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THE LABOR MARKET EFFECTS OF LEGAL RESTRICTIONS ON WORKER MOBILITY

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ABSTRACT

We analyze how the legal enforceability of noncompete agreements (NCAs) affects labor markets. Using newly-constructed panel data, we find that higher NCA enforceability diminishes workers' earnings and job mobility, with larger effects among workers most likely to sign NCAs. These effects are far-reaching: changes in enforceability impose externalities on workers across state borders, suggesting that enforceability broadly affects labor market dynamism. We provide evidence that NCA enforceability primarily affects wages through its effect on workers' outside options; moreover, workers facing high enforceability are unable to leverage tight labor markets to increase earnings. We motivate these findings by embedding NCA enforceability in a search model with bargaining. Finally, higher NCA enforceability exacerbates gender and racial earnings gaps.

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1 Introduction

By several metrics, the U.S. labor market failed to produce economic gains for the majority of workers in the four decades prior to 2020. Average real hourly earnings changed little[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 (Farber et al., 2018), 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 attention is firms' use of postemployment restrictions, the most salient of which are noncompete agreements (NCAs). NCAs contractually limit a worker's ability to enter into a professional position in competition with his or her employer in the event of a job separation. NCAs are common: Starr et al. (2021) find that approximately 18% of workers in 2014 were bound by NCAs, whereas Colvin and Shierholz (2019) found this range to be between 28 and 47 percent in 2019.[3] The legal *enforceability* of NCAs—that is, the terms under which an employer can enforce one—is determined by state employment law. Making NCAs easier to enforce may hinder earnings growth by limiting workers' ability to seek higher-paying jobs or to negotiate higher earnings at their current job. At the same time, others contend that enforceable NCAs can *increase* earnings by making firms more willing to invest in training, knowledge creation, and other portable assets that raise workers' productivity (Rubin and Shedd, 1981; Starr, 2019).

Though the enforceability of NCAs has received increasing scrutiny from policymakers at state and national levels,[4] there remains an incomplete understanding of the labor market effects of NCAs, primarily due to three factors. The first is a lack of comprehensive panel data on NCA enforceability. Researchers have, to date, relied largely on either cross-sectional measures of states' enforceability or case studies of

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

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

[3] Specifically, 15% of the workers in Starr et al. (2021)'s representative sample reported being bound by NCAs, and another 29.7% were unsure if they were bound by NCAs. Starr et al. (2021) report a level of 18.1% based on a multiple imputation methodology. The range reported by Colvin and Shierholz (2019) represents an imputation based on a survey of business establishments and a broad range of assumptions on the percentage of workers within those establishments bound by NCAs.

[4] The Workforce Mobility Act of 2018 (US Senate Bill 2782, introduced by Chris Murphy) states "No employer shall enter into, enforce, or threaten to enforce a covenant not to compete with any employee of such employer" (`https://www.congress.gov/bill/115th-congress/senate-bill/2782/text?r=6.`). The Freedom to Compete Act of 2019 (US Senate Bill 124, introduced by Marco Rubio) has similar language (`https://www.congress.gov/bill/116th-congress/senate-bill/124/all-info`). In January 2023, the Federal Trade Commission issued a Notice of Proposed Rulemaking which would prohibit NCAs, with limited exceptions, across the economy.

a single state or a handful of states with law changes affecting specific segments of the workforce. This approach has drawbacks: cross-sectional variation in enforceability might be correlated with other unobserved differences across states, and small samples of targeted law changes may not generalize to the population. Second, prior work, which we describe below, has found seemingly conflicting evidence regarding the earnings effect of NCA *use* versus *enforceability*, creating challenges for interpreting the effects of NCAs on worker outcomes. Finally, the literature has not yet thoroughly identified the mechanisms through which enforceable NCAs affect labor markets. Without a clear understanding of *why* NCA enforceability affects workers, it is difficult to generalize empirical evidence to, for example, predict how various proposals to change enforceability might affect the functioning of labor markets.

We present comprehensive evidence on the effect of NCA enforceability on workers' earnings and job mobility. We begin by constructing a new panel dataset to use within-state changes in NCA laws to identify the overall labor market effects of NCA enforceability, including spillover effects within local labor markets. We then provide evidence for a key mechanism through which NCA enforceability affects earnings—namely, its effect on workers' outside options and costs of job mobility. Finally, we show that the earnings effect of NCA enforceability exhibits economically meaningful heterogeneity across demographic groups, contributing a new insight into the determinants of earnings inequality in the United States.

We guide our empirical analysis with a model, based on the search model of Bagger et al. (2014), of how changes in NCA enforceability affect workers' earnings. We show that the effect of increasing NCA enforceability on overall earnings can be decomposed into two terms. The first term relates to the difference in earnings between workers who are and are not bound by enforceable NCAs; the sign of this term is ambiguous due to the offsetting ways that an enforceable NCA raises a worker's earnings (via faster human capital accumulation) and lowers it (via reduced job mobility). The second term reflects the spillover effect of stricter enforceability on the earnings of workers not bound by NCAs. We show that this term is unambiguously negative under the assumption that strict NCA enforceability reduces the job offer arrival rate for all workers. We provide empirical evidence to support this assumption.

To identify the causal effects of NCA enforceability, we created a new dataset with annual measures of NCA enforceability for each of the 50 US states and the District of Columbia from 1991 to 2014. This dataset includes both judicial and legislative decisions that change state-level NCA enforceability, coded to match the criteria developed by leading legal scholars to quantify enforceability. The vast majority of these law changes (90.4%) occur due to judicial decisions via court rulings. An important component of the judicial process is *stare decisis*, or the doctrine of precedent. A consequence is that judges are more constrained than legislators in allowing economic or political trends to affect decisions, a fact that is useful for our research design. We combine our enforceability dataset with earnings and mobility outcomes from a range of datasets including the Current Population Survey, Job to Job Flows, and the Quarterly Workforce Indicators, all from the US Census Bureau, as well as the Job Openings and Labor Turnover Survey from the Bureau of Labor Statistics.

We find that increases in NCA enforceability decrease workers' earnings and mobility. Moving from the 25th to the 75th percentile in enforceability is associated with an approximately 2% decrease in the average worker's earnings. The earnings effects are almost entirely driven by declines in implied hourly wages. The effect is even stronger among occupations, industries, and demographic groups in which NCAs are used more frequently (according to Starr et al. (2021)). We also find that NCA enforceability reduces worker mobility, particularly among groups where NCAs are used more frequently. An out-of-sample extrapolation implies that rendering NCAs unenforceable nationwide would increase average earnings among *all* workers by 3.2% to 14.2%. The midpoint of this interval (8.7%) is roughly equal to the estimated effects of very large increases in employer consolidation on affected workers' earnings (Prager and Schmitt, 2019); it is also approximately equal to the estimated earnings premium that accrues to workers who enter occupations with government-mandated licensing, and roughly half the size of the earnings premium associated with membership in a labor union.

To interpret this overall negative earnings effect, we then conduct an empirical test to isolate the spillover effects of NCA enforceability on workers who are not themselves bound by NCAs. We show that these spillovers are negative—as predicted by our model—and are economically meaningful. Focusing on local labor markets that are divided by a state border, we show that a change in NCA enforceability in one state indirectly affects the earnings and mobility of workers located in an adjoining state. This finding suggests that the treatment effects of NCA enforceability impact a larger population than the relatively small share of workers bound by NCAs, and the magnitudes suggest that spillovers account for a meaningful share of the overall earnings effects of enforceability.

We then conduct two empirical tests of our proposed mechanism that strict NCA enforceability reduces earnings through its effect on workers' job offer arrival rates. First, we test for heterogeneity in the earnings effect using two separate proxies for the extent to which changes in state-level NCA enforceability affect workers' outside options. Strict NCA enforceability has an especially negative earnings effect in industries in which workers are less likely to move jobs across state lines (as measured in the Job-to-Job flows dataset), and in occupations in which workers have lower cross-occupational mobility (as measured by Schubert et al. (2021)). That is, strict NCA enforceability reduces earnings the most when it has the largest impact on workers' outside options.

The second test of our proposed mechanism revisits prior research that considers how tight labor markets enable workers to increase their earnings. We embed NCA enforceability in an empirical model, first used by Beaudry and DiNardo (1991), that considers how a worker's current earnings depend on prior labor market conditions. Previous research has found that workers' current earnings are strongly correlated with the most favorable labor market conditions over their current job spell. This relationship is consistent with the extra job offers workers might receive in tight labor markets enabling them to either negotiate a higher wage with their current employer (Beaudry and DiNardo, 1991) or find a job with higher match quality (Hagedorn and Manovskii, 2013). We find that this relationship continues to hold but only in states where NCAs are relatively unenforceable. In contrast, strict NCA enforceability ties workers' earnings to labor market conditions at the start of their job spell, rather than to the most favorable conditions they have experienced since then. This finding implies that strict NCA enforceability erodes workers' ability to leverage tight labor markets to achieve higher earnings, consistent with the hypothesis that NCAs "undermine workers' prospects for moving up the income ladder" (Krueger, 2017).

Finally, we document economically meaningful heterogeneity in the earnings effect of NCA enforceability across demographic groups. Given gender differences in willingness to commute (Le Barbanchon et al., 2019), geographically-restrictive NCAs (or state-level enforceability changes) may have larger effects on womens' outside options than on mens'. Prior work also suggests women tend to be less willing to violate the terms of their NCA than are men (Marx, 2022). Similar evidence suggests that state-level NCA enforceability changes may disproportionately affect the outside options of Black workers, due to racial differences in the propensity to move in response to economic opportunities (Sprung-Keyser et al., 2022). 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. Using a standard earnings decomposition, our estimates imply that the 75-25 differential in NCA enforceability accounts for 1.5-3.8% of the earnings gaps between white men and other demographic groups.

Relationship to the Literature: Our findings most directly contribute to a growing literature on the earnings effects of NCA enforceability. Prior work examining case studies of individual bans on NCAs—including an Oregon ban on NCAs for hourly workers (Lipsitz and Starr, 2021) and a Hawaii ban on NCAs for tech workers (Balasubramanian et al., 2022)—has found that these bans led to higher earnings.[5] Two papers have studied what happens to executives' earnings when NCAs are easier to enforce, with mixed results: Garmaise (2011) uses three NCA law changes and finds that earnings decrease, while Kini et al. (2019) uses a broader set of law changes and interprets their findings as implying that earnings increase. Studies using cross-sectional variation in NCA enforceability have similarly reached mixed results: Starr (2019) finds that earnings are lower in states with higher NCA enforceability, whereas (Lavetti et al., 2018) finds the opposite relationship for doctors.

[5] An exception is Young (2021), who finds that a ban on NCAs in Austria for low-wage workers had limited effects on earnings.

We make several contributions to this literature. Our paper is the first to provide comprehensive panel-based evidence of the earnings effects of enforceability changes for all states and all labor market sectors, using what legal scholars believe is the most accurate measure of NCA enforceability to date (Barnett and Sichelman, 2020). Second, we provide the first panel-based evidence that NCA enforceability has spillover effects onto workers unaffected by legal changes, and that these spillovers account for a meaningful share of the overall earnings effects of NCA enforceability.[6] Finally, we connect our empirical analyses to a job ladder model of the labor market, which provides testable mechanisms through which NCA enforceability affects earnings—namely, by reducing workers' offer arrival rates. The connection to the model aids the interpretation of our empirical findings and provides insight into the types of workers whose earnings would be most affected by proposed policy discussions to make NCAs more or less easily enforceable. We further elaborate on these contributions in Section 8.

We also complement the vibrant literature that considers other economic effects of NCA enforceability, including on entrepreneurship and investment (Jeffers, 2018), employee spinoffs (Starr et al., 2018; Marx, 2022), startup performance (Ewens and Marx, 2018), worker mobility (Marx et al., 2009), and innovation (Johnson et al., 2023).

Our findings also contribute to broader and growing work on employer monopsony power and workers' outside options. Recent work has examined sources of monopsony power, including the role of search frictions (Manning, 2013; Berger et al., 2023; Jarosch et al., 2019), and local employer concentration (Azar et al. (2017), Benmelech et al. (2022), Prager and Schmitt (2019), Berger et al. (2022)). Our results imply that strict NCA enforceability effectively endows employers with a degree of monopsony power, by affecting workers' outside options, even in the absence of explicit changes in employer concentration, which we interpret through a lens of search frictions. In this spirit, our theoretical assumption (and empirical finding) that enforceable NCAs reduce earnings by reducing the value of workers' outside options complements other work showing the importance of outside options on earnings (Caldwell and Danieli, 2018; Schubert et al., 2021). One benefit of our study is that changes in NCA enforceability isolate changes in labor market competition, whereas other factors that might affect labor market power (such as mergers) also directly affect product market competition, though NCAs may have ramifications in product markets as well (Lipsitz and Tremblay, 2021; Johnson et al., 2023).

Finally, our findings provide new insight into a longstanding debate in law and economics regarding freedom of contracting (see, e.g., Bernstein (2008) for an overview).

[6] Starr et al. (2019) also test for spillovers from enforceable NCAs. Our findings complement theirs by 1) focusing on enforceability (rather than on *use* of enforceable NCAs), 2) using within-state variation in enforceability (rather than cross-sectional variation across states), and 3) using a border county design to isolate spillovers from other potential omitted variables that may jointly affect wages and enforceability.

Appealing to the Coase theorem, advocates of the freedom to contract suggest that freely-bargained-for NCAs must increase match surplus, which may be split between workers and employers. Evidence that NCAs are not freely bargained-for (e.g., because employers present them after the beginning of the employment relationship (Marx, 2011), or because workers are unaware of their existence Starr et al. (2021)), already reveals one shortcoming of this argument. Our paper reveals another: enforceable NCAs impose substantial negative externalities on other workers.

2 Conceptual Framework

In this section, we provide a concise overview of NCAs and the role of legal enforceability, and then present a brief conceptual framework (based on a model which is fully described in Appendix A) to guide our empirical analysis.

An NCA prevents a worker from moving to a job at a competing firm. The exact terms of an NCA are contract-specific, and they typically depend on the nature of competition. For example, in a nontradeable industry in which client lists are important for production, an NCA might dictate that the worker cannot move to another job in the same industry and within a specified geographic radius (e.g. within 25 miles, or within the same state). In an industry in which trade secrets are essential for firms to retain a competitive edge, the NCA might dictate that the worker cannot depart for another employer in the same industry anywhere in the country. More generally, the dimensions of employment mobility that an NCA might restrict could be some combination of geographic, temporal, occupational, or industrial.

While in theory any employment contract could include an NCA, the likelihood that an NCA would be upheld in court depends on the conditions under which a court would rule an NCA to be enforceable—that is, the legal enforceability.

Our focus in this paper is on the effects of NCA *enforceability*, as opposed to NCA *use*. One reason for this focus is data limitations: to our knowledge, there do not exist long panel data for a representative sample of workers on the use of NCAs in the US. A more fundamental reason is that restricting attention to *use* would miss at least two important ways that the legal enforceability of NCAs might affect the labor market.

First, changes in the enforceability of NCAs likely impact both the incidence of NCA use (the extensive margin) and the bindingness of NCAs already signed (the intensive margin). On the extensive margin, cross-sectional studies have found that states with higher NCA enforceability have a larger share of physicians (Lavetti et al., 2018), CEOs (Kini et al., 2019), managers (Shi, 2023), and hair stylists (Johnson and Lipsitz, 2019) that sign NCAs.[7] On the intensive margin, a change in enforceability could alter the effect of an NCA for workers who have already signed one. Though

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

NCAs are used in states in which they are unenforceable (Lavetti et al., 2018; Starr et al., 2021), employers are in a better position to leverage a worker's NCA when enforceability is easier.[8] Higher NCA enforceability could also lead employers already using NCAs to write broader and more restrictive NCAs.

Second, as we will discuss, changes in NCA enforceability could have spillover effects on earnings beyond the set of workers that sign NCAs.

To provide a theoretical foundation for how NCA enforceability affects earnings, we extend a job search model of the labor market developed in Bagger et al. (2014) by allowing workers to have NCAs, and by varying levels of NCA enforceability. Briefly, Bagger et al. (2014) is a job ladder model in which workers match with firms of varying productivities, and they subsequently have the opportunity to take higher-paying jobs or leverage outside offers for pay increases. Worker pay also depends on human capital accumulation. The Bagger et al. (2014) model provides a natural foundation for our purpose, as its focus on the role of human capital accumulation versus job mobility highlights two competing channels through which enforceable NCAs could affect earnings.[9]

We briefly summarize here the insights from the model that guide our empirical analysis. We formally present the extended model in Appendix A.

Let $\bar{w}$ denote average earnings, θ denote NCA enforceability, and γ denote the fraction of workers bound by NCAs. As we derive in Appendix A, the effect of a change in NCA enforceability on average earnings is the sum of two terms:

$$\frac{d\bar{w}}{d\theta} = \gamma(\bar{w}^C - \bar{w}^F) + (1 - \theta\gamma)\frac{d\bar{w}^F}{d\theta} \tag{1}$$

Here, $\bar{w}^C$ and $\bar{w}^F$ denote the average earnings of the subset of constrained workers bound by an NCA and unconstrained workers not bound by one, respectively.

The first term reflects the difference in average earnings between workers bound and not bound by NCAs, scaled by the proportion of workers bound by NCAs. The sign of this difference is indeterminate. On the one hand, workers with NCAs might

[8] This argument holds even if a worker is not fully informed about the enforceability of the NCA she has signed. As long as employers *are* informed, and there is some probability that workers can learn, then employers will know the NCA has less bite in expectation when it is not legally enforceable. Put another away, a worker may get a signal of the NCA enforceability regime when she informs her employer of an outside offer she has received: for example, if enforceability is weak, the employer is unlikely to contend it, whereas if enforceability is strict the employer might saliently inform the worker of the legal environment.

[9] We use the term "human capital accumulation" to reflect a range of investments that firms could make in workers. This could include general human capital training (Rubin and Shedd, 1981), but also the sharing of trade secrets or client lists. All of these investments raise a worker's productivity, but they come with different (from the firm's perspective) costs. For example, general training is costly at the time of investment, whereas sharing a client list is only costly in expectation (if a worker takes the list to a competitor). Of course, some forms of investment in workers will be unaffected by NCA enforceability, such as training a worker needs to perform her job. Our focus is on investment in "portable" assets a worker can take with them in the event of a job separation.

experience faster human capital accumulation or require a compensating earnings differential for lost future mobility, both of which could make this term positive. On the other hand, workers with NCAs are unable to climb the job ladder to higher-productivity firms or to leverage outside offers for pay increases, both of which make this term negative. This indeterminacy ultimately makes the effect of NCA enforceability on earnings an empirical question. We provide this empirical evidence in Section 4.

The second term reflects the effect of increased NCA enforceability on the earnings of unconstrained workers not bound by NCAs, scaled by the proportion of workers not bound by enforceable NCAs. We show that this term is strictly negative. This negative spillover effect arises because of a key assumption that we make: higher NCA enforceability reduces the arrival rate of new job offers for all workers.[10] A slower offer arrival rate dampens a key element of earnings growth, namely workers' ability to climb the job ladder and leverage outside offers with their current employer.[11] We provide evidence for the validity of this assumption and estimate the spillover effects of NCA enforceability in Section 5.

While the overall earnings effect of enforceability is indeterminate, the mechanism that drags down earnings, for constrained and free workers alike, is the slowed arrival rate of job offers. We generate two testable predictions to assess the explanatory power of this mechanism. First, the earnings effect of enforceability will be more negative for workers whose outside options enforceability affects the most. This relationship arises because such workers will experience a particularly large slowdown of offer arrival rates (but the human capital accumulation of bound workers will not change). Second, strict NCA enforceability will prevent workers from taking advantage of tight labor markets to move to better matches or to negotiate for higher earnings. We test both of these predictions in Section 6.

[10]One reason this might happen is that higher NCA enforceability could decrease the number of searching firms, for example by depressing new firm entry (Starr et al., 2018; Jeffers, 2018). Additionally, the use of enforceable NCAs by some firms can increase recruitment costs for all firms: if firms cannot directly observe whether a job applicant is currently bound by an NCA, this can slow down the recruiting process on average and decrease the value of posting vacancies (Starr et al., 2019; Goudou, 2022).

[11]An alternative mechanism that could give rise to negative spillovers is if firms using enforceable NCAs pay lower wages, and this leads other firms to be able to also pay lower wages by worsening their workers' outside option (Beaudry et al., 2012). However, it is unlikely that this mechanism could fully explain our results, given our evidence (presented in Section 5) that higher NCA enforceability leads firms to post fewer vacancies, which is hard to rationalize under the Beaudry et al. (2012) framework. In addition, there is no clear empirical consensus that workers who *sign* an NCA earn lower wages: some studies find positive correlations between wages and NCA use (Lavetti et al., 2018; Starr et al., 2021).

3 Data

3.1 State-Level NCA Enforceability

The cornerstone of our paper is a state-level panel dataset with annual measures of states' NCA enforceability. The enforcement of NCAs is governed by employment law, which is determined at the state level. As described by Bishara (2010), NCA laws vary widely across states, and over time within states, in subtle but meaningful ways. For example, there is substantial variation in what is considered a "reasonable" contract, or what is considered a protectable business interest that justifies an NCA. The various aspects that govern the enforceability of NCAs change through case law and, more rarely, through statutes passed by state legislators.

We draw from authoritative legal experts to create an index of each state's legal enforceability of NCAs for each year from 1991 through 2014. Our main primary sources are Bishara (2010), who adopts careful legal analysis to quantify enforceability along a meaningful scale, and a series of legal treatises that Bishara draws from titled "Covenants Not to Compete: A State by State Survey," updated periodically by Malsberger, a leading legal expert on the topic (Malsberger, 2023). Bishara (via Malsberger) identifies seven quantifiable dimensions governing the extent to which an NCA is enforceable. For example, one dimension (Q3a) indicates the extent to which employers are legally required to compensate workers who sign NCAs at the beginning of a job spell. Another dimension (Q8) reflects whether the NCA is enforceable when the employer terminates the employee who signed the NCA (as opposed to a voluntary separation). Table C.1 lists each of the dimensions. Bishara (2010) developed a theoretically-grounded approach to quantify states' treatment of each dimension on an integer scale from 0 (unenforceable) to 10 (easily enforceable). To create an overall enforceability index, Bishara proposed a weighted sum of these seven dimensions, and he chose weights designed to reflect the relative importance of each law component, based on his opinion as a subject expert. Using these rules, Bishara (2010) quantified each dimension and an overall index for each state for the years 1991 and 2009.

