The Labor Market Effects of Legal Restrictions on Worker Mobility*

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Abstract

We analyze how the legal enforceability of contractual restrictions on job mobility affects labor markets. Using a newly-constructed state-year panel of Noncompete Agreement (NCA) enforceability spanning 1991 to 2014, we find that increasing the enforceability of NCAs leads to a decline in workers' earnings and job mobility. An increase from the 10th to 90th percentile of enforceability is associated with three to four percent lower annual earnings among employed workers, and a nine percent decrease in the monthly probability of changing jobs. Examining local labor markets split by state borders, we show that changes to NCA enforceability cause sizable externalities on workers not directly affected by those changes. We then revisit a classic theory of wage-setting based on implicit contracts: in contrast to prior evidence, workers facing strict enforceability are unable to leverage tight labor markets to increase their wage. Finally, we show that enforceable NCAs increase the racial and gender wage gaps—the earnings effects among women and black workers are twice as large as the effect among white men. *JEL Codes:* J31, J42, K31, M55.

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1 Introduction

There is growing consensus that the U.S. 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. Recent evidence has highlighted how common NCAs are: Starr et al. (2018) find that 18 percent of workers in 2016 were bound by NCAs, and nearly 40 percent of workers had signed an NCA at some point in their career. 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, employers contend that NCAs increase incentives to invest in training, knowledge creation, and other portable assets (Rubin and Shedd, 1981) that could make workers more productive and increase 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[3] 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 data on changes in NCA enforceability. Researchers have, to date, relied largely on cross-sectional measures of states' enforceability or a small handful of changes. This approach has drawbacks: cross-sectional variation in enforceability might be correlated with other unobserved differences across states, cross-sectional measures based on a single year can introduce measurement error if

[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]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`).

NCA laws change over time, and a small handful of 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 use within-state changes in NCA laws to identify the broad-based labor market effects of NCA enforceability. Second, we show evidence on the mechanisms through which NCA enforceability—by increasing the costs of mobility—affects earnings. Finally, informed by prior evidence of differences in bargaining power across workers of different demographics, we show that NCA enforceability has a particularly detrimental effect on earnings for workers already in a position of weaker bargaining power, contributing a new insight to the determinants of wage inequality in the U.S.

To identify the effects of NCA enforceability, we use and extend a database compiled by Hausman and Lavetti (2017) with annual measures of NCA enforceability for each of the 50 U.S. states from 1991 to 2014. These data include both judicial and legislative decisions that change state-level NCA enforceability, coded to match the enforceability measures developed by Bishara (2010). The vast majority of these law changes (91.4%) occur due to court rulings, which are useful for our research design. Since courts are frequently constrained by judicial precedent, these law changes are unlikely to be the result of forces that reflect underlying economic or political trends. We combine our enforceability dataset with earnings and mobility outcomes from the Current Population Survey and the Quarterly Workforce Indicators dataset, both from the U.S. Census Bureau.

We find that increases in NCA enforceability lead to significant decreases in workers' earnings and mobility. Moving from the 10th to 90th percentile in enforceability is associated with a 3-4% decrease in earnings and a 9% decrease in the month-to-month probability of changing jobs. The earnings effects are almost entirely driven by declines in implied hourly wages. The effect is stronger among occupations, industries, and demographic groups in which NCAs are used more frequently (according to Starr et al. (2018)), which suggests that the effects on individuals who actually sign NCAs may be even larger. Under some assumptions, our estimates imply that rendering NCAs unenforceable nationwide would increase average earnings by nearly 7%. This increase is approximately equal to estimated wage premium associated with occupational licensing, and roughly one-third the size of the wage premium associated with union membership.

Though only a fraction of workers actually sign NCAs, the use of NCAs might create externalities on other workers by reducing labor market churn or increasing recruitment costs (Starr et al., 2018). To test whether such externalities exist, we show that 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 test whether a change in NCA enforceability in one state indirectly affects the earnings and mobility of workers located in an adjoining state. We find an indirect effect of enforceability that is more than 90% of the average direct effect on workers in the state that experienced the law change. This large spillover effect suggests that the "treatment" of NCA enforceability affects a larger population than the relatively small share of workers actually bound by NCAs.

To investigate the mechanisms through which NCA enforceability reduces earnings, we 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 seminal 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 this average effect masks substantial heterogeneity based on states' NCA policies. In states with weakly enforceable NCA policies, the minimum unemployment rate over a job spell increases current wages, even when conditioning on the initial unemployment rate (in the same manner as the findings of BDN). However, the effect reverses in states with strict NCA enforceability: in these states, the minimum unemployment rate has essentially no effect on a worker's current wage, and the initial unemployment rate has a much stronger effect. This finding suggests 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, motivated by prior literature demonstrating differences in the ways that workers of different demographics bargain in the workplace, we document economically meaningful heterogeneity in the earnings effect of NCA enforceability across demographic groups. Prior work has found that women face higher relative costs of litigation over NCAs with employers, 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 7.2%, 5.8%, and 10.6% of the earnings gap between white men and white women, black women, and black men, respectively.

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, 2018; Starr et al., 2018), 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., 2018). Other papers have used cross-sectional variation to test how enforceability moderates the employment effects the minimum wage (Johnson and Lipsitz, 2019). Other papers have 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, 2019). 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 that NCA enforceability affects the ability of workers to bargain with their employers in a broad representative sample of the US labor force.

Our findings are also relevant to several other literatures, including the literature on the effects of employer power in labor markets, and the impacts of free contracting. 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 Dube et al. (2018)). While it is not necessarily the case that NCAs increase employer power prior to hiring,[4] our results imply that NCAs skew

[4] Some evidence shows this may be the case (Starr et al., 2018).

power dynamics in employment relationships in favor of the employer by diminishing the worker's outside options. This finding is also relevant 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. While we lack sufficient data to examine efficiency at the employment relationship level, our findings of substantial externalities raise questions about the assumptions required for this argument in the case of NCAs. Finally, our work also 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 Data

2.1 State-Level NCA Enforceability Measures

The cornerstone of our project is a state-level panel dataset with annual measures of states' NCA enforceability. As documented by Bishara (2010), NCA laws vary along seven quantifiable dimensions across states and over time (see Table A.1 for a list of the dimensions). For example, one dimension (Q3a) indicates the extent to which employers are legally required to pay workers a compensating differential when the worker signs an NCA at the beginning of her 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).

We start with the dataset constructed by Hausman and Lavetti (2017): a panel containing values representing the stringency of the law on each of these seven legal dimensions for every state between 1991 and 2009. 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 just the years 1991 and 2009. Hausman and Lavetti (2017) 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. They also used detailed notes and decision rules provided by Bishara (2010) to ensure their approach to quantify enforceability followed that of Bishara (2010). After replicating the cross-sectional scores, they filled in the timing of all intervening changes using the same quantification methodology. These data have never previously been used to study the general labor-market effects of NCA laws.

We supplement the Hausman and Lavetti (2017) dataset with changes to NCA enforceability between 2009 and 2014. These changes were originally identified by Jeffers (2018), who used them to study the effect of restrictions to labor mobility on capital investment and entrepreneurship. We code these legal changes according to the criteria set forth in Bishara (2010): for details, see Table A.2. Using the seven dimensions of enforceability, we construct the composite index (the NCA Enforceability Score) introduced in Bishara (2010) for each state-year from 1991-2014.[5]

Differences in how states interpret the dimensions of enforceability lead to substantial differences in the NCA Enforceability Score across states. At the extreme ends of this policy 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.[6] 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 9-08-06 explicitly bans all NCAs in employment contracts.[7] 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, text in the courts decision set new precedent that NCAs

[5]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.

[6]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

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

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.[8] 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 Pennsylvanias 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 70 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 change in the magnitude of the NCA Enforceability Score that is 7–8% of the average score over this period, and the within-state standard deviation in enforceability is equal to roughly 19% of the overall standard deviation. Since our analyses rely on within-state changes in enforceability, these statistics suggest we have a reasonable amount of identifying variation.

Figure 1 shows the timing of NCA law change events from the combined database. 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 increasing over time, with an especially steep increase during the mid to late 1990s.

2.1.1 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 are correlated with states' underlying political, labor, or business characteristics that may also impact earnings growth. One might be concerned that changes to enforceability could be spurred by strong labor unions on the one hand, or 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

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

pressure that could sway legislative decision-making.

To test the assumption that NCA law changes are exogenous to such underlying forces, we estimate 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 (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.[9] 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 as independent variables we include each of the characteristics noted above (lagged by one year to avoid issues of reverse causality), as well as state and year fixed effects. Out of 19 variables, the vast majority have coefficients that are both economically and statistically insignificant. Only one of these nineteen variables is a statistically significant predictor of within-state changes in NCA policies: Senate Democrats ideology score. While this may seem to be a cause for concern, it is not surprising that one out of nineteen predictors is statistically significant.[10] A joint F test on the statistical significance of these nineteen predictors is not significant ($p > 0.10$). For transparency, we present robustness checks for our main estimates that control for these political and economic variables. A second piece of evidence supporting the exogeneity of law changes is that the R^2 of the model, after residualizing on year and state fixed effects, is 0.109, meaning that these predictors

[9]We have also considered regressions including employment, gross state product, unemployment rates, aggregate personal income, and poverty rate as predictors of NCA law changes. While none are close to being significant predictors, we omit them from our regression since they may be dependent on NCA law, similar to what is shown in this paper.

[10]The probability of finding one or more significant predictors out of nineteen, 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 0.86 ($1 - 0.9^{19}$).

collectively explain only 11% of the variance in within-state changes to NCA policy. We also test for pre-trends in event study analyses in Section 3.2.

2.2 Data on Earnings and Mobility

We gather individual-level data on earnings and employment from the Current Population Survey (CPS). In addition to data from the main survey, we use the Annual Social and Economic Supplement (ASEC), along with the Occupational Mobility and Job Tenure Supplement (JTS) of the CPS. The latter includes, for a subset of workers, details about their current job spell, including their tenure with their current employer.

