and this designation will never change). With this inclusion of NCAs, we make three additional primary modifications to the model.

First, we assume that workers with NCAs accumulate human capital at a faster rate. Accumulation of human capital, h_t, is stochastic in Bagger et al. (2014), with the deterministic component of workers' human capital at time t represented by $g(t)$. Here, we define $g^C(t)$ and $g^F(t)$ to be the deterministic component of, respectively, a constrained (i.e., NCA-signing) and free (i.e., non-NCA signing) worker's human capital at time t[53]. Since human capital evolves faster for those with NCAs, if $g^C(t-1) = g^F(t-1)$, then $g^C(t] > g^F(t)$. This assumption is a natural implication of the argument that NCAs solve a hold-up problem. Firms might be unwilling to invest in human capital of workers who can freely leave, because they do not expect to recoup the returns on their investmen. NCAs, by ensuring that workers cannot freely leave, incentivize firms to invest in workers, causing their human capital to develop more rapidly.

The second primary medication is that workers with NCAs are unable to change jobs: the offer arrival rate of new jobs for employed workers with NCAs is zero, or $\lambda_1^C = 0$. In other words, if a worker has an NCA, they will continue to work for the

[53]The superscripts C and F will be used frequently to differentiate functions and parameters that differ between signers and non-signers.

same employer unless they experience an exogenous separation.[54] Though assuming that NCAs strictly prohibit job changing may seem drastic (because, for example, workers may be able to buy out of NCAs or can move to firms in different industries or geographic locations), this assumption substantially improves tractability and does not change the predictions of the model. For example, the model generates qualitatively similar predictions to a model in which workers must pay a cost to change jobs (e.g. representing an explicit buyout cost); indeed, if the cost is steep enough, in the limit assuming a cost is identical to assuming that the worker is unable to change jobs.

The third modification we make, outlined in Section A.4, is assuming that the offer arrival rate is lower for workers in thinner labor markets (i.e., markets with a lower measure of workers available to match to new firms). Specifically, we allow the offer arrival rate for employed workers in jobs with no NCA, λ_1, to vary with T. T represents the thickness of the freely mobile labor market (i.e., the measure of workers available to match in non-NCA jobs) defined as $T \equiv N^F + (1-\gamma)U$ (where N^F is the proportion of workers employed in non-NCA jobs, and $(1-\gamma)U$ is the proportion of unemployed workers who will randomly match to firms without an NCA). We assume that $\frac{d\lambda_1(T)}{dT} > 0$: the thicker the labor market, the more often workers will be contacted on-the-job. This modification, which generates spillover effects in the model, has sound empirical justification. As shown in Starr et al. (2019), when NCAs are used at a higher rate in a given state-industry combination, workers who do not sign NCAs in that state-industry receive fewer job offers. Furthermore, in thin labor markets (which may result from NCAs since, when a worker signs an NCA, the supply and demand sides of the labor market are diminished as workers and vacancies disappear from the unmatched pool), workers and firms match less often (Bleakley and Lin, 2012; Gan and Li, 2016), resulting in lower contact rates in this model.

Under these modifications, we now generate multiple predictions which relate directly to the empirical work found in this paper.

[54]We make two additional modifications related to this one. First, we assume that, after an exogenous separation, a worker who had previously signed an NCA will continue to work in a job with an NCA. This assumption significantly increases tractability by limiting flows between the two types of jobs. One way to view this assumption is that workers work in industries that use NCAs or in industries that do not; this could occur due to the value of accumulated industry-specific human capital. The second assumption is that workers immediately find new work upon an exogenous separation with their employer. This assumption increases tractability of the model and does not change the qualitative predictions. Furthermore, we view it as reasonable: roughly half of states do not enforce NCAs when employees are fired, leaving such workers able to find other jobs quickly in the event of an involuntary separation.

A.3 Direct Effects of NCAs on Wages of NCA Signers

First, we examine the wages of a worker who signs versus does not sign an NCA, from which we can extrapolate the average wages when the proportion of workers who sign NCAs, γ, changes. Wages depend on human capital (which develops more rapidly when workers have NCAs) and mobility (which is restricted when workers sign NCAs). This tension forms the substance of the direct effects.

The wage of a worker is given by $w_{i,t} = \alpha_i + g^j(t) + \varepsilon_{i,t} + p_{i,t} + r$, where α_i is a worker heterogeneity parameter, $g^j(t)$ is the deterministic component of human capital accumulation of the worker for $j \in \{C, F\}$ for workers who are constrained by an NCA or free to change employers, respectively, and $\varepsilon_{i,t}$ is a stochastic worker human capital shock. Firm productivity, $p_{i,t}$ (where i represents the worker and t represents time), and r (the piece rate of the worker) round out wages.

In order to compare wages across workers, we compare the individual components of wages. By assumption, ε is distributed identically across workers and across time, and α is distributed identically across workers, so in expectation, there are no differences in ε or α for workers with and without NCAs.

By assumption, human capital evolves at a higher rate for those with NCAs: if $g^C(t-1) = g^F(t-1)$, then $g^C(t) > g^F(t)$.

What is left to compare are firm productivities and the piece rates of workers. Intuitively, workers with NCAs will face a worse distribution of firm productivities because they are unable to search for higher-paying jobs—*i.e.* they are unable to climb the job ladder. In fact, since they are immobile and exit occurs independently of firm productivity, the distribution of firm productivities at which NCA-constrained workers are employed ($L^C(p)$) is exactly equal to the exogenous productivity distribution for a worker entering employment: $L^C(p) = F(p)$.

The steady state distribution for those who do not sign NCAs is derived in Bagger et al. (2014) (equation A14): $L^F(p) = \frac{(\mu+\delta)F(p)}{\mu+\delta+\lambda_1 \bar{F}(p)}$, where $\bar{F}(p) = 1 - F(p)$. Since workers only move *up* the job ladder, $L^F(p)$ first-order stochastically dominates $L^C(p)$.

