

# The Labor Market Effects of Legal Restrictions on Worker Mobility

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Matthew S. Johnson

*Duke University and National Bureau of Economic Research (NBER)*

Kurt Lavetti

*Ohio State University and NBER*

Michael Lipsitz

*Federal Trade Commission*

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: increasing enforceability imposes externalities on workers across state borders, suggesting broad effects on labor market dynamism. We show that enforceability affects wages by reducing outside options and preventing workers from leveraging tight labor markets to increase earnings. We motivate these findings with a model of search and bargaining. Finally, higher NCA enforceability exacerbates gender and racial earnings gaps.

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## I. Introduction

By several metrics, the US labor market failed to produce economic gains for most workers in the four decades prior to 2020. Average real hourly earnings changed little (Desilver 2018), and the share of income accruing to labor declined from 64% in 1980 to 58% in 2016 (CEA 2016). Various forces have been posited to underlie these trends, including domestic outsourcing (Weil 2014; Goldschmidt and Schmieder 2017), the decline of labor unions (Farber et al. 2021), and the rise of superstar firms (Autor et al. 2020).

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, though their exact incidence is difficult to measure: estimates of the share of workers bound by NCAs vary from 18% of workers in 2014 (Starr, Prescott, and Bishara 2021), 28%–47% in 2019 (Colvin and Shierholz 2019), and 11.4% in 2022 (Boesch et al. 2023).<sup>1</sup> 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, or other assets that raise workers' productivity (Rubin and Shedd 1981; Starr 2019; Lavetti, Simon, and White 2020).

Though the enforceability of NCAs has received increasing scrutiny from policymakers at state and national levels,<sup>2</sup> there remains an incomplete

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<sup>1</sup> The 18.1% estimate from Starr, Prescott, and Bishara (2021) comes from a multiple imputation based on the share of workers in a representative survey who reported being bound by NCAs (15%) and the share who reported being unsure if they were bound by one (30%). The range reported by Colvin and Shierholz (2019) represents an imputation based on a survey of business establishments and various assumptions on the percentage of workers within those establishments bound by NCAs. Boesch et al. (2023) use the Federal Reserve Bank's Survey of Household Economics and Decisionmaking, which is broadly representative of the population.

<sup>2</sup> 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>

understanding of the labor market effects of NCAs primarily because of three factors. The first is a lack of comprehensive panel data on NCA enforceability. To date, researchers have largely relied on either cross-sectional measures of states' enforceability or case studies of 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 effects 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 which workers would be most affected by various proposals to change enforceability.

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.

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 because of 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.

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/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 that would prohibit NCAs, with limited exceptions, across the economy.

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 as a result of judicial decisions via court rulings, which is useful for our research design, as judges are more constrained by judicial precedent (*stare decisis*) than legislators in allowing economic or political trends to affect decisions. We combine our enforceability dataset with earnings and mobility outcomes from a range of datasets from the US Census Bureau and the Bureau of Labor Statistics (BLS).

We find that increases in NCA enforceability decrease workers' earnings and mobility. Moving from the 25th to the 75th percentile of the distribution of state-year 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, Prescott, and Bishara 2021). We also find that NCA enforceability reduces worker mobility, particularly among groups in which NCAs are used more frequently. An out-of-sample extrapolation implies that rendering NCAs unenforceable nationwide would increase average earnings among all workers by 3.5%–13.7%. The midpoint of this interval (8.6%) is roughly half the size of the labor union wage premium and roughly equal to estimates of the earnings effects of a large decrease in employer consolidation or of entering occupations with government-mandated licensing.

To interpret this overall effect, we conduct an empirical test to isolate the spillover effects of NCA enforceability on workers who are not themselves bound by NCAs. Focusing on local labor markets (commuting zones [CZs]) 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. Moreover, this evidence implies that our baseline estimate of the overall earnings effect of enforceability may be understated: the stable unit treatment value assumption is violated for some border counties in control states. Consistent with this logic, we find slightly larger earnings effects of enforceability when we restrict the analysis to counties that are at least 50 miles from any state border.

We then conduct two 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 and in occupations in which workers have lower cross-occupational mobility (as measured by Schubert, Stansbury, and Taska 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. 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, Rathelot, and Roulet 2021), geographically restrictive NCAs (or state-level enforceability changes) may have larger effects on women's outside options than on men's. State-level NCA enforceability changes may also disproportionately affect the outside options of Black workers because of racial differences in the propensity to move in response to economic opportunities (Sprung-Keyser, Hendren, and Porter 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 75th–25th differential in NCA enforceability accounts for 1.5%–3.8% of the earnings gaps between White men and other demographic groups, depending on which demographic group.

*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 2022) and a Hawaii ban on NCAs for tech workers (Balasubramanian et al. 2022)—has found that these bans led to higher earnings.<sup>3</sup> Balasubramanian et al. (2022) and Starr (2019) also use a pseudo difference-in-difference design, with cross-sectional variation in NCA enforceability across states as the first difference and variation in NCA prevalence across occupations as the second difference. Both find that earnings are lower in states that enforce NCAs.<sup>4</sup> 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, Williams, and Yin (2021) use a broader set of law changes and conclude that earnings increase with stronger enforceability. Studies using cross-sectional variation in NCA enforceability have similarly reached mixed results. Lavetti, Simon, and White (2020) find that the earnings of physicians are higher in states with stricter NCA enforceability. Finally, Gottfries and Jarosch (2023) and Potter, Hobijn, and Kurmann (2024) embed NCAs into Burdett-Mortensen-style models of wage posting, and both show theoretically how NCAs can reduce wages.<sup>5</sup>

Our paper also contributes to related work on NCAs and worker mobility. Nearly all of the studies referenced above examining earnings also find that NCA use and enforceability reduce mobility. In addition, Marx, Strumsky, and Fleming (2009) find that worker mobility (especially for workers with firm-specific technical skills) decreased after Michigan increased NCA enforceability in 1985.

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.<sup>6</sup> Finally, we connect our empirical analyses to a job ladder model of

<sup>3</sup> An exception is Young (2021), who finds that a ban on NCAs in Austria for low-wage workers had limited effects on earnings.

<sup>4</sup> The variation in NCA prevalence across occupations in Starr (2019) is based on whether an occupation's NCA use is above or below the national average, according to tabulations from Starr, Prescott, and Bishara (2021); the variation in prevalence across occupations in Balasubramanian et al. (2022) is based on comparing workers in the high-tech sector with workers in other sectors.

<sup>5</sup> Shi (2023), using a model of wage bargaining applied to the labor market for managers, finds that managers with an NCA have higher initial earnings but lower earnings growth. This paper is distinct from ours, as we focus on NCA enforceability (rather than use) and on the broader labor market (rather than on managers).