We use the Annual Social and Economic supplement (ASEC, otherwise known as the March Supplement) to estimate the effect of NCA enforceability on earnings and wages, which are deflated using the Consumer Price Index. The ASEC is a CPS supplement collected each March that contains information about the wage and salary income of respondents. Each dataset also includes demographic and geographic information.[11] We use the unsupplemented CPS to estimate the effect of NCA enforceability on worker mobility. The CPS contains a variable "...[indicating] whether or not the respondent was employed by the same employer and the same job he/she reported working as his/her main job in the previous month's survey."[12]

We restrict the CPS and 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 observations for which earnings or hours variables have been topcoded. The resulting CPS dataset contains approximately 2.4 million observations[13], and the resulting ASEC dataset contains approximately 1.5 million observations, 1.2 million of which represent full-time workers. We match NCA enforceability measures by state and year.

We also make use of the Quarterly Workforce Indicators (QWI) dataset, both to corroborate earnings estimates from the CPS and also to investigate spillovers

[11] 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, however, are qualitatively unchanged with the ACS.

[12] 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`

[13] We further restrict the CPS to include only observations in which individuals answered the survey themselves (as opposed to proxy respondents). Results using the full sample are significantly more noisy, but largely unchanged otherwise. Results using the ASEC are largely unchanged when limiting the sample to self-respondents.

from NCA enforceability. The QWI is a public use file from the U.S. Census Bureau that aggregates data from the Longitudinal Employer-Household Dynamics (LEHD) dataset. The QWI dataset used in this paper 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. 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. For this reason, we are left with only 41 legal changes (instead of the universe of 70 legal changes) when using the QWI. Thus, the ASEC serves as our primary dataset when estimating overall effects on earnings.

Finally, 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 history of employment, such as "How long have you been working [for your present employer]?"[14] We use responses to this question to calculate the year that the worker began his or her job spell, which allows us to match individuals to the enforceability score at the time of hiring. Our primary outcome variable of interest is weekly earnings, and we use additional variables as controls. We also use national and state-level annual unemployment rates between 1947 and 2014 from the Bureau of Labor Statistics website.

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

Economic theory yields an ambiguous prediction regarding the effect of NCA enforceability on earnings: while enforceable NCAs may diminish the bargaining power of workers in wage negotiations, employers may be more willing to invest in the skills of workers with NCAs, leading to greater worker productivity and compensation. Furthermore, employers may need to pay compensating wage differentials at the outset of employment to induce workers to sign NCAs.

Before turning to our main identification strategy leveraging within-state law changes, we assess the extent to which cross-sectional variation in enforceability can explain variation in earnings across workers and over time. Figure 3 plots average Mincerized log earnings over the period 1991–2014,[15] separately for workers in high

[14] 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.

[15] The figure plots the residuals from a regression of log earnings on sex, race, Hispanic status, age, age squared, marital status, metropolitan area status, as well as Census division, occupation,

and low enforceability states (defined as states with enforceability scores in 2009 that are above or below the median). Earnings in high enforceability states are lower than earnings in low enforceability states at all time points. Furthermore, the gap between high- and low-enforcing states appears to widen in the years following the Great Recession of 2007-2009. We revisit this observations in Section 4.

However, this cross-sectional evidence may conflate unobserved differences across states with differences in NCA policy. To address this problem, in this section we use within-state changes in NCA policy over time to estimate the effect of NCA enforceability on earnings, as well as its effect on worker mobility. Additionally, in Section 3.5 we also examine whether state enforceability policies have spillover effects on workers in different legal jurisdictions.

3.1 Main Results on Earnings and Mobility

We use a difference-in-difference design to estimate the effects of NCA enforceability on earnings and mobility, 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 2, 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.[16] The coefficient of interest, β, is identified from changes in earnings in states that have changed their NCA enforceability, relative to other states in the same Census division. Standard errors are clustered by state. A key identifying assumption is $E(Enforceability_{st}\varepsilon_{ist}|\rho_s, \delta_{dt}) = 0$: conditional on state and division-year effects, changes in enforceability are uncorrelated with the error term. The evidence in Section 2.1.1 supports this assumption.

Results are reported in Table 3. Columns 1-4 use data from the ASEC, restricted to full-time workers between the age of 18-64 who reported working for wage and salary income at a private employer the prior year.[17] The coefficient in Column 1 suggests that going from NCA enforceability of 0 (completely unenforceable) to 1

and industry fixed effects, estimated on individuals in the CPS ASEC who are between 18 and 64 working in the private for-profit sector.

[16] 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 the *Enforceability* on earnings, corresponding to Column 1 of Table 3, is -0.150 ($p < .01$) when year fixed effects are used in lieu of Division by year fixed effects.

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

(the strictest enforceability observed in our sample) leads to a 9.2 percent decline in earnings ($exp(-.096) - 1, p = .019$). Adding fixed effects for broad occupation codes in Column 2 leaves the point estimate essentially unchanged but improves its precision ($p < .01$). To get a sense of the magnitude of this estimate, the 10th and 90th percentiles of *Enforceability* observed in our sample are 0.55 and 0.9, respectively. The estimates thus imply that moving from the 10th to the 90th percentile in *Enforceability* leads to a 3.1 percent average decline in annual earnings.

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[18]: while the point estimate is negative, it is relatively small and statistically insignificant. 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 essentially identical to the coefficient on annual earnings.

Finally, in Column 5, we corroborate estimates in Columns 1–4 (using the CPS ASEC sample) using data from the QWI. We run essentially the same regressions specification as Column 1, except that we can include fixed effects for each county (rather than state)[19] and each Division-Quarter (rather than Division-Year). The estimate is very similar in magnitude to that in Column 1.

A useful benchmark against which to compare our results is the estimated wage effects of other labor market institutions. For example, Farber et al. (2018) find that the household income premium associated with union membership has been 15-20 log points over the past 80 years. Gittleman et al. (2018) estimate that a required, government-issued occupational license increases wages for workers in the licensed occupation by 7.5%.[20] To compare the effects of NCA enforceability against these institutions, we can extrapolate our within-state estimates to consider what would happen to earnings under a national policy that rendered all NCAs unenforceable. To do so, we generate predicted earnings for each individual in the 2014 ASEC sample using 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 would increase by 7.0% nationally if NCAs were made unenforceable.[21]

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

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

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

[21]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

This magnitude is similar to the premium attained by workers in occupations with government-mandated licenses and roughly one-third to half the household premium from union membership.

Figure 4 visually illustrates the relationship between annual earnings and NCA enforceability using binned scatterplots. To isolate the identifying variation use in regressions, in all four graphs earnings and NCA enforceability are plotted net of state and census division by year effects. Panels (a) and (c) include no additional controls, and panels (b) and (d) includes the additional controls used in Column 2 of Table 3 (1-digit occupation codes and individual-level demographic controls). Panels (a) and (b) use all state-years, and panels (c) and (d) drop the two states with *Enforceability* that represent extremely low outliers (California and North Dakota). All of the figures clearly depict a 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 A.3, 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 A.1.[22] With one exception (which is insignificant at the 5% level), the effect of each score is negative, and is significant at the 5% level for three out of seven components. In line with Starr (2018), we find that the dimensions yielding the most negative earnings effect are those requiring consideration, both at the outset of employment (Q3a) and after employment has already begun(Q3bc). The existence of a state statute (Q1) has a slightly negative but insignificant earnings effect. This lack of an effect for Q1 is perhaps not surprising: some states that do not have statutes regarding NCA enforceability (e.g., Kansas and Connecticut) nonetheless enforce NCAs more readily than many other states. Since this dimension

individual, i, in 2014. Let γ be the vector of respective coefficients estimated in the same regression, and let β be the coefficient on $Enforceability_i$, the NCA Enforceability Score for individual i's state of residence in 2014. Then, if $\hat{Y}_{i,1} = \gamma X_i + \beta Enforceability_i$ represents predicted earnings for individual i, and $\hat{Y}_{i,2} = \gamma X_i$ represents predicted earnings for individual i when $Enforceability_i = 0$, the predicted earnings increase is calculated as the average of $\hat{Y}_{i,2}$ minus the average of $\hat{Y}_{i,1}$, divided by the average of $\hat{Y}_{i,1}$.

[22] 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 A.3.

might not accurately portray true enforceability of NCAs, in the final row of Table A.3 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.

The final column of Table 3 presents estimates of the average effect of *Enforceability* on mobility. To measure workers' mobility, we use individuals' response to the CPS question about whether the respondent's current employer was the same as her employer in the previous month. The outcome variable is a dummy equal to 1 if the answer to this question was "Yes" and equal to 0 if the answer was "No" (we omit a small share of observations in which the response was "I don't know" or "refusals to answer"). The estimated effect of *Enforceability* is -0.6 percentage points, which is marginally statistically significant at the 10% level. The sample mean rate of monthly job changing is approximately 2.3%, implying that moving from the 10th to 90th percentile in enforceability decreases monthly mobility by about 9%.

3.2 Event-Study Estimates on Earnings: Pre-Trends and Dynamic Effects

Two concerns are common with difference-in-difference designs. A first concern is to ensure a lack of pre-trends, or divergence in the outcome variable between treatment and control groups in the years prior to the onset of treatment. In our context, this pattern could arise if earnings or mobility patterns influence judicial or legislative decisions on NCA enforceability, or if business or labor advocacy organizations impact earnings and mobility as well as legal decisions. Both constitute forms of reverse causality that would violate the model assumptions. A second concern is imbalance in treatment timing. Our regression design leverages changes in NCA laws that occurred in different states in different years. This variation in treatment timing can give differential weight to states depending on at what point in the sample period they experience a law change; this weighting could cause our estimate to depart from the average treatment effect (Goodman-Bacon, 2018).