Finally, we turn to piece rates. Piece rates for nonsigners evolve identically to those in the baseline model of Bagger et al. (2014). However, the piece rate for signers does not evolve over time: lacking the ability to change the piece rate by leveraging outside offers or engaging in job-to-job mobility, the piece rate for a worker with an NCA is determined at the advent of their job spell.

In Bagger et al. (2014), the piece rate (r) is a function of the most recent firm from which the worker was able to, or would have been able to, extract all available surplus (by virtue of having a high enough competing offer)[55]:

[55]Note that the piece rate is negative: wages are given by $w_t = r + p + h_t$, where $p + h_t$ is the

$$r = -\int_{q_{i,t}}^{p_{i,t}} \phi(x)dx$$

where $\phi(x) = (1-\beta)\frac{\rho+\delta+\mu+\lambda_1\bar{F}(x)}{\rho+\delta+\mu+\lambda_1\beta\bar{F}(x)}$, $F(x) = 1-\bar{F}(x)$ is the exogenous distribution of firm productivities from which workers draw upon matching with a firm, and $q_{i,t}$ represents the productivity of the last firm from which the worker was able to extract all surplus, by virtue of leveraging a competing offer (see Equation 6 in Bagger et al. (2014) for details on the derivation of this equation). The greater is $q_{i,t}$, the greater the worker's wage will be. If $q_{i,t} = p_{i,t}$, then the worker was able to extract all surplus from their current firm and therefore $r = 0$.

In the case of an NCA signer, the last "job" from which the worker was able to extract all surplus was unemployment, since workers cannot leverage outside options or job hop. The piece rate of signers is therefore determined by the worker having outside option p_{min} (the lowest productivity a firm can have), since by assumption, the value of unemployment is equal to the value of employment in the least productive firm. Simplifying (since $\lambda_1^C = 0$ for signers by assumption), the piece rate of NCA signers will be:

$$\begin{aligned} r &= -\int_{p_{min}}^{p_{i,t}} \phi(x)dx \\ &= -\int_{p_{min}}^{p_{i,t}} (1-\beta)\frac{\rho+\delta+\mu+\lambda_1^C\bar{F}(x)}{\rho+\delta+\mu+\lambda_1^C\beta\bar{F}(x)}dx \qquad = -(p_{i,t}-p_{min})(1-\beta) \end{aligned}$$

The wage processes of signers versus nonsigners are given by:

$$\text{Nonsigners: } w_{i,t}^F = \alpha_i + g^F(t) + \varepsilon_{i,t} + p_{i,t} - \int_{q_{i,t}}^{p_{i,t}} \phi(x)dx$$

$$\text{Signers: } w_{i,t}^C = \alpha_i + g^C(t) + \varepsilon_{i,t} + p_{i,t} - (p_{i,t}-p_{min})(1-\beta)$$

We now compare expected wages for workers with and without an NCA. First, we examine workers new to the workforce:

Proposition A.1. *In steady state, workers signing NCAs will receive higher initial wages in expectation than workers not signing NCAs:* $E_{i,t-1}[w_{i,t}^C] > E_{i,t-1}[w_{i,t}^F]$

Proof. In the first period in which workers match, the firm productivity distributions are identical (since workers have not had a chance to switch jobs). In expectation, α_i

marginal product of the worker (p is the firm's productivity and h_t is the worker's productivity due to human capital accumulation). Therefore, the piece rate r represents the share of the worker's productivity that is allocated to the firm.

and $\varepsilon_{i,t}$ are identical for those with and without NCAs. By assumption, $E_{t-1}[g^C(t)] > E_{t-1}[g^F(t)]$, so the proposition is proven if

$$E_{i,t}[(p_{i,t} - p_{min})(1-\beta)] < E_{i,t}\left[\int_{p_{min}}^{p_{i,t}} \phi(x)dx\right],$$

since the worker initially bargains with outside option p_{min}.

Rewriting the right hand side, we must show that

$$E_{i,t}\left[\int_{p_{min}}^{p_{i,t}} (1-\beta)dx\right] < E_{i,t}\left[\int_{p_{min}}^{p_{i,t}} \phi(x)dx\right],$$

which is true since $\phi(x) > (1-\beta) > 0$. □

This proposition highlights two reasons for greater (initial) pay under NCAs: first, a greater accumulation of human capital leading to greater productivity, and second, the compensating differential associated with NCAs. Workers who initially match with NCAs are compensated to some extent for their limited future mobility.

However, as workers remain at their jobs longer, three things happen: first, workers with NCAs accumulate more human capital. Second, workers without NCAs climb the job ladder, moving to jobs with greater firm productivities, $p_{i,t}$. Third, when they leverage outside offers, they negotiate better piece rates, r. The first increases earnings by more for those who sign NCAs, while the latter two increase earnings by more for those who do not sign NCAs. The overall comparison, then, is indeterminate: if human capital grows more quickly than mobile workers climb the job ladder and negotiate better piece rates, workers with NCAs will have earnings that grow more quickly than those without, and vice versa. We summarize in Proposition A.2, but first introduce the condition used in the proposition. The condition states that the growth rate of human capital is lower than the growth rate of the lost ability of the worker to bargain for higher wages, and as such, the proposition is just an algebraic simplification. While there is no major intuitive leap contained in the proposition, its goal is to show that there is a direct tradeoff between human capital growth and job mobility which governs wage dynamics.