We use these legal texts to create a panel version of each state's enforceability from 1991–2014 as follows. We obtained Bishara's internal notes that provide explanations of the legal aspects behind each of his coding decisions.[12] We hired law students to familiarize themselves with the quantification system by going through the Malsberger texts and Bishara's notes for the 1991 enforceability scores. The law students then attempted to use the Malsberger texts to match Bishara's 2009 scores for all of the legal components in every state. After calibrating their own scoring of 2009 with Bishara's, they quantified the changes in enforceability between 1991 and 2009 using the Malsberger texts, imposing Bishara's 1991 and 2009 scores as endpoints. They then extended the panel to 2014. See Section C.1 for a more detailed discussion of

[12]We thank Norm Bishara for generously sharing this dataset with us.

the methods, procedures, and principles we used to construct this database.

Once the seven dimensions of enforceability were coded, we constructed a composite *NCA Enforceability Score* for each state-year from 1991-2014 using the same weights for each of the seven dimensions proposed by Bishara (2010).[13]

Differences in how states interpret these dimensions have led to substantial differences in the *NCA Enforceability Score* across states. At the extreme ends of this spectrum, Florida Statute 542.335 explicitly allows the use of NCAs as long as a legitimate business interest is being protected, the agreement is in writing, and the agreement is reasonable in time, area, and line of business.[14] 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.[15] 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. Law changes can occur through either statutory provisions (by the state legislature) or through precedent-setting court decisions. Over 90% of the law changes during our sample period arise from court decisions. Each of these involves an instance in which an employer or worker filed a dispute over an NCA, and in deciding whether the NCA was enforceable the judge overruled legal precedent. Consider, for example, a state Superior Court case in Pennsylvania: Insulation Corporation of America v. Brobston (1995). The case concerned an employee of an insulation sales company who had signed an NCA. After being terminated for poor performance, he was hired by a competitor of his original employer, in alleged violation of the NCA. While the NCA in question was ultimately not enforced, the court's decision set new precedent that NCAs may generally be enforced following employer termination: "...the circumstances under which the employment relationship is terminated are an important factor to consider in assessing... the reasonableness of

[13]In some state-years, there is no legal precedent for a particular dimension of the enforceability index. Following Bishara (2010), we code these values as missing. The composite NCA enforceability index is a weighted average of scores on each of the seven legal dimensions. When the score for one of the dimensions is missing, we omit it from the calculation of that weighted average, as in Bishara (2010). Though we defer to Bishara (2010) that this is the appropriate way to treat missing values, there are other sensible approaches. In Section C.2, we show that missingness is ultimately quite rare, and we show that our main estimates are insensitive to how we treat missing values.

[14]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`

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

enforcing the restrictive covenant."[16] Future cases cited this precedent in adjudicating matters concerning employee termination: for example, in *All-Pak, Inc., v. Johnston* the court wrote that "We emphasized [in Brobston]...that the reasonableness of enforcing such a restriction is determined on a case by case basis. Thus, the mere termination of an employee would not serve to bar the employer's right to injunctive relief."[17] That is, *Brobston* set a precedent that NCAs *could* be enforceable even if the employee was terminated. *Insulation Corp. of America v. Brobston* therefore resulted in the component of the NCA Enforceability Score specific to treatment following employer termination (Q8) to change from 4 (out of 10) to 7 in Pennsylvania; the resulting change in Pennsylvania's overall NCA Enforceability Score was equal to roughly a third of a standard deviation in the distribution across our sample period.

Table 1 summarizes differences in levels of NCA enforceability across the country and within states over time, between 1991 and 2014. With the exception of the numbers of law changes, states, index increases, and index decreases, the descriptive statistics in Table 1 are weighted to reflect population demographics by matching the scores from each state-year to corresponding observations in the CPS ASEC and using the relevant weights provided by the Census Bureau.

There are 73 within-state NCA law changes over our sample period, and these are dispersed roughly evenly across the Northeast, Midwest, South, and West regions. The average law change results in a change in the magnitude of the NCA Enforceability Score that is about 6.4% of the average score over this period, and the within-state standard deviation in enforceability is equal to roughly 12% of the overall standard deviation. Figure B.1 displays this variation visually. Panel A is a histogram of the level of NCA enforceability across all states over our sample period 1991–2014. Panel B is a histogram of the magnitude (in absolute value) of NCA law changes over this same sample period. Ninety-five percent of law changes result in a score change of 0.15 or less; 0.15 is roughly the difference between the 25th (0.66) and 75th (0.81) percentiles of the NCA score distribution (in levels) over our sample period.

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

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

[17] All-Pak, Inc. v. Johnston, 694 A.2d 347 (1997).

3.1.1 Are NCA Law Changes Predictable?

If changes in NCA enforceability were correlated with underlying legal, economic, political, or social trends, this could reflect a potential source of endogeneity that would make it challenging to use these changes to isolate the effects of enforceability on earnings. For example, changes to enforceability might be preceded by an increasingly litigious business climate that could itself be caused by changing labor market conditions.

A priori, there are good reasons to expect this concern to be minimal. In most cases, the judicial decisions that change legal precedent are initiated by a case that is idiosyncratic to a particular occupation, industry, or employment relationship; however, the consequences of these decisions affect the state's labor law more broadly. Relative to legislators, judges are less influenced by stakeholder pressure that could sway their decision-making because of the doctrine of *stare decisis*.[18] Furthermore, evidence overwhelmingly suggests that judges do not base their decisions purely on policy preferences, but rather on a wide range of motivations (Epstein and Knight, 2013), implying that judges' decisions to break from precedent in an NCA case are unlikely to be related to underlying trends in the labor market.

Nonetheless, we use two approaches to empirically test this possibility more thoroughly. First, we test whether NCA law changes are preceded by a spike in court cases involving NCA litigation. Second, we test whether states' political, social, and economic characteristics predict NCA law changes.

As our first approach, we test whether changing litigiousness predicts NCA law changes. Following Hiraiwa et al. (2023) and Marx (2022), we use data from Courthouse News Service to identify instances of a filed dispute over an NCA in a US court. As in Hiraiwa et al. (2023), we collect all filings containing the strings "noncompetition," "non-competition," "not to compete," "noncompete," "restrictive covenant," or "postemployment restraint."[19] The data begin in 2002, and we collapse to the state-year level, tabulating counts of cases.[20]

For each state that experiences an NCA law change, we consider the window of time starting five years prior to the law change,[21] and we use state-year observations with no legal change during the same window as the controls for that state. We refer to a treatment state and its matched controls as a "block." We use a stacked

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

[19] We omit cases including the term "sale," which often refers to NCAs ancillary to the sale of a business, as these cases are typically handled differently than standard employee NCAs under state law

[20] From 2002–2014, there were roughly 700 court filings about NCAs per year. Compare this number to the roughly 2.5 NCA law changes due to court decisions that occur per year during that same period. That is, roughly 0.38% of court filings result in a decision in which the judge overturned precedent. Interestingly, this proportion is quite similar to the proportion (0.5%) of Supreme Court decisions in which the Court reversed its own Constitutional precedent (Schultz, 2022).

[21] We obtain qualitatively similar results if we choose different time windows.

event study (focusing only on the pre-period) to test whether NCA law changes are preceded by a spike in case counts. Formally, we use a Poisson pseudo-maximum likelihood model (due to the dependent variable being count data) to estimate:

$$Y_{s,b,t} = \sum_{\tau=0}^{5} \alpha_\tau I_{s,b}^{\tau} + \mu_{s,b} + \rho_{b,t} + \varepsilon_{s,b,t}$$

where $Y_{s,b,t}$ is the count of cases in state s at time t, observed in estimation block b; α_τ is the event-time coefficient of interest on $I_{s,b}^{\tau}$, which is an indicator for whether a legal change occurred τ years after the observation time t in state s; $\mu_{s,b}$ are fixed state-by-block effects; and $\rho_{b,t}$ are fixed block-by-time effects. $\varepsilon_{s,b,t}$ is the error term. The estimation blocks (b) correspond to sub-experiments in the stacked difference-in-difference design (Cengiz et al., 2019; Deshpande and Li, 2019); see Section 4.2.2 for more details.

We present the $\hat{\alpha}_\tau$ coefficient estimates in Appendix Figure B.2. There is no positive trend in cases prior to legal changes. This alleviates concerns that NCA law changes are due to an increased trend toward conflict or toward legal interest in NCAs, which may itself be due to changing labor market or business conditions.

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

Table 2 presents the results from a regression in which the dependent variable is a state's annual NCA enforceability, and the independent variables are each of the 20 characteristics noted above (lagged by one year), as well as state and Census division by year fixed effects (we use these same fixed effects in our subsequent analysis). Out of 20 variables, the vast majority have coefficients that are both economically and statistically insignificant. Only two of these 20 variables are statistically significant at the 10% level (the minimum wage and the State Senate Democrats ideology score), and only the minimum wage is significant at the 5% level. A joint F test on the statistical significance of these predictors is insignificant at the 10% level ($p = 0.197$).[22] Furthermore, the partial R^2 of the model, after residualizing on division by year and state fixed effects, is 0.114, implying that these predictors collectively explain only 11% of the variance in within-state changes to NCA policy. Thus, these results provide supportive evidence that NCA law changes are not strongly determined by underlying economic, political, or social trends. In subsequent analysis, we provide further corroborating evidence by showing that earnings do not differentially change in years *prior* to an NCA law change.

3.2 Data on Earnings and Mobility

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

First, we gather individual-level data on earnings and employment from the CPS ASEC (otherwise known as the March Supplement).[23] The ASEC is a CPS supplement collected each March that contains information about the wage and salary income of respondents. The CPS also includes respondents' demographic and geographic information.[24] We restrict the ASEC sample to include individuals who reported having worked for a private-sector employer (not self-employed) in the year prior to being surveyed. We include the years 1991 to 2014, restrict to individuals who were between the ages of 18 and 64 at the time they were surveyed, and remove observations for which earnings or hours variables have been topcoded. The resulting ASEC dataset contains approximately 1.5 million observations, 1.2 million of which represent full-time workers. We deflate earnings and wages in the ASEC using the Consumer Price Index. We match NCA enforceability measures by state and year.

Second, we use the Job-to-Job Flows (J2J) dataset from the U.S. Census Bureau to examine the effect of enforceability on job mobility. Derived from the Longitudinal-

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

[23]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

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

Employer Household Dynamics dataset,[25] these data contain aggregate job flows between cells defined by combinations of age, sex, quarter, origin job state, destination job state, origin employer industry, and destination employer industry. We aggregate these data to the level of the state-industry-year, and we create multiple measures of job mobility that could potentially be affected by NCA enforceability: (1): the *total count* of job-to-job separations; (2): the count of job-to-job separations in which the separating worker's destination job is in a *different* industry or (3): *the same* industry, respectively, than his or her origin job; and (4): the count of job-to-job separations in which the separating worker's destination job is in a *different* state or (5): *the same* state, respectively, than his or her origin job.

Third, we use the Quarterly Workforce Indicators (QWI) dataset from the Census Bureau. Like the J2J, the QWI is a public use file that aggregates data from the LEHD, and it contains data on earnings, as well as numbers of hires and separations, at the county-quarter level for the near-universe of private workers, stratified by sex and age group. We use the QWI both to complement the CPS in our estimation of the earnings effects of NCA enforceability, and also to investigate spillovers from enforceability. One drawback with the QWI for our purposes is that the QWI is not a balanced panel over our sample period, as some states did not begin reporting the necessary data until the late 1990s or later. For this reason, we are left with only 44 legal changes (instead of the universe of 73 legal changes) when using the QWI.

Fourth, in our investigation of the mechanism underlying the relationship between enforceability and earnings, we use data from the CPS Occupational Mobility and Job Tenure Supplement (JTS) over the years 1996 to 2014. The JTS is conducted biannually in either January or February. Among other things, it includes questions about the respondent's history of employment, such as "How long have you been working [for your present employer]?"[26] 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 hire. Our outcome variable of interest is weekly earnings, and we use additional variables as controls. We merge in annual national unemployment rates between 1947 and 2014 from the Bureau of Labor Statistics website for the analysis, which we describe in Section 6.

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

[26]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.

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

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

4.1 Main Results on Earnings and Mobility

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

$$Y_{ist} = \beta * Enforceability_{st} + X_{it}\gamma + \rho_s + \delta_{d(s)t} + \varepsilon_{ist}, \qquad (2)$$

where Y_{ist} is the outcome of interest, $Enforceability_{st}$ is a state's annual composite NCA enforceability score across the 7 dimensions described in Section 3, X_{it} is a vector of individual-level controls, ρ_s is a fixed effect for each state, and $\delta_{d(s)t}$ is a fixed effect for each Census division by year.[27] The coefficient of interest, β, is identified from changes in earnings in states that change their NCA enforceability, relative to other states in the same Census division over the same period. Standard errors are clustered by state. A key identifying assumption is $E(Enforceability_{st}\varepsilon_{ist}|\rho_s, \delta_{d(s)t}) = 0$: conditional on state and division-year effects, changes in enforceability are uncorrelated with the error term. The evidence in Section 3.1.1 supports this assumption.

We report results in Table 3. Columns 1-4 use data from the ASEC, restricted to full-time workers between the ages of 18 and 64 who reported working for wage and salary income at a private employer the prior year.[28] The coefficient in Column 1 implies that an enforceability increase equal to 10% of the observed variation in our sample period leads to a 1.2 percent decline in earnings ($exp(-0.118 * 0.1) - 1, p = 0.002$). As another way to convey the magnitude of this estimate, consider that the 25^{th} and 75^{th} percentiles of *Enforceability* observed in our sample are 0.66 and 0.81, respectively. Moving from the 25^{th} to the 75^{th} percentile in *Enforceability* thus leads to a 1.7 percent average decline in annual earnings ($exp(-0.1175*0.15) - 1 = 0.017$). Adding fixed effects for broad occupation codes in Column 2 diminishes the point estimate slightly but improves its precision ($p < 0.001$).

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

[27]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.

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

dependent variable is instead the log of a worker's reported weekly hours:[29] While the point estimate is negative, it is relatively small and statistically insignificant ($p = 0.24$). 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 nearly identical to the coefficient on annual earnings.

Finally, in Column 5, we corroborate the estimates in Columns 1–4 that used the CPS ASEC sample by using data from the QWI. We run essentially the same regression specification as Column 1, except that we are able to include fixed effects for each county (rather than state)[30] and each division-year-quarter (rather than division-year). We weight the regression by county-level employment. The estimated coefficient is slightly larger than that in Column 1 and statistically significant.

Figure 3 visually illustrates the joint distribution of NCA enforceability and log annual earnings using binned semiparametric scatterplots. The dots in each graph depict the conditional mean log annual earnings for bins of NCA enforceability levels, controlling for the same variables included in Column 2 of Table 3 (state fixed effects, Census division-by-year effects, 1-digit occupation effects, and individual demographic controls). The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023).

Panel (a) shows the full joint distribution for all states and years. Panel (b) excludes California and North Dakota to visually focus on the states and years that provide nearly all of the identifying variation in our estimates. Both figures depict a clear negative relationship between enforceability and earnings. Using the test developed in Cattaneo et al. (2023), we fail to reject the hypothesis that the relationship between log earnings and NCA enforceability is linear in the full distribution (p=0.992). This test reinforces the choice of a linear regression specification in Equation 2.

In Appendix Table B.1 we report estimates from the same models shown in Table 3, but in each model we include the additional political and economic controls described in Section 3.1.1. The point estimates are slightly attenuated but similar with these controls: the coefficients in the ASEC log earnings and log wage models are -0.087 and -0.085, respectively ($p < 0.01$ in each model) and the coefficient in the QWI log average earnings model is -0.121 ($p < 0.01$).

It is instructive to benchmark our results against the estimated earnings effects of other labor market characteristics or institutions. One particularly instructive comparison is the effect of explicit employer concentration on earnings: Prager and Schmitt (2019) find that large changes in employer concentration, caused by local hospital mergers, caused a 6.5 percent decline in earnings among the most affected workers. As two comparable institutions, the household income premium associated with membership in a labor union is an estimated 15-20 log points (Farber et al.,

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

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

2018); the income premium for workers in an occupation that requires a government-issued occupational license is estimated to be 7.5% Gittleman et al. (2018).[31] To derive a comparable effect of NCA enforceability, we can extrapolate our estimates to consider what would happen to earnings under a national policy that rendered all NCAs unenforceable. We generate predicted earnings for each individual in the 2014 ASEC sample using coefficients from Column 1 of Table 3, for two different levels of NCA score: first, the NCA score observed in 2014 in that individual's state, and second, at the lowest observed NCA enforceability level (0). These predictions imply that average earnings among *all* workers would likely increase by 3.2% to 14.2% nationally if NCAs were made unenforceable.[32] The midpoint of this interval (8.7%) is similar to the effect of a large change in employer concentration, roughly one-half the household premium from labor union membership, and comparable to the premium attained by workers in occupations with government-mandated licenses.[33]

Our NCA Enforceability Score pools seven dimensions of NCA enforceability, but these dimensions might differ in their earnings effects. In Appendix Table B.2, we reestimate the effect of changes in NCA law on earnings in a specification analogous to Column 1 of Table 3, but focusing on each individual component of the composite NCA score separately. The first seven rows represent separate regressions identical to Equation 2, except that $Enforceability_{st}$ is replaced with each respective element of the NCA score described in Table C.1.[34] With two exceptions (which are both insignificant at the 10% level), the estimated effect of each score is negative; among those that are negative, the coefficients are significant at the 5% level for three components. Two of the dimensions yielding the largest negative earnings effect are those requiring consideration (i.e. compensation), both at the outset of employment (Q3a)

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

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

[33]This prediction of the effect of a national ban on NCAs requires a strong assumption of linearity, since such a ban would lead the average worker to experience an NCA score change far outside the distribution of identifying variation underlying our regressions in Table 3. However, the roughly linear relationship between earnings and NCA enforceability illustrated in Figure 3 suggests that this assumption is not unreasonable.

[34]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 B.2.

and after employment has already begun (Q3bc), consistent with evidence in Starr (2019). No single dimension drives our results, and the dimensions with the largest effects are consistent with what one might expect based on theory and prior results.

4.1.1 Effects of Enforceability on Job Mobility

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

Table 4 presents estimates based on job-to-job flows data from the J2J dataset. We measure the number of job-to-job changes at the state-year-quarter-sex-age group-industry level. We then estimate a Poisson pseudo-maximum likelihood model with the following specification:

$$\mathbb{E}\left[J_{stia}\right] = \exp\left[\beta * NCA_{st} + \lambda * High\ Ind_i\ \times NCA_{st} + \gamma X_{ia} + \theta_{si} + \phi_{d(s)ti} + \varepsilon_{stia}\right]$$

where J_{stia} is the count of job-to-job changes[35] in state s, quarter t, origin industry i, and demographic group (age and sex) cell a. NCA_{st} is the NCA enforceability score, and $High\ Ind_i\ \times NCA_{st}$ is an interaction between industries with high rates of NCA use (as measured in Starr et al. (2021): see Section 4.3.2 for more detail), and the NCA enforceability score. X_{ia} contains indicator variables for male workers and each of the age bins in the J2J data.[36] θ_{si} is a fixed state by origin industry effect, and $\phi_{d(s)it}$ is a fixed census division by origin industry by quarter-year effect.

In Column 1 we estimate the effect of the origin state NCA enforceability score on the overall number of job-to-job changes and find a small and statistically insignificant effect. However, in Column 2 we interact NCA enforceability with an indicator for whether the origin job was in a high NCA-use industry, and find that NCA enforceability substantially reduces job-to-job separations in high-use industries. The coefficient on $High\ Ind_i \times NCA_{st}$ is negative (-0.241) and highly significant ($p < .01$). The estimate implies that moving from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile of NCA enforceability decreases the number of job-to-job changes by 3.7% in high-use industries.

In Columns 3 through 6 we test whether NCA enforceability affects not just the *level*, but also the *direction* of job mobility, based on two forms of restrictions often used in NCA contracts. In Columns 3 and 4 we test for effects on job-to-job transi-

[35] Following Johnson et al. (2023), we use job change counts, instead of rates, as our dependent variable. We do this because NCA enforceability also affects the denominator of the rate—employment—which makes interpretation difficult. In untabulated results, we find that a regression of log employment on NCA enforceability (using QWI data in a specification identical to Column 5 of Table 3, using baseline employment as weights) yields a coefficient of -0.13 ($p = 0.047$), corresponding to a 1.9% decrease in employment when moving from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile of enforceability.

[36] These are age ranges 14-18, 19-21, 22-24, 25-34, 35-44, 45-54, and 55-64.

tions that occur across (Col. 3) and within (Col. 4) the origin job industry. Focusing on high-use industries, we find no statistically significant impact of NCA enforceability on across-industry job transitions, but we find a large and significant negative effect on transitions within-industry in high-use industries. Specifically, we estimate that moving from the 25^{th} to the 75^{th} percentile of NCA enforceability decreases the number of within-industry job changes by 5.9% in high-use industries. This evidence is consistent with Marx (2011) and Mueller (2022), who find that technical professionals and inventors bound by NCAs or subject to stricter NCA enforceability take "career detours" to different industries and occupations to avoid potential lawsuits.

In Columns 5 and 6 we test for effects on job-to-job transitions that occur across (Col. 5) and within (Col. 6) the state of the origin job. We again find no statistically significant impact of NCA enforceability in high-use industries on across-state job transitions, but we find a large and significant negative effect on transitions within the origin state in high-use industries. We estimate that moving from the 25^{th} to the 75^{th} percentile of NCA enforceability decreases the number of within-state job changes by 4.1% in high-use industries. This evidence is consistent with the fact that the restrictions in many NCAs are geography-specific, so are more likely to affect the rates of in-state moves.

This evidence illustrates that our measures of NCA enforceability influence mobility decisions: exactly what NCAs are designed to do. The results also motivate our investigation into one mechanism through which NCA enforceability affects earnings, which we describe in Section 6.

4.2 Dynamic Effects on Earnings and Robustness to Heterogeneous Treatment Effects

We use a distributed lag model to check whether earnings exhibit differential pre-trends in the years prior to an NCA law change, and how earnings evolve in the subsequent years after a law change. We corroborate this analysis with an event study model centered around a state's first NCA law change, which also addresses potential bias from heterogeneous treatment effects that might affect our baseline estimates.

4.2.1 Distributed Lag Estimates on Earnings

Two potential concerns with the estimates from difference-in-difference specifications are 1) the plausibility of the parallel trends assumption that treatment and control states would counterfactually follow common trends in the absence of a law change in the treated state, and 2) whether the regression estimates reported in Table 3 mask dynamic treatment effects that change over time.