To address these concerns, we complement our difference-in-difference estimates with event-study analyses. For each state that experiences an NCA enforceability change, we isolate a four year window before and after the law change, and we identify 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 data for each treatment window and the corresponding set of control states, and estimate the difference in outcomes between treated and control states in each year relative to the law change. Figure 5 shows the event study estimates. The first pattern that emerges from the figure is that there is no evidence of a pre-trend, supporting our argument above that NCA law changes were likely exogenous to underlying economic trends. The second pattern in the figure is a decline in hourly earnings the year of the law change that grows in magnitude over 4 years following the law change. These estimates imply that moving from the 10th to 90th percentile of *Enforceability* leads to a reduction in hourly wages that is as high as 6.1 percentage points four years after the law change, which is meaningfully larger than our difference-in-differences estimates of the overall effect on earnings.

3.3 Addressing Other Threats to Identification

In Section 2.1.1, we provided evidence that economic, social, and political variables 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, an additional way we can bolster causality is to ensure that no other economic, social and political factors are changing at the same time as NCA law changes. We assess the robustness of our estimates to this concern in Table A.6. We replicate the structure of Table 3, but we include additional controls for each of the predictors included in Table 2.[23] 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 an appealing aspect of our research design is that the vast majority of NCA law changes arise from judicial decisions, which are arguably 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 in particular, 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 judge 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 (whether or not the elections are partisan), in Tables A.4 and A.5, respectively. If

[23] 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.

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). Thus, there is no evidence that our estimates are biased by impartial judicial decision-making.

3.4 Heterogeneous Effects Based on Prevalence of NCA Use

The results in Table 3, Figure 4, and Figure 5 imply that stricter NCA enforceability leads to lower earnings for the average worker. However, this average effect is unlikely to be constant across all workers. One likely source of heterogeneity is prevalence of actual NCA use. All else equal, we would expect NCA enforceability to affect earnings more in settings in which they are used more often; in the limiting case, if NCAs are never used for a certain group of workers (and there are no spillover effects), we should expect no effect of NCAs on earnings for those workers.

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 3.1. If we found that enforceability has larger earnings effects among groups less likely to be bound by NCAs, it might raise questions about the research design. Second, identifying the effects of NCA enforceability on groups more likely to use NCAs moves our estimates closer to a "treatment effect on the treated". This allows policymakers to assess the impact that changes in NCA enforceability will have on those mostly likely to sign NCAs.

While we do not observe whether individual workers have or have not signed an NCA, Starr et al. (2018) report several sources of heterogeneity in NCA use by worker characteristics. We focus on three sources: worker education, worker occupation, and worker industry. First, Starr et al. (2018) 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. (2018) find heterogeneity in use across 22 occupation categories and 19 industry categories. We use the occupation and industry an individual reported working in the prior year to the CPS to classify workers as working in *High or Low NCA Use Occupations* and *High or Low NCA Use Industries*.[24] We replicate our main difference-in-difference specification, Equation 1, except that we now add

[24]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; Accomodation 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. (2018).

an interaction term of *Enforceability* with an indicator for *College Educated Worker*, *High NCA Use Occupation*, or *High NCA Use Industry*.

Table 4 reports these heterogeneity estimates. Column 1 reports the baseline average effect on earnings, corresponding to Column 2 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 sum of the main effect on NCA Score and the interaction effect implies that going from the 10th to 90th percentile of enforceability leads to a 5.2% decrease in earnings for college-educated workers ($p < .01$), and the interaction term implies that the effect is 4.8 percentage points greater in magnitude for college educated workers relative to those without a college education ($p < .01$). The main effect itself is close to zero and statistically insignificant, implying little to no effect of enforceability on earnings for non-college educated workers.

Column 3 reports heterogeneity by occupational use of NCAs. Going from the 10th to 90th percentile of enforceability leads to a 4.2% decrease in earnings in high-use occupations ($p < .01$), an effect that is 2.0 percentage points larger than for low-use occupations ($p < .01$). Finally, Column 4 reports heterogeneity by industries' use of NCAs. Going from the 10th to 90th percentile of enforceability leads to a 4.7% decrease in earnings in high-use industries ($p < .01$), an effect that is 2.3 percentage points larger than for low-use industries ($p < .01$).

Estimating the heterogeneous impact of NCA enforceability separately by education, occupation, and industry might be misleading if college education is correlated with working in a high NCA use occupation or industry, confounding estimates. In Column 5, we simultaneously estimate the heterogeneous impacts of NCA enforceability for those with and without college degrees, and those in high and low use occupations and industries. The coefficients on the interactions of NCA Score with *High Use Occupation* and *High Use Industry* attenuate, but remain negative and significant. The interaction of NCA Score with *College Educated* changes little and remains statistically significant; 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. (2018) that NCA use is increasing in workers' annual earnings.

These sources of heterogeneity imply that NCA enforceability has a larger effect on earnings in settings in which NCAs are more frequently used. This supports the causal interpretation of our initial findings, and it also suggests that the earnings effect of NCA enforceability is substantially larger among groups most likely to sign NCAs.

3.5 Spillover Effects of NCA Enforceability

An important qualifier about our results thus far is that we do not observe which workers actually *sign* NCAs. If NCA enforceability only affected the earnings of workers actually bound by an NCA, one could interpret our results as "intent-to-treat" effects, in which the "treatment" is signing an (enforceable) NCA. If this were the case, we could easily estimate the "treatment effect on the treated" by scaling the intent to treat estimate by the proportion of workers bound by NCAs. However, in this section we show that this approach is invalid in our setting by demonstrating that changes in NCA laws have spillover effects on workers in different legal jurisdictions.

We examine whether legal changes to NCA enforceability in a "donor" state impact 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 workers employed on the Missouri side of the St. Louis metro area? Ex ante, it is plausible that such spillover effects could be present: NCA use by a subset of firms can reduce labor market dynamism and increase recruitment costs for all firms (Starr et al., 2018), resulting in fewer vacancies and lower wages (Diamond, 1982).

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 (Autor et al., 2017). We identify commuting zones that straddle state borders: these commuting zones are local labor markets that include business establishments in two states and therefore may be 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. 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. We also report estimates that include all 742 counties in these commuting zones (including counties insulated from the state border).

We employ data from the Quarterly Workforce Indicators (QWI) dataset, which includes quarterly earnings and employment flows at the county level, separated by various firm characteristics and worker demographics. In particular, each observation in the dataset represents a unique year, quarter, county, sex, and age group cell, as defined in the QWI.

To test for spillovers, we use an analog of the difference in differences model corresponding to Equation 1 to estimate the impact of a change in NCA enforceability across a state border, when the two states share a commuting zone. 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 5. Columns 1–3 report estimates on the subset of counties directly on state borders in the border commuting zones. Column 1 verifies the basic relationship between a state's NCA score and earnings. The coefficient on *Own State NCA Score* is close to that reported in Table 3, confirming that the direct effect of enforceability in this subsample of counties is similar to that of the entire country. The model in Column 2 additionally includes *Donor State NCA Score*. The estimate reveals evidence of spillover effects of NCA enforceability: an increase in NCA enforceability in a donor state reduces the earnings of workers by an amount that is nearly as large as the effect of an increase in a worker's own state (-0.141, $p = .049$ vs. -0.154, $p = .017$).

The magnitude of this spillover effect, however, could differ depending on county characteristics. One plausible source of heterogeneity is the share of a county's employment in the employment of the commuting zone as a whole. Intuitively, a law change in a donor state should have little to no effect on earnings in a focal county if the focal county makes up a large portion of (i.e. dominates) the commuting zone's employment. On the other hand, a law change outside of a focal county would have a larger impact on a focal county that comprises a very small part of its commuting zone.

We test for such heterogeneity in Column 3. Along with their main effects, we include interactions of *Own State NCA Score* and *Donor State NCA Score* with the ratio of sex- and age-group-specific employment in the focal county to sex- and age-group-specific employment in the commuting zone as a whole (*Own Cty Emp / CZ*

Emp).[25] The results demonstrate that spillover effects of donor-state enforceability are heterogeneous in a manner consistent with the logic above. The main effect of *Donor State NCA Score*, representing the spillover effect for a county that comprises zero percent of its commuting zone's employment, is negative (-.166, $p < .01$) and actually with a magnitude slightly *larger* than the coefficient on *Own State NCA Score*. In contrast, the spillover effect is substantially smaller for counties that contribute a large share of employment in their commuting zone: adding the main effect of *Donor State NCA Score* with its interaction with *Own Cty Emp / CZ Emp* implies a spillover effect close to zero (-0.166 + 0.149 = -0.017) for a county comprising 100% of its CZ employment.[26]

The evidence of spillover effects is further demonstrated in Figure 6, which uses the methodology in Section 3.2 applied to changes in *Donor State NCA Score*, on the sample of physical border counties. The event study graph exhibits no evidence of any pre-trend in earnings, supporting the causal interpretation of our estimates. The effect of border state NCA Enforceability appears only after a few years: this lag (relative to Figure 5) could be due to adjustment times by firms that result in a relatively slow spread of labor market conditions across the commuting zone.

To ensure that our estimates are detecting true spillovers in *local* labor markets, and not just sampling variation, Columns 4–6 replicate Columns 1–3 but also include the counties in the border CZs that do not physically lie on the state border. Even though such counties are in a common labor market, their insulation from the state border should make them slightly less affected by the labor market effects of an NCA law change in a donor state. As an extreme example, Los Angeles shares a commuting zone with La Paz county in Arizona, even though Los Angeles is roughly 200 miles from the Arizona state border: one would not expect an NCA law change in Arizona to affect Los Angeles county. The results in Column 4 are consistent with this prediction: the overall spillover effect is weaker in this subsample than in the whole sample (-0.111, $p = .145$ vs. -0.141, $p = .049$).