Condition 1.

$$
\begin{aligned}
&E_t[(g^C(t+1) - g^C(t)) - (g^F(t+1) - g^F(t))] \\
&\qquad < \left(\int_{q_{j,t}}^{p_{j,t}} \int_{p_{j,t-1}}^{p} \phi(x)dxdF(p) \right) \\
&\qquad + \left(\int_{p_{j,t}}^{p_{max}} p - p_{j,t} - \left(\int_{p_{j,t}}^{p} \phi(x)dx - \int_{q_{j,t}}^{p_{j,t}} \phi(x)dx \right) dF(p) \right)
\end{aligned}
$$

Proposition A.2. *If worker i has an NCA and worker j does not, then conditional on remaining employed and experiencing identical shocks in period t ($\varepsilon_{i,t} = \varepsilon_{j,t}$), $E_t[w_{i_t+1}] - w_{i,t} < E_t[w_{j,t+1}] - w_{j,t}$ whenever Condition 1 holds, and $E_t[w_{i_t+1}] - w_{i,t} > E_t[w_{j,t+1}] - w_{j,t}$ when it does not.*

Proof. The condition is an algebraic simplification of the inequality $E_t[w_{i_t+1}] - w_{i,t} < E_t[w_{j,t+1}] - w_{j,t}$. The left hand side may be rewritten as:

$$
E_t[\alpha_i + \varepsilon_{i,t+1} + g^C(t+1) + p_{i,t+1} - (1-\beta)(p_{i,t+1} - p_{min})] - [\alpha_i + \varepsilon_{i,t} + g^C(t) + p_{i,t} - (1-\beta)(p_{i,t} - p_{min})]
$$

Since $p_{i,t} = p_{i,t+1}$ for i, who has an NCA, this reduces to $E_t[g^C(t+1) - g^C(t) + \varepsilon_{i,t+1} - \varepsilon_{i,t}]$. The right hand side may be rewritten as

$$
\begin{aligned}
&E_t[\alpha_j + \varepsilon_{j,t+1} + g^F(t+1) + p_{j,t+1} - \int_{q_{j,t+1}}^{p_{j,t+1}} \phi(x)dx] - [\alpha_j + \varepsilon_{j,t} + g^F(t) + p_{j,t} - \int_{q_{j,t}}^{p_{j,t}} \phi(x)dx] \\
&= E_t[g^F(t+1) - g^F(t) + \varepsilon_{j,t+1} - \varepsilon_{j,t}] \\
&\quad - \left[\int_{q_{j,t}}^{p_{j,t}} \left(\int_{p}^{p_{j,t}} \phi(x)dx - \int_{q_{j,t}}^{p_{j,t}} \phi(x)dx \right) dF(p) \right] \\
&\quad + \left[\int_{p_{j,t}}^{p_{max}} p - p_{j,t} - \left(\int_{p_{j,t}}^{p} \phi(x)dx - \int_{q_{j,t}}^{p_{j,t}} \phi(x)dx \right) dF(p) \right] \\
&= E_t[g^F(t+1) - g^F(t) + \varepsilon_{j,t+1} - \varepsilon_{j,t}] \\
&\quad + \left(\int_{q_{j,t}}^{p_{j,t}} \int_{q_{j,t}}^{p} \phi(x)dxdF(p) \right) \\
&\quad + \left[\int_{p_{j,t}}^{p_{max}} p - p_{j,t} - \left(\int_{p_{j,t}}^{p} \phi(x)dx - \int_{q_{j,t}}^{p_{j,t}} \phi(x)dx \right) dF(p) \right]
\end{aligned}
$$

We expand the expectation by using the fact that the lowest productivity level a worker will be able to leverage to achieve an increase in earnings is $q_{j,t}$. If the worker contacts a new employer whose productivity is less than qj, t, productivity will not change and the worker will not renegotiate the piece rate. If the worker contacts a new employer with productivity between $q_{j,t}$ and $p_{j,t}$, they will remain employed at productivity $p_{j,t}$ but will renegotiate the piece rate. Finally, if the worker contacts a new employer with productivity above $p_{j,t}$, the worker will change jobs, changing both productivity and the piece rate.

Combination of the reduced right and left hand sides yields the condition stated in the proposition. □

Proposition A.2 simplifies the condition under which workers have larger wage growth with NCAs versus without. An alternative way of interpreting this proposition is that, when the inequality condition holds, workers without NCAs will see wage increases relative to workers with NCAs.

Let $\bar{w}_t^S$ represent average wages for workers with tenure t (where $t = 0$ represents workers new to the labor market) with or without NCAs ($S \in \{C, F\}$). Since workers initially match using an identical productivity distribution, since α and ε are distributed identically for workers with and without NCAs, and since separation is independent of wage or productivity, induction on Proposition A.2 generates the following corollary immediately:

Corollary A.3. *$\bar{w}_0^C - \bar{w}_0^F > \bar{w}_t^C - \bar{w}_t^F$ whenever Condition 1 holds, and $\bar{w}_0^C - \bar{w}_0^F < \bar{w}_t^C - \bar{w}_t^F$ when it does not.*

Corollary A.3 generates an indeterminate prediction regarding the relationship between average earnings and NCA enforceability, explored in Section 4. The balance of the tradeoff between human capital growth and climbing the job ladder cannot be assessed theoretically, and we therefore test it empirically.

A.4 Indirect Effects NCAs on Wages of non-NCA Signers

As described above, NCAs thin labor markets, which reduces on-the-job contact rates even for workers without NCAs (Bleakley and Lin, 2012; Gan and Li, 2016). For the remainder of this section, we therefore assume that the on-the-job contact rate (λ_1) is a function of market thickness. As described above, we define market thickness as $T \equiv N^F + (1-\gamma)U$, where N^F is the proportion of workers employed in jobs with no NCA, and U is the proportion of unemployed workers. Multiplying U by $1-\gamma$ yields the proportion of workers who are unemployed and will randomly match to firms without an NCA. We assume, then, that $\frac{d\lambda_1(T)}{dT} > 0$: the thicker the labor market, the more often workers will be contacted on-the-job.

To derive conditions under which increased frequency of NCA use yields lower earnings for workers without NCAs, we must first generate expressions representing the proportion of matched workers, in steady state, without an NCA (N^F), with an NCA (N^C), and who are unemployed ($U \equiv 1 - N^C - N^F$). We do this using flow equations for each type of worker. The flow into unemployment is $(N^C + N^F)(\mu + \delta(1-\kappa))$, and the flow out is $\lambda_0(1 - N^C - N^F)$, generating the flow balance equation

$$1 - N^C - N^F = \frac{\mu + \delta(1-\kappa)}{\mu + \delta(1-\kappa) + \lambda_0},$$

which is effectively identical to equation A7 in Bagger et al. (2014).