<sup>6</sup> Starr, Frake, and Agarwal (2019) also test for spillovers from NCAs. Our findings complement theirs by (1) focusing on enforceability (rather than use of enforceable NCAs),

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 elaborate on these contributions in section VIII.

We also complement the vibrant literature that considers other economic effects of NCA enforceability, including on entrepreneurship and investment (Jeffers 2024), employee spin-offs (Starr, Balasubramanian, and Sakakibara 2018; Marx 2022), startup performance (Ewens and Marx 2018), and innovation (Johnson, Lipsitz, and Pei 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; Jarosch, Nimczik, and Sorkin 2024), idiosyncratic worker preferences (Lamadon, Mogstad, and Setzler 2022), and local employer concentration (Prager and Schmitt 2021; Azar, Marinescu, and Steinbaum 2022; Benmelech, Bergman, and Kim 2022; Berger, Herkenhoff, and Mongey 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. 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 2024; Schubert, Stansbury, and Taska 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 (Johnson, Lipsitz, and Pei 2023; Lipsitz and Tremblay 2024).

Finally, our findings provide new insight into a long-standing debate in law and economics regarding freedom of contracting (for an overview, see, e.g., Bernstein 2008). Appealing to the Coase theorem, advocates of the freedom to contract suggest that NCAs must increase match surplus, which may be split between workers and employers. Evidence that NCAs are not freely bargained for—for example, because employers present them after the beginning of the employment relationship (Marx

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(2) using within-state (rather than cross-sectional) variation in enforceability, and (3) using a border county design to isolate spillovers from omitted variables that may jointly affect wages and enforceability. Gottfries and Jarosch (2023) show theoretically that NCAs cause wage spillovers in a wage posting model.



2011) or because workers are unaware of their existence (Starr, Prescott, and Bishara 2021)—already reveals one shortcoming of this argument. Our paper reveals another: enforceable NCAs impose substantial negative externalities on other workers.

## II. 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 that is fully described in app. A [apps. A–D are available online]) to guide our empirical analysis.

An NCA prevents a worker from moving to a job at a competing firm. The exact terms are contract specific and typically depend on the nature of competition. 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 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, an NCA might restrict some combination of geographic, temporal, occupational, or industrial mobility.

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, no long panel data for a representative sample of US workers' use of NCAs exist. 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 NCA enforceability likely impacts both the incidence of NCA use (the extensive margin) and the bindingness of NCAs already signed (the intensive margin). On the extensive margin, states with higher NCA enforceability in the cross section have a larger share of physicians (Lavetti, Simon, and White 2020), chief executive officers (CEOs; Kini, Williams, and Yin 2021), managers (Shi 2023), and hair stylists (Johnson and Lipsitz 2022) that sign NCAs.<sup>7</sup> On the intensive margin, a change in enforceability could alter the effect of an NCA for workers

<sup>7</sup> This evidence is not unanimous, however: Starr, Prescott, and Bishara (2021) find essentially no difference in NCA use by states' enforceability in a representative sample of US workers.



who have already signed one. Though NCAs are used in states in which they are unenforceable (Starr, Prescott, and Bishara 2021), employers are in a better position to leverage a worker's NCA when enforceability is easier.<sup>8</sup> Higher NCA enforceability could also lead employers 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 who sign NCAs.

To provide a theoretical foundation for how NCA enforceability affects earnings, we extend a 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 human capital accumulation and job mobility highlights two competing channels through which enforceable NCAs could affect earnings.<sup>9</sup>

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,  $\theta$  denote NCA enforceability, and  $\gamma$  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:

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

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.

<sup>8</sup> 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 that the NCA has less bite in expectation when it is unenforceable. Put another way, a worker gets a signal of the NCA enforceability regime when she informs her employer of an outside offer she has received: if enforceability is weak, the employer is unlikely to contend it, whereas if enforceability is strict, the employer might inform the worker of the legal environment.

<sup>9</sup> We use human capital accumulation to reflect a range of ways that firms could invest in workers. This could include general training as well as 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. General training is costly at the time of investment, whereas sharing a client list is costly in expectation only if a worker takes the list to a competitor. Of course, some investments—like training a worker to perform her job—are unaffected by NCA enforceability. Our focus is on investment in portable assets a worker can take with them in the event of a job separation.

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 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 may push this term downward. This indeterminacy makes the effect of NCA enforceability on earnings an empirical question. We provide this empirical evidence in section IV.

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 we make: higher NCA enforceability reduces the arrival rate of new job offers for all workers.<sup>10</sup> A slower offer arrival rate dampens workers' ability to climb the job ladder and leverage outside offers with their current employer.<sup>11</sup> We test the validity of this assumption and estimate spillover effects of NCA enforceability in section V.

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, workers who experience larger declines in offer arrival rates are more negatively affected by increases in enforceability. 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 VI.

<sup>10</sup> This might happen if higher NCA enforceability decreases the number of searching firms, e.g., by depressing new firm entry (Starr, Balasubramanian, and Sakakibara 2018; Jeffers 2024). Additionally, the use of enforceable NCAs by some firms may increase recruitment costs for all firms: if firms cannot observe whether a job applicant is currently bound by an NCA, this can slow down the recruiting process and decrease the value of posting vacancies (Starr, Frake, and Agarwal 2019; Goudou 2022).

<sup>11</sup> An alternative mechanism that could lead to negative spillovers is if firms using enforceable NCAs pay lower wages, enabling other firms to pay lower wages by worsening workers' outside options (Beaudry, Green, and Sand 2012). However, it is unlikely that this mechanism fully explains our results, given our evidence (presented in sec. V) that higher NCA enforceability leads firms to post fewer vacancies, which is hard to rationalize under the Beaudry, Green, and Sand (2012) framework. In addition, there is no empirical consensus that workers who sign an NCA earn lower wages: some studies find positive correlations between wages and NCA use (Lavetti, Simon, and White 2020; Starr, Prescott, and Bishara 2021).