To further bolster the causal interpretation of our spillover estimates, in Table A.7, we consider a falsification test for a set of counties that should be completely immune to NCA law changes in border states. We consider counties that lie over 100 miles from any state border and are not in a commuting zone that straddles a state border. We assign to each county a donor state NCA score that corresponds

[25]We also include the main effect of that employment ratio but do not report its coefficient in the table.

[26]Column 3 also reveals that the earnings effect of *Own State NCA Score* is much larger for counties that comprise a large share of commuting zone's employment, though the estimate is imprecisely estimated.

to the state geographically closest to that county.[27] Reassuringly, the point estimate on *Donor State NCA Score* is close to zero and nowhere near statistically significant ($p = .806$).

Finally, we present estimates of the spillover effects of enforceability on mobility in Table 6. The structure mirrors Table 5. Our dependent variables are the log quarterly number of accessions (hires) and separations in the top and bottom panels, 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. The spillover effects (reported in Columns 2 and 5) are imprecisely estimated, though they are negative and of a magnitude that is 70-80 percent as large as the direct effect. 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's mobility among counties comprising a small portion of the commuting zone's total employment, compared to counties comprising a large share. Finally, comparing Columns 2 and 5 reveal that spillover effects of an NCA law change in a donor state attenuate among counties not physically on the state border, consistent with the patterns we find for earnings. While we interpret these estimates as suggestive, given their imprecision, they are consistent with the hypothesis that NCA enforceability has spillover effects on worker 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 do not measure which workers do and do not sign NCAs, these results suggest that NCA use has external effects on workers and firms that do not use them, consistent with evidence found in Starr et al. (2018).

4 Why Does NCA Enforceability Reduce Earnings? The Effects of Costly Mobility

Recall that Figure 3 plots average Mincerized log earnings in high and low enforceability states. In addition to the overall gap in earnings between the two types of states, earnings in high versus low enforceability states appear to widen in the years following recessions. This pattern suggests that the overall health of the economy plays a role in determining the strength of the relationship between earnings and NCA enforceability.

[27] Specifically, we calculate the distance between county centroids. If the centroid of a county in a different state is less than 100 miles from the centroid of the focal county, we exclude that focal county from this analysis. Donor state NCA scores are similarly assigned by finding the county in a different state whose centroid is closest to the focal state's centroid, and using that donor state's NCA score.

In this section, we explore this relationship by examining a channel through which restricting workers' mobility would reduce their earnings: by diminishing workers' abilities to take advantage of favorable labor markets over the course of their job tenure.

A longstanding theory in labor economics is that wages are determined by "implicit contracts" in which firms insure workers against declines in their wage. This theory implies that wages are not determined in a spot market, but rather set by implicit contracts with terms that depend in part on the worker's outside option. The seminal 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 and her employer has no incentive to increase her wage. 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 workers' wages. Therefore, the best labor market conditions over the course of a worker's job spell will be correlated with the worker's current wage.

BDN develop a simple empirical method to test between these models, and 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 contemporaneous conditions (which would be predicted by a spot market) or those 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)).

An immediate prediction is that NCAs–by making mobility more costly–change the nature of implicit contracts in the labor market. In other words, we expect that, when NCAs are more enforceable, workers are no longer able to 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.

We begin by replicating BDN. We use the CPS JTS (which collects data in even-numbered years), limiting our analysis to full-time, private sector workers, for the years 1996-2014[28] (compared to BDN, who used the years 1976 to 1984). We estimate

[28]We omit years prior to 1996 due to a lack of data availability: though BDN use CPS data

the model:

$$\ln w(i, t + j, t) = \Omega_1 X_{i,t+j} + \Omega_2 C(t, j) + \varepsilon_{i,t+j}, \tag{3}$$

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 is model dependent. Possible inclusions in $C(t, j)$ are *Initial UR* (the unemployment rate at the beginning of the individual's job spell) and *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, we control for 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 to accommodate our analysis. 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 for confidentiality reasons). In their stead, we use dummy variables for metropolitan area status (comparable to those 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 year fixed effects. Each of these adjustments ultimately has little bearing on our estimates.[29]

We report these results in Table 7. 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

collected prior to 1996, the dataset we employ (the CPS JTS) has only been collected since 1996.

[29]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. Our results are finally robust 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.

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:

$$\ln w(i, t+j, t, s) = \Omega_1 X_{i,t+j} + \Omega_2 C(t, j) + \Omega_3 Enf_{t,s} + \Omega_4 C(t, j) * Enf_{t,s} + \varepsilon_{i,t+j}, \quad (4)$$

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 that the coefficient on $Enf_{t,s} \times$ *Minimum UR* will 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 that the coefficient on $Enf_{t,s} \times$ *Initial UR* will 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 change, indicating that NCA enforceability is not acting as a de facto proxy for one of the unemployment rates.[30]

In Column 5, we include the interactions demonstrating the change in the cost of mobility. The coefficient on $Enf_{t,s} \times$ *Initial UR* (-0.017; $p < .05$) shows that labor market conditions at time of hire affect earnings much more negatively when NCAs are more enforceable. Similarly, estimates show heterogeneous wage effects of the most favorable labor market conditions over the course of the employee's tenure. The main effect on *Minimum UR* (-0.029, $p < .01$) shows workers in a state with the lowest enforceability experience a substantial wage benefit from an improvement in

[30]Caution should be taken when interpreting the coefficient on *Initial NCA Score*, 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. By controlling for tenure, we are shutting down an important channel between NCA law changes and earnings. Indeed, omitting tenure and tenure squared as controls in the regression in Column 3 slightly increases the coefficient on *Initial NCA Score* to -0.083, though it is not statistically significant. We note that exclusion of tenure controls does not meaningfully impact the magnitude or significance of the coefficients of interest in subsequent regressions.

their outside option. In contrast, the interaction term $Enf_{t,s} \times \textit{Minimum UR}$ (0.022; $p < .05$) shows that these wage benefits are much more modest in states with higher enforceability: for workers in a state with the highest observed enforceability, the most favorable labor market condition over the course of their tenure has essentially no effect on their wage ($-0.029 + 0.022 = -0.007, p = .26$).

These estimates imply that NCA enforceability fundamentally changes the way that workers and employers negotiate wages. To visualize the real implications of these findings, we can use our estimates from Table 7 to predict how the wage path of a worker beginning a job spell in a particular year differs depending on the NCA enforceability in her state. We consider the predicted earnings path for two hypothetical workers—one in a low- and the other in a high-enforceability state—who each began job spells in 2009 at identical wages and held their job through 2019. This period is of particular interest given that 2009 was the onset of the Great Recession and witnessed a large increase—then decrease—in the unemployment rate. We predict earnings each year based on a regression identical to that reported in Column 5 of Table A.8, and we plot the path of normalized predicted earnings[31] for both individuals in Figure 7, alongside the monthly national unemployment rate. The two wage paths move in perfect tandem until 2013–the year that the unemployment rate begins to drop below the initial unemployment rate at the start of the workers' tenure (2009). Beginning in 2013, however, the paths diverge. The worker in the low-enforcing state is able to take advantage of the improvement in her outside option and increase her earnings above and beyond the initial earnings path. The path of earnings for the worker in the high enforceability state, on the other hand, is significantly less responsive to the labor market tightening: this worker's earnings continue to rise at a relatively constant rate according to their tenure at the firm. Ultimately, this divergence leads earnings in the high enforceability state to be 2.4% lower than in low enforceability states (\$935.12 vs. \$912.67) in 2019.

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 mobility, NCAs reduce workers' bargaining power. In other words, enforceable NCAs change the terms that govern how workers and employers bargain over wages.

[31] The normalization simply subtracts the difference in low enforceability versus high enforceability state earnings from each predicted value for high enforceability in order to consider two hypothetical workers with identical initial wages. The initial gap (in January, 2009) is \$3.26.

More broadly, these estimates illustrate a means through which NCA enforceability has plausibly contributed to the declining labor share of income and wage stagnation in recent decades. The cross-sectional evidence in Figure 3 implied that the "earnings penalty" of high NCA enforceability widened during and in the years following recessionary periods. The estimates in this section reveal a reason why. As the unemployment rate decreases during recovery periods following recessions, workers in states that enforce NCAs more strictly do not see wage gains commensurate with the general improvements in the labor market. A plausible implication of these results would be that productivity gains translate less into wage gains when NCAs are more enforceable.

5 NCA Enforceability Reduces Earnings More for Women and Black Workers

NCA enforceability might have a stronger impact on earnings for some groups of workers than others—even holding prevalence of NCA use constant. For example, even though NCA use is not systematically different for male and female workers (Starr et al., 2018), NCA enforceability may have a differential effect on the wages of men and women for several reasons. First, evidence suggests that NCAs might be implicitly more binding for women than for men. Marx (2018) finds that strict NCA enforceability decreases entrepreneurship among women at a greater rate than among men, which he attributes to women facing higher costs of litigation (e.g., over violation of an NCA) than men do. Second, if NCAs shift the bargaining power between workers and employers (which the results in Section 4 imply is the case), this shift could be more drastic for women than for men. Female workers face higher barriers to bargaining, and receive lower shares of job-specific surplus, than do male workers (Card et al., 2015). At the same time, improvements in outside options lead to higher wages for both male and female workers (Caldwell and Harmon, 2019; Caldwell and Danieli, 2018). By restricting outside options, strict NCA enforceability may then exacerbate bargaining disparities between male and female workers[32] (see Bertrand (2011) for an overview of this literature). Third, if strict NCA enforceability reduces the competitiveness of labor markets, it may give firms monopsony power to

[32] Put another way, prior literature has shown that men are more likely than women to negotiate for salary increases when the "rules of wage negotiation" are ambiguous, but are equally likely to do so when employers signal clearly that negotiation is allowed (Leibbrandt and List, 2014). To the extent that receiving an outside job offer provides an unambiguous rationale to negotiate with one's employer, removing this option will also exacerbate inequalities in bargaining power between men and women in the workplace. Similarly, Exley and Kessler (2019) show that women are less likely to self-promote, which may be their only option when alternative job opportunities are more limited.

discriminate between male and female workers (Robinson, 1933; Barth and Dale-Olsen, 2009). For each of these reasons, NCA enforceability might reduce wages more for female workers than for similar male workers.