The flow balance equations for jobs with and without NCAs, respectively, at productivity p or less are given by

$$\text{NCA: } \lambda_0(1 - N^C - N^F)F(p)\gamma + \delta\kappa(1 - L^C(p))N^C F(p) \\ = [\mu + \delta(1-\kappa) + \delta\kappa\bar{F}(p)]L^C(p)N^C$$

$$\text{No NCA: } \lambda_0(1 - N^C - N^F)F(p)(1-\gamma) + \delta\kappa(1 - L^F(p))N^F F(p) \\ = [\mu + \delta(1-\kappa) + (\delta\kappa + \lambda_1(T))\bar{F}(p)]L^F(p)N^F$$

Setting $p = p_{max}$ and solving generates closed form solutions for N^C and N^F, as well as for $1 - N^C - N^F$:

$$N^F = \frac{\lambda_0(1-\gamma)}{\mu + \delta(1-\kappa) + \lambda_0}$$
$$N^C = \frac{\lambda_0\gamma}{\mu + \delta(1-\kappa) + \lambda_0}$$
$$1 - N^C - N^F = \frac{\mu + \delta(1-\kappa)}{\mu + \delta(1-\kappa) + \lambda_0}$$

Therefore, $T = 1 - \gamma$, and $\frac{dT}{d\gamma} = -1$: market thickness is negatively associated with the probability that workers sign an NCA, all else equal. Under the assumption that $\frac{d\lambda_1(T)}{dT} > 0$, it follows that $\frac{d\lambda_1(T)}{d\gamma} < 0$.

The remaining analysis of spillover effects mirrors the analysis of the direct effect of NCAs on wages, to some extent. Workers who take a new job will receive slightly higher initial wages when γ increases. This is because the firm's job offer includes an implicit promise of future mobility, which is cheapened when future mobility is partially compromised due to a thin market. Therefore, the firm must increase the worker's piece rate to continue to guarantee a β share of that firm's surplus:

Proposition A.4. $\frac{dE_{t=0}[w_{i,1}]}{d\gamma} > 0$

Proof. The workers expected wage before matching with an employer is

$$E_{t=0}[w_{i,1}] = (1-\lambda_0)V_0(h_0) + \lambda_0 E_{t=0}\left[\alpha_i + g(1) + \varepsilon_{i,1} + p_{i,1} - \int_{p_{min}}^{p_{i,t}} \phi(x)dx\right]$$

Therefore:

$$\begin{aligned}
\frac{dE_{t=0}[w_{i,1}]}{d\gamma} &= -\int_{p_{min}}^{p_{i,t}} \frac{d\phi(x)}{d\gamma}dx \\
&= -\int_{p_{min}}^{p_{i,t}} \frac{1-\beta}{(\rho+\delta+\mu+\lambda_1(T(\gamma))\bar{F}(x))^2}(\bar{F}(x)(1-\beta)(\rho+\delta+\mu)\frac{d\lambda_1}{d\gamma})dx \\
&> 0,
\end{aligned}$$

where the last inequality follows because $\frac{d\lambda_1}{d\gamma} < 0$. $\square$

Wage growth after a match follows a pattern similar to that found in Proposition A.2, except that workers not bound by NCAs who are subject to spillovers from NCAs do not experience faster than usual growth of human capital. Therefore, workers with NCAs experience slower wage growth than workers without NCAs, which will eventually lead to lower wages for those workers, even in the presence of the compensating differential described in Proposition A.4.

A.5 Empirical Implications of Theoretical Results

Suppose we found that, in the cross section, stricter NCA enforceability led to lower wages on average. How would we rationalize such a result using the theoretical model we have just described? Ignoring any spillover effects, this result would imply, given Corollary A.3, that Condition 1 holds: the lower wage growth due to diminishment of the gains from job search dominates any benefit of faster human capital accumulation (the "late-career effect"), and this negative effect outweighs any initial compensating differential the worker receives (the "early career effect"). Other models could yield alternative explanations. For example, even though the Bagger et al. (2014) does not admit a compensating differential for human capital accumulation, a different model could imply that workers accept a *negative* compensating differential for signing an NCA, since workers anticipate faster future wage growth due to faster human capital accumulation. In this latter explanation, a negative relationship between

NCA enforceability and wages on average would not imply that workers are worse off under stricter enforceability.

While we cannot rule out this latter explanation, we present empirical results in Section 6 that support an interpretation that is consistent with the mechanisms implied by our model. In that section, we show evidence that NCA enforceability reduces the benefits from job search by limiting workers' mobility, particularly in strong labor markets. Strong labor markets will positively impact the right hand side of Condition 1, for example by increasing the arrival rate of offers for workers able to accept them. Therefore, strong labor markets make Condition 1 more likely to be satisfied. An unanticipated positive labor market shock enabled a worker not bound by an NCA to leverage his improved outside option to bargain for a higher wage; a similar worker bound by an NCA cannot credibly threaten to leave, and thus is unable to take advantage of the stronger labor market to secure a wage increase. We describe these results more fully in Section 6.

B Appendix: Additional Event Study Analysis

In Section 4.2.2, we report our primary estimates of the dynamic effects of legal changes to NCA enforceability over time, as well as the lack of a trend in earnings prior to implementation of such a legal change, using a distributed lag model. However, given the range of choices available to researchers in examining such dynamic effects, in this Appendix we consider whether our results reported in Section 4.2.2 are robust to three alternative specifications.