To address these concerns, we complement our difference-in-difference estimates with a distributed lag model, which allows us to assess the dynamic effects of an

NCA law change in the years immediately before and after the change takes place. A distributed lag model is similar to an event study model: Schmidheiny and Siegloch (2020) show that a distributed lag model with leads and lags is in fact numerically identical to an event study model with binned endpoints.

We estimate the distributed lag regression in first differences, similar to the approach used by Fuest et al. (2018)[37] using the QWI sample, which is based on the universe of jobs in the U.S.. In this specification, the unit of observation is a county $c(s)$, demographic group g (defined as combinations of sex and age), and quarter t. The model we estimate using QWI data is:

$$\ln w_{c(s),g,t} - \ln w_{c(s),g,t-1} = \sum_{k=-3}^{k=5} \beta_k [Enforceability_{s,t-k} - Enforceability_{s,t-k-1}] \\ + \Omega_g + \gamma X_{s,t} + \delta_{d(s),t} + \varepsilon_{c(s),g,t}.$$

The dependent variable, $\ln w_{c(s),g,t}$, is the natural logarithm of average earnings in the relevant bin. Ω_g contains indicator variables for worker sex and each age bin. $X_{s,t}$ includes the same state-level political, economic, and social measures described in Section 4.1. $\delta_{d(s),t}$ is a fixed Census division-by-year-quarter effect. We weight observations by employment and cluster standard errors by state.

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

We report the results from this model in Panel A of Figure 4. The figure depicts two noteworthy features. First, there is little evidence of a pre-trend in earnings, supporting the assumptions (and the evidence in Section 3.1.1) that NCA law changes were conditionally exogenous to underlying economic trends and to underlying changes in the frequency of litigation or the use of NCAs which could simultaneously impact earnings. Second, earnings begin to decline in the first year following the law change, and the effects grow in magnitude until year three, becoming statistically significant by year two.[38]

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

[38]The gradual increase in the earnings effect could be due to delays in knowledge about law changes, frictions in adjusting contracting terms, or grandfathering of contractual provisions, among other factors. The earnings effect growing over time is also consistent with our proposed mechanism that higher enforceability leaves workers less able to benefit from outside job offers to improve their earnings—a mechanism we test for in Section 6—which is an effect that would compound over time. Lipsitz and Starr (2021) and Young (2021), who study the effects of NCA bans in the state of Oregon and in Austria, respectively, both also find that the earnings effects of NCA bans grew over time.

4.2.2 Stacked Event Study

While the distributed lag model reported in Panel A of Figure 4 corroborates our baseline two-way fixed effects (TWFE) model, recent research has illustrated that both of these approaches can be biased in the presence of heterogeneous treatment effects. Our empirical design leverages differential timing in changes across states to a continuous treatment that can change multiple times over the sample period. Several recent papers have highlighted that staggered timing of changes can cause TWFE to be biased because of comparisons where states that experience early law changes serve as controls for states with later law changes (Goodman-Bacon, 2018)). While alternative estimators have been proposed to overcome this bias for a binary treatment (e.g., Callaway and Sant'Anna (2021)), continuous variation in treatment can create additional complications that are the subject of ongoing research (De Chaisemartin and D'Haultfoeuille, 2022b).

To address these concerns, we draw inspiration from recent work and conduct a stacked event-study around a state's first law change during our sample period. The stacked design has been used in other recent applied settings (Cengiz et al., 2019; Deshpande and Li, 2019), and De Chaisemartin and D'Haultfoeuille (2022a) show that the treatment effect of a unit's first change can be estimated without bias. We identify the subset of NCA law changes that satisfy the following criteria: 1) they are a state's first law change during the sample period, 2) they occur at least 4 years after the start of the QWI sample period (which varies by state since states entered QWI in different years), 3) they occur at least 5 years before the end of the sample period (2014), and 4) they are not followed by subsequent countervailing law changes.

We use the 11 states that never experienced a law change during our sample period (never changers) as the set of eligible control states. For each treatment state, we create a panel dataset for that treatment and its control states, comprising the three years prior and five years following the treatment state's law change. We consider two sets of control states for each treatment state: 1) all 11 never changer states, and 2) the subset of never changers in the same Census region.[39] Two treatment states satisfy requirements (1) to (4) above but lack a control state in their Census region with QWI data in the pre-period; these two treatment states get dropped from the specification restricting to control states in the same region. Overall, the sample restrictions leave us with 10 law changes (14% of the 73 total changes) when we require controls to be in the same region, and 12 law changes when we allow control states to be out-of-region. Thus, a tradeoff with this specification is that, while it

[39] This model is different than our baseline that compares treated states to control states in the same Census division. The reason is that in this model there are only 11 eligible controls control states, leaving an overly sparse set of control states if we required they be in the same Census division (of which there are 9). We present estimates that do and do not require control states to be in the Census *region* (of which there are four) to balance the tradeoff between accounting for geographic-specific shocks that could matter for wages, while also ensuring we have a large enough comparison group.

helps us overcome the potential biases associated with TWFE, it is not guaranteed that the estimates we obtain will represent a population-level average.

We then stack these individual panel datasets (estimation blocks) and estimate the difference in outcomes between treated and control states in each year relative to the law change. We estimate the following regression equation:

$$\ln w_{c,b,g,t} = \sum_{\tau=-4}^{\tau=6} \alpha_\tau I^{\tau}_{s(c),b} \times \text{Score Change}_{s(c),b} + \mu_{c,b} + \rho_{r(c),b,t} + \Omega_g + \gamma X_{s,t} + \varepsilon_{c,b,g,t} \quad (3)$$

where $\ln w_{c,b,g,t}$ is log average earnings of group g in county c in estimation block b in year t. $I^{\tau}_{s(c),b}$ is equal to 1 if year t is τ years relative to state $s(c)$'s first NCA law change (where state $s(c)$ contains county c), and Score Change$_{s(c),b}$ is equal to the magnitude of the law change that defines block b—i.e., the NCA score from that first law change (and is therefore zero for all control states). $\mu_{c,b}$ is a fixed county–block effect, $\rho_{r(c),b,t}$ a fixed block–region–year effect, where $r(c)$ is the Census region containing county c (or simply block–year when not requiring that controls be in the same Census region). As in the distributed lag model, Ω_g contains indicators for sex and age categories and $X_{s,t}$ contains state-level political, economic, and social variables. Following Cengiz et al. (2019), we cluster standard errors by state–block. We weight observations by employment.

Panel B of Figure 4 graphically displays the estimates of the α_τ coefficients from two versions of Equation 3 that do and do not require that control states be in the same Census region. In both specifications, the pre-period coefficients have some noise but are close to (and statistically indistinguishable from) zero. As with the distributed lag model, the coefficients grow for several years following the law change, and are statistically significant in both specifications after year three. The coefficient magnitudes are quite similar across the two models. Using a stacked difference-in-difference (as opposed to a two-way fixed effects) model,[40] we estimate an overall earnings effect of -0.246 ($p < .01$), as reported in Column 1 of Table B.3.[41] This magnitude is quite a bit larger than the baseline TWFE coefficient of -0.137 using the QWI data (Table 3), though the estimates are not directly comparable since they are estimated on a different set of law changes and over a different time horizon.

Another advantage of the stacked model is that we can estimate separate treatment effects for each individual law change. This exercise is useful because, for example, it enables us to check whether our estimates are driven by one or two law

[40] This regression model is:

$$\ln w_{c,b,g,t} = \beta \times \text{Enforceability}_{s(c),b,t} + \mu_{c,b} + \rho_{b,r(c),t} + \Omega_g + \varepsilon_{c,b,g,t} \quad (4)$$

[41] For this table, we report results from the specification that requires that control states be in the same Census region and that does not condition on the additional state-year level variables in $X_{s,t}$ in Equation 3.

changes, or whether the earnings effect of enforceability is negative in a broad range of states. Figure B.3 reports point estimates and 95% confidence intervals on *Enforceability* from different regressions that each estimate the stacked diff-in-diff model analogous to Equation 4, separately for each of the 10 treatment states in the estimation sample. The point estimates are negative for 8 of the 10 states, implying that our estimated earnings effects are not driven by a few outliers, but rather are broadly represented in a range of states.

4.2.3 Long-Panel Event Study

While our stacked model in Section 4.2.2 addresses the potential sources of bias common to difference-in-difference models with staggered treatment timing, an additional complication in our setting is the non-absorbing nature of NCA policies: states have the ability to change NCA enforceability multiple times, such as reversing or enhancing previously changed laws. We address this issue by employing a long-panel event study design, in which the event in each treated state is simply the change in NCA enforceability between the beginning and end of the panel. To do so, we include the years 1991-1993 and 2012-2014 (the first and last three years in our panel) for each state, and we calculate the change in the NCA enforceability score over this time period.[42] We use the CPS ASEC data for this analysis, since many states only started reporting data to QWI after 1993.

Figure B.4 displays results. As in the stacked event studies and the distributed lag model, there is no evidence of a trend in earnings that is different for treated versus untreated states. Earnings are substantially lower (higher) in states that experienced NCA enforceability increases (decreases) in the intervening years, with coefficients that are significantly different than zero and of essentially identical magnitude to our estimates in Panels A and B of Figure 4.

This result provides evidence that our results are not being driven by peculiarities of the methods we employ, as well as demonstrating that the effects of NCA enforceability changes appear to persist in the long run.

4.3 Assessing Robustness of Our Estimates to a Range of Concerns

4.3.1 Interpreting Estimates from a Continuous Treatment Variable

Recent research reveals that difference-in-difference estimates can be challenging to interpret when the treatment variable is continuous (Callaway et al., 2021). In light of this concern, we can use our stacked event study model to assess whether our

[42]For states in which there were enforceability changes in the first three years or in the last three years, we omit the odd year out (and keep the two identical years). There were no states with multiple changes in either of those periods.

estimated earnings effects are driven by the scaling of our enforceability variable or by particular types of law changes. We report results in Table B.3. Column 1 reports the overall estimated earnings effect from the stacked difference-in-difference model. In Column 2 we replace the continuous NCA score with a signed indicator variable that is equal to 1 in the years following a positive law change, to -1 following a negative change, and to 0 otherwise. This model yields a coefficient of -0.018 ($p < 0.01$). To interpret this coefficient, consider that the average NCA law change in this estimation sample resulted in an absolute change in the enforceability index of 0.077; together, these imply an effect size of NCA enforceability of $-0.018/0.077 = -0.234$), similar to the effect size we directly estimate with the continuous variable.

We then estimate if the *direction* of the law change matters. In Columns 3 and 4 we separately estimate the effects of positive and negative enforceability changes, using the same signed indicator variable in place of the continuous enforceability measure. We obtain an estimate of -0.018 in each model ($p = 0.019$ and $p = 0.012$, respectively). The symmetric effects illustrate that our estimated earnings effects are general to both increases and decreases in enforceability.

Finally, in Columns 5 and 6 we estimate separate effects for small and large NCA law changes, as defined by whether the treatment state's NCA score change (in absolute value) is below or above the median. The average small change leads the mean treated state's NCA score to change by 0.039 in absolute value, and the estimated earnings effect (using the signed indicator variable for treatment) is -0.017 ($p = 0.008$). The average large change leads the mean treated state's score to change by 0.121 in absolute value, and the estimated earnings effect is -0.024 ($p = .026$). These differences suggest that the scale of our enforceability measure has economic content: the magnitude of NCA law changes, and not just the sign of the change, affects wages.

These estimates show that the earnings effects are not driven by a particular direction or magnitude of law change.

4.3.2 Heterogeneous Earnings Effects Based on Prevalence of NCA Use

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

While we do not observe whether individual workers have or have not signed an NCA, Starr et al. (2021) report several sources of heterogeneity in NCA use by worker characteristics. We focus on three sources: workers' education, occupation, and industry. First, Starr et al. (2021) find that workers with a Bachelor's degree or higher are significantly more likely to sign NCAs than workers without a college degree. Second, Starr et al. (2021) find heterogeneity in use across 22 occupation categories and 19 industry categories. We use the occupation and industry in which an individual reports working to the CPS to classify workers as working in *High or Low NCA Use Occupations* and *High or Low NCA Use Industries*.[43] We replicate our main difference-in-difference specification, Equation 2, except that we now add an interaction term of *Enforceability* with an indicator for *College Educated Worker*, *High NCA Use Occupation*, or *High NCA Use Industry* (as well as an indicator for the respective main effects).

Table 5 reports these heterogeneity estimates. Column 1 reports the baseline average effect on earnings, corresponding to Column 1 in Table 3. Column 2 includes an interaction of NCA Enforceability Score with an indicator for whether a worker has a college degree (*College Educated Worker*). The main effect on *NCA Enforceability Score* is close to zero and statistically insignificant, implying that enforceability has little to no effect on earnings for non-college-educated workers. On the other hand, the interaction term ($-0.138, p < .01$) implies that enforceability has a much stronger effect on the earnings of college-educated workers. The sum of the main effect on *NCA Enforceability Score* and the interaction effect implies that going from the 25^{th} to 75^{th} percentile of enforceability leads to a 2.6% decrease in earnings for college-educated workers ($exp((-0.038 - 0.138) * 0.15) - 1 = -0.026, p < .01$), an earnings effect that is over 50 percent larger than the earnings effect for the whole population implied by Column 1 of Table 3.

Column 3 reports heterogeneity by occupational use of NCAs. The estimates imply that going from the 25^{th} to 75^{th} percentile of enforceability leads to a 2.1% decrease in earnings in high-use occupations ($exp((-0.085 - 0.059) * 0.15) - 1 = -0.021, p < 0.01$); the effect for low-use occupations is about 60% as large ($p = 0.02$), and the difference is statistically significant ($p < 0.01$). Finally, Column 4 reports heterogeneity by industries' use of NCAs. Going from the 25^{th} to 75^{th} percentile of enforceability leads to a 2.4% decrease in earnings in high-use industries ($p < 0.01$); the effect for low-use industries is roughly 60% as large ($p < 0.01$), and the difference is statistically significant ($p < 0.01$).

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

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

ative and significant. The interaction of NCA Score with *College Educated* changes little and remains statistically significant.[44]

4.3.3 Accounting for Potentially Endogeneous NCA Law Changes

Considering that the vast majority of NCA law changes arise from court decisions rather than statutory changes; that economic, social, political, and legal factors do not collectively predict changes in NCA enforceability (Table 2 and Figure B.2); and that there is no evidence of pre-trends in the distributed lag and event study models, it is exceedingly unlikely that NCA law changes are endogenous to omitted variables that could contaminate our estimates. Still, we can conduct some additional analyses to further address this concern.

Even though the majority of NCA law changes arise through court decisions, one might worry that the few changes arising from statutory changes might be endogenous to underlying trends in ways that could bias our results. We directly address this concern in Panel A of Table B.4, where we re-estimate our baseline TWFE model but exclude the 8 states that ever experience a statutory NCA law change. The estimated coefficient on *NCA Enforceability Score* is similar to our baseline estimates in Table 3; the standard errors (unsurprisingly) increase in size, though the estimates remain statistically significant.

While judicial decisions are less prone to endogeneity than are statutory changes from legislative action, there is some evidence that judges' decision-making can be swayed by external forces like business interests, particularly for judges that are elected rather than appointed (Katz, 2018). To ensure that our results are not driven by confounding influences on elected judges, we obtained data on how judges are selected across states from Bannon (2018). We recreate our main TWFE analyses a) excluding the 6 states that have partisan judicial elections (i.e., judges are selected via election and the judge's political party is listed on the ballot) and b) excluding the 21 states in which judges are elected (whether or not the elections are partisan). We report results in Panels B and C of Table B.4, respectively. If anything, our point estimates are *larger* in magnitude with these restricted samples (they become substantially more imprecise in Panel C, which is to be expected since we are eliminating over 40% of the states in our sample). Since judicial elections are a key mechanism through which political or economic preferences of voters might affect judicial decisions, this evidence provides further reassurance against this potential form of endogeneity.

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

4.3.4 Robustness to Construction of NCA Enforceability Index

Though our construction of the NCA Enforceability index reflects the reasoning and judgment of leading legal scholars, a natural question is whether some of the decisions that go into this index affect our results. Two such decisions are how we treat missing values of individual enforceability components and the weights we give to each individual component in constructing the aggregate index. In Appendices C.2 and C.3, we show that our estimates are insensitive to alternative approaches to both of these decisions.

5 Spillover Effects of NCA Enforceability on Earnings

The results in Section 4 demonstrate that NCA enforceability has a negative effect on overall earnings. How do these estimates relate to our model? As described in Section 2 (and shown in Equation 13 in Appendix A), the effect of enforceability on average earnings is a weighted sum of two terms: 1) the average difference in earnings between workers that are and are not bound by NCAs and 2) the spillover effect of enforceability on earnings of workers not bound by NCAs. Theoretically, this second term is unambiguously negative: strict NCA enforceability will decrease the earnings of workers not bound by NCAs. This effect arises due to the assumption that strict enforceability slows down the job offer arrival rate for workers who are not constrained by NCAs, reducing their ability to leverage outside offers and climb the job ladder. In this section, we discuss existing evidence supporting this assumption and provide new evidence to corroborate it. We then show that enforceability does have spillover effects that are present and economically meaningful. Finally, we provide a brief discussion of what our results can say about the first term in Equation 13, the difference in average earnings between constrained and free workers, which our model suggests is indeterminate.

5.1 Effects of Enforceability on Job Vacancies

Our model predicts that NCA enforceability reduces earnings of workers not bound by NCAs under the assumption that NCAs cause offer arrival rates to fall for all employed workers in a labor market, not just those bound by NCAs. Prior work supports this assumption. Using survey data, Starr et al. (2019) find a large and significant negative effect of the interaction of incidence of NCA use in a state-industry cell and NCA enforceability on job offers received in either the prior year or over the course of their job spell—even among workers who are not bound by NCAs. Similarly, Goudou (2022) finds a decreased job-finding rate in industries with greater NCA incidence, consistent with his model that enforceable NCAs make job vacancies more difficult for firms to fill.[45]

We provide additional corroborating evidence for the prediction that NCAs reduce offer arrival rates using data on job vacancy posting rates. Vacancy rates measure the existence of potential jobs both for workers bound by NCAs and those who are not (and, arguably, more so for those who are not, since those bound by NCAs are unable to take certain jobs) (Bagger et al., 2022). Our primary proxy for offer arrival rates is the number of unemployed people per job opening, a metric used by the Bureau of Labor Statistics that reflects how tight or slack the labor market is. A higher ratio indicates that it would take longer for a worker to receive a job offer, on average. We additionally consider the number of job openings to demonstrate that changes in the ratio are not solely driven by changes in the number of unemployed people. Both of these measures are available at the state–year level starting in 2001 from the Job Openings and Labor Turnover Survey (JOLTS) conducted by the BLS.[46]

In Table 6, we present estimates of the impact of NCA enforceability on these measures of job offer arrival rates. Formally, we estimate an analog of Equation 2 at the state-time level, with no individual controls, and with t representing a month-year. Column 1 shows that stricter NCA enforceability leads to increases in the count of unemployed individuals per job opening: going from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile of enforceability leads to a reduction in that rate of 0.27 ($p = 0.094$), or 10.7% relative to a mean of 2.51. In other words, when enforceability is stricter, the number of individuals vying for any given vacancy increases. Column 2 shows that, while statistically insignificant, this effect is driven, at least in part, by changes in the count of job openings: going from the 25$^{\text{th}}$ to the 75$^{\text{th}}$ percentile of enforceability leads to a reduction in job openings of 3.4%.

These results, taken together with the existing literature, corroborate the assumption that NCA enforceability reduces offer arrival rates to workers in the labor market, especially for those who are not bound by NCAs.

5.2 Estimating Spillover Effects of NCA Enforceability

Having provided empirical support for our model's assumption that NCA enforceability affects offer arrival rates for all workers, we now turn to the implication of this assumption: that changes to NCA enforceability have spillover effects on the earnings of workers not bound by NCAs.

To test this prediction, we examine whether changes in NCA enforceability in

[45] Other factors, however, could push this relationship the other way: in theory, NCAs could encourage recruitment by providing more flexible contracting structures. See Potter et al. (2022) for the implications that follow from that assumption.

[46] We use monthly data aggregated across industries (total nonfarm) at the state level, seasonally adjusted. The BLS does not report data at a more granular level. See `https://www.bls.gov/jlt/data.htm`

a "donor" state affect workers who share a local labor market with that state but work in a different state. Our goal is to directly assess the extent of spillovers onto workers not directly affected by a change in NCA enforceability. Consider the St. Louis metro area, which includes counties in Missouri but also several counties across the state border in Illinois. If Illinois experiences an NCA law change, does it affect the earnings of workers employed on the Missouri side of the St. Louis metro area? And vice versa if Missouri experiences a law change?

We measure local labor markets as commuting zones, which are clusters of counties that have strong commuting ties and have been used in many prior studies as measures of local labor markets (e.g., Autor et al. (2013)). We identify commuting zones that straddle state borders: these commuting zones are local labor markets that include business establishments in two states and are therefore subject to two different NCA enforcement regimes. We remove 8 commuting zones that contain counties in more than 2 states to ensure clarity in defining the donor state. These restrictions leave us with a set of 137 commuting zones and 742 counties in them. In our main analysis, we focus on the 545 counties in these commuting zones that themselves lie directly on state borders; with this restriction, we avoid counties such as Los Angeles County, which shares a commuting zone with counties in Arizona but is nearly 200 miles driving distance from anywhere in Arizona.

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

To test for spillovers, we use an analog of the difference-in-difference model corresponding to Equation 2 to estimate the impact of a change in NCA enforceability across a state border, among workers employed in a commuting zone that straddles the state border. The outcome variable is the log of average quarterly earnings within each cell for all private sector employees. We estimate the model:

$$\begin{aligned} Y_{ctga} = \phi_0 + \phi_1 * Enforce_{ct} + \phi_2 * BorderEnforce_{ct} \\ + \phi_3 * Female_g + \psi_a + \zeta_c + \Omega_{d(c)t} + \varepsilon_{ctga}, \end{aligned} \tag{5}$$

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. ψ_a and ζ_c are fixed age group and county effects, respectively. $\Omega_{d(c)t}$ 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 7. Column 1 verifies that the direct relationship between (own) state NCA scores and earnings holds in this restricted sample. The coefficient on *Own State NCA Score* is -0.160 and statistically significant ($p < 0.01$). This magnitude is slightly larger than the main estimates reported in Table 3. Column 2 includes the *Donor State NCA Score*. In this model the direct effect of *Own State NCA Score* increases slightly to -0.181, $p < 0.01$, while the coefficient on *Donor State NCA Score* reveals evidence of meaningful spillover effects: the coefficient is -0.137 ($p = 0.059$), which equals 76% of the own state effect.

In the next section we conduct several tests to evaluate the reliability and clarify the interpretation of these spillover estimates.