By similar arguments, NCA enforceability could have a stronger effect on the earnings of racial minorities than white workers. There is anecdotal evidence that NCAs can be more implicitly binding for black workers than their white co-workers.[33] As with gender, there is evidence that black workers face lower negotiating power in the workplace than do white workers (Hernandez et al., 2018).

Motivated by these findings, in this section we investigate whether the earnings effect of NCA enforceability is heterogeneous on the basis of sex or race. Column 1 of Table 8 displays the overall "earnings" gaps for non-black women, black women, and black men, relative to (the omitted category) white men,[34] from a regression corresponding to Equation 1. The regressions underlying this table differ from Equation 1 in two ways. 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.[35] 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 table. Column 2 reports estimates from a regression that additionally includes NCA Score interacted with our four demographic categories (dummy variables indicating whether the respondent was a non-black woman, a black woman, a black man, or men that are not white nor black). The main effect of NCA Score shows that NCA enforceability has a moderate negative impact on earnings of white men, the omitted group, though the effect is smaller in magnitude than the overall effect (-0.063 vs. -0.107) and statistically insignificant at conventional levels ($p = .148$).

The coefficients on the interaction terms (representing the additional effect of NCA enforceability on women and black workers) are all negative and significant at the 10% level (and at the 1% level for black men). The magnitudes are slightly greater than the main effect of NCA Score, implying the earnings penalty from NCA enforceability is over twice as large for women and black men as for white men[36].

[33]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. However, 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)

[34]The regressions in this table also include an indicator for men who are neither white nor black. This coefficient, and its resulting interaction, are omitted from the table for brevity.

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

[36]The coefficients on each of the three interactions of interest are not significantly different from each other, according to pairwise F-tests for equality of coefficients.

These results suggest that NCA enforceability not only reduces earnings *on average*, but also likely exacerbates existing disparities across demographic groups. This point is illustrated two ways. First, the coefficients in Column 2 imply that moving from the 10th to 90th percentile of the NCA Score distribution (NCA score = 0.55 and 0.9, respectively) would decrease average earnings of white men by approximately 2.2%, vs. a decrease of 4.8%, 4.8%, and 5.1% for non-black women, black women, and black men, respectively. Together with the estimates in Column 1, these results imply that if a state that enforces NCAs at the 90th percentile of the distribution were to switch to enforcing NCAs at the 10th percentile of the distribution, the earnings gap between white men and non-black women, black women, and black men would close by 5.6%, 4.5%, and 8.4%, respectively.

The estimated coefficients would be misleading if sex or race correlate with differences in education or occupational choice, which Section 3.4 showed moderates the effect of NCA enforceability on earnings. To address this concern, in Columns 3–5 of Table 8 we include interactions of NCA Score with college education and high NCA use occupation (separately and then together) to control for this possibility. The coefficients of interest do not qualitatively change. NCA enforceability has differential earnings effects for women and black workers, independent of education and occupation.

In Section 4 we found that changing the nature of implicit contracts was a mechanism through which NCA enforceability reduced earnings. Is this mechanism stronger for the demographic groups facing the largest earnings penalty from NCA enforceability? Though answering this question imposes substantial demands on the relatively small sample in the CPS JTS, we can reestimate the model shown in Column 5 of Table 7, including triple interaction terms that allow the relationship between *Initial NCA Score*, and *Initial UR* and *Minimum UR*, to differ by race and sex.[37] In other words, we are estimating whether the earnings effect of making mobility costly—rendering workers unable to take advantage of strong labor markets to get wage increases—is stronger for non-white-male workers. The results, reported in Table A.8, show that this is indeed the case, at least for black workers: when NCAs are highly enforceable, earnings for black workers (both men and women) are tied much more to labor market conditions at the outset of their job than they are to improvements in the job market over their tenure.

The evidence provided in this section shows that, in addition to affecting average earnings across workers in the US workforce, strict NCA enforceability specifically

[37] We also include double interactions of demographic groups with NCA Score and the two unemployment rates, but do not report their coefficients in the table.

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.

6 Conclusion

In this article, we estimate the impact of NCA enforceability on workers' earnings, and we investigate the mechanism underlying this relationship. Using panel data on state-level NCA enforceability, we show that stricter NCA enforceability leads to a decline in workers' earnings and mobility. Our estimates imply that moving from the 10th to the 90th percentile of the distribution throughout our sample period causes a 3-4% decrease in earnings and a 9% decrease in month over month job mobility. We find that the earnings effect is greater for workers more likely to work under NCAs, and greater for female and black workers. We also find spillover effects on workers who share a labor market with other workers who experience a change in NCA enforceability, but who do not necessarily experience a change themselves. The external effect of changes in NCA enforceability on workers not directly exposed to the change is demonstrated using within-local labor market spillovers to employees in states that did not experience changes. Such workers experience earnings losses associated with increases in NCA enforceability quite close to workers in states that are directly affected. This effect may be due to decreases in labor market churn.

Furthermore, we identify and find evidence of a primary mechanism underlying the relationship between earnings and NCA enforceability: that stricter NCA enforceability undermines workers' ability to negotiate for pay increases when the labor market tightens over the course of their job tenures. This finding suggests that making NCAs enforceable fundamentally changes what has long been considered a key feature of the functioning of labor markets. While a long literature has found evidence that wages are most consistent with a model of implicit contracts between workers and employers and *costless* mobility of workers, we find that in states with stricter enforceability, wages are better explained by a model 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 results 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 not made better off by doing so. However, at the *market* level, our findings imply that freedom to contract harms workers. This relationship might be due to frictions in the labor market that prevent efficient contracts or could reflect externalties from NCA use. From this perspective, the evidence presented in this paper may be viewed as evidence that labor markets do not meet all the assumptions of perfect competition, though it is difficult to identify exactly which assumption is violated.

Our results inform policy centered around NCAs on two major dimensions. First, enforceability of NCAs seriously inhibits growth of labor income. In theory, decreases in employee income may be more than compensated by increases in employer income. Therefore, short of limiting NCA enforceability, policymakers could consider other levers to override this transfer of income when NCAs are enforceable. Second, enforceability of NCAs causes negative externalities that affect workers in nearby states, and (plausibly) workers who have not signed NCAs. Internalization of externalities is one of the most widely accepted roles of government, suggesting that the existence of such external effects is an especially compelling rationale for government intervention in the use and enforceability of NCAs.

A limitation of our study is that we do not observe whether an individual worker has signed an NCA. However, our results inform what is the actual lever at policymakers' disposal: the enforceability of NCAs. Thus, our paper provides insight to state and federal lawmakers that are considering laws that amend, or even outright ban, employers' ability to use NCAs. As more data becomes available that measures NCA use at the worker- or firm-level over time, this will allow further studies into the earnings effects of NCA *use*, as opposed to *enforceability.* We look forward to future work in this domain.

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7 Tables and Figures

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

Region	Northeast	Midwest	South	West	Total
Average Index	0.76	0.79	0.76	0.41	0.69
Standard Deviation of Index	0.10	0.12	0.13	0.34	0.24
Maximum Index	0.97	0.97	1.00	0.91	1.00
Minimum Index	0.63	0.00	0.50	0.08	0.00
Number of Law Changes	11	22	22	15	70
Number of States in Region	9	12	17	13	51
Number of Index Increases	8	15	14	9	46
Number of Index Decreases	3	7	8	6	24
Average Magnitude Positive Index Change	0.05	0.05	0.08	0.06	0.06
Maximum Positive Index Change	0.15	0.10	0.34	0.19	0.34
Average Magnitude Negative Index Change	-0.04	-0.04	-0.06	-0.04	-0.05
Maximum Negative Index Change	-0.04	-0.06	-0.17	-0.10	-0.17
Between-State Standard Deviation	0.09	0.25	0.12	0.21	0.18
Within-State Standard Deviation	0.03	0.02	0.04	0.03	0.03

Notes: Statistics in the table represent data from 1991-2014 for each state-year in which a legal precedent exists. The minimum of the score is 0 and the maximum is normalized to 1.