### III. Data

#### A. *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 primary sources are Bishara (2010), who adopts careful legal analysis to quantify enforceability, and a series of legal treatises that Bishara draws from by Malsberger (2023), a leading legal expert on the topic. Bishara (via Malsberger) identifies seven quantifiable dimensions governing NCA enforceability. 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 an employee (as opposed to a voluntary separation). Table C.1 (tables B.1–D.1 are available online) 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 component, based on his opinion as a legal 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 to 2014 as follows. We obtained Bishara's internal notes that provide explanations of the legal aspects behind each of his coding decisions.<sup>12</sup> 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 end points. They then extended the panel to 2014. Appendix section C.1

<sup>12</sup> We thank Norm Bishara for generously sharing this dataset with us.

provides a more detailed discussion of the methods, procedures, and principles we used to construct this database.<sup>13</sup>

Once the seven dimensions of enforceability were coded, we constructed a composite NCA enforceability score for each state-year from 1991 to 2014 using the same weights for each of the seven dimensions proposed by Bishara (2010).<sup>14</sup>

Differences in how states interpret these dimensions have led to substantial differences in the NCA enforceability score across states. At the extreme ends, 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.<sup>15</sup> 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.<sup>16</sup> 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 precedent-setting court decisions. Over 90% of the law changes during our sample period arise from court decisions.<sup>17</sup> 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

<sup>13</sup> Our approach mirrors that of Hausman and Lavetti (2021), who created an analogous dataset for NCA enforceability specific to physicians from 1991–2009.

<sup>14</sup> In some state-years, there is no legal precedent for a particular dimension of enforceability. Following Bishara (2010), we code these values as missing. The composite NCA enforceability index is a weighted average of scores on the seven dimensions. When the score for a dimension 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 app. sec. C.4, we show that missingness is rare and that our estimates are insensitive to how we treat missing values.

<sup>15</sup> See [http://www.leg.state.fl.us/statutes/index.cfm?App\\_mode=Display\\_Statute&URL=0500-0599/0542/Sections/0542.335.html](http://www.leg.state.fl.us/statutes/index.cfm?App_mode=Display_Statute&URL=0500-0599/0542/Sections/0542.335.html).

<sup>16</sup> See <https://www.legis.nd.gov/cencode/t09c08.pdf>.

<sup>17</sup> More recently (and outside of our sample period), statutory changes to NCA enforceability have become more common. For example, effective July 1, 2023, a Minnesota statute prohibits NCAs for the vast majority of the workforce (Minn. Stat. 2022 181.988).

TABLE 1  
DESCRIPTIVE STATISTICS ON NCA LAW CHANGES, 1991–2014

	Region				Total
	Northeast	Midwest	South	West	
Average index	.75	.79	.76	.40	.69
SD of index	.10	.12	.13	.35	.25
Maximum index	.97	.97	1.00	.91	1.00
Minimum index	.63	.00	.47	.07	.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
Positive index change:					
Average magnitude	.03	.05	.08	.05	.05
Maximum	.15	.11	.24	.19	.24
Negative index change:					
Average magnitude	-.05	-.03	-.04	-.02	-.04
Maximum	-.06	-.06	-.17	-.09	-.17
Between-state SD	.09	.25	.12	.22	.18
Within-state SD	.03	.03	.04	.03	.03

NOTE.—Statistics 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 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 US Census Bureau.

being terminated for poor performance, he was hired by a competitor of his original employer in alleged violation of the NCA. While the NCA in question was ultimately not enforced, the court's decision set new precedent that NCAs may generally be enforced following employer termination: "The circumstances under which the employment relationship is terminated are an important factor to consider in assessing . . . the reasonableness of enforcing the restrictive covenant."<sup>18</sup> Future cases cited this precedent in adjudicating matters concerning employee termination: in *All-Pak, Inc. v. Johnston*, the court wrote, "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."<sup>19</sup> That is, *Brobston* set a precedent that NCAs could be enforceable even if the employee was terminated. *Insulation Corporation 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

<sup>18</sup> *Insulation Corporation of America v. Brobston*, 667 A.2d 729, 446 Pa. Superior Ct. 520, 446 Pa. Super. 520 (Super. Ct. 1995).

<sup>19</sup> *All-Pak, Inc. v. Johnston*, 694 A.2d 347 (1997).

enforceability score was equal to roughly one-third of a standard deviation in the distribution across our sample period.

Table 1 summarizes differences in levels of NCA enforceability across the country and within states over time between 1991 and 2014.

There are 73 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 (figs. B.1–B.5 are available online) displays this variation visually. Figure B.1*a* is a histogram of the level of NCA enforceability across all states over our sample period 1991–2014. Figure B.1*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 Current Population Survey (CPS) Annual Social and Economic Supplement (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.

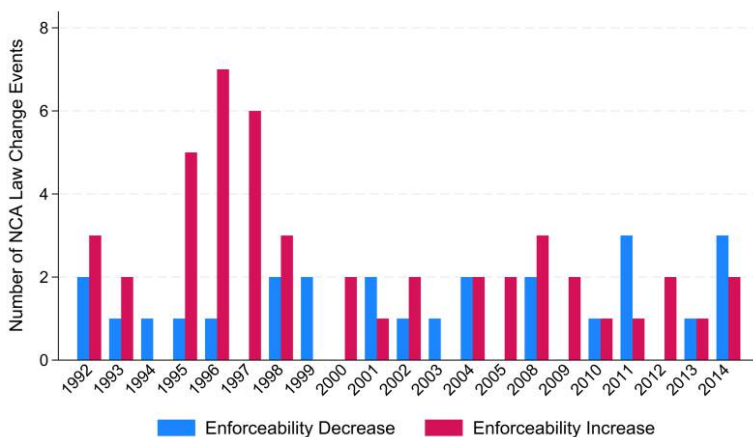


FIG. 1.—Timing of NCA law changes from 1991 to 2014.

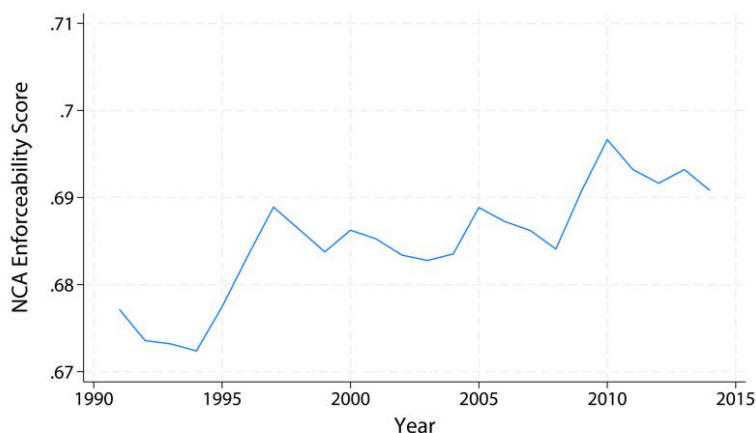


FIG. 2.—Average NCA enforceability score from 1991 to 2014. The series represents the population-weighted average NCA score in the United States in each year.

### B. *Are NCA Law Changes Predictable?*

If changes in NCA enforceability were correlated with underlying legal, economic, political, or social trends, it would be 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 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* (Knight and Epstein 1996). 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 economic trends.