B.1 Stacked Event Study

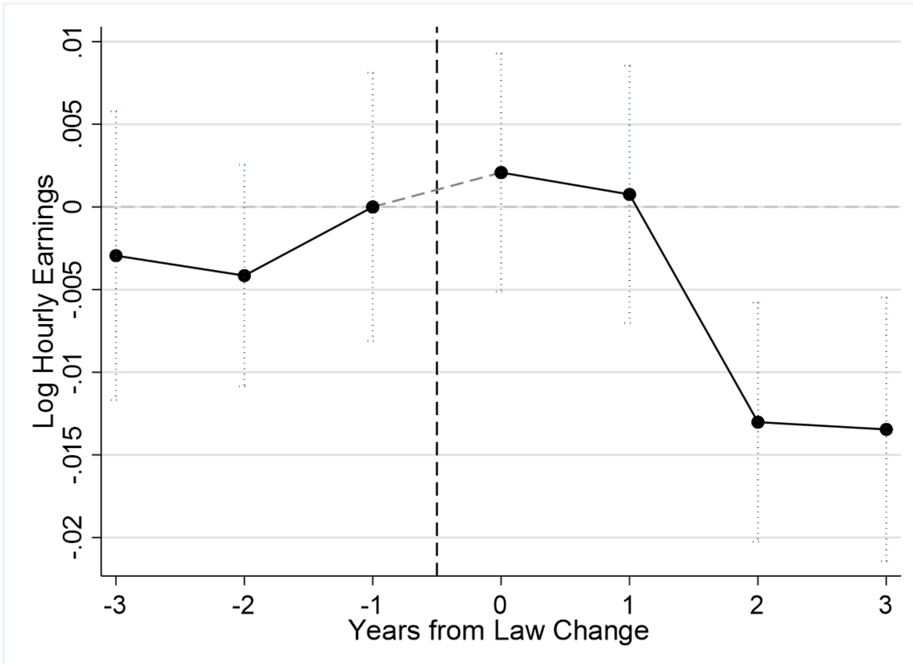
The first approach we consider is a "stacked" event-study analysis, following prior work (e.g., Cengiz et al. (2019)). For each state that experiences an NCA enforceability change, we generate a subsample by isolating a three year window before and after the year of the law change, and by identifying a set of "control" states in the same Census division that did not experience a law change at any point during that window. We then stack the subsamples and estimate the difference in outcomes between treated and control states in each year relative to the law change. If the treated state experienced more than one law change during an event window, we drop years of data to ensure a consistent NCA score for at least two years each in the pre- and post-periods, and we drop subsamples altogether when this is not possible. The regression equation is:

$$\ln w_{is\overline{s}t} = \sum_{k=-3}^{k=3} \beta_k D_s^{tk} + X_{it}\gamma + \rho_{s\overline{s}} + \delta_{d(s)t} + \varepsilon_{is\overline{s}t} \tag{6}$$

where $\ln w_{is\overline{s}t}$ is log hourly earnings of worker i in state s, matched to treatment state $\overline{s}$ in year t. D_s^{tk} is equal to 1, -1, or 0, indicating positive, negative, or no changes in state s's composite NCA enforceability score from a law change that occurred at year $t+k$ (and is therefore zero for all k when $s \neq \overline{s}$, *i.e.*, when the state is a control state). X_{it} is a vector of individual-level controls, $\rho_{s\overline{s}}$ is a fixed effect for each state by treatment-state combination, and $\delta_{d(s)t}$ is a fixed effect for each Census division by year. We cluster standard errors by state.

Figure B.1 shows the event study estimates for each $\widehat{\beta_k}$ (we normalize the coefficient $\widehat{\beta_{-1}}$ to be zero). The figure is qualitatively very similar to our distributed lag estimates reported in Figure 4. As in our main figure, there is little evidence of a pre-trend in earnings in the years prior to a law change, and there is a decline in hourly earnings in the years following a law change. As in Figure 4, the effect only becomes substantial two years following the law change; we discussed reasons why this might be in Section 4.2.2. These estimates imply that, on average, an increase

Figure B.1: Event Study Estimates of the Effect of NCA Enforceability Changes on Log Hourly Earnings



The sample includes three-year windows around NCA law change events, as well as control states in the same Census division with no corresponding event in the three-year window, dropping treatment-state observations which introduce additional changes in the pre- or post-periods. The estimating equation includes controls for sex, age, age squared, level of education, race, Hispanic status, and whether or not the respondent lives in a metropolitan area, as well as state and Census division-by-year fixed effects. Coefficient estimates and 95% confidence intervals pictured (normalized to coefficient estimate one year prior to law change).

in *Enforceability* reduces earnings by up to 1.3%.

B.2 "Clean" Stacked Event Study with Balanced Panel and Continuous Treatment Measure

In Figure B.1, for each event, we used a three-year event window and dropped non-event years in which a state experienced a separate NCA law change. One potentially unappealing feature of this approach is that one might worry that NCA law changes in non-event years could "contaminate" our estimate the effect of a law change in the focal event year; furthermore, we might want to examine effects beyond a three year window. To address these concerns, we drop any subsample in which the treatment state experiences a law change in any of the years that is not the event year, and we also extend the event window to four years. The tradeoff is statistical power versus cleanliness: the approach we use in Figure B.1 contains more events (62 events), but loses specific years in which other NCA enforceability changes cause pollution of the event window, creating an unbalanced panel. This alternative approach contains fewer events (41 events), but each window is unpolluted by other changes. We additionally use a continuous metric for NCA enforceability. While this type of dose-response approach is not typically employed in event studies (see, e.g., Fuest et al. (2018)), we use it here to again refine the alternative approach and demonstrate the robustness of our initial estimates.