5.3 Assessing the Interpretation of Spillover Estimates

We conduct three tests to corroborate the interpretation that the estimates in Table 7 reflect spillover effects of NCA enforceability across state borders. First, we test whether the magnitude of spillover effects varies in proportion to the relative sizes of the labor forces on each side of a bisected commuting zone. Second, we estimate heterogeneity in the magnitude of spillover effects by distance from state borders. Finally, we consider whether alternative mechanisms can explain our spillover results.

We first examine heterogeneity in spillover effects among border counties. Intuitively, in a commuting zone bisected by a state border, the magnitude of a spillover effect from a donor state's law change should be smaller if the donor state comprises a small share of total employment in the commuting zone. Conversely, if the donor state is the primary location of employers in the commuting zone, a change in NCA enforceability in the donor state should create a larger change in job offer arrival rates (and thus earnings) across the border in the neighboring state.

Column 3 of Table 7 shows our estimates of this heterogeneity. Along with their main effects, we include interactions of the 'own state' and 'donor state' NCA Scores with the share of the commuting zone labor force that is employed on the 'own state' side of the border. Since the unit of observation in this regression is at the county-demographic group-quarter level, we calculate these shares at the demographic group (age-sex combinations) level.[47] The results show that spillover effects are heterogeneous in a manner consistent with the logic above. The main effect of *Donor State NCA Score*, representing the spillover effect in a county that comprises zero percent of its CZ's employment (and thus where the donor state comprises essentially all of the CZ's total employment), is negative (-.167, $p = 0.032$). However, the spillover effect is substantially smaller in counties that account for a large share of employment in their commuting zone. In the extreme case in which a county contains 100% of commuting zone employment, the estimated spillover effect is close to zero (-0.009 =

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

-0.167 + 0.157) and statistically insignificant ($p = 0.891$).[48]

Our main estimates of spillover effects consider earnings in adjacent pairs of counties bisected by state borders. Our second test of the interpretability of these estimates relies on the intuition that the magnitude of spillovers should attenuate with distance to the state border; if they did not one might worry our spillover estimates are driven by a spurious correlation. In Table B.6 we present three supplemental estimates from samples that include (1) interior counties that are neither in commuting zones that straddle state borders nor on state borders; (2) the subset of these interior counties that lie at least 50 miles from any state border; and (3) the subset that lie at least 100 miles from a border. We assign to each county a 'Donor State NCA Score' that corresponds to the state geographically closest to that county.[49] Reassuringly, the point estimate on *Nearest Neighboring State's NCA Score* is substantially attenuated in each of these three subsamples, with coefficients -0.059, -0.027, and -0.036, respectively.[50] None of the coefficients are statistically significant.

As a third test, we examine whether spillover effects of NCA enforceability could be driven by alternative mechanisms that we have not considered. We have argued theoretically (and shown empirically in Section 5.1) that strict NCA enforceability slows job offer arrival rates, and that this is the mechanism that underlies negative spillover effects on earnings. However, other explanations are possible. For example, workers may decide to find a job across state lines if their own state increases NCA enforceability. Such behavior would cause an outward shift in labor supply in border states, causing the market-clearing wage to decline. We find no evidence, however, that such worker behavior can explain the spillover effects on earnings. In Table B.7, we present estimates of the spillover effects of enforceability on workers' *mobility*. The structure mimics Table 7, except that our dependent variables are the log quarterly

[48]Unlike the analysis with the QWI dataset that we reported in Table 3 and Figure 4, we leave the regressions in Table 7 unweighted. We do this for two reasons. First, we weight the prior QWI analysis by employment to estimate an average treatment effect for the US population; because the sample in Table 7 is limited to border counties, weighting serves no such purpose. Second, spillover effects (as we show) are more pronounced in counties with a small share of employment. Therefore, an estimate that weights observations by employment would likely reveal little to no average impact of Donor State NCA Score. We report a weighted version of Table 7 in Table B.5, which indeed shows an attenuated average effect. However, Column 3 reveals that the heterogeneity based on employment shares in the CZ in Column 3 persists in the weighted specification, as expected.

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

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

number of hires and separations from QWI in Columns 1 to 3 and 4 to 6, respectively. Across all six columns, enforceability in a worker's *own* state has a negative effect—of roughly similar magnitude—on hires and separations, corroborating the mobility results we found in Section 4.1.1 using the J2J dataset. The spillover effects (reported in Columns 2 and 5) are imprecisely estimated, though they are negative and of a magnitude that is 53-66% smaller than the direct effect.[51] Thus, there is no evidence that workers move across state lines in response to an NCA law change in their own state; if anything, these estimates suggest that strict NCA enforceability *reduces* cross-border mobility.

Collectively, these results on earnings and mobility provide evidence that NCA enforceability reduces earnings and labor market churn, even across state borders. Though we cannot observe which workers sign NCAs, these results suggest that NCA use has external effects on workers and firms that do not use them, consistent with the theoretical considerations discussed in Section 2.

5.4 Interpreting Enforceability Effects in the Presence of Spillovers

The spillover effects reported above have two important implications for interpreting our estimates of the overall earnings effect of NCA enforceability.

The first implication is theoretical. As described in Section 2, the overall effect of enforceability on average earnings depends not just on spillovers, but also on a second term: the average difference in earnings between constrained workers bound by an enforceable NCA and unconstrained workers not bound by one. This term can be positive or negative and is what makes the overall effect on average earnings indeterminate. We are not able to directly estimate this term in this paper; nevertheless, the spillover results allow us to provide some perspective on it.

We first note that, even if a panel dataset on NCA use existed (which, to our knowledge, does not), it is not obvious that the causal effect of signing an NCA is straightforward to identify. The decision by workers and firms to use NCAs is likely to be correlated with many unobserved worker and firm characteristics, such as intangible capital and opportunities for investments, causing endogenous selection into employment contracts with NCAs (Starr et al., 2021). This endogeneity makes it challenging to estimate the causal effect of signing an NCA on earnings. Some prior correlational studies indicate that workers who are bound by NCAs have 5–6% higher earnings than observationally similar workers not bound by one (Starr et al., 2021; Starr and Rothstein, 2022). However, these comparisons likely suffer from omitted variable bias; Balasubramanian et al. (2023) estimate a *negative* effect of signing an NCA on earnings when accounting for plausible selection effects.

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

That said, our results can provide some perspective on the magnitude of this term. As shown in Table 7, the spillover effect of NCA enforceability in a border state is roughly three-quarters of the magnitude of the direct effect in a worker's focal state, our empirical analog of $\frac{d\bar{w}}{d\theta}$ from Equation 1. If our estimate of spillovers is a perfect empirical analog of $\frac{d\bar{w}^F}{d\theta}$, this comparison suggests that $\bar{w}^C - \bar{w}^F$ is *negative* (that is, earnings for workers bound by NCAs are less than earnings for workers without NCAs). On the other hand, if our spillovers analysis underestimates $\frac{d\bar{w}^F}{d\theta}$ (for example, if "true" local labor markets are smaller than Commuting Zones), then our results still leave open the possibility that $\bar{w}^C - \bar{w}^F$ is positive. Regardless, this comparison indicates that, whatever the sign of $\bar{w}^C - \bar{w}^F$, a meaningful share of the overall earnings effect of NCA enforceability is borne by workers not actually bound by NCAs.

The second implication is econometric. Our primary estimating equation (Equation 2) relies on the stable unit treatment value assumption (SUTVA): that control units—states not experiencing legal changes—do not have counterfactual earnings trajectories that are affected by treated units (states experiencing law changes). However, our spillover estimates indicate that this assumption is violated for some control units—namely, counties in control states that are located near the border of a treated state. Since the direction of contamination is the same as the direction of the main effect, this suggests that our primary specification, which includes these contaminated counties, may underestimate the earnings effect of enforceability. We examine this concern in Table B.8, which replicates Column 5 of Table 3, but restricts the sample to counties progressively further away from a state border. Excluding counties near state borders increases the magnitude of the coefficient, though the estimates also become noisier due to the decrease in the number of counties included in the sample.

6 Does NCA Enforceability Reduce Earnings By Worsening the Value of Outside Options?

According to our model, the key channel through which NCA enforceability lowers earnings is by slowing down the arrival rate of new job offers. For constrained workers, NCAs explicitly prevent workers from considering outside job offers that compete with their current employer. For unconstrained workers not bound by an NCA, Corollary A.6 demonstrates that this slowdown occurs if high enforceability leads employers to post fewer vacancies (as shown in Section 5.1). Fewer job offers mean that workers have less ability to use improvements in outside options to negotiate for higher earnings and to climb the job ladder (that is, find better-paying jobs).

In this section, we use two approaches to test whether this "outside options" channel explains the negative earnings effect of NCA enforceability. First, we show that the earnings effect of changes in NCA enforceability is largest for those workers whose outside options are most affected by changes in enforceability in their state.

Second, we show that NCA enforceability disrupts workers' ability to take advantage of tight labor markets to raise earnings.

6.1 Heterogeneous Earnings Effects Based on Workers' Outside Options

As demonstrated in the second part of Corollary A.6, if strict NCA enforceability reduces earnings by preventing workers from leveraging outside options, then changes in enforceability will have a larger effect on the earnings of workers whose set of outside options is most affected by NCA enforceability.

We consider two margins that could govern the impact of enforceability on workers' outside options: the likelihood that a worker can move across state lines, or switch occupations. The ease with which a worker can move across state lines could directly affect the outside option bite of NCA enforceability among both constrained and unconstrained workers. Because NCAs often restrict movement within a local geographic area, all else equal an NCA eliminates a smaller share of outside options for workers who are more mobile across state lines. If higher state-level NCA enforceability slows down in-state job offer arrival rates, this has less of a bite for unconstrained workers who are more mobile across state lines. Similarly, NCAs often restrict within-occupation mobility (Marx, 2011; Johnson and Lipsitz, 2019). For workers who are outwardly occupationally mobile, such limitations will be less restrictive, since a smaller portion of potential job offers are limited by the use of enforceable NCAs.

We measure variation in cross-state mobility at the industry level using the J2J data (described above in Section 4.1.1). J2J includes a variable equal to the share of job-to-job changes that are across state lines at the state-industry-year (where industry corresponds to 2-digit NAICS code). We collapse this measure to the industry level by averaging across all states for the years 2000—2006.[52] This process gives us a measure of the share of job changes that are across state lines for each 2-digit NAICS industry. One complication for our purposes is that (as shown in Table 4) the share of job changes across state lines is potentially endogenous to NCA enforceability. To partially address this issue, in some specifications we also control for each industry's incidence of NCA *use* as used in Section 4.3.2.

We measure variation in cross-occupational mobility at the occupation level using data from Schubert et al. (2021). Schubert et al. (2021) use data from 16 million resumes compiled by Burning Glass Technologies over the period 2002–2018 to construct the "occupational leave share:"[53] the share of job transitions in which a worker

[52] We choose this time-window to avoid any confounding effects from the 2007–2009 Great Recession.

[53] We are incredibly grateful to the authors, who directly provided us with the dataset on each occupation's share of job changes that are to a different occupation.

switches occupations, at the 6-digit SOC occupation level.[54]

We first consider heterogeneity in the earnings effects of NCA enforceability across industries, based on the share of job changes in each industry that are across state lines (the "cross-state leave share"). Panel (a) of Figure 5 displays this relationship graphically. The figure is a scatterplot in which the unit of observation is a 2-digit NAICS industry: on the vertical axis is the earnings effect of NCA enforceability in that industry,[55] and on the horizontal axis is the industry's share of job changes across state lines. The relationship is positive, meaning that the earnings effect of enforceability is attenuated when workers can more easily move across state lines. Column 1 of Table B.9 displays corresponding regression results:[56] a one standard deviation increase in the share of an industry's job changes that are across state lines attenuates enforceability's negative effect on earnings by 0.050 log points ($p = 0.052$), or roughly half of the main effect. Column 2 shows that this estimate is robust to also interacting NCA enforceability with each industry's NCA incidence.

We next consider heterogeneity in the earnings effect across occupations, based on the "occupational leave share." Panel (b) of Figure 5 displays a scatterplot in which the unit of observation is a 6-digit SOC occupation: on the vertical axis is the earnings effect of NCA enforceability in that occupation,[57] and on the horizontal axis is the occupation's share of job changes in which the worker switches occupations. The relationship is positive, which again demonstrates that the earnings of workers whose outside options are less affected by NCAs are less affected by enforceability. Column 3 of Table B.9 displays corresponding regression results:[58] a one SD increase in the share of an occupation's job changes that are to a different occupation attenuates enforceability's negative effect on earnings by 0.011 log points ($p < .01$), or roughly 17% of the main effect. Column 4 shows that this estimate is robust to also interacting NCA enforceability with each occupation's NCA incidence.

These analyses show remarkably consistent evidence that strict NCA enforceability has the largest effect on the earnings of workers whose outside options are most

[54]In theory, this measure could also be endogenous to NCA enforceability, for example if workers bound by NCAs are more likely to switch occupations to escape their NCA (Marx, 2011). Unfortunately, the occupational leave share measure is only measured nationally, so we cannot construct it for the state of California (like we did for industry-level cross-state job transitions.)

[55]Using the QWI dataset, we separately regress earnings on NCA enforceability for each industry, and we save the coefficient from each regression. In each regression, we include fixed effects for state, sex, age group, and year–quarter–region, and we weight observations by employment.

[56]Here, we run a single regression with an interaction term. We also normalize the "cross-state leave share" to be mean 0 and standard deviation 1 for interpretability.

[57]Using the CPS ASEC (which is required since it includes information on workers' occupations), we separately regress earnings on NCA enforceability for each occupation, and we save the coefficient from each regression. In each regression we include fixed effects for state, year–region, and we include basic demographic controls. For this plot, we restrict attention to occupations with at least 5,000 observations in our sample period, comprising roughly the most common 100 occupations.

[58]Here, we run a single regression with an interaction term. We also normalize the "cross-occupation leave share" to be mean 0 and standard deviation 1 for interpretability.

plausibly impacted by the use and stringency of NCAs in their state.

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

The results in the prior section corroborate our model's implication that strict NCA enforceability reduces earnings by slowing down workers' arrival rate of outside offers, thus interrupting an important channel of workers' overall earnings growth (Bagger et al., 2014). In this section, we consider a second way that NCA enforceability might interrupt this channel of earnings growth: by reducing workers' ability to take advantage of tight labor markets to raise their earnings.

We embed NCA enforceability in an empirical model, first used by Beaudry and DiNardo (1991), that considers how a worker's current earnings depend on prior labor market conditions. Beaudry and DiNardo (1991) (hereafter, BDN) consider a model in which firms insure workers against negative productivity shocks using implicit contracts. Their model implies that improvements in labor market conditions enable workers to bargain for higher earnings that persist through their job spell—but only if their mobility is costless (that is, they can easily switch jobs). In this case, because the worker can threaten to quit if her outside option improves, improvements in labor market conditions compel employers to raise wages. If, instead, workers' mobility is costly, they cannot credibly threaten to leave, and improvements in labor market conditions will not translate into higher earnings.

BDN develop a simple empirical test of their model. If mobility is costless, a worker's current earnings will be correlated with the most favorable labor market conditions over the course of her current job spell; if mobility is costly, her earnings will be correlated with the initial market conditions at the start of the spell. BDN find strong evidence consistent with costless mobility: the effect of the most favorable labor market conditions over a worker's job spell (measured as the minimum unemployment rate over the spell) exceeds and washes out any effect of the unemployment rate at the time of hire (predicted by an implicit contracts model with costly mobility) or the contemporaneous unemployment rate (predicted by a spot market).[59]

More recently, Hagedorn and Manovskii (2013) (hereafter, HM) propose a different explanation for why current earnings could be tied to prior labor market conditions. HM model workers' earnings as set in spot markets (in contrast with Beaudry and DiNardo (1991)). However prior labor market conditions still affect a worker's current earnings through their effect on a worker's current match quality. In favorable labor markets, workers receive many job offers and are able to climb the job ladder, enabling workers to choose a job with a higher match quality. HM show that their model rationalizes the same reduced form relationship between current earnings and

[59] Other papers in this literature have replicated this baseline result, using different datasets and time periods (e.g., Molloy et al., 2016; Schmieder and Von Wachter, 2010).

history of unemployment rates, but they provide evidence to suggest their model better explains this relationship than BDN.

While BDN and HM provide differing reasons for why prior labor market conditions matter for current earnings, they both illustrate ways that strict NCA enforceability attenuates workers' ability to take advantage of tight labor markets. By slowing down the arrival rate of job offers that workers might otherwise expect, strict NCA enforceability interrupts both channels through which tight labor markets translate to higher earnings, by preventing them from climbing the job ladder (in the spirit of HM) and by diminishing the *threat* of climbing the job ladder (in the spirit of BDN). Both of these mechanisms are important elements of earnings growth in the search model of Bagger et al. (2014).

To test this idea, we revisit the empirical model used by BDN and HM that relates a worker's earnings to prior labor market conditions. We hypothesize that when NCAs are more easily enforceable, a worker's current earnings will be less correlated with the most favorable market conditions during her job spell—and more correlated with initial labor market conditions—relative to workers in states where NCAs are less enforceable.

We begin by replicating the baseline analysis of BDN using the CPS JTS,[60] and limiting our analysis to full-time, private sector workers, for the years 1996-2014 (compared to BDN, who used the years 1976 to 1984).[61] We estimate the model:

$$\ln w_{(i,t+j,t)} = \Omega_1 X_{i,t+j} + \Omega_2 C(t,j) + \rho_{s(i,t)} + \delta_{d(i,t)t} + \varepsilon_{i,t+j}, \tag{6}$$

where $w_{(i,t+j,t)}$ is the earnings of individual i at time $t+j$ who began her job spell at time t. $X_{i,t+j}$ is a vector of individual level characteristics. Following BDN, in $X_{i,t+j}$ we include race, Hispanic status, sex, marital status, age, age squared, tenure, tenure squared, education, and industry dummies. $C(t,j)$ is a vector of unemployment rates which, depending on the model, include *Initial UR* (the unemployment rate at the beginning of the individual's job spell) and/or *Minimum UR* (the lowest unemployment rate between the beginning of the job spell and the time of measurement of earnings). Following BDN, we use annual national unemployment rates from the Bureau of Labor Statistics. $\rho_{s(i,t)}$ is a fixed effect for the state in which worker i lives in year t. $\delta_{d(i,t)t}$ is a fixed census division by year effect.[62]

This model departs in some ways from the BDN specification. 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

[60] Hagedorn and Manovskii (2013) use a similar specification to Beaudry and DiNardo (1991), though they use the National Longitudinal Survey of Youth rather than the CPS.

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

[62] BDN do not use state fixed effects; we include them to harmonize this model with our benchmark earnings models and to only use within-state variation in enforceability.

from public use extracts of CPS supplements). In their stead, we use dummy variables for metropolitan area status (as used in Equation 2). Second, we do not consider the contemporaneous unemployment rate, which is collinear with $\delta_{d(i,t)t}$. Each of these adjustments ultimately has little impact on our estimates.[63]

We report these results in Table 8. Columns 1–3 replicate the Beaudry and DiNardo (1991) main results for our sample period. In Column 1 we include only the unemployment rate at the 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 statistically significant ($p < 0.01$). Column 2 uses, instead, the minimum unemployment rate over the course of the worker's job spell (*Minimum UR*); 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 significant ($p < 0.01$). In other words, on average, prior experience with tight labor markets leads to higher current earnings—consistent with either a model of implicit contracts with costless mobility (Beaudry and DiNardo, 1991) or a model in which match quality matters for earnings (Hagedorn and Manovskii, 2013).

To test the hypothesis that NCA enforceability shuts down the ability of workers to leverage strong labor markets (via either improvements in bargaining position or moves to stronger matches), we estimate the model:

$$\ln w_{(i,t+j,t,s)} = \Omega_1 X_{i,t+j} + \Omega_2 C(t,j) + \Omega_3 Enf_{t,s} + \Omega_4 C(t,j) * Enf_{t,s} + \varepsilon_{i,t+j}, \tag{7}$$

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, or if NCA enforceability prevents workers from attaining better match quality, we expect two effects. First, we expect the coefficient on $Enf_{t,s} \times$ *Minimum UR* to be positive, indicating that employees have *less* ability to leverage favorable labor markets over the course of their job spell when NCA enforceability is high. Second, we expect the coefficient on $Enf_{t,s} \times$ *Initial UR* to be *negative*, indicating that earnings 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

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

($Enf_{t,s}$). 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.

In Column 5, we include the interactions demonstrating the change in the cost of mobility. First, consider the main effects of *Initial UR* and *Minimum UR*, which indicate the effect of initial and most favorable labor market conditions, respectively, for a state with the lowest NCA enforceability. These coefficients mirror, and amplify, the findings from BDN and HM: a higher initial unemployment rate for a worker in a low-enforcing state does not reduce her earnings today—if anything it leads to *higher* earnings—whereas the main effect of *Minimum UR* indicates that a worker's earnings today are strongly responsive to her most favorable labor market condition over her tenure. In other words, earnings in a state with low NCA enforceability are *even more* aligned with an implicit contracts model of costless mobility, or alternatively reflect a *greater* ability of workers to find high-quality matches, relative to the overall population.

Next, consider the two interaction terms, indicating the differential effects of these conditions for a worker in the highest enforcing state. The coefficient on $Enf_{t,s} \times$ *Initial UR* (-0.017; $p < 0.01$) shows that a higher unemployment rate at the time of hire affects current earnings much more negatively when NCAs are more enforceable. The coefficient on the other interaction term, $Enf_{t,s} \times$ *Minimum UR* (0.020; $p < 0.05$), shows that the most favorable labor market condition over job tenure has a much more muted effect on current earnings for workers in states with higher enforceability. Combining the main effect on *Minimum UR* with this interaction term reveals that the most favorable labor market condition over the course of tenure has essentially no effect on the earnings of a worker in a state with the highest observed enforceability ($-0.028 + 0.020 = -0.008, p = .19$).

These results provide even more evidence to support the theory that strict NCA enforceability reduces earnings by limiting workers' outside options. The increased rate of job offers that workers can expect in tight labor markets can have long-lasting positive effects on their earnings, either by increasing their bargaining power or by enabling them to switch to better matches. The estimates in Table 8, however, show that this effect is effectively shut down when NCAs are strictly enforced.