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

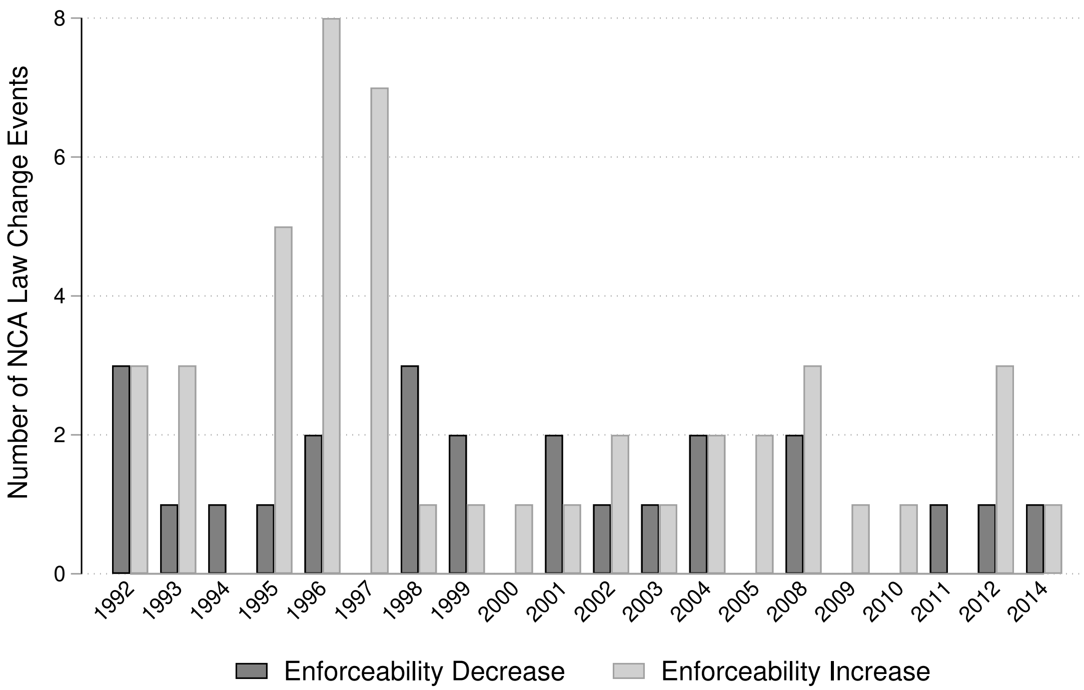


Figure 2: Average NCA Enforceability Score from 1991 to 2014

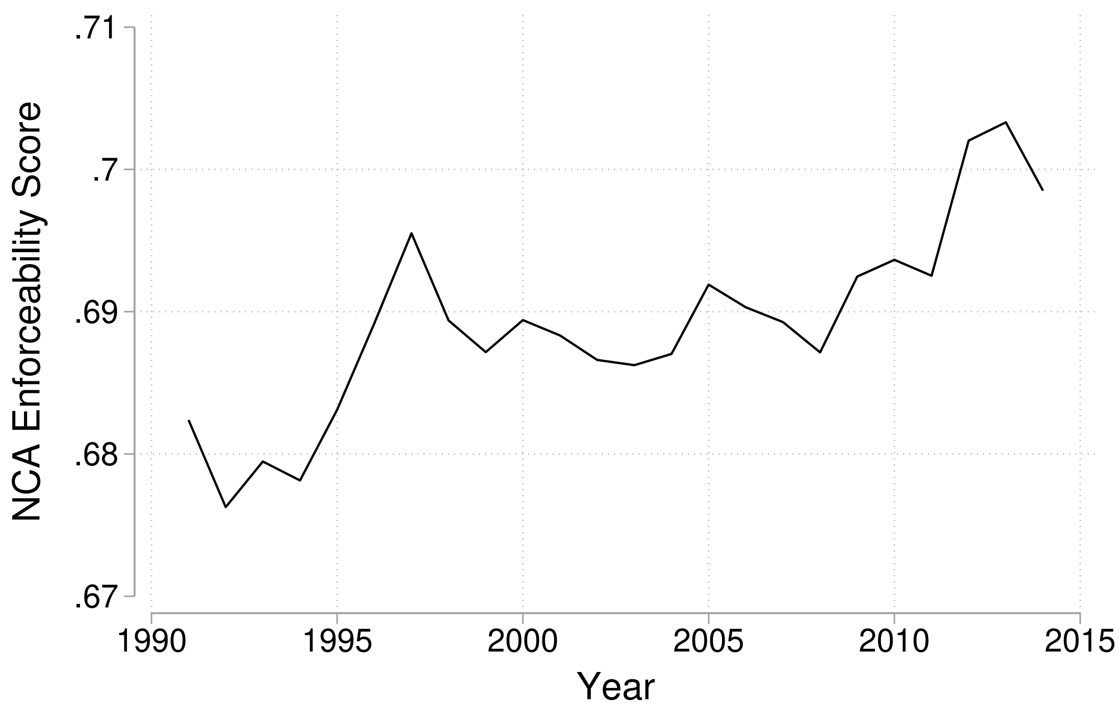
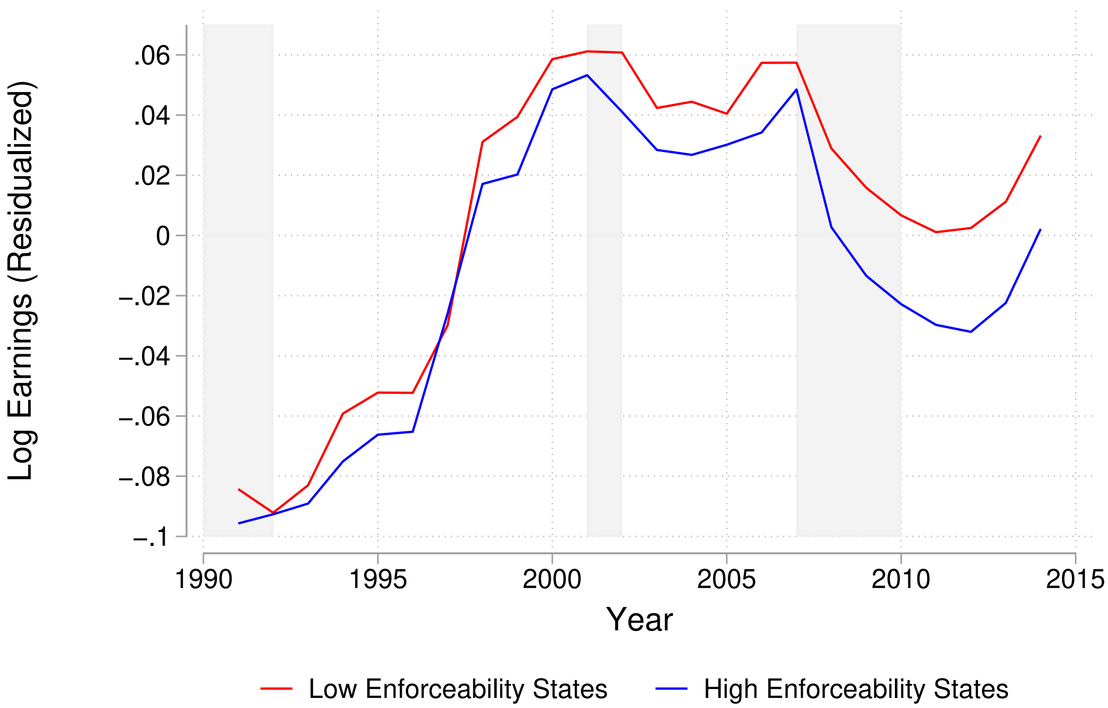


Table 2: Can Economic and Political Factors Explain Changes in NCA Enforceability?

Dependent Variable:	NCA Enforceability	
Population (100,000s)	–0.00	(0.00)
Number of Workers Compensation Beneficiaries	–0.00	(0.00)
Democratic Party Governor	–0.00	(0.00)
% of State House from Democratic Party	0.02	(0.05)
% of State Senate from Democratic Party	0.02	(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	(0.01)
Economic Policy Liberalism Score	–0.02	(0.01)
Social Mass Liberalism Score	–0.01	(0.01)
Economic Mass Liberalism Score	0.03	(0.03)
Democratic Party ID Count	0.04	(0.25)
State House Ideology Score	–0.00	(0.01)
State Senate Ideology Score	0.00	(0.00)
House Democrats Ideology Score	–0.03	(0.03)
House Republicans Ideology Score	0.06	(0.04)
Senate Democrats Ideology Score	–0.03*	(0.02)
Senate Republicans Ideology Score	–0.00	(0.01)
Union Membership	–0.00	(0.00)
N	837	
R^2	0.109	
F-Test p-Value	0.106	

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. *** $p< 0.01$, ** $p< 0.05$, * $p< 0.1$

Figure 3: Mincerized Log Earnings Residuals by NCA Enforceability



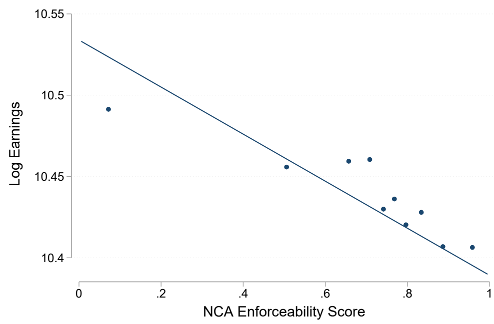
Each series plots the residuals from a regression of log earnings on sex, race, Hispanic status, age, age squared, marital status, metropolitan area status, as well as Census division, occupation, and industry fixed effects, estimated on individuals in the CPS ASEC who are between 18 and 64 working in the private for-profit sector. Low enforceability states have a 2009 NCA score that is less than or equal to the median, and high enforceability states have a 2009 NCA score greater than the median.

Table 3: The Effect of NCA Enforceability on Earnings

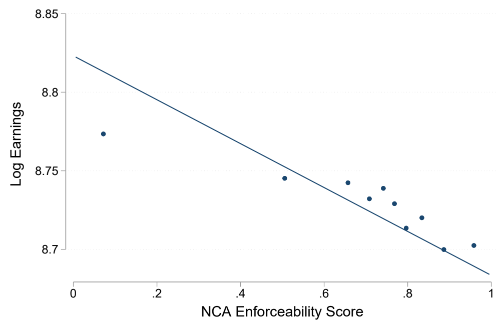
	Log Earnings (1)	(2)	Log Hours (3)	Log Wage (4)	Log Average Earnings (5)	Changed Jobs (6)
NCA Enforceability Score	-0.096***	-0.091***	-0.015	-0.093***	-0.118**	-0.006*
	(0.030)	(0.022)	(0.019)	(0.023)	(0.045)	(0.004)
Observations	1216726	1216726	1545874	1216726	3562313	2360904
R^2	0.275	0.357	0.132	0.346	0.891	0.005
Geographic FE	State	State	State	State	County	State
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter	Div x Month
Occupation FE	N	Y	Y	Y	N	Y
Sample	ASEC	ASEC	ASEC	ASEC	QWI	CPS

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).
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

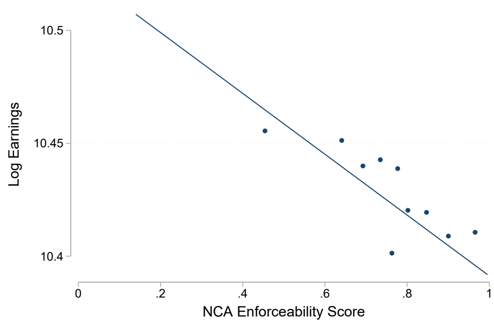
Figure 4: The Relationship between NCA Enforceability and Earnings: Binned Scatterplots



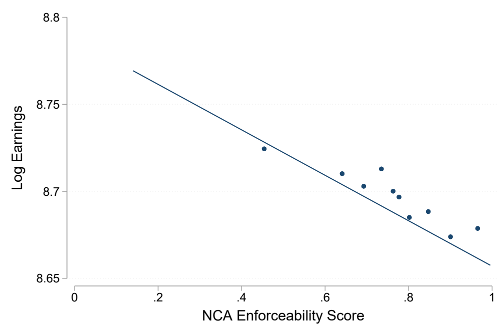
(a) All state-years with no controls.