Nonetheless, we use two approaches to empirically test this possibility. First, we test whether changing litigiousness predicts NCA law changes. Following Marx (2022) and Hiraiwa, Lipsitz, and Starr (2023), we use data from Courthouse News Service to identify instances of a filed dispute over an NCA in a US court. As in Hiraiwa, Lipsitz, and Starr



(2023), we collect all filings containing the strings “noncompetition,” “non-competition,” “not to compete,” “noncompete,” “restrictive covenant,” or “postemployment restraint.”<sup>20</sup> The data begin in 2002, and we collapse to the state-year level, tabulating counts of cases.<sup>21</sup>

For each state that experiences an NCA law change, we consider the window of time starting 5 years prior to the law change,<sup>22</sup> 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 event study (focusing on only the preperiod) to test whether a spike in case counts precedes NCA law changes. We use a Poisson pseudo maximum likelihood model (because of 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 block  $b$ ;  $\alpha_{\tau}$  is the event time coefficient of interest on  $I_{s,b}^{\tau}$ , which is an indicator for whether a legal change occurred  $\tau$  years after the observation time  $t$  in state  $s$ ;  $\mu_{s,b}$  are fixed state  $\times$  block effects;  $\rho_{b,t}$  are fixed block  $\times$  time effects; and  $\varepsilon_{s,b,t}$  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 sec. IV.B.2 for more details).

We present the  $\hat{\alpha}_{\tau}$  coefficient estimates in 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 test whether changes in political, social, or economic characteristics predict NCA law changes. We use data from the University of Kentucky Center for Poverty Research (UKCPR 2018) on population, workers' compensation beneficiaries, an indicator for whether the state governor is a member of the Democratic party, the share of state house and senate representatives in the Democratic party, minimum wage,

<sup>20</sup> We omit cases including the term “sale,” which often refers to NCAs ancillary to the sale of a business; these cases are typically handled differently than standard employee NCAs.

<sup>21</sup> From 2002–14, there were roughly 700 court filings about NCAs per year. Compare this number with 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. 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).

<sup>22</sup> We obtain qualitatively similar results if we choose different time windows.

and the number of Medicaid beneficiaries. We also use measures from Caughey and Warshaw (2018) of state-level policy liberalism (as reflected by government policy) and mass liberalism (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. 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 score and the independent variables are each of the characteristics noted above (lagged by 1 year) as well as state and census division  $\times$  year fixed effects. 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

TABLE 2  
CAN ECONOMIC AND POLITICAL FACTORS EXPLAIN CHANGES IN NCA ENFORCEABILITY?

Dependent Variable	NCA Enforceability	SE
Population (100,000s)	-.00	.00
Unemployment rate	.00	.00
Number of workers' compensation beneficiaries	-.00	.00
Democratic party governor	-.01	.00
% of state house from Democratic Party	.03	.06
% of state senate from Democratic Party	.05	.03
State minimum wage	-.01*	.01
Number of Medicaid beneficiaries (100,000s)	.00	.00
Liberalism score:		
Social policy	-.01	.02
Economic policy	-.02	.01
Social mass	.00	.02
Economic mass	.04	.04
Democratic party ID count	-.07	.31
Ideology score:		
State house	-.00	.01
State senate	.01	.01
House Democrats	-.05	.04
House Republicans	.02	.05
Senate Democrats	-.04**	.02
Senate Republicans	-.00	.02
Union membership	-.00	.00
<i>N</i>	829	
<i>R</i> <sup>2</sup>	.114	
<i>F</i> -test <i>p</i>	.197	

NOTE.—Models also include state and year fixed effects. Reported *R*<sup>2</sup> calculated after residualizing on state and year fixed effects. Standard errors are clustered by state.

\* *p* < .10.

\*\* *p* < .05.

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 ( $p = .197$ ).<sup>23</sup> Furthermore, the partial  $R^2$  of the model is 0.114 after residualizing on division  $\times$  year and state fixed effects, 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 underlying economic, political, or social trends do not themselves cause NCA law changes.

### C. *Data on Earnings and Mobility*

We gather data on earnings, employment, mobility, and other labor market outcomes from four sources: the CPS ASEC, the Job-to-Job Flows (J2J) 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 (Flood et al. 2018). The ASEC (also known as the March supplement) is collected each March and contains respondents' wage and salary income. The CPS also includes respondents' demographic and geographic information.<sup>24</sup> We restrict the ASEC sample to individuals who reported having worked for a private sector employer (not self-employed) in the year before being surveyed. We include the years 1991–2014, restrict to individuals ages 18–64 at the time they were surveyed, and remove observations for which earnings or hours variables have been top coded. The resulting ASEC dataset contains approximately 1.5 million observations, 1.2 million of which represent full-time workers. We deflate earnings and wages using the Consumer Price Index. We match NCA enforceability measures by state and year.

Second, we use the J2J dataset from the US Census Bureau to examine the effect of enforceability on job mobility. Derived from the Longitudinal Employer-Household Dynamics (LEHD) dataset (US Census Bureau 2019), these data contain aggregate job flows between cells defined by combinations of age, sex, quarter, origin job state, destination job state,

<sup>23</sup> It is not surprising that two out of 20 predictors are statistically significant. The probability of finding two or more significant predictors (at the 10% level) out of 20, 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 ( $1 - 0.90^{20}$ ).

<sup>24</sup> The American Community Survey also measures earnings. Its coverage begins in 2001, 10 years after our enforceability data begin. Results are quite similar if we instead use this survey.

origin employer industry, and destination employer industry. We aggregate these data to the level of the state  $\times$  industry  $\times$  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 QWI dataset from the US Census Bureau. Like the J2J, the QWI aggregates data from the LEHD, and it contains data on earnings as well as numbers of hires and separations at the county  $\times$  quarter level for the near universe of private workers, stratified by sex and age group. We use the QWI 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 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 JTS over the years 1996–2014. 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]?"<sup>25</sup> 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. We merge in annual national unemployment rates between 1947 and 2014 from the BLS for the analysis, which we describe in section VI.

#### **IV. 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 find that strict NCA enforceability reduces workers' earnings and mobility. These effects are more pronounced among workers who are most likely to have signed an NCA, and our estimates are stable to numerous robustness checks and sensitivity analyses.

<sup>25</sup> For more details, see <http://www.nber.org/cps/cpsjan2016.pdf>.