7 Heterogeneity in NCA Enforceability's Earnings Effect by Sex and Race

We have shown that strict NCA enforceability has a particularly detrimental earnings effect in industries and occupations in which state-level NCA enforceability has the largest effect on workers' outside options. Extending this logic suggests that the earnings effect of NCA enforceability may differ across demographic groups. For example, it is plausible that NCA enforceability has a larger effect on women's outside options than men's. Women tend to be less willing than women to commute far distances for their job (Le Barbanchon et al., 2019; Caldwell and Danieli, 2018), and married women are less likely to relocate in response to labor market opportunities than are married men (Jayachandran et al., 2023), both of which could be due to imbalanced household gender norms. Women are also less willing (and able) to violate NCAs than are men (Marx, 2022). These differences would imply that geographically-restrictive NCAs (or state-level enforceability changes) would have a larger effect on women's outside options than on men's. Similar differences could arise for racial minorities relative to White individuals: Black individuals are less likely to migrate far away from their hometown, and they are less likely to migrate in response to earnings increases elsewhere (Sprung-Keyser et al., 2022). Together with our model, these differences predict that NCA enforceability will cause greater earnings penalties for historically disfavored workers.

Figure 6 displays results from two regressions that add demographic group indicators, alone and interacted with NCA Score, to the regression reported in Column 1 of Table 3.[64] (Table B.10 reports the underlying regression estimates.) The coefficients reported in the Figure are on the interaction of the relevant group indicator with the *NCA Enforceability Score*, and they represent the impact of NCA enforceability on the earnings of individuals in that group. We report coefficients from two models: our main estimate and a second model that includes interactions between the *NCA Enforceability Score* and indicators for college-educated, high-NCA-use occupations, and high-NCA-use industries, alone and interacted with *NCA Enforceability Score*, in order to account for the fact that workers in different demographic groups may hold different jobs and have different education levels, on average.

The figure reveals meaningful heterogeneity in the earnings effect across demographic groups. In the baseline model the estimates are negative and statistically significant for all demographic groups; however, the magnitudes of earnings effects for Black men and other female minority workers are 94% and 145% larger, respectively, than the effect for White men.[65] A test of equality of the earnings effects across all six groups is strongly rejected ($p < 0.001$). These differences persist in the regression specification with additional controls—the test of equality in coefficients yields a p-value below 0.001.[66]

[64] We make two additional modifications to the regression specification. 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, though the results do not meaningfully change if we reimpose the full-time restriction. Second, we include more detailed (interacted) demographic categories in the model.

[65] The p-values of pairwise comparisons reported in Figure 6 are Bonferroni-corrected to account for five pairwise comparisons.

[66] We note that our results do not accord with a model in which the penalties faced by non-White workers and women are additive; this pattern has been observed in other work on racial and gender earnings gaps (Paul et al., 2022).

These results suggest that strict NCA enforceability not only reduces earnings *on average*, but it also exacerbates existing disparities across demographic groups. In Column 2 of Appendix Table B.10 we show that these coefficients imply that moving from the 25^{th} to 75^{th} percentile of the NCA Score distribution would decrease average earnings of white men by approximately 1.3%, vs. decreases ranging from 1.5% to 3.2% for the other demographic groups. Together with the estimates in Column 1, these results imply that if a state that enforces NCAs at the 75^{th} percentile of the distribution were to switch to enforcing NCAs at the 25^{th} percentile of the distribution, the earnings gap between white men and each other demographic group would close by 1.5% for nonblack, nonwhite men, 1.9% for black women, 2.3% for white women, 3.6% for black men, and 3.8% for nonblack, nonwhite women.

Of course, we cannot say conclusively that the disparate impacts of NCA enforceability by sex and race arise from differential impacts on outside options. Still, these results do provide further (albeit indirect) evidence that our model has explanatory power for understanding the mechanism through which strict NCA enforceability reduces earnings. A promising avenue for future research would be to more comprehensively examine the ways in which NCAs differentially impact workers of different demographic groups.

8 Comparison to Prior Studies: How Generalizable Are the Earnings Effects of NCA Enforceability?

Ours is not the first paper to consider the earnings effect of NCAs and NCA enforceability. Prior work on this topic has considered the effects of NCA use and/or enforceability for specific subsets of workers or subsets of law changes. Relative to this important work, our paper provides the first estimates of earnings effects of NCA enforceability for a broad, representative sample of the US labor force using all law changes over a 24-year period. We also connect our empirical analysis to a theoretical model, which both helps interpret the reduced form effect of NCA enforceability on earnings and implies sources of heterogeneity in those effects. Collectively, these features of our paper allow us to revisit these prior studies, some of which find facially contrasting results.

First, our paper helps make sense of seemingly conflicting findings on the effects of NCA *use* versus NCA *enforceability*. Prior work tends to find that NCA use has either no association or a positive association with earnings (Balasubramanian et al., 2023; Lavetti et al., 2018; Starr and Rothstein, 2022; Starr et al., 2021). In contrast, studies of enforceability of NCAs (including ours) tend to find negative impacts on earnings

(Lipsitz and Starr, 2021; Balasubramanian et al., 2022; Garmaise, 2011).[67] Our paper rationalizes these disparate findings. Our model shows that the effect of increasing enforceability on earnings is the sum of two terms: the difference in earnings between workers who do and do not sign enforceable NCAs (which we show can be positive or negative), and the spillover effect on non-signers (which we show theoretically and empirically is unambiguously negative).[68] Thus, our model provides an explanation for why there could be positive/null earnings effects of use and negative earnings effects of enforceability.[69]

Second, our paper can help rationalize heterogeneity in the estimated earnings impacts of NCA enforceability among existing studies. For example, Lipsitz and Starr (2021) find a 2-3% earnings effect of a ban on NCAs for low-wage workers in Oregon, while Balasubramanian et al. (2022) find a 4-5% earnings impact of a ban on NCAs for high-tech workers in Hawaii. Our model suggests that the differences in the magnitudes of these effects could be due to disparities in the outside options of workers in these different segments of the labor force. In Section 6.1, we find that workers whose outside options are most impacted by NCA enforceability (for example, because NCAs typically cover specific locations, occupations, or industries) are those whose earnings are most affected by changes in enforceability. There is evidence that low-wage workers are more mobile across industries than are high-wage workers, perhaps due to differences in the industry-specificity of human capital.[70] By comparison, high-tech workers may have skills that are more industry-specific, meaning their outside options would be more affected by NCA use and enforceability. [71] At a more extreme tail of the labor market, Garmaise (2011) estimates that CEOs at large publicly-traded US firms have 8.2% lower earnings growth under stricter NCA enforceability. This especially large earnings effect is consistent with CEOs having

[67]An exception is (Young, 2021), who finds that an NCA ban in Austria for low-wage workers had a limited effect on earnings.

[68]This insight is particularly useful for interpreting the results from Kini et al. (2019), who estimate the interaction effect of NCA enforceability and NCA use on CEO earnings. They find a *positive* effect of this interaction term (suggesting CEOs with enforceable NCAs get an earnings premium) but a *negative* effect on the main effect of enforceability, which is consistent with negative spillovers. See Table 7, Column 1 of that paper.

[69]Another potential explanation for these differences is that the correlation between NCA use and earnings may not reflect a causal effect, since factors such as access to proprietary knowledge may simultaneously contribute to the use of NCAs and higher earnings. See Starr and Rothstein (2022) for a deeper discussion of this point.

[70]Figure 1 of Lipsitz and Starr (2021) shows that workers in lower earnings brackets are much more likely to change industries than are workers in higher brackets.

[71]At the same time, high-tech workers might be more mobile across state lines than the typical worker, enabling them to escape increases in NCA enforceability in their origin state, which could explain why the 4–5% earnings increase from the Hawaii ban from Balasubramanian et al. (2022) is smaller than our implied overall earnings increase from a nationwide NCA ban (8.7%). Indeed, in the J2J data, the share of job changes that are across state lines in NAICS code 51 (which contains several high-tech industries based on Balasubramanian et al. (2022)'s definition) is 20%, compared to 15% across all other sectors.

substantially lower outside-occupation mobility than other occupations (which the data from Schubert et al. (2021) shows is the case).

Finally, our paper offers the most comprehensive understanding of the labor market effects of NCA enforceability to date. We show that the effect on earnings is negative for a wide range of states (as displayed in Figure B.3), implying that the negative effects in prior case studies are not aberrations. At the same time, we show substantial heterogeneity in the earnings effects across industries and occupations—something not feasible to estimate in a single case study. These analyses can inform which groups are likely to be most affected by ongoing policy discussions to restrict or ban NCAs. Finally, we offer (and provide evidence for) a theoretical channel through which NCA enforceability affects earnings; this extends prior work that has, for example, referenced the role of worker mobility but has been unable to explicitly test why lower mobility would translate to lower earnings.

9 Conclusion

Using newly-assembled panel data on state-level NCA enforceability, we show that stricter NCA enforceability leads to a decline in workers' earnings and mobility. The earnings effect of NCA enforceability extends across legal jurisdictions, illustrating that NCA enforceability has far-reaching consequences on labor market outcomes that likely extend far beyond the subset of workers that actually sign NCAs. Multiple sources of evidence indicate that strict enforceability reduces earnings by dampening workers' outside options, shutting down a primary way that workers can otherwise attain higher pay over the course of their careers. Finally, strict enforceability has an especially negative effect on the earnings of women and racial minorities and thus exacerbates existing disparities in the labor market.

Our results also inform a longstanding debate regarding freedom of contract. An argument frequently cited in this debate is that workers would not sign NCAs if they were made worse off by doing so. Evidence that workers sign NCAs either unwittingly or after they have any chance to bargain over them (Marx, 2011) already casts doubt on this argument. Our findings that NCAs create negative *market-level* externalities provide a further challenge to this argument.

Our findings suggest several avenues for future research. An important question is how incomplete markets interact with workers' willingness to sign NCAs: for example, liquidity-constrained workers might sign NCAs that are damaging to their lifetime earnings if they are unable to alternatively accept an initial earnings cut to pay for training or other human capital investment; in this case, NCA enforceability might exacerbate inequality between high- and low-wealth individuals. The earnings effects of NCA enforceability might also interact with unionization and other labor market institutions. Finally, given our findings that strict NCA enforceability reduces the extent to which strong labor markets translate into higher earnings, it is possible that increases in NCA enforceability (or in NCA use) have contributed to the decline in the labor share of income over the past several decades.

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10 Tables and Figures

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

Region	Northeast	Midwest	South	West	Total
Average Index	0.75	0.79	0.76	0.40	0.69
Standard Deviation of Index	0.10	0.12	0.13	0.35	0.25
Maximum Index	0.97	0.97	1.00	0.91	1.00
Minimum Index	0.63	0.00	0.47	0.07	0.00
Number of Law Changes	15	19	23	16	73
Number of States in Region	9	12	17	13	51
Number of Index Increases	11	14	13	9	47
Number of Index Decreases	4	5	10	7	26
Average Magnitude Positive Index Change	0.03	0.05	0.08	0.05	0.05
Maximum Positive Index Change	0.15	0.11	0.24	0.19	0.24
Average Magnitude Negative Index Change	-0.05	-0.03	-0.04	-0.02	-0.04
Maximum Negative Index Change	-0.06	-0.06	-0.17	-0.09	-0.17
Between-State Standard Deviation	0.09	0.25	0.12	0.22	0.18
Within-State Standard Deviation	0.03	0.03	0.04	0.03	0.03

Notes: Statistics in the table represent data from 1991–2014, and the unit of observation is a state-year. The minimum and maximum of the NCA Score are normalized to 0 and 1, respectively. With the exception of the numbers of law changes, states, index increases, and index decreases, the descriptive statistics in Table 1 are weighted to reflect population demographics by matching the scores from each state-year to corresponding observations in the CPS ASEC and using the relevant weights provided by the Census Bureau

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

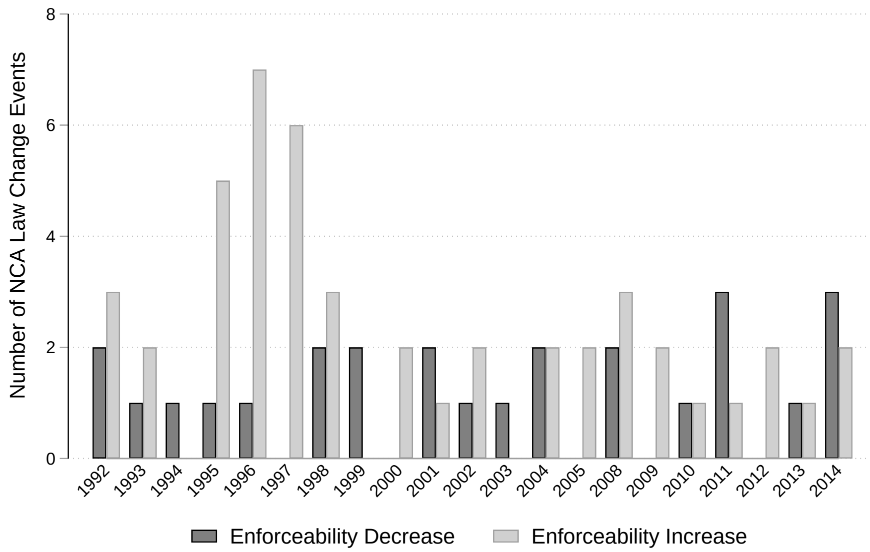
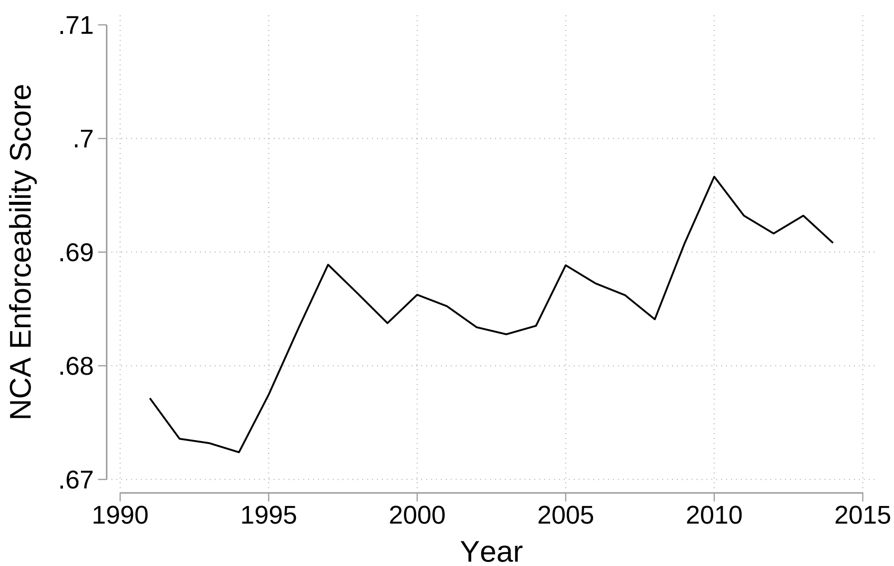


Figure 2: Average NCA Enforceability Score from 1991 to 2014



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

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

Dependent Variable:	NCA Enforceability	
Population (100,000s)	–0.00	(0.00)
Unemployment Rate	0.00	(0.00)
Number of Workers Compensation Beneficiaries	–0.00	(0.00)
Democratic Party Governor	–0.01	(0.00)
% of State House from Democratic Party	0.03	(0.06)
% of State Senate from Democratic Party	0.05	(0.03)
State Minimum Wage	–0.01*	(0.01)
Number of Medicaid Beneficiaries (100,000s)	0.00	(0.00)
Social Policy Liberalism Score	–0.01	(0.02)
Economic Policy Liberalism Score	–0.02	(0.01)
Social Mass Liberalism Score	0.00	(0.02)
Economic Mass Liberalism Score	0.04	(0.04)
Democratic Party ID Count	–0.07	(0.31)
State House Ideology Score	–0.00	(0.01)
State Senate Ideology Score	0.01	(0.01)
House Democrats Ideology Score	–0.05	(0.04)
House Republicans Ideology Score	0.02	(0.05)
Senate Democrats Ideology Score	–0.04**	(0.02)
Senate Republicans Ideology Score	–0.00	(0.02)
Union Membership	–0.00	(0.00)
N	829	
R^2	0.114	
F-Test p-Value	0.197	

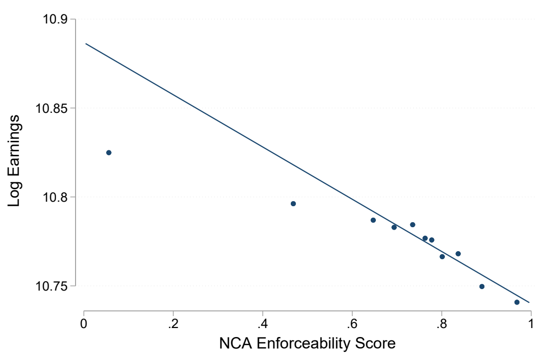
Notes: Models also include state and year fixed effects. Reported R^2 calculated after residualizing on state and year fixed effects. Standard errors reported in parentheses are clustered by state. *** p<0.01, ** p<0.05, * p<0.1

Table 3: The Effect of NCA Enforceability on Earnings

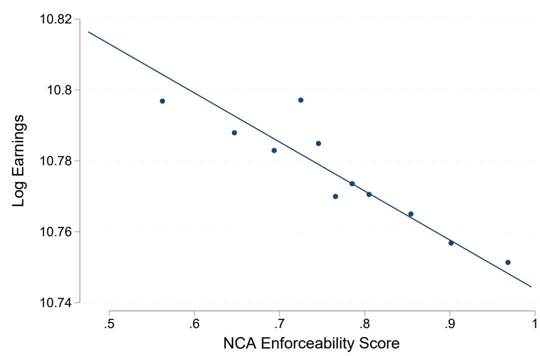
	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.118***	-0.107***	-0.021	-0.106***	-0.137***
	(0.036)	(0.028)	(0.017)	(0.027)	(0.034)
Observations	1216726	1216726	1545874	1216726	3548827
R^2	0.275	0.357	0.132	0.346	0.941
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

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

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



(a) Full Joint Distribution



(b) Joint Distribution Excluding CA & ND

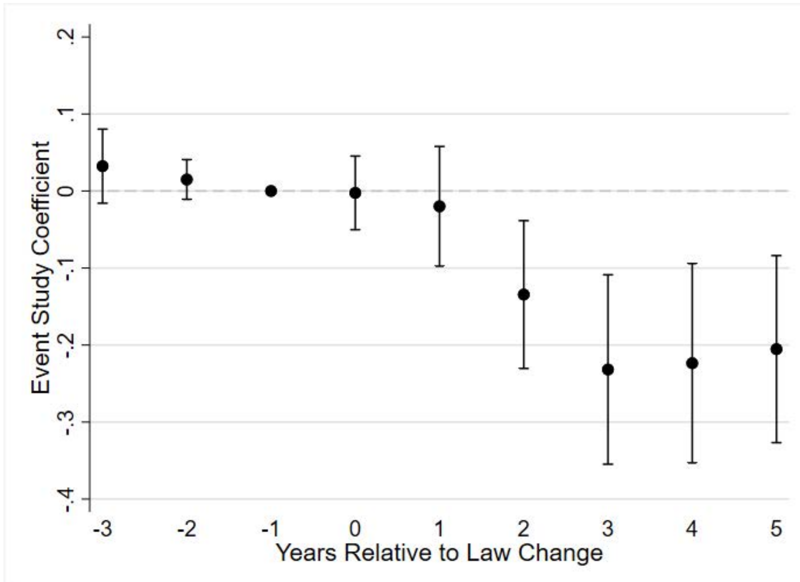
Figures are binned scatterplots depicting the conditional joint distribution of NCA enforceability and log annual earnings, controlling for the same variables included in Column 2 of Table 3 (fixed state effects, census division-by-year effects, 1-digit occupation effects, age, age-squared, and indicators for white, Hispanic, male, less than college education, and metro area status.) Conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2023). Panel (a) includes all states and years, panel (b) excludes California and North Dakota to visually focus on the main sources of identifying variation that we use for estimation.

Table 4: The Effects of NCA Enforceability on Job Mobility

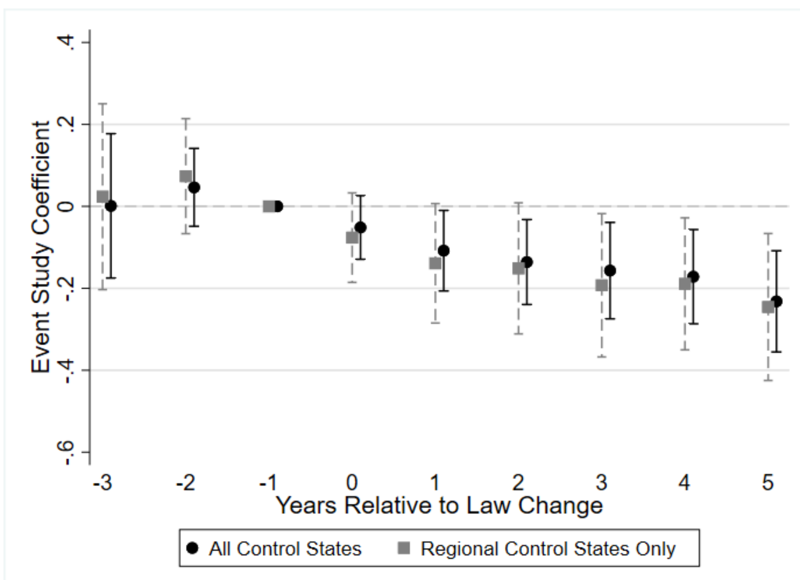
	All J2J Separations		Across Ind.	Within Ind.	Across State	Within State
	(1)	(2)	(3)	(4)	(5)	(6)
NCA Enforceability Score	0.064	0.112	0.102	0.121	-0.008	0.130
	(0.114)	(0.108)	(0.127)	(0.089)	(0.070)	(0.120)
High NCA Use Ind × NCA Score		-0.241***	-0.122	-0.380***	-0.058	-0.270**
		(0.085)	(0.089)	(0.109)	(0.126)	(0.110)
Observations	652024	652024	651664	619283	638444	650404
Mean Dep Var	1,421.69	1,421.69	794.65	627.60	165.38	1,256.38

Estimates are Poisson pseudo-likelihood coefficients from a model using LEHD Job-to-Job flows data from 1991-2014. Each observation is a state-sex-age group-quarter-industry cell. All regressions include controls for sex, age group, and fixed state-by-origin-industry effects and census-division-by-origin-industry-by-year-by-quarter effects. Regressions are weighted by employment, and standard errors are clustered by state. ***P<.01, **P<.05, *P<.1.

Figure 4: Dynamic Effects of NCA Enforceability Changes on Earnings from Two Different Models



(a) Distributed Lag Model



(b) Stacked Event Study

The graphs plot two estimates of the dynamic effects of NCA law changes on earnings, from a distributed lag model (Panel A), and a stacked event study model (Panel B). Both regressions use data from QWI. See Section 4.2.1 for the regression equations and further details. The coefficients represent the effect of an NCA law change that occurred j years ago ($j \in \{-4, 5\}$) on log earnings. The coefficient representing one year prior to law change is normalized to zero. In Panel A, the dependent variable is the yearly change in the log average earnings in a county-group; in Panel B the dependent variable is the log average earnings in a county-group. Standard errors are clustered by state.