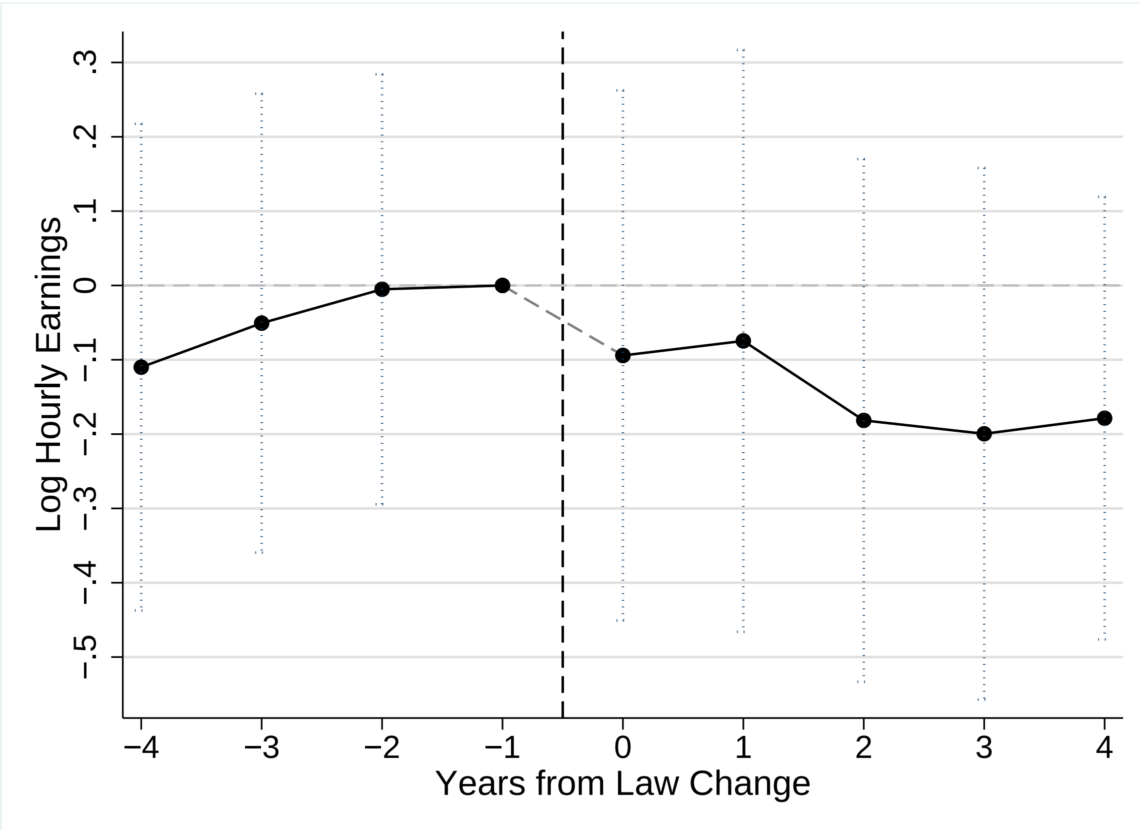
The results of this event study analysis are displayed in Figure B.2. Not surprisingly (due to the loss of about 1/3 of the events), the uncertainty associated with each coefficient is greater, and the post-period coefficients are no longer statistically significant. However, the qualitative pattern that emerges using cleaner, longer event windows is quite similar to the pattern displayed in Figure B.1, demonstrating that the event selection was not substantially skewing the results. Furthermore, the magnitude of the coefficients in the post-period years, while not statistically significant, are nearly identical to the magnitudes in our preferred distributed lag approach reported in Figure 4. As with our prior approaches, there is no evidence of a trend in earnings prior to a law change.

B.3 Long-Panel Approach

A concern common to each of the methods we have employed thus far is the differential treatment timing in different states (so-called "staggered" event study designs), as well as "non-absorbing" policies: in other words, states have the ability to change NCA enforceability multiple times, such as reversing or enhancing previously changed laws. One way to address this issue is to employ a "long-panel" event study design, in which the "event" in each treated state is simply the change in NCA enforceability between the beginning and end of the panel. To do so, we include the years 1991-1993 and 2012-2014 (the first and last three years in our panel) for each state, and we calculate the change in the NCA enforceability score over this time period. For states in which there were enforceability changes in the first three years or in the last three years, we omit the odd year out (and keep the two identical years). There were no states with multiple changes in either of those periods.

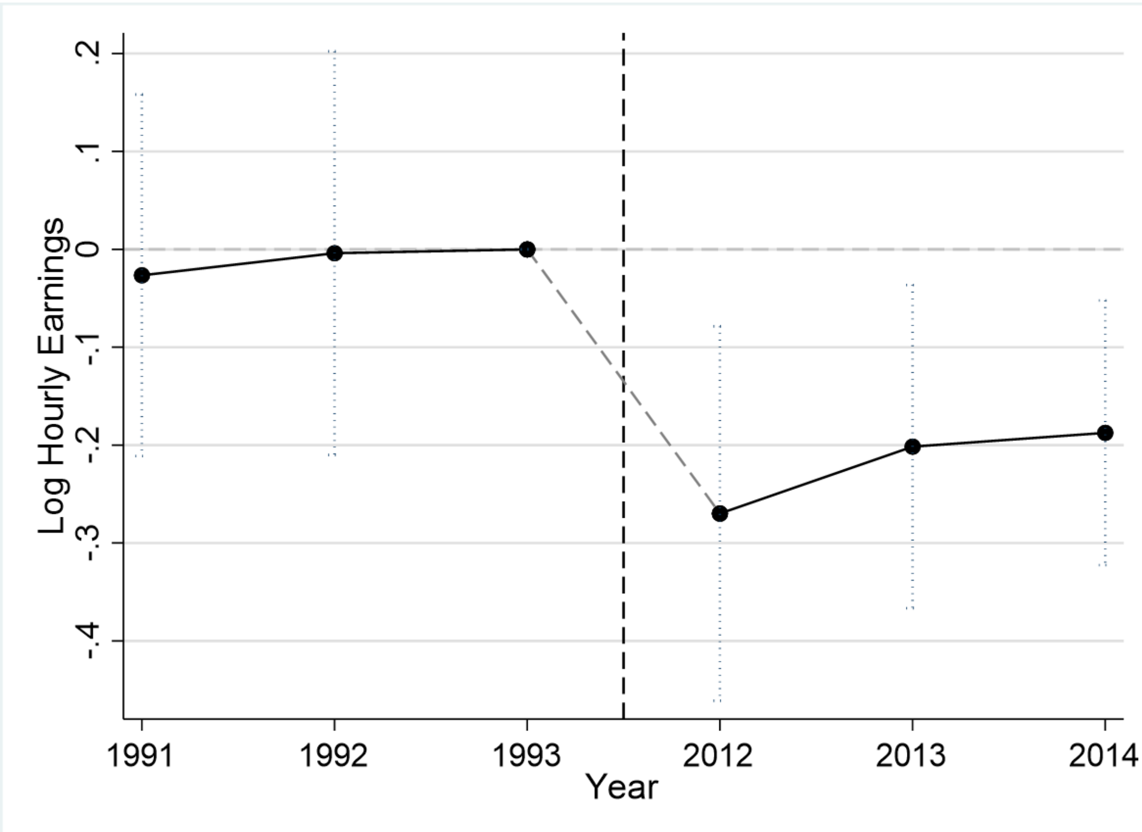
This approach alleviates issues associated with staggered designs, since all changes occur in years that intervene those included in the estimation sample. The approach also alleviates the issue of non-absorbing policies by effectively assuming that each state only had one change over the sample period. In this way, the approach closely mirrors the canonical 2-by-2 difference in difference design, though there are many states with enforceability changes and a treatment that varies in magnitude.