### A. Main Results on Earnings

We use intrastate variation in enforceability over time to estimate the effect of NCA enforceability on earnings using a difference-in-difference regression model:

$$Y_{ist} = \beta \times \text{Enforceability}_{st} + X_{it}\gamma + \rho_s + \delta_{d(s)t} + \varepsilon_{ist}, \quad (2)$$

where  $Y_{ist}$  is the outcome of interest,  $\text{Enforceability}_{st}$  is a state's annual composite NCA enforceability score across the seven dimensions described in section III,  $X_{it}$  is a vector of individual-level controls,  $\rho_s$  is a fixed effect for each state, and  $\delta_{d(s)t}$  is a fixed effect for each census division by year.<sup>26</sup> The coefficient of interest,  $\beta$ , 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  $E(\text{Enforceability}_{st}\varepsilon_{ist}|\rho_s, \delta_{d(s)t}) = 0$ : conditional on state and division  $\times$  year effects, changes in enforceability are uncorrelated with the error term. The evidence in section III.B supports this assumption.

We report results in table 3. Columns 1–4 use data from the ASEC restricted to full-time workers ages 18–64 who reported working for wage and salary income at a private employer the prior year.<sup>27</sup> 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% decline in earnings ( $\exp(-0.118 \times 0.1) - 1$ ,  $p = .002$ ). As another way to convey the magnitude of this estimate, consider that the 25th and 75th percentiles of *Enforceability* observed in our sample are 0.66 and 0.81, respectively. Moving from the 25th to the 75th percentile in *Enforceability* thus leads to a 1.7% average decline in annual earnings ( $\exp(-0.1175 \times 0.15) - 1 = 0.017$ ). Adding fixed effects for broad occupation codes in column 2 diminishes the point estimate slightly but improves its precision ( $p < .001$ ).

A negative effect of *Enforceability* on annual earnings could reflect either a decline in hours worked or a decline in hourly wages. In column 3, the dependent variable is instead the log of a worker's reported weekly hours:<sup>28</sup> while the point estimate is negative, it is close to zero and statistically insignificant ( $p = .24$ ). In column 4, the dependent variable is the individual's implied log hourly wage (calculated as annual earnings divided by 52 times usual weekly hours). The estimated coefficient is nearly identical to the coefficient on annual earnings.

<sup>26</sup> There are nine census divisions that partition the United States. We include division  $\times$  year fixed effects to account for potential time-varying shocks to different areas of the country.

<sup>27</sup> All results are very similar if we include part-time workers.

<sup>28</sup> We include part-time workers in this regression to avoid selecting on the dependent variable.

TABLE 3  
EFFECT OF NCA ENFORCEABILITY ON EARNINGS

	Log Earnings		Log Hours (3)	Log Wage (4)	Log Average Earnings (5)
	(1)	(2)			
NCA enforceability score	-.118*** (.036)	-.107*** (.028)	-.021 (.017)	-.106*** (.027)	-.137*** (.034)
Observations	1,216,726	1,216,726	1,545,874	1,216,726	3,548,827
R <sup>2</sup>	.275	.357	.132	.346	.941
Geographic fixed effects	State	State	State	State	County
Time fixed effects	Division × year	Division × year	Division × year	Division × year	Division × quarter
Occupation fixed effects	No	Yes	Yes	Yes	No
Sample	ASEC	ASEC	ASEC	ASEC	QWI

NOTE.—CPS ASEC samples use years 1991–2014 and include individuals 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 and age group as well as county fixed effects. The dependent variable in col. 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 × 52). Columns 1, 2, and 4 include full-time workers only, while col. 3 includes part-time workers to avoid selection on the dependent variable. Standard errors (in parentheses) are clustered by state.

\*\*\*  $p < .01$ .

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 in column 1, except that we are able to include fixed effects for each county (rather than state) and each division × year × quarter (rather than division × year).<sup>29</sup> We weight the regression by county-level employment. The estimated coefficient is slightly larger than that in column 1 and statistically significant ( $p < .01$ ).

Figure 3 illustrates the joint distribution of NCA enforceability and log annual earnings in the CPS using binned semiparametric scatterplots. Circles 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 × year effects, one-digit occupation effects, and individual demographic controls). The conditional means are constructed using the semiparametric partial linear regression approach developed in Cattaneo et al. (2024).

Figure 3A shows the full joint distribution for all states and years. Figure 3B excludes California and North Dakota to visually focus on the states and years that provide nearly all of the identifying variation in

<sup>29</sup> The estimate is essentially unchanged if we instead use state fixed effects.

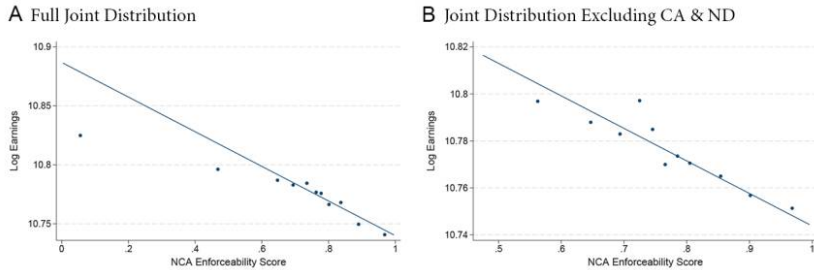


FIG. 3.—Relationship between NCA enforceability and earnings: binned scatterplots. Scatterplots depict the conditional joint distribution of NCA enforceability and log annual earnings, controlling for the same variables included in column 2 of table 3 (fixed state effects, census division  $\times$  year effects, one-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. (2024). *A* includes all states and years; *B* excludes California and North Dakota to visually focus on the main sources of identifying variation that we use for estimation.

our estimates. Both panels depict a clear negative relationship between enforceability and earnings. Using the test developed in Cattaneo et al. (2024), we fail to reject the hypothesis that the relationship between log earnings and NCA enforceability is linear in the full distribution ( $p = .992$ ). This test reinforces the choice of a linear regression specification in equation (2).

In table B.1, we report estimates from the same models in table 3, but we include the additional political and economic controls described in section III.B. The point estimates are slightly attenuated but similar to our baseline models: the coefficients in the ASEC log earnings and log wage models are  $-0.087$  and  $-0.085$ , respectively ( $p < .01$  in each model), and the coefficient in the QWI log average earnings model is  $-0.121$  ( $p < .01$ ). In table B.2, we also show that the estimates are similar when including government and self-employed workers in the sample.