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



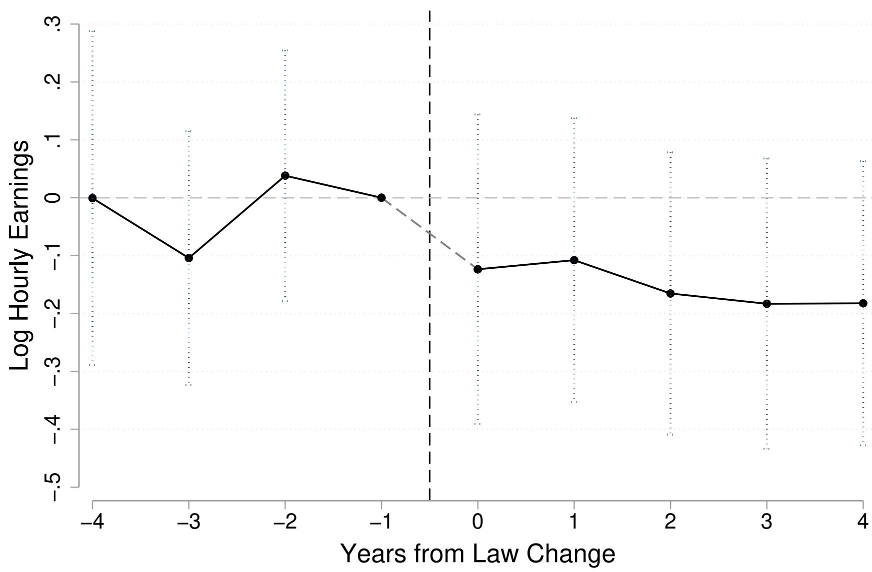
(c) Drop 2 low outlier states (ND and CA) with no controls.



(d) Drop 2 low outlier states (ND and CA) 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 panels (c) and (d), states with an enforceability score of less than 0.1 are dropped. In all graphs, both variables are residualized on state and Census division by year fixed effects, and are further residualized by broad occupation class fixed effects, age and age-squared, and indicators for white, Hispanic, male, not having completed college, and metro area status in panels (b) and (d).

Figure 5: Event Study Estimates of the Effect of NCA Enforceability Changes on Log Hourly Earnings



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. Estimating equations include 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).

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

	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.096***	-0.012	-0.065**	-0.073**	-0.007
	(0.030)	(0.034)	(0.030)	(0.031)	(0.034)
College Educated Worker	0.415***	0.512***	0.376***	0.391***	0.444***
	(0.013)	(0.022)	(0.012)	(0.010)	(0.016)
College Educated Worker × NCA Score		-0.140***			-0.120***
		(0.033)			(0.023)
High NCA Use Occ			0.255***		0.194***
			(0.008)		(0.005)
High NCA Use Occ × NCA Score			-0.059***		-0.015*
			(0.015)		(0.008)
High NCA Use Ind				0.268***	0.219***
				(0.009)	(0.007)
High NCA Use Ind × NCA Score				-0.066***	-0.035***
				(0.014)	(0.010)
Observations	1216726	1216726	1216726	1216726	1216726
R^2	0.275	0.275	0.290	0.292	0.304

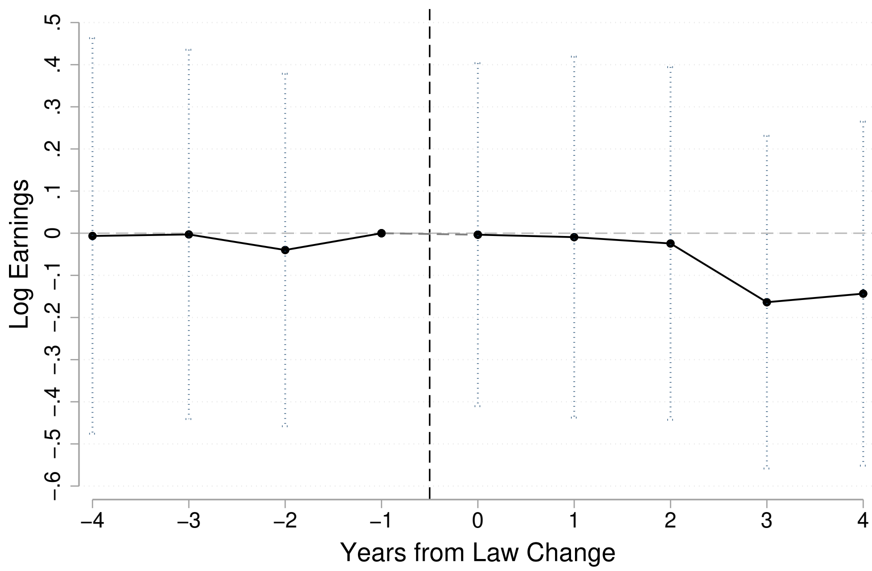
The sample in all columns is the CPS ASEC from 1991-2014 and includes individuals between ages 18-64 who reported working for wage and salary income at a private employer the prior year. All regressions include fixed effects for state, fixed effects for Census region by year, fixed effects for broad 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. (2018).
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table 5: The External Effects of NCA Enforceability on Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
Own State NCA Score	-0.129**	-0.154**	-0.132**	-0.119**	-0.139**	-0.118*
	(0.055)	(0.062)	(0.063)	(0.054)	(0.062)	(0.061)
Donor State NCA Score		-0.141**	-0.166**		-0.111+	-0.130+
		(0.070)	(0.075)		(0.075)	(0.079)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.111			-0.112
			(0.148)			(0.157)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.149***			0.132**
			(0.055)			(0.052)
Observations	615097	615097	613679	840565	840565	838701
R^2	0.898	0.898	0.901	0.899	0.899	0.901
Border County	Y	Y	Y	N	N	N

The dependent variable is log earnings. The sample is the QWI from 1991-2014 restricted to counties 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. "Border County" restricts to the sample of counties that are physically on state borders. 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 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, +P<.15

Figure 6: Event Study Estimates of the Effect of Bordering State NCA Enforceability Changes on Log Earnings



The sample includes four year windows around NCA law change events in the border county sample, as well as control counties in the same Census division with no corresponding event in a bordering county in the four year window. Estimating equations include controls for sex and age group, 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).

Table 6: The External Effects of NCA Enforceability on Mobility: Hires and Separations

Log Accessions	(1)	(2)	(3)	(4)	(5)	(6)
Own State NCA Score	-0.157*	-0.179*	-0.108	-0.122+	-0.140	-0.050
	(0.087)	(0.102)	(0.125)	(0.081)	(0.096)	(0.113)
Donor State NCA Score		-0.124	-0.192		-0.101	-0.154
		(0.153)	(0.175)		(0.152)	(0.171)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.426			-0.578
			(0.547)			(0.585)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.393**			0.384**
			(0.150)			(0.161)
Observations	604322	604322	603466	826749	826749	825599
R^2	0.951	0.951	0.951	0.953	0.953	0.954
Border County	Y	Y	Y	N	N	N
Log Separations	(1)	(2)	(3)	(4)	(5)	(6)
Own State NCA Score	-0.155+	-0.179+	-0.096	-0.112	-0.131	-0.026
	(0.101)	(0.114)	(0.143)	(0.092)	(0.105)	(0.132)
Donor State NCA Score		-0.135	-0.197		-0.109	-0.156
		(0.152)	(0.174)		(0.151)	(0.170)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.500			-0.672
			(0.581)			(0.635)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.386**			0.384**
			(0.151)			(0.167)
Observations	604512	604512	603659	827050	827050	825925
R^2	0.950	0.950	0.950	0.952	0.952	0.953
Border County	Y	Y	Y	N	N	N