Figure B.2: Clean Window Event Study



The sample includes four-year windows around NCA law change events, as well as control states in the same Census division with no corresponding event in the four-year window, dropping entire events which introduce additional changes for the "treated state" in the pre- or post-periods. The estimating equation includes controls for sex, age, age squared, level of education, race, Hispanic status, and whether or not the respondent lives in a metropolitan area, as well as state and Census division-by-year fixed effects. Coefficient estimates and 90% confidence intervals pictured (normalized to coefficient estimate one year prior to law change).

Figure B.3: Long-Panel Event Study



The sample includes the years 1991-1993 and 2012-2014 for each state, dropping "odd year out" observations for each state (for states which there were enforceability changes in the first three years or in the last three years). The estimating equation includes controls for sex, age, age squared, level of education, race, Hispanic status, and whether or not the respondent lives in a metropolitan area, as well as state and Census division-by-year fixed effects. Coefficient estimates and 95% confidence intervals pictured (normalized to coefficient estimate for 1993).

Results are depicted in Figure B.3. As in the stacked event studies and the distributed lag model, there is no evidence of a trend in earnings that is different for treated versus untreated states. Earnings are substantially lower (higher) in states that experienced NCA enforceability increases (decreases) in the intervening years, with coefficients that are significantly different than zero and of essentially identical magnitude to our estimates in Figures 4 and B.2. This graph provides evidence that our results are not being driven by peculiarities of the methods we employ, as well as demonstrating that the effects of NCA enforceability changes appear to persist in the long run.

C Appendix Figures & Tables

Table C.1: Dimensions of NCA Enforceability, According to Bishara (2010)

Question Number	Question
Q1	Is there a state statute that governs the enforceability of covenants not to compete?
Q2	What is an employer's protectable interest and how is that defined?
Q3	What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?
Q3b/c	b) Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun? / c) Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?
Q4	If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenant enforceable? If so, under what circumstances will the courts allow reduction and what form of reduction will the courts permit?
Q8	If the employer terminates the employment relationship, is the covenant enforceable?

Source: Bishara (2010).

Table C.2: The Effect of NCA Enforceability on Earnings, by Component of NCA Score

Q1: State Statute	-0.031	(0.024)
Q2: Protectable Interest	-0.045*	(0.025)
Q3: Plaintiff Burden of Proof	0.040	(0.028)
Q3a: Consideration, Start of Employment	-0.055***	(0.014)
Q3bc: Consideration, Continued Employment	-0.031**	(0.012)
Q4: Judicial Modification	-0.023	(0.016)
Q8: Enforceable if Employer Terminates	0.011	(0.039)
NCA Score without Question 1	-0.120***	(0.037)
Observations	1216726	

Each of the first seven rows represents a separate regression (corresponding to Column 1 of Table 3) in which the variable $Enforceability_{st}$ in Equation 1 has been replaced with each component of the NCA Enforceability Score separately. The coefficient on the score component is reported, alongside SEs clustered by state in parentheses. The final row uses as an independent variable a modified NCA Enforceability Score that omits the score for Q1 (whether there exists a state statute that governs NCA enforceability) in the calculation, but is otherwise equivalent to the NCA Enforceability Score used in the main analysis.
***P<.01, **P<.05, *P<.1

Table C.3: The Effect of NCA Enforceability on Earnings: Robustness to Political & Economic Controls

	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.077**	-0.071***	-0.030**	-0.068***	-0.113***
	(0.033)	(0.024)	(0.013)	(0.023)	(0.020)
Observations	1139890	1139890	1448431	1139890	3431264
R^2	0.274	0.357	0.132	0.346	0.942
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

This table replicates Table 3, but additionally controls for all variables (except ideology variables) introduced in Table 2.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table C.4: The Effect of NCA Enforceability on Earnings: Excluding States with Partisan Judicial Elections

	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.135***	-0.121***	-0.046***	-0.122***	-0.153***
	(0.042)	(0.032)	(0.015)	(0.032)	(0.037)
Observations	989854	989854	1262128	989854	2695840
R^2	0.272	0.356	0.130	0.345	0.941
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Year-Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

This table replicates Table 3, but drops the 6 states in which judges are selected via partisan election. SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table C.5: The Effect of NCA Enforceability on Earnings: Excluding States with Judicial Elections (Partisan or Non-partisan)

	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.116	-0.112	-0.045**	-0.107	-0.125
	(0.092)	(0.074)	(0.019)	(0.074)	(0.082)
Observations	699036	699036	890737	699036	1531543
R^2	0.272	0.359	0.128	0.348	0.941
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Year-Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