To interpret the magnitude of our estimates, it is helpful to compare them with the earnings effects of other labor market characteristics or institutions. For example, Prager and Schmitt (2021) find that large changes in employer concentration induced by hospital mergers caused a 6.5% decline in earnings among the most affected workers. Farber et al. (2021) estimate that the union wage premium is 15–20 log points. Gittleman, Klee, and Kleiner (2018) estimate that mandated occupational licensing increases earnings by 7.5%.<sup>30</sup> We can extrapolate our estimates to predict

<sup>30</sup> Estimates of the earnings premium associated with occupational licensing vary widely. For example, Redbird (2017) finds no premium using a 30-year comprehensive panel of licensing laws.



the earnings effect of a national ban on NCAs. To do this, we use coefficients from column 1 of table 3 to generate predicted earnings in the ASEC sample for two different levels of NCA score: at the average NCA score over our sample period and at the lowest observed NCA enforceability level (0). These predictions imply that average earnings would increase by 3.5%–13.7% nationally. The midpoint of this interval (8.6%) is similar to the effect of a large change in employer concentration—roughly one-half the union earnings premium—and comparable to the premium attained by workers in occupations with government-mandated licenses.<sup>31</sup>

Our NCA enforceability score pools seven dimensions of NCA enforceability, but these dimensions might have different earnings effects. In table B.3, we estimate the earnings effects of changing each individual component of the NCA enforceability score separately.<sup>32</sup> 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) 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 on the basis of theory and prior results.

### *B. Dynamic Effects on Earnings and Robustness to Heterogeneous Treatment Effects*

We use three approaches to examine the dynamic effects of NCA enforceability and the robustness of our estimates to potential bias from heterogeneous treatment effects.

#### *1. Distributed Lag Estimates on Earnings*

Two potential concerns with estimates from the difference-in-difference specifications are (1) the plausibility of the parallel trends assumption

<sup>31</sup> This predicted effect of a national ban on NCAs requires a strong linearity assumption since a ban would lead the average worker to experience an NCA score change far outside the range of identifying variation underlying our regressions in table 3. However, the roughly linear relationship between earnings and enforceability in fig. 3 suggests that this assumption is not unreasonable.

<sup>32</sup> Estimating a model with each score component separately likely introduces omitted variable bias if score components are correlated with each other. However, including all individual components in the same regression causes the sample size to shrink significantly because of 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.3.

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 use 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. We estimate the distributed lag regression in first differences, similar to the approach used by Fuest, Peichl, and Siegloch (2018) using the QWI data.<sup>33</sup> We estimate a model in which the unit of observation is a county  $c(s)$ , demographic group  $g$  (defined as combinations of sex and age), year  $t$ , and quarter  $q$ :

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

The dependent variable,  $\ln w_{c(s),g,t,q} - \ln w_{c(s),g,t-1,q}$ , is the 1-year difference in the natural logarithm of average earnings in the relevant bin.  $\Omega_g$  contains indicator variables for worker sex and each age bin.  $X_{s,t,q}$  includes the same state-level political, economic, and social measures described in section III.B.  $\delta_{d(s),t,q}$  is a fixed census division  $\times$  year-quarter effect. We weight observations by employment and cluster standard errors by state. 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,  $k = -1$  (Schmidheiny and Siegloch 2023).

We report results in figure 4A. Two features are noteworthy. First, there is little evidence of a pretrend in earnings, supporting the assumptions (and the evidence in sec. III.B) that NCA law changes were conditionally exogenous to underlying trends that 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 3 before leveling off, becoming statistically significant by year 2.<sup>34</sup>

<sup>33</sup> Fuest, Peichl, and Siegloch (2018) 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.

<sup>34</sup> 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 (which we test in sec. VI) that higher enforceability renders workers less able to use outside job offers to increase their earnings, which is an effect that would compound over time. Young (2021) and Lipsitz and Starr (2022), who study the effects of NCA bans in Austria and the state of Oregon, respectively, both also find that the earnings effects of NCA bans grew over time.

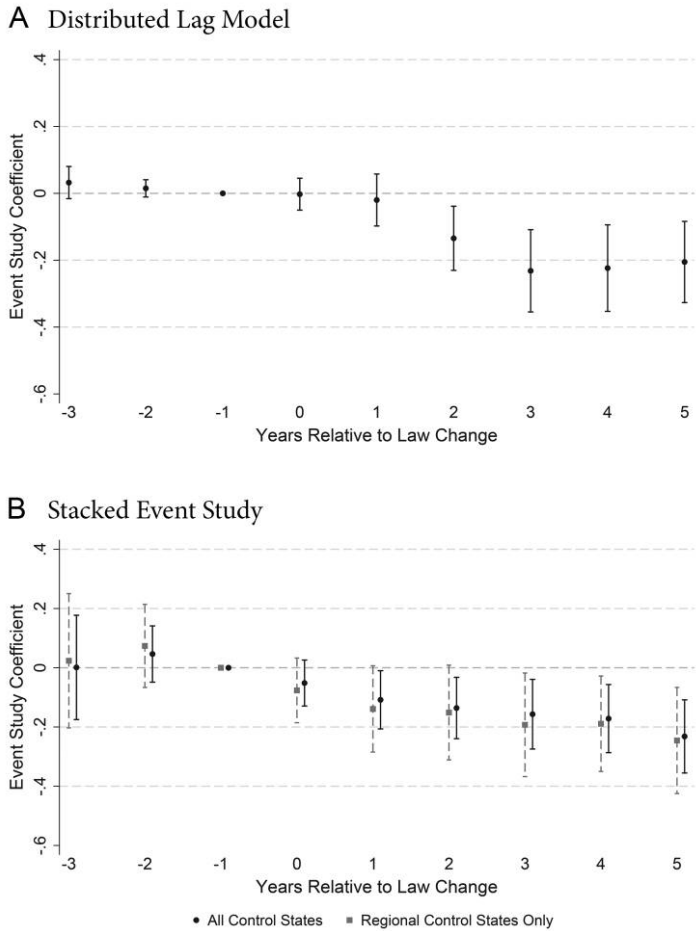


FIG. 4.—Dynamic effects of NCA enforceability changes on earnings from two different models. The figure plots two estimates of the dynamic effects of NCA law changes on earnings from a distributed lag model (A) and a stacked event study model (B). Both regressions use data from the QWI. See section IV.B.1 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 1 year prior to law change is normalized to zero. The dependent variable is the yearly change in the log average earnings in a county-group (A) and the log average earnings in a county-group (B). Standard errors are clustered by state.

2. Stacked Event Study

While the distributed lag model corroborates the 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. It is now well known that staggered timing of changes can cause TWFE to be biased because of comparisons in which states that experience early law changes serve as controls for states with later law changes (Goodman-Bacon 2021). 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 2023; Callaway, Goodman-Bacon, and Sant'Anna 2024).

To address these concerns, we 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 (2024) 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 (1) are a state's first law change during the sample period, (2) occur at least 4 years after the start of the QWI sample period (which varies by state since states entered the QWI in different years), (3) occur at least 5 years before the end of the sample period (2014), and (4) 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 3 years prior and 5 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.<sup>35</sup> Two treatment states satisfy requirements 1–4 above but lack a control state in their census region with the QWI data in the preperiod; these two treatment states are 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 trade-off with this specification is that while it helps us overcome the potential biases associated with TWFE, it is not guaranteed that the estimates we obtain will represent a population-level average.