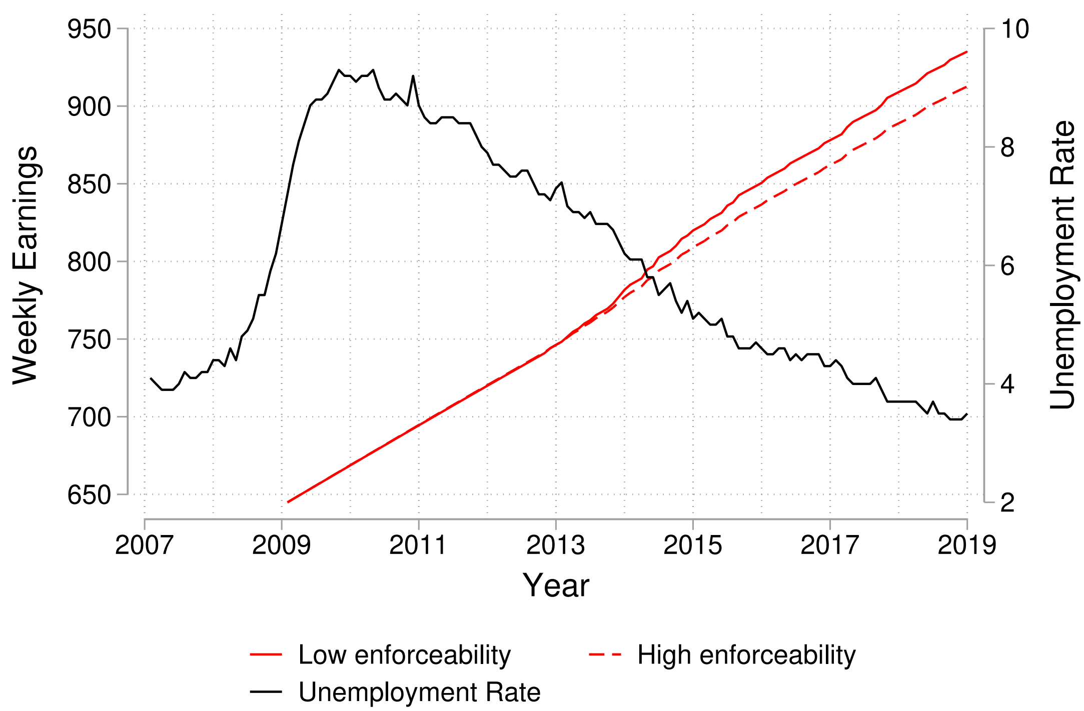
The sample is the QWI from 1991-2014 and includes individuals between ages 19-64. All regressions include controls for male, age group, as well as division by year by quarter and county fixed effects. "Border County" indicates counties that are physically on state borders, as opposed to simply being part of a commuting zone that straddles a state border.
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, +P<.15

Table 7: 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***	-0.029***
		(0.003)	(0.005)	(0.005)	(0.006)
Init. NCA Score × Init. UR					-0.017***
					(0.006)
Init. NCA Score × Min. UR					0.022**
					(0.009)
Initial NCA Score				0.002	-0.029
				(0.071)	(0.085)
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, and female.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Figure 7: Divergence in Weekly Earnings in States with High versus Low NCA Enforceability



The figure depicts simulated earnings for workers who began their jobs in 2009 and held them through 2019. As the labor market improves, workers in low enforceability states receive wage increases according to the tightness of the labor market, while workers in high enforceability states are unable to do so due to the increased costs of mobility imposed by NCAs.

Table 8: Heterogeneous Effects of NCA Enforceability on Earnings by Race and Sex

	(1)	(2)	(3)	(4)	(5)
NCA Score	-0.107**	-0.063	-0.003	-0.047	-0.002
	(0.044)	(0.043)	(0.048)	(0.044)	(0.048)
Female & Non-black	-0.471***	-0.420***	-0.427***	-0.421***	-0.428***
	(0.011)	(0.025)	(0.024)	(0.025)	(0.024)
Female & Black	-0.572***	-0.520***	-0.526***	-0.514***	-0.520***
	(0.011)	(0.027)	(0.025)	(0.030)	(0.028)
Male & Black	-0.339***	-0.281***	-0.283***	-0.272***	-0.275***
	(0.008)	(0.015)	(0.016)	(0.014)	(0.014)
Female & Non-black × NCA Score		-0.075*	-0.065	-0.067*	-0.058
		(0.040)	(0.039)	(0.040)	(0.039)
Female & Black × NCA Score		-0.074*	-0.065*	-0.082*	-0.074*
		(0.039)	(0.035)	(0.044)	(0.041)
Male & Black × NCA Score		-0.082***	-0.079***	-0.060***	-0.057***
		(0.015)	(0.015)	(0.014)	(0.014)
College Educated Worker × NCA Score			-0.111***		-0.105***
			(0.027)		(0.023)
High NCA Use Occ × NCA Score				-0.036***	-0.015
				(0.013)	(0.012)
Observations	1537454	1537454	1537454	1537454	1537454
R^2	0.275	0.275	0.275	0.289	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. (2018). 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

A Appendix Figures & Tables

Table A.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 A.2: Description of Legal Changes from Jeffers (2018), 2010-2014

State	First Year of Updated Score	Score Change	Justification
Wisconsin	2010	Increase Question 4 value to 3	Established enforceability with divisible contract terms
South Carolina	2011	Decrease Question 4 value to 0	Judicial modification of NCAs disallowed
Georgia	2011	Increase Question 1 value to 8, Question 2 value to 10, and Question 4 value to 8	Statute enacted expanding protectable interest and allowing modification
Colorado	2012	Increase Question 3b & c value to 10	Continued employment deemed sufficient consideration
Montana	2012	Change Question 8 value to 1 (previously undecided)	NCAs deemed unenforceable if employer terminates employment relationship
Illinois	2012	Increase Question 2 value to 8	Expanded scope of protectable interests
Texas	2012	No change made	"Goodwill" already protectable and ancillary agreement test already standard under Covenants Not to Compete Act
Illinois	2014	Decrease Question 3a value to 5	Consideration necessary for enforceable NCA increased
Virginia	2014	Increase Question 3 value to 6	Plaintiff allowed to present evidence of reasonableness, reducing dismissals of challenges

See Table A.1 for a detailed description of the questions referenced above. Though no change was made in our data, the entry on Texas is included in this table due to its inclusion in Jeffers (2018).

Table A.3: The Effect of NCA Enforceability on Earnings, by Component of NCA Score

Q1: State Statute	-0.023	(0.020)
Q2: Protectable Interest	-0.043*	(0.023)
Q3: Plaintiff Burden of Proof	0.042*	(0.025)
Q3a: Consideration, Start of Employment	-0.055***	(0.013)
Q3b/c: Consideration, Continued Employment	-0.028**	(0.013)
Q4: Judicial Modification	-0.022**	(0.010)
Q8: Enforceable if Employer Terminates	-0.016	(0.054)
Full Score, Excluding Existence of Statute	-0.108***	(0.031)

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

	Log Earnings		Log Hours	Log Wage	Log Average Earnings	Changed Jobs
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Score	-0.108***	-0.104***	-0.031	-0.107***	-0.139***	-0.006
	(0.034)	(0.026)	(0.019)	(0.025)	(0.048)	(0.005)
Observations	989854	989854	1262128	989854	2706622	1929111
R^2	0.272	0.356	0.130	0.345	0.891	0.005
Geographic FE	State	State	State	State	County	State
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter	Div x Month
Occupation FE	N	Y	Y	Y	N	Y
Sample	ASEC	ASEC	ASEC	ASEC	QWI	CPS

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

	Log Earnings (1)	Log Earnings (2)	Log Hours (3)	Log Wage (4)	Log Average Earnings (5)	Changed Jobs (6)
NCA Enforceability Score	-0.131	-0.130*	-0.057***	-0.127*	-0.197*	-0.022*
	(0.085)	(0.067)	(0.016)	(0.069)	(0.098)	(0.011)
Observations	699036	699036	890737	699036	1537282	1328960
R^2	0.272	0.359	0.128	0.348	0.896	0.006
Geographic FE	State	State	State	State	County	State
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter	Div x Month
Occupation FE	N	Y	Y	Y	N	Y
Sample	ASEC	ASEC	ASEC	ASEC	QWI	CPS

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 A.6: The Effect of NCA Enforceability on Earnings: Robustness to Political & Economic Controls

	Log Earnings (1)	Log Earnings (2)	Log Hours (3)	Log Wage (4)	Log Average Earnings (5)	Changed Jobs (6)
NCA Enforceability Score	-0.065**	-0.069***	-0.015	-0.071***	-0.113**	-0.007
	(0.026)	(0.019)	(0.019)	(0.019)	(0.046)	(0.005)
Observations	1139890	1139890	1448431	1139890	3442359	2289184
R^2	0.274	0.357	0.132	0.346	0.893	0.005
Geographic FE	State	State	State	State	County	State
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter	Div x Month
Occupation FE	N	Y	Y	Y	N	Y
Sample	ASEC	ASEC	ASEC	ASEC	QWI	CPS

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 A.7: The External Effects of NCA Enforceability on Earnings on Counties over 100 Miles from State Borders

	(1)	(2)
Own State NCA Score	-0.254+	-0.272
	(0.162)	(0.197)
Donor State NCA Score		-0.022
		(0.088)
Observations	579159	544608
R^2	0.873	0.877

The dependent variable is log earnings. The sample is the QWI from 1991-2014 and includes individuals between ages 19-64, restricted to counties over 100 miles from a state border. 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. Standard errors are clustered by own state in column (1) and two-way clustered by own state and commuting zone in column (2). ***P<.01, **P<.05, *P<.1, +P<.15

Table A.8: Heterogeneity in the Role of NCA Enforceability in the Implicit Contract Mechanism

	Log Earnings (1)
Initial UR	0.007 (0.012)
Minimum UR	-0.036*** (0.008)
Init. NCA Score × Init. UR	-0.009 (0.017)
Init. NCA Score × Min. UR	0.005 (0.012)
Non-black Female × Init. NCA Score × Init. UR	-0.000 (0.024)
Non-black Female × Init. NCA Score × Min. UR	-0.001 (0.018)
Black Female × Init. NCA Score × Init. UR	-0.085** (0.033)
Black Female × Init. NCA Score × Min. UR	0.072 (0.048)
Black Male × Init. NCA Score × Init. UR	-0.068** (0.029)
Black Male × Init. NCA Score × Min. UR	0.115*** (0.028)
No. Obs.	92724
R^2	0.406

All regressions include state, Census division by year, and industry fixed effects, controls for quadratics in age and tenure, and indicators for college education, black, hispanic, married, union member, and female, as well as the unreported interaction terms between demographic groups, initial NCA score, and initial and minimum unemployment rates.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1