This table replicates Table 3, but drops the 21 states in which judges are selected via election (partisan or non-partisan).
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table C.6: The External Effects of NCA Enforceability on Earnings (Weighted by Employment)

	(1)	(2)	(3)
Own State NCA Score	-0.063	-0.067	-0.050
	(0.042)	(0.045)	(0.049)
Donor State NCA Score		-0.014	-0.116*
		(0.054)	(0.067)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.078
			(0.093)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.257**
			(0.109)
Observations	613679	613679	613679
R^2	0.943	0.943	0.943

The dependent variable is log earnings. The sample is the QWI from 1991-2014 and includes individuals between ages 19-64. All regressions include controls for male, age group, as well as division by year by quarter and county fixed effects. Own Cty Emp/CZ Emp is the ratio of sex- and age-group-specific employment in own county divided by sex- and age-group-specific employment in the entire commuting zone. Each regression is weighted by cell-specific employment. Standard errors are clustered by own state in Column (1), and two-way clustered by own state and commuting zone in columns (2) and (3). ***P<.01, **P<.05, *P<.1

Table C.7: The External Effects of NCA Enforceability on Earnings on Counties Far from State Borders

	(1)	(2)	(3)	(4)
Own State NCA Score	-0.210***	-0.214***	-0.192***	-0.216
	(0.061)	(0.060)	(0.058)	(0.193)
Nearest Neighboring State's NCA Score	-0.198***	-0.084	-0.058	-0.011
	(0.066)	(0.062)	(0.062)	(0.083)
Observations	615097	2015741	1594870	545696
R^2	0.898	0.889	0.887	0.877
Border Sample	Y	N	N	N
Distance to Nearest State Restriction	None	None	50 miles	100 miles

The dependent variable is log earnings. The sample is the QWI from 1991-2014 and includes individuals between ages 19-64. Column 1 uses the sample from Table 6, while Columns 2, 3, and 4 use counties that are neither on state borders nor members of border-straddling commuting zones. Columns 3 and 4 further restrict by the distance from the focal county's centroid to the nearest county centroid in a different state. All regressions include controls for male, age group, as well as division by year by quarter and county fixed effects. Standard errors are clustered by own state. ***P<.01, **P<.05, *P<.1

Table C.8: The External Effects of NCA Enforceability on Mobility: Hires and Separations

	Hires			Separations		
	(1)	(2)	(3)	(4)	(5)	(6)
Own State NCA Score	-0.286**	-0.305**	-0.223	-0.269**	-0.291**	-0.193
	(0.111)	(0.122)	(0.144)	(0.132)	(0.140)	(0.167)
Donor State NCA Score		-0.115	-0.182		-0.141	-0.200
		(0.165)	(0.187)		(0.167)	(0.190)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.476			-0.568
			(0.557)			(0.590)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.413**			0.407**
			(0.162)			(0.163)
Observations	604322	604322	603466	604512	604512	603659
R^2	0.951	0.951	0.951	0.950	0.950	0.950
Sample	Border	Border	Border	Border	Border	Border

The sample is the QWI from 1991-2014 and includes individuals between ages 19-64. All regressions include controls for male, age group, as well as division by year by quarter and county fixed effects.
Standard errors are clustered by own state in columns (1) and (4), and two-way clustered by own state and commuting zone in columns (2), (3), (5), and (6). ***P<.01, **P<.05, *P<.1

Table C.9: Heterogeneous Effects of NCA Enforceability on Earnings by Race and Sex

	(1)	(2)	(3)	(4)
NCA Score	-0.136***			
	(0.046)			
Female & White=1	-0.469***	-0.417***	-0.423***	-0.416***
	(0.011)	(0.027)	(0.026)	(0.027)
Female & Black=1	-0.572***	-0.519***	-0.526***	-0.513***
	(0.011)	(0.026)	(0.024)	(0.030)
Male & Black=1	-0.339***	-0.280***	-0.282***	-0.271***
	(0.008)	(0.016)	(0.017)	(0.015)
Female & Not Black or White=1	-0.502***	-0.424***	-0.438***	-0.436***
	(0.019)	(0.016)	(0.015)	(0.016)
Male & Not Black or White=1	-0.146***	-0.132***	-0.144***	-0.142***
	(0.010)	(0.017)	(0.016)	(0.015)
White Male × NCA Score		-0.091*	-0.031	-0.069
		(0.047)	(0.053)	(0.047)
Female & White=1 × NCA Score		-0.166***	-0.097*	-0.138**
		(0.057)	(0.054)	(0.054)
Female & Black=1 × NCA Score		-0.166***	-0.097*	-0.154***
		(0.053)	(0.050)	(0.052)
Male & Black=1 × NCA Score		-0.175***	-0.112*	-0.132***
		(0.048)	(0.056)	(0.048)
Female & Not Black or White=1 × NCA Score		-0.221***	-0.141***	-0.199***
		(0.045)	(0.045)	(0.043)
Male & Not Black or White=1 × NCA Score		-0.106**	-0.029	-0.083*
		(0.048)	(0.046)	(0.044)
College Educated Worker=1 × NCA Score			-0.114***	
			(0.026)	
High NCA Use Occ=1 × NCA Score				-0.038***
				(0.012)
Observations	1537454	1537454	1537454	1537454
R^2	0.275	0.275	0.276	0.289

The dependent variable is log weekly earnings. The sample in all columns is the CPS ASEC from 1991-2014 and includes individuals between ages 18-64 who reported working for wage and salary income at a private employer the prior year. All regressions include fixed effects for state, fixed effects for Census division by year, fixed effects for broad occupational class, and individual controls for male, white, Hispanic, age, age squared, whether the individual completed college, and indicators for the metropolitan city center status of where the individual lives. In Column (4), High NCA Use Occupations are occupations with NCA use greater than the national average, as tabulated by Starr et al. (2021). A separate indicator for High NCA Use Occupation is included in those regressions.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1