<sup>35</sup> This differs from our baseline model that compares treated states with control states in the same census division. The reason is that in this model there are only 11 eligible control states, leaving an overly sparse set of controls if we required them to be in the same census division (of which there are nine). 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 trade-off between accounting for geographic-specific shocks that could matter for wages while also ensuring that we have a large enough comparison group.

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:

$$\ln w_{c,b,g,t} = \sum_{\tau=-3}^{\tau=5} \alpha_{\tau} I_{s(c),b}^{\tau} \times \text{Score change}_{s(c),b} + \mu_{c,b} + \rho_{r(c),b,t} + \Omega_g + \gamma X_{s(c),t} + \varepsilon_{c,b,g,t}, \quad (3)$$

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

Figure 4B graphically displays the estimates of the  $\alpha$ , 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 preperiod 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 3. The coefficient magnitudes are quite similar across the two models. Using a stacked difference-in-difference (as opposed to a TWFE) model,<sup>36</sup> we estimate an overall earnings effect of  $-0.246$  ( $p < .01$ ), as reported in column 1 of table B.4. 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 treatment effects for each individual law change. Figure B.3 reports point estimates and 95% confidence intervals on *Enforceability* from separate regressions that estimate the stacked difference-in-difference model in equation (4) for each of the 10 treatment states in the estimation sample.

<sup>36</sup> This regression model is

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

The point estimates are negative for eight of the 10 states, implying that our estimated earnings effects are not driven by a few outlier states but rather are broadly represented in a range of states.

### 3. Long Panel Event Study

While the stacked model in section IV.B.2 accounts for potential bias from staggered treatment timing, another complication in our setting is the nonabsorbing nature of NCA policies: states might change NCA enforceability multiple times, potentially in opposing directions. We use a long panel event study to address this issue, in which the event in each treated state is simply the change in NCA enforceability between the beginning and the end of the panel. We include the years 1991–93 and 2012–14 (the first and last 3 years in our panel) and calculate each state's change in the NCA enforceability score over this period.<sup>37</sup> We use the CPS ASEC data for this analysis, since many states started reporting data to the QWI only after 1993.

Figure B.4 displays results. 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 from zero and of essentially identical magnitude to our estimates in figure 4.

This result shows that our results are not driven by peculiarities of the methods we employ and that the effects of NCA enforceability changes persist in the long run.

### C. *Heterogeneous Effects Based on Prevalence of NCA Use*

We next examine heterogeneity in the effect of enforceability by prevalence of NCA use, which serves two useful purposes. First, it informs the robustness of the results in section IV.A: if we found that enforceability has larger earnings effects among groups less likely to be bound by NCAs, it might raise questions about the research design. Second, this exercise offers a closer sense of the impact that changes in NCA enforceability have on the earnings of groups more likely to be exposed to NCAs.

While we do not observe whether individual workers have signed an NCA, we use three sources of heterogeneity in NCA use: workers' education, occupation, and industry. Starr and Rothstein (2022) show that workers with a high school degree or higher are twice as likely to sign NCAs

<sup>37</sup> For states in which there were enforceability changes in the first 3 years or in the last 3 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.

TABLE 4  
HETEROGENEOUS EFFECTS OF NCA ENFORCEABILITY ON EARNINGS  
BY EDUCATION, OCCUPATION, AND INDUSTRY

	(1)	(2)	(3)	(4)	(5)
NCA enforceability score	−.113*** (.037)	−.004 (.040)	−.091** (.036)	−.100*** (.037)	.003 (.037)
High school degree plus × NCA score		−.121*** (.010)			−.113*** (.007)
High-use occupation × NCA score			−.042*** (.008)		−.016** (.007)
High-use occupation			.195*** (.004)		.155*** (.004)
High-use industry × NCA score				−.044*** (.009)	−.027*** (.008)
High-use industry				.207*** (.006)	.176*** (.005)
Observations	1,216,726	1,216,726	1,216,726	1,216,726	1,216,726
R <sup>2</sup>	.333	.333	.343	.344	.351

NOTE.—The sample is the CPS ASEC from 1991 to 2014 and includes individuals 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 × year, and individual controls for male, White, Hispanic, age, age squared, the individual’s years of education, and indicators for the metropolitan city center status of where the individual lives. In cols. 3 and 4, high-NCA-use occupations are occupations with NCA use greater than the national average, as tabulated by Starr, Prescott, and Bishara (2021). Standard errors (in parentheses) are clustered by state.

\*\*  $p < .05$ .  
\*\*\*  $p < .01$ .

relative to workers without a high school degree, and Starr, Prescott, and Bishara (2021) find heterogeneity in use across 22 occupation categories and 19 industry categories. We classify workers to high- or low-NCA-use occupations and high- or low-NCA-use industries on the basis of the occupation and industry in which an individual reports working in the CPS.<sup>38</sup> We replicate our main difference-in-difference specification (eq. [2]), except that we now add an interaction term of *Enforceability* with an indicator for high school degree, high-NCA-use occupation, or high-NCA-use industry (as well as an indicator for the respective main effects).

Table 4 reports these estimates. Column 1 reports the baseline average effect on earnings, corresponding to column 1 in table 3. Column 2

<sup>38</sup> 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 we define 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. High-NCA-use industries are all others. These occupations and industries represent those with NCA use below or above the national average, according to figs. 5 and 6 in Starr, Prescott, and Bishara (2021).



includes an interaction of NCA enforceability score with an indicator for high school degree. The main effect on NCA enforceability score is close to zero and statistically insignificant, implying that enforceability has essentially no effect on earnings for workers without a high school degree. On the other hand, the interaction term ( $-0.138$ ,  $p < .01$ ) implies that enforceability has a much stronger effect on the earnings of high school-educated workers. The sum of these two coefficients implies that going from the 25th to 75th percentile of enforceability leads to a 2.6% decrease in earnings for college-educated workers ( $\exp((-0.038 - 0.138) \times 0.15) - 1 = -0.026$ ,  $p < .01$ ), an effect that is over 50% larger than the earnings effect for the whole population.

Column 3 reports heterogeneity by occupation. Going from the 25th to 75th percentile of enforceability leads to a 2.1% decrease in earnings in high-use occupations ( $\exp((-0.085 - 0.059) \times 0.15) - 1 = -0.021$ ,  $p < .01$ ); the effect for low-use occupations is about 60% as large ( $p = .02$ ), and the difference is statistically significant ( $p < .01$ ). Column 4 reports heterogeneity by industry. Going from the 25th to 75th percentile of enforceability leads to a 2.4% decrease in earnings in high-use industries ( $p < .01$ ); the effect for low-use industries is roughly 60% as large ( $p < .01$ ), and the difference is statistically significant ( $p < .01$ ).