Table 5: Heterogeneous Effects of NCA Enforceability on Earnings by Education, Occupation, and Industry

	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.118***	-0.038	-0.085**	-0.097***	-0.033
	(0.036)	(0.040)	(0.035)	(0.035)	(0.038)
College Educated Worker	0.415***	0.510***	0.376***	0.391***	0.442***
	(0.013)	(0.020)	(0.012)	(0.010)	(0.014)
College Educated Worker × NCA Score		-0.138***			-0.118***
		(0.030)			(0.022)
High NCA Use Occ × NCA Score			-0.059***		-0.015*
			(0.014)		(0.008)
High NCA Use Occ			0.254***		0.194***
			(0.007)		(0.005)
High NCA Use Ind × NCA Score				-0.065***	-0.035***
				(0.013)	(0.010)
High NCA Use Ind				0.267***	0.219***
				(0.008)	(0.007)
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, and individual controls for male, white, Hispanic, age, age squared, whether the individual did not complete college, and indicators for the metropolitan city center status of where the individual lives. In Columns (3) and (4), High NCA Use Occupations are occupations with NCA use greater than the national average, as tabulated by Starr et al. (2021).
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Table 6: The Effects of NCA Enforceability on Job Openings

	Unemployed People Per Job Opening (1)	Job Openings (2)
NCA Enforceability Score	1.783*	-0.225
	(1.045)	(0.233)
Observations	8568	8568
R^2	0.922	0.9308
Estimation Methodology	OLS	Poisson

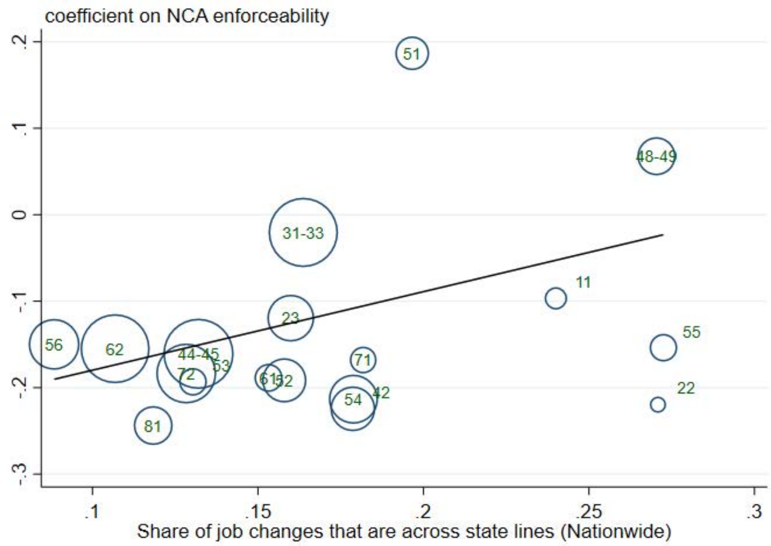
Estimates are OLS or Poisson pseudo-likelihood coefficients from a model using BLS JOLTS data from 2001-2014. Each observation is a state-year-month cell. All regressions include fixed state and census-division-by-year-by-month effects. Regressions are weighted by employment, and standard errors are clustered by state. ***P<.01, **P<.05, *P<.1.

Table 7: The External Effects of NCA Enforceability on Earnings

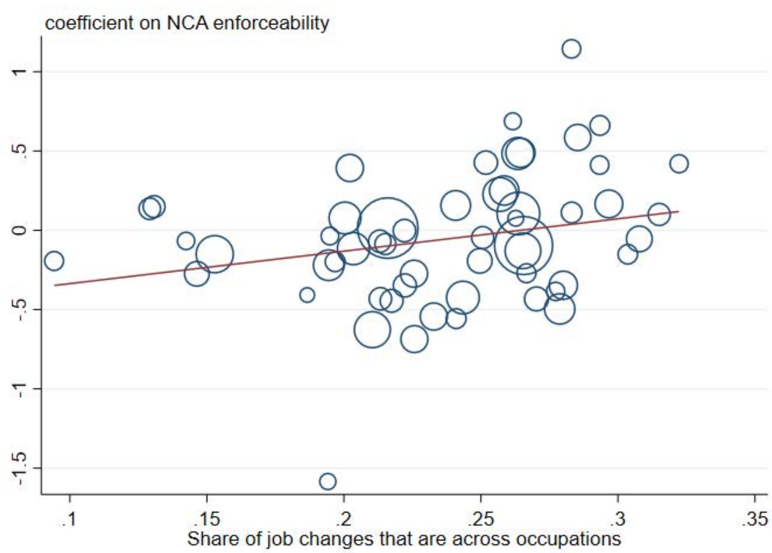
	(1)	(2)	(3)
Own State NCA Score	-0.160***	-0.181***	-0.161**
	(0.058)	(0.066)	(0.069)
Donor State NCA Score		-0.137*	-0.167**
		(0.071)	(0.075)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.110
			(0.150)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.157***
			(0.054)
Observations	615191	615191	613762
R^2	0.899	0.899	0.902

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

Figure 5: NCA Enforceability Has a Larger Effect on Earnings When it Has a Bigger Impact on Workers' Outside options



(a) Industry-level cross-state mobility [QWI]



(b) Occupation-level cross-occupation mobility [CPS]

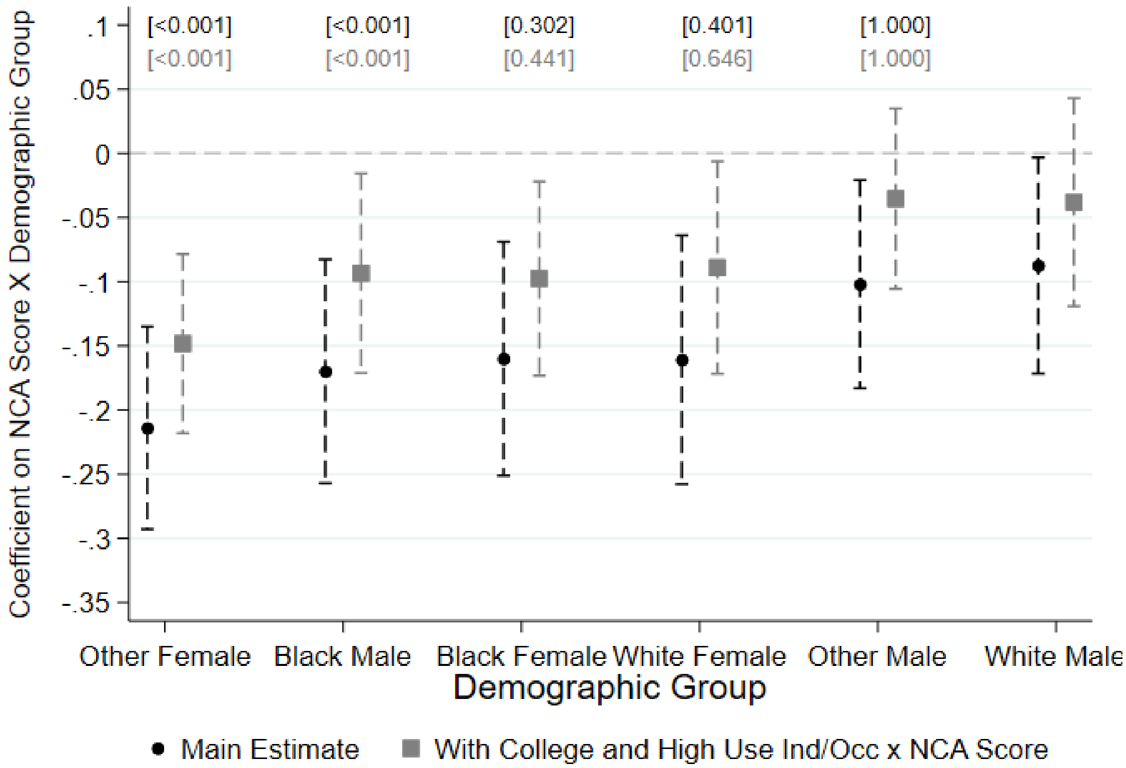
Each figure is a scatterplot relating the earnings effect of NCA enforceability against the "bite" of enforceability on workers' outside options, using two dimensions of this "bite." In Panel (a), a unit of observation is a 2-digit NAICS industry: on the vertical axis is the earnings effect of NCA enforceability in that industry (estimated using the QWI dataset) and on the horizontal axis is the share of job transitions in that industry that are across state lines (measuring using the J2J dataset). In Panel (b), a unit of observation is a 6-digit SOC occupation: on the vertical axis is the earnings effect of NCA enforceability in that occupation (estimated using the CPS ASEC dataset) and on the horizontal axis is the share of job transitions in that occupation that to different occuaptions (based on data from Schubert et al. (2021)). See Section 6.1 for details.

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

	Log Earnings				
	(1)	(2)	(3)	(4)	(5)
Initial UR	-0.008***		-0.002	-0.002	0.010**
	(0.002)		(0.003)	(0.003)	(0.004)
Minimum UR		-0.017***	-0.014***	-0.014***	-0.028***
		(0.003)	(0.005)	(0.005)	(0.006)
Initial NCA Score				-0.013	-0.033
				(0.059)	(0.074)
Init. NCA Score × Init. UR					-0.017***
					(0.006)
Init. NCA Score × Min. UR					0.020**
					(0.009)
No. Obs.	76350	76350	76350	76350	76350
R^2	0.364	0.364	0.364	0.364	0.364

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

Figure 6: Heterogeneous Effects of NCA Enforceability on Earnings by Race and Sex



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

A Formalization of Theory

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

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

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

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

We selected this model as a baseline due to the harmony between the drivers of earnings growth in the model and the channels through which NCAs could affect earnings that we discussed in Section 2. In the baseline model, workers' earnings growth occurs because of growth in their human capital, h_t , and their ability to search for higher-paying jobs. These two mechanisms for earnings growth match well to potential roles for NCAs. First, NCAs are typically justified as a solution to a hold-up problem, where firms are not willing to invest in workers' human (or other) capital (e.g., training, imparting trade secrets, client lists, etc.) for fear that the worker will depart the firm and therefore deny the firm its return on investment.[72] Therefore, an NCA in this model should cause h_t to grow at a greater rate, as the firm is more willing to invest in the worker. Second, NCAs prevent workers from changing jobs or threatening to change jobs, meaning that workers will not be able to increase earnings by moving to a firm offering higher earnings, or by leveraging an outside offer to increase their earnings at their current firm. The tradeoff between these two competing mechanisms will partially determine the difference in the rates of earnings growth with and without an NCA for the worker.

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

We hypothesize that NCAs and NCA enforceability impact labor markets through three primary channels: first, via the offer arrival rates, second, via human capital accumulation, and third, via the ability of constrained workers to change jobs (and, similarly, to use the threat of changing jobs in earnings bargaining). We model NCA enforceability as an exogenous parameter, θ, which may be viewed as the probability that a randomly selected NCA will be enforced (therefore, $\theta \in [0, 1]$).

The first modification we make is that workers with enforceable NCAs are unable to change jobs. We let workers sign NCAs with exogenous probability γ when they commence their first employment relationship, which are enforceable with probability θ. The offer arrival rate of new jobs for employed workers with NCAs is zero, or $\lambda_1^C = 0$, where C indicates that the worker is *constrained* by an enforceable NCA.[73] This modification means that if a worker has an enforceable NCA, they will continue to work for the same employer unless they experience an exogenous separation.[74] Though assuming that NCAs strictly prohibit job changing is a simplification (because, for example, workers may be able to buy out of NCAs or can move to firms in different industries or geographic locations), this assumption substantially improves tractability and does not change the predictions of the model, assuming the friction to job switching is great enough. We could instead model NCAs as introducing a cost

[72] One reason that enforceable NCAs might raise investment is due to incomplete markets: namely, that liquidity-constrained workers cannot "pay" for general human capital training in the form of lower initial earnings, but they *can* sign an NCA. See (Rubin and Shedd, 1981) for more discussion on this topic.

[73] The superscript C and F will be used frequently to differentiate functions and parameters that differ between signers (constrained workers) and non-signers (free workers).

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

on job switching. In the limit, if the cost is steep enough to limit job changes, this is identical to assuming that the worker is unable to change jobs.

The second modification we make is assuming that the offer arrival rate for workers without NCAs is lower when NCA enforceability is stricter (θ is larger). One plausible foundation for this assumption is that, when enforceability is nonexistent, firms can be sure that a worker to whom they offer a job will be unencumbered by an NCA. However, when enforceability is strict, firms may worry that they will ultimately have to pay high costs to buy workers out of their NCA (see, e.g., Shi (2023)) or that the worker ultimately will not be able to work for the offering firm. This higher expected cost or greater uncertainty effectively raises the recruitment cost to the firm, reducing the rate at which firms are willing to make offers (see Starr et al. (2019)). Whether or not this foundation is exactly accurate, the relationship between NCA enforceability and job posting is empirically testable: indeed, we find in Section 5 that NCA enforceability causes lower rates of *vacancy posting* (which, notably, does not simply affect workers bound by NCAs) and higher ratios of unemployed workers to vacancies. These results directly underpin this modification to the model.

Specifically, we allow the offer arrival rate for employed workers without enforceable NCAs (workers who are *free* to move), λ_1^F, to vary with θ. We assume that $\frac{d\lambda_1^F(\theta)}{d\theta} < 0$: the more strictly NCAs are enforced in the labor market, the less often workers will be contacted on-the-job.

The final modification we make is to assume that workers with enforceable NCAs accumulate human capital at a faster rate. In Bagger et al. (2014), accumulation of human capital, h_t, is stochastic, with the deterministic component of workers' human capital at time t represented by $g(t)$. Here, we define $g^C(t)$ and $g^F(t)$ to be the deterministic component of, respectively, a constrained and free worker's human capital at time t. Since human capital evolves faster for those with NCAs, if $g^C(t-1) = g^F(t-1)$, then $g^C(t) > g^F(t)$. This assumption is a natural implication of the argument that NCAs solve a hold-up problem. Firms might be unwilling to invest in human capital of workers who can freely leave, because they do not expect to recoup the returns on their investment. NCAs, by ensuring that workers cannot freely leave, incentivize firms to invest in workers, causing workers' human capital to develop more rapidly.[75]

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

[75]Rubin and Shedd (1981) formalize this argument in a model of incomplete markets, in which liquidity-constrained workers cannot "pay" for general skills training in the form of lower initial earnings, so signing NCAs is an alternative way to facilitate such training that would not otherwise occur.

A.3 Effects of Enforceability on Average Earnings

First, we examine what happens to average earnings when NCAs become more easily enforceable (that is, when enforceability becomes "stricter"). Earnings depend on human capital (which develops more rapidly for workers with enforceable NCAs) and on mobility (which is is lower when NCAs are more easily enforceable). This tension generates the ambiguous effect of (enforceable) NCAs on earnings.

Since we do not observe NCA use, our empirical investigation focuses on average earnings (across enforceable NCA signers and non-signers). For notational simplicity, we define $\bar{w}_t^k \equiv E[w_{i,t}|j(i) = k]$ for $k \in \{C, F\}$, where $j(i)$ denotes whether worker i is constrained by an enforceable NCA or free to change jobs. These values represent average earnings, at time t, for the two respective types of workers. Thus, the average earnings in period t, which we denote $\bar{w}_t$, is given by $\bar{w}_t = \theta\gamma\bar{w}_t^C + (1 - \theta\gamma)\bar{w}_t^F$.[76] The value $\theta\gamma$ is the probability that the worker is bound by an enforceable NCA (the product of the probability of having an NCA, γ, and the probability that it is enforceable, θ).

The quantity we are therefore interested in computing is $\frac{d\bar{w}}{d\theta}$: the change in average earnings which results from a change in NCA enforceability. Omission of the subscript, t, indicates that we are interested in the derivative of average earnings in steady state. Taking the derivative and rearranging, this quantity has three components:

$$\frac{d\bar{w}}{d\theta} = \gamma(\bar{w}^C - \bar{w}^F) + \theta\gamma\frac{d\bar{w}^C}{d\theta} + (1 - \theta\gamma)\frac{d\bar{w}^F}{d\theta} \tag{8}$$

We consider each component in turn.

A.3.1 Difference in Average Earnings

We begin with $\gamma(\bar{w}^C - \bar{w}^F)$. Intuitively, this term captures the additional weight put on earnings of workers subject to enforceable NCAs in overall average earnings. As θ rises, more workers are subject to enforceable NCAs, and the overall average is pushed closer to average earnings for constrained workers, $\bar{w}^C$.

As in Bagger et al. (2014), with our modifications, the earnings of worker i at any time t is given by $w_{i,t} = \alpha_i + g^{j(i)}(t) + \varepsilon_{i,t} + p_{i,t} + r$, where α_i is a worker heterogeneity parameter, $g^{j(i)}(t)$ is the deterministic component of human capital accumulation of the worker, and $\varepsilon_{i,t}$ is a stochastic worker human capital shock. Firm productivity, $p_{i,t}$ (where i represents the worker and t represents time), and r (the piece rate of the worker) round out earnings.

In order to calculate the difference in earnings across workers with and without enforceable NCAs, we compare the individual components of earnings. By assump-

[76] Note that flow balance into and out of unemployment implies that an identical proportion of C and F type workers are employed in steady state, and we therefore may omit that proportion in calculation of average earnings.

tion, ε is distributed identically across workers and across time, and α is distributed identically across workers, so in expectation, there are no differences in ε or α for workers with and without enforceable NCAs.

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

What is left to compare are firm productivities and the piece rates of workers. Workers with NCAs will face a worse (i.e., first order stochastically dominated) distribution of firm productivities because they are unable to search for higher-paying jobs—*i.e.* they are unable to climb the job ladder. In fact, since they are immobile and exit occurs independently of firm productivity, the distribution of productivities at firms at which NCA-constrained workers are employed (denoted by $L^C(p)$) is exactly equal to the exogenous productivity distribution for a worker entering employment: $L^C(p) = F(p)$.[77]

The steady state distribution for those who do not have enforceable NCAs is derived in Bagger et al. (2014) (equation A15): $L^F(p) = \frac{(\mu+\delta)F(p)}{\mu+\delta+\lambda_1(\theta)\bar{F}(p)}$, where $\bar{F}(p) = 1 - F(p)$. Since workers only move *up* the job ladder, $L^F(p)$ first-order stochastically dominates $L^C(p)$, regardless of the value of θ. Note that, since $\lambda_1'(\theta) < 0$ by assumption, as enforceability becomes stricter, the distribution of firm productivities shifts leftwards (i.e., $\frac{dL^F(p)}{d\theta} \geq 0 \ \forall p$).

Finally, we turn to piece rates. Piece rates for workers without enforceable NCAs evolve identically to those in the baseline model of Bagger et al. (2014). However, the piece rate for enforceable NCA signers does not evolve over time: lacking the ability to change the piece rate by leveraging outside offers or engaging in job-to-job mobility, the piece rate for a worker with an NCA is determined at the advent of their job spell.

In Bagger et al. (2014), the piece rate (r) is a function of the most recent firm from which the worker was able to, or would have been able to, extract all available surplus (by virtue of having a high enough competing offer)[78]:

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

[77] We note that an alternate modeling assumption would be that NCAs directly affect the productivity distribution of firms. For example, strict NCA enforceability could directly reduce productivity, as might be suggested by work showing that firms are less innovative when NCAs are more enforceable (Johnson et al., 2023). One concern might be that this assumption generates dynamics in *average* wages that are similar to the effects of enforceability on average earnings that we present in Section 4, making it hard to disentangle whether our proposed mechanism or this alternative assumption drives these empirical results. However, this alternative assumption cannot explain other results, such as those in Sections 5 and 6.2 that show heterogeneous earnings effects, which *can* be explained by our own modeling assumptions.

[78] Note that the piece rate is negative: earnings are given by $w_t = r + p + h_t$, where $p + h_t$ is the marginal product of the worker (p is the firm's productivity and h_t is the worker's productivity due to human capital accumulation). Therefore, the piece rate r represents the share of the worker's productivity that is allocated to the firm.

where $\phi(x,\theta) = (1-\beta)\frac{\rho+\delta+\mu+\lambda_1(\theta)\bar{F}(x)}{\rho+\delta+\mu+\lambda_1(\theta)\beta\bar{F}(x)}$, $\bar{F}(x) = 1 - F(x)$ is the exogenous distribution of firm productivities from which workers draw upon matching with a firm, and $q_{i,t}$ represents the productivity of the last firm from which the worker was able to extract all surplus, by virtue of leveraging a competing offer (see Equation 6 in Bagger et al. (2014) for details on the derivation of this equation). The greater is $q_{i,t}$, the greater the worker's earnings will be. If $q_{i,t} = p_{i,t}$, then the worker was able to extract all surplus from their current firm and therefore $r = 0$: they return none of the full value of productivity to the employer.

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

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

The earnings processes of signers of enforceable NCAs versus nonsigners are therefore given by:

$$\begin{aligned} &\text{Nonsigners: } w_{i,t}^F = \alpha_i + g^F(t) + \varepsilon_{i,t} + p_{i,t} - \int_{q_{i,t}}^{p_{i,t}} \phi(x,\theta)dx \\ &\text{Signers: } w_{i,t}^C = \alpha_i + g^C(t) + \varepsilon_{i,t} + p_{i,t} - (p_{i,t}-p_{min})(1-\beta) \end{aligned} \tag{9}$$

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

Proposition A.1. *In steady state, workers signing enforceable NCAs will receive higher initial earnings in expectation than workers not signing NCAs: for i that transition from unemployment to work in period t,* $E_{i,t-1}[w_{i,t}|j(i) = C] > E_{i,t-1}[w_{i,t}|j(i) = F]$.

Proof. In the first period in which workers match, the firm productivity distributions are identical (since workers have not had a chance to switch jobs). In expectation, α_i and $\varepsilon_{i,t}$ are identical for those with and without NCAs. By assumption, $E_{t-1}[g^C(t)] > E_{t-1}[g^F(t)]$, so the proposition is proven if

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

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

Rewriting the left hand side, we must show that

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

which is true since $\phi(x,\theta) > (1-\beta) > 0$, regardless of the value of θ. □

The proof of this proposition highlights two reasons for greater (initial) pay with enforceable NCAs: first, a greater accumulation of human capital leading to greater productivity, and second, the compensating differential associated with NCAs (which is embedded in $\phi(x,\theta)$). Workers who initially match with NCAs are compensated to some extent for their limited future mobility.

However, as workers remain at their jobs longer, three things happen: first, workers with enforceable NCAs accumulate more human capital. Second, workers without enforceable NCAs climb the job ladder, moving to jobs with greater firm productivities, $p_{i,t}$. Third, when workers without enforceable NCAs leverage outside offers, they negotiate better piece rates, r. The first increases earnings by more for those who sign enforceable NCAs, while the latter two increase earnings by more for those who do not sign enforceable NCAs. The overall comparison, then, is indeterminate: if human capital grows more quickly than mobile workers climb the job ladder and negotiate better piece rates, workers with NCAs will have earnings that grow more quickly than those without, and vice versa. We summarize in Proposition A.2, but first introduce the condition used in the proposition. The condition states that the growth rate of human capital is lower than the growth rate of the lost ability of the worker to bargain for higher earnings. Ultimately, the goal of the proposition is to show that there is a direct tradeoff between human capital growth and job mobility which governs earnings dynamics.

Condition 1.

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

Proposition A.2. *Suppose worker i has an enforceable NCA and worker k does not. Conditional on remaining employed and experiencing identical shocks in period t (i.e., $\varepsilon_{i,t} = \varepsilon_{k,t}$), in steady state, expected earnings growth is faster for k than for i under Condition 1: i.e., $E_t[w_{i,t+1}] - w_{i,t} < E_t[w_{k,t+1}] - w_{k,t}$ whenever Condition 1 holds, and $E_t[w_{i_t+1}] - w_{i,t} > E_t[w_{k,t+1}] - w_{k,t}$ when it does not.*

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

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

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

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

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

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

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

Averaging over workers in the population, Propositions A.1 and A.2 immediately generates an indeterminacy with respect to the overall rank ordering of average earnings. When Condition 1 does not hold, average *initial* earnings are greater for workers with enforceable NCAs and earnings growth is faster for workers with enforceable

NCAs, meaning that average earnings for workers with enforceable NCAs are greater than for those without. However, when Condition 1 holds, greater earnings growth for workers without enforceable NCAs may overtake greater initial earnings for workers with enforceable NCAs, leading to the possibility that average earnings are greater for workers without enforceable NCAs.

Corollary A.3. *Condition 1 is necessary, but not sufficient, for* $\bar{w}_t^F > \bar{w}_t^C$.

A.3.2 Effects on Average Earnings for Constrained and Free Workers

The impact of θ on $\bar{w}_t^C$ is straightforward:

Proposition A.4. $\frac{d\bar{w}_t^C}{d\theta} = 0$

Proof. Using Equation 9:

$$\frac{d\bar{w}_t^C}{d\theta} = \frac{d}{d\theta}\left[E[\alpha_i + g^C(t) + \varepsilon_{i,t} + p_{i,t} - (p_{i,t} - p_{min})(1-\beta)]\right]$$

Since the distribution of $p_{i,t}$, $L^C(p)$, is independent of θ (since it is always equal to $F(p)$), and since $\frac{dE[\alpha_i]}{d\theta} = \frac{dE[\varepsilon_{i,t}]}{d\theta} = \frac{dE[g^C(t)]}{d\theta} = 0$, the proposition is shown. $\square$

The impact of θ on $\bar{w}_t^F$ is less straightforward. In Bagger et al. (2014), the value function for a given worker is given by $V(r, h_t, p)$, and the value function of an unemployed worker (who does not have a piece rate, r, or a productivity, p) is given by $V_0(h_t)$. It is straightforward to write $V_0(h_t)$ recursively, using the transition probabilities given in Bagger et al. (2014), as:

$$V_0(h_t) = w_u + \frac{\lambda_0}{1+\rho}\int_{p_{min}}^{p_{max}} E_t[V(r_0, h_{t+1}, x)]dF(x) + \frac{1-\lambda_0}{1+\rho}V_0(h_t), \tag{10}$$

where w_u represents the flow value of unemployment.

We index workers such that workers $i \in [0, u]$ are unemployed, and workers $i \in [u, 1]$ are employed. Let average earnings in period t for workers who do not have enforceable NCAs be given by $\bar{w}_t^F = \int_{i=u}^{1} w_{i,t}di$, and let $\bar{w}$ represent average earnings in steady state. Then:

Proposition A.5. *In steady state, average earnings are increasing in the arrival rate of offers to employed workers. Formally,* $\frac{d\bar{w}}{d\lambda_1} > 0$.

Proof. Consider the generic value functions for employed and unemployed workers, $V(r, h_t, p)$ and $V_0(h_t)$. Integrating each across workers and summing the two expressions yields

$$\int_0^u V(0, h_{i,t})di + \int_u^1 V(r_i, h_{i,t}, p_i)di,$$

where variables indexed by i represent worker i's human capital, piece rate, or the productivity of their matched firm, respectively.

Using the recursive definition of $V(r, h_t, p)$ given by Equation 5 in Bagger et al. (2014), as well as the recursive definition of $V_0(h_t)$ given in Equation 10, and simplifying (making use of the fact that, in steady state, the distribution of h is identical across time periods), this expression may be written as:

$$\frac{1+\rho}{\rho}\left(\int_{i=0}^{u} V(0, h_{i,t})di + \int_{i=u}^{1} V(r_i, h_{i,t}, p_i)di\right) = \int_{i=0}^{u} w_u di + \int_{i=u}^{1} w_{i,t} di$$

This expression is intuitive: the sum of the per-period value accrued by workers in the model is given by the sum of payments to unemployed workers and payments to employed workers. Taking derivatives of both sides with respect to λ_1, and exchanging the order of differentiation and integration (since u is not a function of λ_1, as shown in Bagger et al. (2014)), we generate the following expression for $\frac{d\bar{w}}{d\lambda_1}$:

$$\frac{d\bar{w}_t}{d\lambda_1} = \int_{i=0}^{u} \frac{dV(0, h_{i,t})}{d\lambda_1} di + \int_{i=u}^{1} \frac{dV(r_i, h_{i,t}, p_i)}{d\lambda_1} di \tag{11}$$

It therefore suffices to show that the right hand side is positive.

The first term may be rewritten to simplify the proof of this fact. First, we substitute for $V(r_0, h_t+1, x)$ using Equation (3) in Bagger et al. (2014) into Equation 10:

$$V_0(h_t) = w_u + \frac{\lambda_0}{1+\rho}\int_{p_{min}}^{p_{max}} (1-\beta)V_0(h_t) + \beta E_t[V(0, h_{t+1}, x)]dF(x) + \frac{1-\lambda_0}{1+\rho}V_0(h_t),$$

Next, we solve for $V_0(h_t)$:

$$V_0(h_t) = \frac{1+\rho}{\rho+\lambda_0\beta} w_u + \frac{\lambda_0\beta}{1+\rho}\int_{p_{min}}^{p_{max}} E_t\left[V(0, h_{t+1}, x)\right] dF(x)$$

Therefore, for worker i:

$$\frac{dV(0, h_{i,t})}{d\lambda_1} = \frac{\lambda_0\beta}{1+\rho}\int_{p_{min}}^{p_{max}} E_t[\frac{dV(0, h_{t+1}, x)}{d\lambda_1}]dF(x) \tag{12}$$

Moving to the second term of the right hand side of Equation 11, Equation (2), the unnumbered equation which follows (2), and Equation (3) from Bagger et al. (2014) show that each $V(r_i, h_{i,t}, p_i)$ may be rewritten as either:

$$(1-\beta)E_t[V(0, h_{t+1}, p')] + \beta E_t[V(0, h_{t+1}, p)]$$

or

$$(1-\beta)V_0(h_t) + \beta E_t[V(0, h_{t+1}, p)]$$

Therefore, given these expressions and Equation 12, the proposition is proven if $\frac{dV(0,h_t,p)}{d\lambda_1} > 0$, $\forall h_t, p$.

This fact is straightforward. Consider Equation (5) in Bagger et al. (2014), the recursive definition of $V(r, h_t, p)$. Since $r = 0$ in the case we are considering, an increase in λ_1 simply increases the probability that the worker moves to a higher quality firm to get paid more (the third line of Equation (5)) or stays at their current firm but negotiates better earnings (the fourth line), and decreases the probability that the worker stays at their current firm. Therefore, the result is shown. $\square$

Since $\frac{d\bar{w}^F}{d\theta} = \frac{d\bar{w}^F}{d\lambda_1} \cdot \frac{d\lambda_1}{d\theta}$, and since $\frac{d\lambda_1}{d\theta} < 0$ by assumption, we immediately get the following results:

Corollary A.6. $\frac{d\bar{w}^F}{d\theta} < 0$ *and* $\frac{d\left[\frac{d\bar{w}^F}{d\theta}\right]}{d\left[\frac{d\lambda_1}{d\theta}\right]} > 0$

The first result says that earnings for free workers are decreasing in NCA enforceability. The second result says that the relationship between NCA enforceability and earnings for free workers is steeper when NCA enforceability has a greater (negative) impact on the arrival rate of offers.

A.3.3 Overall Effect on Average Earnings

We now return to the overall effect of θ on average earnings, $\frac{d\bar{W}}{d\theta}$. First, we may reduce Equation 8 using Proposition A.4:

$$\frac{d\bar{w}}{d\theta} = \gamma(\bar{w}^C - \bar{w}^F) + (1-\theta\gamma)\frac{d\bar{w}^F}{d\theta} \tag{13}$$

Due to the indeterminacy in the sign of $\bar{w}^C - \bar{w}^F$, the sign of the overall expression is also indeterminate. If $\bar{w}^C - \bar{w}^F < 0$, then by A.6, $\frac{d\bar{w}}{d\theta} < 0$. If $\bar{w}^C - \bar{w}^F > 0$, then $\frac{d\bar{w}}{d\theta}$ may be positive or negative.

A.4 Empirical Implications of Theoretical Results

Overall, our empirical results are able to address several of the model's implications.

First, our results in Section 4 resolve the indeterminacy of the sign of $\frac{d\bar{w}}{d\theta}$.

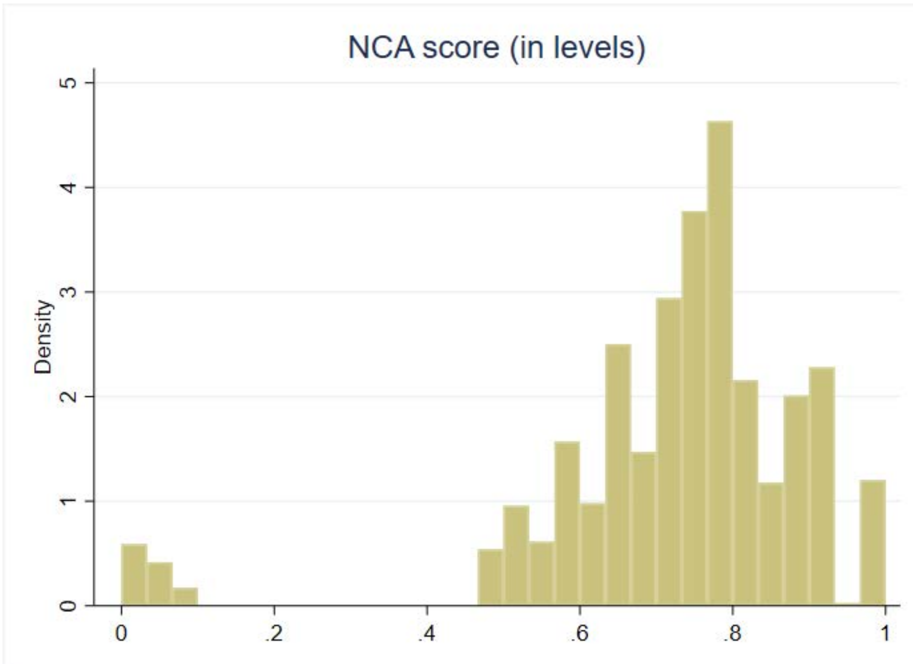
Second, our results in Section 5 test the model's prediction that $\frac{d\bar{w}^F}{d\theta} < 0$ (the first half of Corollary A.6).

Third, in Section 6, we test the second half of Corollary A.6: that stricter NCA enforceability will have a more negative effect on earnings when enforceability has a larger impact on a worker's offer arrival rate. We test this corollary two ways. In Section 6.1, we directly test this prediction by estimating whether the earnings effect of NCA enforceability are heterogeneous depending on the degree to which workers' offer arrival rates would be affected by NCA enforceability. In Section 6.2, we indirectly test this prediction by estimating whether strict NCA enforceability attenuates the degree to which strong labor market conditions translate into higher earnings over the course of a worker's job spell.

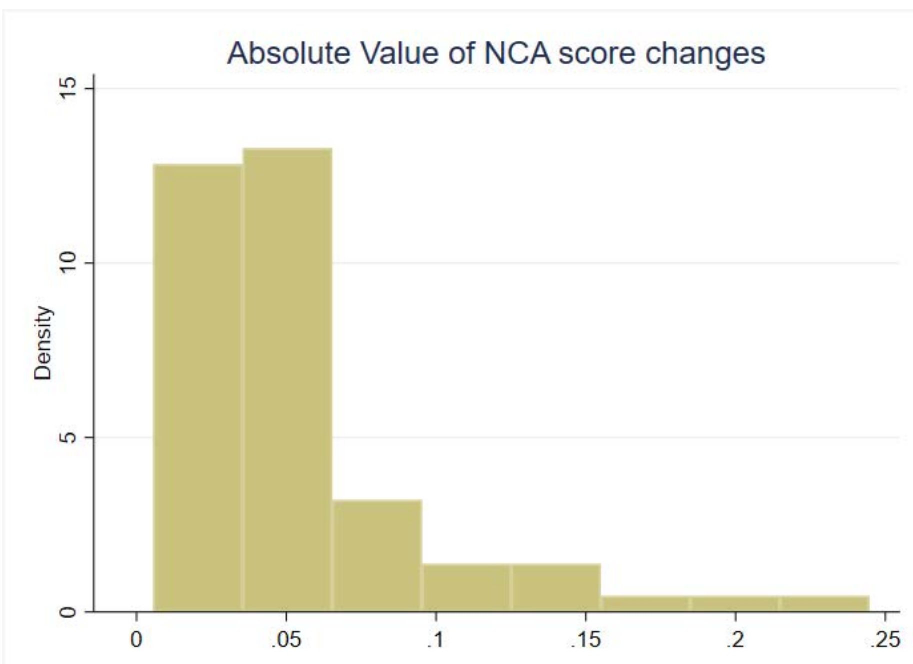
B Appendix Figures & Tables

Figure B.1: The Distribution in NCA Scores Across states, 1991–2014 (in Levels and Changes)

(a) NCA score levels

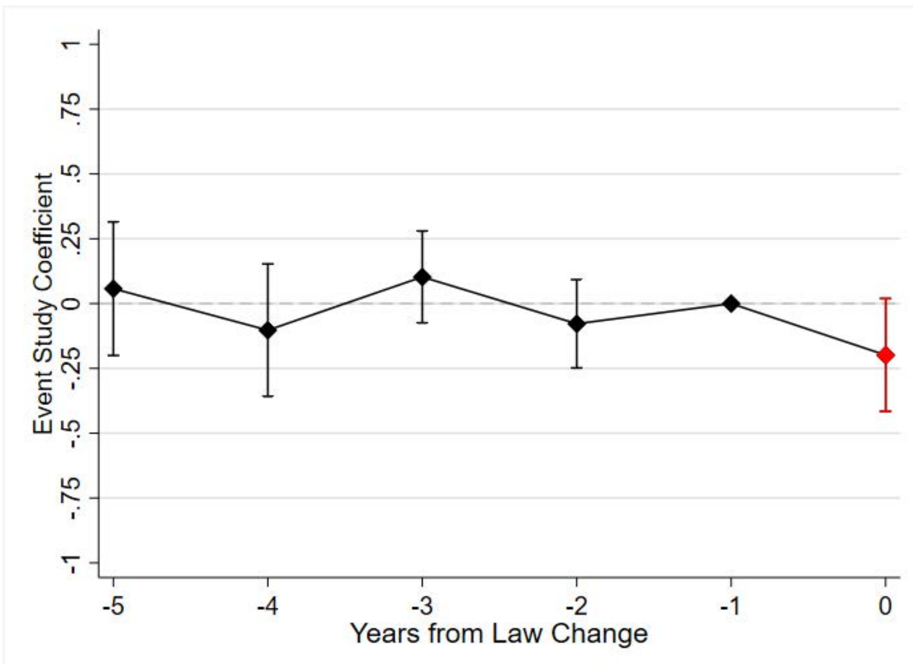


(b) NCA score changes



Notes. Panel (a) is a histogram of the NCA enforceability score in levels, at the state-year level over our sample period 1991–2014. Panel (b) is a histogram of the size (in absolute value) of score changes over this same sample period.

Figure B.2: Do NCA Court Filings Increase Prior to Legal Changes?



Notes: This figure presents the pre-period of a stacked difference-in-difference design, where the coefficients (vertical axis) represent the net impact of being in the state which has a future legal change versus states which do not.

Table B.1: The Effect of NCA Enforceability on Earnings:
Robustness to Political & Economic Controls

	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
NCA Enforceability Score	-0.095***	-0.087***	-0.025*	-0.085***	-0.121***
	(0.031)	(0.023)	(0.013)	(0.022)	(0.030)
Observations	1184797	1184797	1506230	1184797	3459572
R^2	0.275	0.357	0.132	0.346	0.941
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

This table replicates Table 3, but additionally controls for all variables introduced in Table 2 except ideology variables and variables that are themselves directly related to labor market outcomes (unemployment, Medicaid enrollment, and union membership). SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

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

Q1: State Statute	-0.029	(0.025)
Q2: Protectable Interest	-0.051**	(0.025)
Q3: Plaintiff Burden of Proof	0.033	(0.031)
Q3a: Consideration, Start of Employment	-0.051***	(0.013)
Q3b/c: Consideration, Continued Employment	-0.029**	(0.012)
Q4: Judicial Modification	-0.023	(0.016)
Q8: Enforceable if Employer Terminates	0.001	(0.035)
NCA Score without Question 1	-0.117***	(0.037)
Observations	1216726	

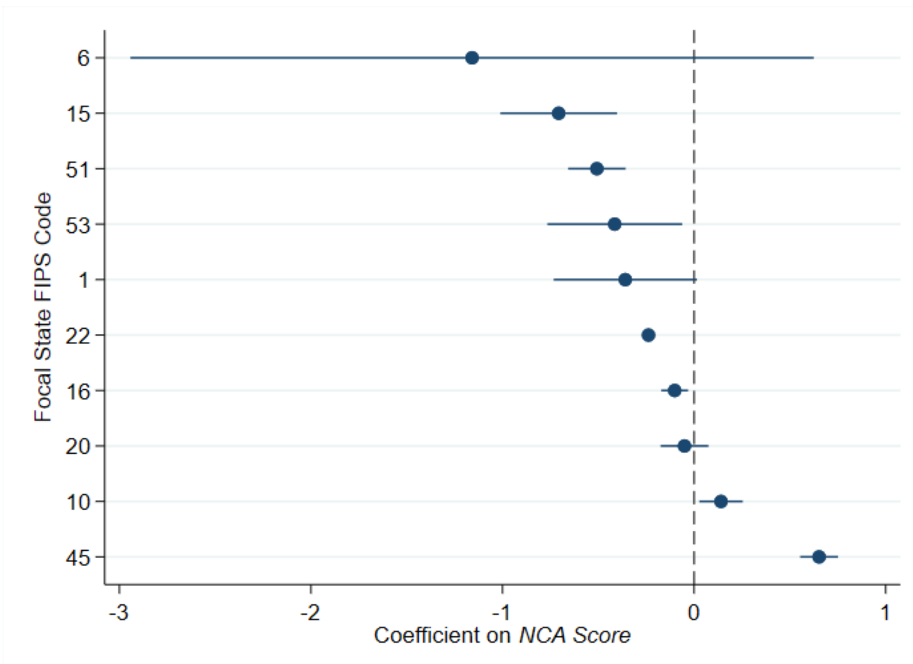
Each of the first seven rows represents a separate regression (corresponding to Column 1 of Table 3) in which the variable $Enforceability_{st}$ in Equation 2 has been replaced with each component of the NCA Enforceability Score separately. The coefficient on the score component is reported, alongside 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 B.3: The Effect of NCA Enforceability on Earnings: Heterogeneity by Magnitude, Direction, and Source of Law Changes (Stacked Design)

	(1) Baseline	(2) Extensive	(3) + change	(4) - change	(5) small change	(6) big change
NCA score	-0.246***					
	(0.070)					
Has NCA change (signed)		-0.018***	-0.018**	-0.018**	-0.017***	-0.024**
		(0.005)	(0.008)	(0.007)	(0.006)	(0.010)
Observations	5,698,274	5,698,274	3,971,622	1,726,652	2,854,985	2,843,289
R^2	0.94	0.94	0.94	0.94	0.94	0.95
Mean NCA score change		0.077	0.095	0.045	0.039	0.121

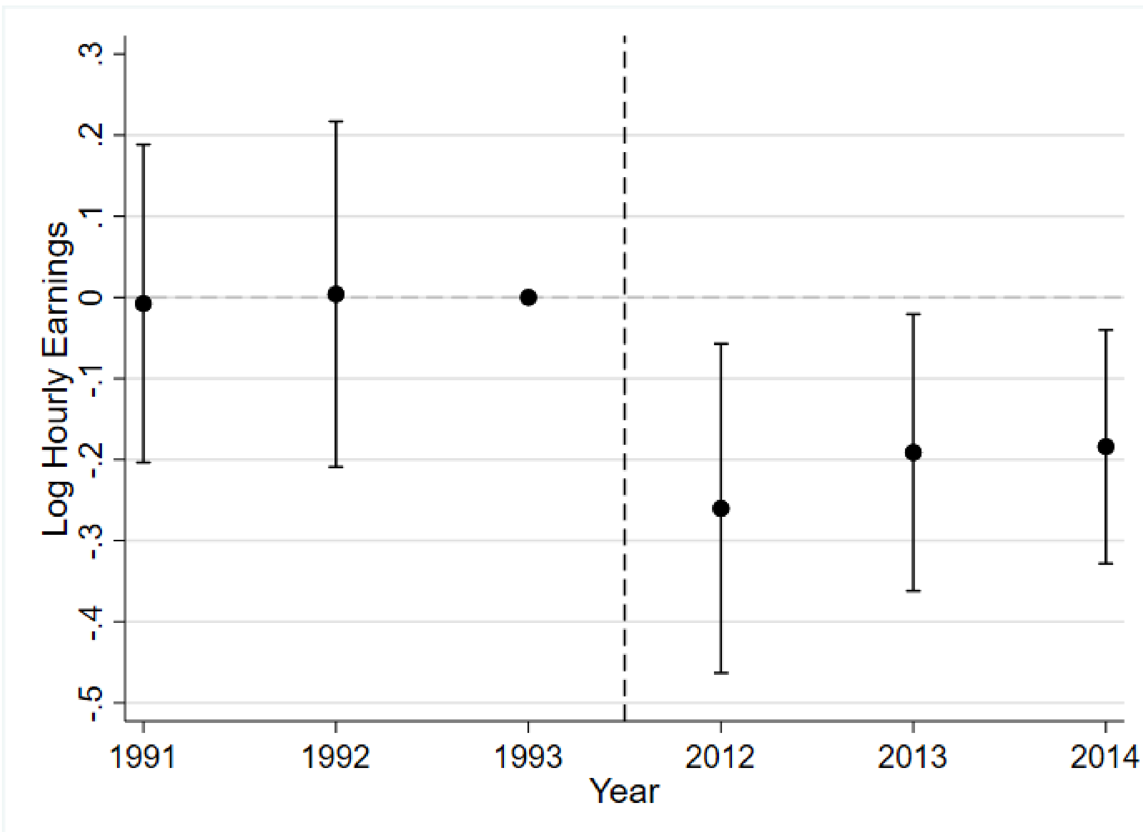
Each column reports the main regression coefficient from the stacked diff-in-diff model in Equation 3, with various modifications described in the table footer.
SEs clustered by state in parentheses. ***P<.01, **P<.05, *P<.1

Figure B.3: Estimated Effect of NCA Enforceability on Earnings, from Separate Stacked Diff-in-diff Models for Each Focal State



Notes: This figure presents the point estimate and 95% confidence interval from separate stacked difference-in-difference models estimated separately for each "focal" treatment state in the estimation sample for the stacked event study model described in Section 4.2.2.

Figure B.4: Long-Panel Event Study



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

Table B.4: The Effect of NCA Enforceability on Earnings: Excluding States in which NCA Law Changes Could in Theory be Endogenous

	(1)	(2)	(3)	(4)	(5)
	Log Earnings		Log Hours	Log Wage	Log Average Earnings
Panel A: Drop States with a Legislative NCA Law Change					
NCA Enforceability Score	-0.136**	-0.120***	-0.013	-0.122***	-0.109
	(0.056)	(0.044)	(0.027)	(0.042)	(0.071)
Observations	1055609	1055609	1346663	1055609	2926080
R^2	0.278	0.362	0.134	0.350	0.942
Panel B: Drop States with Partisan Judicial Elections					
NCA Enforceability Score	-0.135***	-0.121***	-0.041***	-0.122***	-0.156***
	(0.043)	(0.033)	(0.013)	(0.033)	(0.039)
Observations	989854	989854	1262128	989854	2696241
R^2	0.272	0.356	0.130	0.345	0.941
Panel C: Drop States with Judicial Elections (Partisan or Non-Partisan)					
NCA Enforceability Score	-0.128	-0.122	-0.038*	-0.117	-0.113
	(0.095)	(0.078)	(0.019)	(0.077)	(0.090)
Observations	699036	699036	890737	699036	1531774
R^2	0.272	0.359	0.128	0.348	0.942
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Year-Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

This table replicates Table 3, but with different sample restrictions in each panel. Panel A drops the 8 states that ever experience a legislative NCA enforceability change. Panel B drops the 6 states in which judges are selected via partisan election. Panel C 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 B.5: The External Effects of NCA Enforceability on Earnings (Weighted by Employment)

	(1)	(2)	(3)
Own State NCA Score	-0.067*	-0.067*	-0.057
	(0.035)	(0.036)	(0.047)
Donor State NCA Score		-0.002	-0.109
		(0.056)	(0.067)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.054
			(0.091)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.263**
			(0.110)
Observations	613762	613762	613762
R^2	0.944	0.944	0.944

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

Table B.6: The External Effects of NCA Enforceability on Earnings on Counties Far from State Borders

	(1)	(2)	(3)	(4)
Own State NCA Score	-0.184***	-0.182***	-0.147***	-0.073
	(0.061)	(0.060)	(0.053)	(0.181)
Nearest Neighboring State's NCA Score	-0.152**	-0.059	-0.027	0.036
	(0.060)	(0.061)	(0.059)	(0.092)
Observations	615191	2015843	1595005	545732
R^2	0.899	0.889	0.887	0.874
Border Sample	Y	N	N	N
Distance to Nearest State Restriction	None	None	50 miles	100 miles

The dependent variable is log earnings. The sample is the QWI from 1991-2014 and includes individuals between ages 19-64. Column 1 uses the sample from Table 7, while Columns 2, 3, and 4 use counties that are neither on state borders nor members of border-straddling commuting zones. Columns 3 and 4 further restrict by the distance from the focal county's centroid to the nearest county centroid in a different state. All regressions include controls for male, age group, as well as division by year by quarter and county fixed effects. Standard errors are clustered by own state. ***P<.01, **P<.05, *P<.1

Table B.7: The External Effects of NCA Enforceability on Mobility: Hires and Separations

	Hires			Separations		
	(1)	(2)	(3)	(4)	(5)	(6)
Own State NCA Score	-0.277**	-0.292**	-0.221	-0.256*	-0.275*	-0.189
	(0.129)	(0.141)	(0.159)	(0.152)	(0.162)	(0.182)
Donor State NCA Score		-0.099	-0.171		-0.129	-0.198
		(0.143)	(0.166)		(0.145)	(0.169)
Own Cty Emp/CZ Emp × Own State NCA Score			-0.429			-0.518
			(0.533)			(0.570)
Own Cty Emp/CZ Emp × Donor State NCA Score			0.396**			0.396**
			(0.169)			(0.165)
Observations	603965	603965	603108	604160	604160	603300
R^2	0.951	0.951	0.952	0.950	0.950	0.951
Sample	Border	Border	Border	Border	Border	Border

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

Table B.8: The Effect of NCA Enforceability on Earnings as Potentially Contaminated Control Groups Are Removed

	(1)	(2)	(3)	(4)
Own State NCA Score	-0.137***	-0.159***	-0.293***	-0.603***
	(0.034)	(0.033)	(0.073)	(0.194)
Observations	3548827	1860301	1078739	602968
R^2	0.941	0.941	0.941	0.941
Sample Restriction	No restriction	Distance > 50 miles	Distance > 75 miles	Distance > 100 miles

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, and are identical to Column 5 of Table 3 with different samples. Columns (2), (3), and (4) include only counties whose centroids are at least the specified distance away from the nearest county centroid in a different state. Standard errors are clustered by state. ***P<.01, **P<.05, *P<.1

Table B.9: Heterogeneous Earnings Effects Based on the “Bite” of NCA Enforceability on Workers’ Outside Options

	(1)	(2)	(3)	(4)
Dependent variable:	Log (Average Quarterly Earnings)		Log (Weekly Earnings)	
Sample:	QWI		CPS	
NCA Enforceability Score	-0.091**	-0.109**	-0.088*	-0.065
	(0.027)	(0.030)	(0.043)	(0.042)
NCA Enforceability Score × Industry’s State leave share [US]	0.050+	0.043+		
	(0.025)	(0.021)		
NCA Enforceability Score × Occupation’s occupational leave share			0.011**	0.011**
			(0.003)	(0.003)
High NCA Use Industry=1 × NCA Enforceability Score		0.049		
		(0.046)		
High NCA Use Occ=1 × NCA Enforceability Score				-0.044**
				(0.016)
Observations	1075767	1075767	739219	739219

Each column contains coefficients from a pooled regression across industries or occupations, comparable to Equation 2. Columns (1) - (2) interact NCA Enforceability with the industry’s state leave share (defined as the share of job-to-job changes in that industry from 2001–2006 in which the worker moved across state lines) using J2J data. Columns (3) and (4) use occupational leave share (defined as the share of job changes in an occupation in which the worker moved to a different occupation), calculated using data from Schubert et al. (2021)).
**P<.01, *P<.05, +P<.1

Table B.10: Heterogeneous Effects of NCA Enforceability on Earnings by Race and Sex

	(1)	(2)	(3)	(4)
NCA Score	-0.131***			
	(0.049)			
Female & White	-0.469***	-0.418***	-0.424***	-0.417***
	(0.011)	(0.025)	(0.025)	(0.025)
Female & Black	-0.572***	-0.521***	-0.528***	-0.515***
	(0.011)	(0.025)	(0.024)	(0.029)
Male & Black	-0.339***	-0.281***	-0.283***	-0.272***
	(0.008)	(0.016)	(0.017)	(0.015)
Female & Not Black or White	-0.502***	-0.427***	-0.441***	-0.439***
	(0.019)	(0.015)	(0.013)	(0.015)
Male & Not Black or White	-0.146***	-0.133***	-0.144***	-0.142***
	(0.010)	(0.016)	(0.015)	(0.014)
White Male × NCA Score		-0.087*	-0.029	-0.067
		(0.050)	(0.056)	(0.050)
Female & White × NCA Score		-0.161***	-0.094*	-0.135**
		(0.058)	(0.056)	(0.055)
Female & Black × NCA Score		-0.160***	-0.092*	-0.148***
		(0.054)	(0.052)	(0.053)
Male & Black × NCA Score		-0.170***	-0.109*	-0.129**
		(0.052)	(0.059)	(0.051)
Female & Not Black or White × NCA Score		-0.214***	-0.136***	-0.194***
		(0.047)	(0.048)	(0.045)
Male & Not Black or White × NCA Score		-0.102**	-0.027	-0.080*
		(0.048)	(0.048)	(0.045)
College Educated Worker × NCA Score			-0.110***	
			(0.025)	
High NCA Use Occ × NCA Score				-0.037***
				(0.012)
Observations	1537454	1537454	1537454	1537454
R^2	0.275	0.275	0.276	0.289

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

C Appendix: Creating our Database of Noncompete Laws

C.1 Law Database Construction Procedures and Principles

The state-year level NCA database that we constructed for this paper was guided by the method developed in Bishara (2010) for quantifying the enforceability of state NCA laws on seven dimensions. These seven dimensions are themselves defined by the organization system used in a series of legal reference books by Brian Malsberger titled "Covenants Not to Compete: A State-by-State Survey." There are currently fourteen editions of this reference book, published respectively in 1991 (1st), 1996 (2nd), 2002 (3rd), 2004 (4th), 2006 (5th), 2008 (6th), 2010 (7th), 2012 (8th), 2013 (9th), 2015 (10th), 2017 (11th), 2018 (12th), 2021 (13th), 2022 Edition (Ebook). There are additionally several supplemental editions of the Malsberger text that update new information between these editions. The supplements include: 1999 Cumulative Supplement, 2003 Supplement, 2005 Supplement, 2009 Supplement, and 2016 Supplement.

The Malsberger series is organized around 12 guiding legal questions, in addition to 11 sub-components of these questions. For each of these 23 components in each state, the series describes the current state of the law, including detailed descriptions of relevant case decisions or statues, and discussion of how the law has changed, including which cases were precedential. In constructing a method to quantify the enforceability of NCAs, Bishara (2010) chose seven of these questions and sub-components to be used in an enforceability index. Bishara's quantification method also includes his expert opinion on weights that should be used for each of these seven elements to construct a weighted index that reflects the relative legal importance of the components. The rationales behind the choices of these weights is discussed in Bishara (2010). The weighted index is designed to measure cardinal differences in laws, as opposed to an ordinal ranking of states.

Table C.1 shows the seven components and weights used to construct the enforceability index, along with a few benchmark enforceability scores for each legal component.

Bishara (2010) uses these questions, along with the Malsberger series, to develop two cross-sectional measures of the enforceability index, for every state in 1991 and 2009. Accompanying the paper, Professor Bishara also shared with us a document that contains his internal notes that helped guide the decision-making process behind the assignment of the scores. These internal notes provide important context for decisions about scores that do not perfectly align with the approximate benchmarks shown in Table C.1.

In the construction of our panel measures of NCA enforceability, our guiding principle was to treat the expert opinion expressed in Bishara (2010), and the ac-

Table C.1: Bishara (2011) Rating of the Restrictiveness of Non-Compete Agreements

Question #	Question	Criteria	Question Weight
Q1	Is there a state statute that governs the enforceability of covenants not to compete?	10 = Yes, favors strong enforcement 5 = Yes or no, in either case neutral on enforcement 0 = Yes, statute that disfavors enforcement	10
Q2	What is an employer's protectable interest and how is that defined?	10 = Broadly defined protectable interest 5 = Balanced approach to protectable interest 0 = Strictly defined, limiting the protectable interest of the employer	10
Q3	What must the plaintiff be able to show to prove the existence of an enforceable covenant not to compete?	10 = Weak burden of proof on plaintiff (employer) 5 = Balanced burden of proof on plaintiff 0 = Strong burden of proof on plaintiff	5
Q3a	Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?	10 = Yes, start of employment always sufficient to support any CNC 5 = Sometimes sufficient to support CNC 0 = Never sufficient as consideration to support CNC	5
Q3b/c	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? Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?	10 = Continued employment always sufficient to support any CNC 5 = Only change in terms sufficient to support CNC 0 = Neither continued employment nor change in terms sufficient to support CNC	5
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?	10 = Judicial modification allowed, broad circumstances and restrictions to maximum enforcement allowed 5 = Blue pencil allowed, balanced circumstances and restrictions to middle ground of allowed enforcement 0 = Blue pencil or modification not allowed	10
Q8	If the employer terminates the employment relationship, is the covenant enforceable?	10 = Enforceable if employer terminates 5 = Enforceable in some circumstances 0 = Not enforceable if employer terminates	10

Source: Bishara (2010). Notes: The questions in the table correspond to the NCA law components used in the IV estimates throughout the paper. In the paper and tables, we refer to Q1 as the 'Statutory Index', to Q2 as the 'Protectible Interest Index', to Q3 as the 'Burden of Proof Index', to Q3a as 'Consideration Index Inception', to Q3b and Q3c together as 'Consideration Index Post-Inception', to Q4 as 'Blue Pencil Index', and to Q8 as 'Employer Termination Index'. In the raw data, the laws are scaled in each state-year from 0 to 10, as indicated by this table. In the estimations, each component is rescaled to range from 0 to 1, where 0 is the least restrictive observation in the data and 1 is the most.

companying replication materials, as truth, and to find the timing of law changes between 1991 and 2009 that align with the cross-sectional measures and reflect as closely as possible the decision-making process used by Bishara in the construction of the cross-sectional measures.

Operationally, we implemented this database construction process by hiring two third-year law student research assistants (one at Ohio State University and one at Duke University) to make the decisions about the timing and magnitude of law changes. The research assistants were first trained by reading Bishara (2010), reading the relevant components of Malsberger (1991), and going through the notes from Prof. Bishara to understand how different scores were assigned in 1991. The law students then attempted to blindly match Bishara's scores in 2009 for each of the seven law components for all states. They were told which of the components were scored correctly and iterated the calibration process until there was a match with the Bishara 2009 index. The students then went through all of the editions of Malsberger between 1991 and 2009 and coded the timing of changes in enforceability for each of the components in each year. The same RAs then extended the index forward beyond 2009 using subsequent editions of Malsberger. The RAs did not interact directly with each other and were hired in series such that independent revisions and refinements to the database were made over time.

After these two law students completed the raw state-question-year enforceability scores, we hired a third law student at Duke to go over the index completely and construct an accompanying file that includes citations to each case or statute that generated each of the law changes in the database, citations to the locations in the Malsberger series that describe each change, and write brief overviews of the legal substance of each change.

Using the raw component scores, we constructed a weighted average NCA enforceability index using the same quantification system developed in Bishara (2010). In this system, the index score is calculated by taking the weighted total score in each state-year. This quantification system sometimes yields missing values for particular components of the NCA enforceability index in certain state-years. Missing values exist when a state has never had a court case or written a legislative statute that codified a particular dimension of NCA law. In constructing the weighted average enforceability index, Bishara (2010) adjusts for missing components by calculating the weighted sum of non-missing components and scaling the total upwards by the maximum possible score (550) divided by the maximum achievable score given the missing values in a state-year. Since our primary guiding principle is to follow the approach developed in Bishara (2010), we do the same.

One nominal (but important) way that we deviate from Bishara is that we normalize the scale of the index by dividing all scores by the maximum observed score in any state-year. This results in an index that ranges from 0 to 1 and has an interpretation as the range of the observed policy space.

C.2 Sensitivity of Results to Treatment of Missing Values

A natural concern is whether our estimated earnings effects of NCA enforceability hinge on the treatment of missing values in the Bishara NCA enforceability index. Here we discuss the sensitivity of our approach to decisions about the treatment of missing values.

Of the 8,568 component-state-year law measures in our sample period (51 states*24 years*7 components), 900 (10.5%) are missing. Given that our empirical models use within-state variation, the NCA components that are always missing in a state do not meaningfully contribute to our identifying variation. Of the 900 missing values, 744 (83%) fall into this category of being always missing for all years in the corresponding states. The remaining 156 missing values (1.8%) change from being missing to non-missing over time, which typically means that a new case was decided in which a judge opined on the issue the index is measuring.

We also consider alternative ways one might treat missing values. One alternative approach is to replace missing values with their future non-missing values. This approach might be reasonable if judicial decisions that go from missing to non-missing reflect cases in which a judge's decision reflected reasoning that was implicitly known by legal experts but not yet codified in the law. Redefining the index in this way causes switches to/from missing to become static values, so they no longer contribute to identification. We reconstruct the NCA index using this different assumption and rerun the main results, which are presented in Table C.2.

Table C.2: Robustness to Changes in Assumption about Missing NCA Index Components

	Log Earnings		Log Hours	Log Wage	Log Average Earnings
	(1)	(2)	(3)	(4)	(5)
Baseline Estimates	-0.118***	-0.107***	-0.021	-0.106***	-0.137***
	(0.036)	(0.028)	(0.017)	(0.027)	(0.034)
Alternative NCA Enforceability Score	-0.108***	-0.095***	-0.023	-0.095***	-0.135***
	(0.037)	(0.029)	(0.018)	(0.028)	(0.034)
Observations	1216726	1216726	1545874	1216726	3548827
R^2	0.275	0.357	0.132	0.346	0.941
Geographic FE	State	State	State	State	County
Time FE	Div x Year	Div x Year	Div x Year	Div x Year	Div x Quarter
Occupation FE	N	Y	Y	Y	N
Sample	ASEC	ASEC	ASEC	ASEC	QWI

The point estimates are slightly attenuated under this alternative assumption, but the qualitative patterns (and 95% confidence intervals) all overlap with our baseline estimates.

C.3 Sensitivity of Results to Weights Used in Enforceability Index

The weights used to construct the enforceability index were chosen by Professor Bishara to reflect the legal importance of each dimension in determining whether an NCA was enforceable. Bishara notes that "Because this data includes an element of assigning weights to influence the ranking based on the importance of the question to the dependent variable of strength of enforcement, the data can easily be utilized to highlight other outcomes by adjusting the emphasis and rationale for the weight factors" (Bishara, 2010).

We assess the sensitivity of our main results from Table 3 to choices of alternative weights. To do this, we sequentially increased or decreased the weight of each NCA law component by 50%, recalculated the weighted average index, and used the reweighted index to rerun the main earnings, hours, and wage models. As shown below in Table C.3, the main estimates are not very sensitive to these changes in weights. In both the log earnings and log wage models the largest deviation of any coefficient is 11% of the baseline estimate. In all cases, the estimates remain statistically significant.

A second approach we take to gauge the sensitivity of our estimate to the choice of weights is to use the weights from Starr (2019), which uses a confirmatory factor analysis model to infer the weights that optimize model fit. We reconstruct the weighted average NCA index using Starr (2019) statistical weights and again find estimates that are quite similar to our baseline results, as shown in Table C.4.

Table C.3: Robustness to Changes in NCA Index Weights

	Log Earnings		Log Hours	Log Wage
	(1)	(2)	(3)	(4)
Baseline Estimates	-0.118***	-0.107***	-0.021	-0.106***
	(0.036)	(0.028)	(0.017)	(0.027)
Increase Q1 Weight 50%	-0.115***	-0.105***	-0.023	-0.103***
	(0.036)	(0.028)	(0.018)	(0.028)
Increase Q2 Weight 50%	-0.117***	-0.105***	-0.019	-0.103***
	(0.035)	(0.027)	(0.017)	(0.027)
Increase Q3 Weight 50%	-0.116***	-0.106***	-0.021	-0.105***
	(0.038)	(0.029)	(0.018)	(0.029)
Increase Q3a Weight 50%	-0.125***	-0.113***	-0.021	-0.112***
	(0.036)	(0.028)	(0.018)	(0.027)
Increase Q3bc Weight 50%	-0.118***	-0.106***	-0.018	-0.106***
	(0.035)	(0.027)	(0.018)	(0.027)
Increase Q4 Weight 50%	-0.105***	-0.094***	-0.018	-0.094***
	(0.035)	(0.026)	(0.014)	(0.026)
Increase Q8 Weight 50%	-0.116***	-0.110***	-0.023	-0.108***
	(0.037)	(0.027)	(0.017)	(0.027)
Decrease Q1 Weight 50%	-0.119***	-0.107***	-0.018	-0.108***
	(0.036)	(0.028)	(0.017)	(0.027)
Decrease Q2 Weight 50%	-0.111***	-0.104***	-0.022	-0.104***
	(0.036)	(0.027)	(0.017)	(0.027)
Decrease Q3 Weight 50%	-0.117***	-0.106***	-0.020	-0.104***
	(0.035)	(0.026)	(0.016)	(0.026)
Decrease Q3a Weight 50%	-0.108***	-0.099***	-0.020	-0.098***
	(0.035)	(0.027)	(0.017)	(0.027)
Decrease Q3bc Weight 50%	-0.110***	-0.102***	-0.023	-0.100***
	(0.036)	(0.027)	(0.016)	(0.027)
Decrease Q4 Weight 50%	-0.124***	-0.114***	-0.022	-0.112***
	(0.038)	(0.030)	(0.020)	(0.031)
Decrease Q8 Weight 50%	-0.117***	-0.101***	-0.018	-0.101***
	(0.036)	(0.028)	(0.017)	(0.028)
Observations	1216726	1216726	1545874	1216726

Table C.4: Robustness to Changes in NCA Index Weights

	Log Earnings		Log Hours	Log Wage
	(1)	(2)	(3)	(4)
Baseline Estimates	-0.118***	-0.107***	-0.021	-0.106***
	(0.036)	(0.028)	(0.017)	(0.027)
NCA Index using Weights from Starr (2019)	-0.130***	-0.116***	-0.015	-0.115***
	(0.038)	(0.032)	(0.021)	(0.032)
Observations	1216726	1216726	1545874	1216726