In column 5, we include these three sources of heterogeneity in the same regression. The coefficients on the interactions of NCA score with high-use occupation and high-use industry attenuate but remain negative and significant. The interaction of NCA score with college educated changes little and remains statistically significant.

#### *D. Robustness of Earnings Estimates to Various Concerns*

Our earnings estimates are robust to a range of reasonable concerns.

In light of potential challenges to interpreting continuous treatment variables in difference-in-difference designs (Callaway, Goodman-Bacon, and Sant'Anna 2024), in appendix section C.2 we assess whether our estimated earnings effects are driven by the scaling of our enforceability variable or by particular types of law changes. We find proportionally similar effect sizes when using a signed indicator variable in place of our continuous treatment variable, find symmetric effects for positive and negative law changes, and find larger effect sizes for law changes resulting in larger NCA score 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; fig. B.2); and that there is no evidence of pretrends in the distributed lag and event study models, it is unlikely that NCA law changes are endogenous to omitted variables that could contaminate our estimates.

In appendix section C.3, we describe additional tests that show that our estimates are insensitive to dropping a subset of law changes that are enacted through statute as well as law changes in states where judges are selected via elections.

Though our construction of the NCA enforceability index reflects the reasoning and judgment of leading legal scholars, a natural question is whether 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 each component to construct the aggregate index. In appendix sections C.4 and C.5, we show that our estimates are insensitive to alternative approaches to both decisions.

### *E. Effects of Enforceability on Job Mobility*

We next estimate the effect of NCA enforceability on worker mobility using data from the J2J dataset. This analysis validates that the variation in enforceability is capturing what NCAs are designed to do: restrict workers' mobility.

Table 5 presents estimates. We measure the number of job-to-job changes at the state  $\times$  year  $\times$  quarter  $\times$  sex  $\times$  age group  $\times$  industry level. We then estimate a Poisson pseudo maximum likelihood model with the following specification:

$$\begin{aligned} \mathbb{E}[J_{stia}] = & \exp [\beta \times NCA_{st} + \lambda \times High\ ind_i \times NCA_{st} + \gamma X_{ia} + \theta_{si} \\ & + \phi_{d(s)ti} + \varepsilon_{stia}], \end{aligned}$$

where  $J_{stia}$  is the count of job-to-job changes in state  $s$ , quarter  $t$ , origin industry  $i$ , and demographic group (age and sex) cell  $a$ .<sup>39</sup>  $NCA_{st}$  is the NCA enforceability score, and  $High\ ind_i$  is an indicator for industries with above-median NCA use (as defined in sec. IV.C).  $X_{ia}$  contains indicator variables for male workers and each of the age bins in the J2J data.<sup>40</sup>  $\theta_{si}$  is a fixed state  $\times$  origin industry effect, and  $\phi_{d(s)it}$  is a fixed census division  $\times$  origin industry  $\times$  quarter-year effect.

In column 1, we estimate the effect of origin state NCA enforceability 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

<sup>39</sup> Following Johnson, Lipsitz, and Pei (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, a regression of log employment on NCA enforceability (using QWI data in a specification identical to col. 5 of table 3, using baseline employment as weights) yields a coefficient of  $-0.13$  ( $p = .047$ ), corresponding to a 1.9% decrease in employment when moving from the 25th to the 75th percentile of enforceability.

<sup>40</sup> These are ages 14–18, 19–21, 22–24, 25–34, 35–44, 45–54, and 55–64.

TABLE 5  
EFFECTS OF NCA ENFORCEABILITY ON JOB MOBILITY

	All J2J Separations		Across Industries	Within Industries	Across State	Within State
	(1)	(2)				
NCA enforceability score	.064 (.114)	.112 (.108)	.102 (.127)	.121 (.089)	−.008 (.070)	.130 (.120)
High-NCA-use industry × NCA score		−.241*** (.085)	−.122 (.089)	−.380*** (.109)	−.058 (.126)	−.270** (.110)
Observations	652,024	652,024	651,664	619,283	638,444	650,404
Mean dependent variable	1,421.69	1,421.69	794.65	627.60	165.38	1,256.38

NOTE.—Estimates are Poisson pseudo maximum likelihood coefficients from a model using J2J data from 1991 to 2014. Each observation is a state × sex × age group × quarter × industry cell. All regressions include controls for sex and age group as well as fixed state × origin industry effects and census division × origin industry × year × quarter effects. High-NCA-use industry is an indicator for industries with NCA use above the national average, according to fig. 6 in Starr, Prescott, and Bishara (2021). Regressions are weighted by employment, and standard errors (in parentheses) are clustered by state.

\*\*  $p < .05$ .  
\*\*\*  $p < .01$ .

industry, and we find that NCA enforceability substantially reduces job-to-job separations in high-use industries. The coefficient on  $High\ ind_i \times NCA_{it}$  is negative (−0.241) and highly significant ( $p < .01$ ). Combined with the coefficient on the main effect of  $NCA_{it}$ , the estimates imply that moving from the 25th to the 75th percentile of NCA enforceability decreases the number of job-to-job changes by 1.95% in high-use industries.

In columns 3–6, we test whether NCA enforceability affects not just the level but also the direction of job mobility on the basis of two forms of restrictions often used in NCAs. In columns 3 and 4, we test for effects on job-to-job transitions 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. Moving from the 25th to the 75th percentile of NCA enforceability decreases the number of within-industry job changes by 3.8% in high-use industries.

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 find no detectable 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. Moving from the 25th to the 75th percentile of NCA enforceability decreases the number of within-state job changes by 2.1% in high-use industries. This evidence is consistent with the fact that the restrictions in many NCAs

are geography specific and thus are more likely to affect the rates of in-state moves.

Our measures of NCA enforceability influence workers' mobility—exactly what NCAs are designed to do. These results also motivate our test of one mechanism through which NCA enforceability affects earnings, which we describe in section VI.

## V. Spillover Effects of NCA Enforceability

We have found that strict NCA enforceability reduces overall earnings. How do these estimates relate to our model? As shown in equation (1), the effect of enforceability on average earnings is a weighted sum of two terms: (1) the average difference in earnings between workers who 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, given the assumption that strict enforceability slows down the job offer arrival rate for all workers. In this section, we first discuss existing evidence supporting this assumption and provide new evidence to corroborate it. We then show that enforceability does have economically meaningful spillover effects. Finally, we briefly discuss what our results can say about the first term in equation (1), which corresponds to the direct earnings effect of being bound by an NCA.

### A. *Effects of Enforceability on Job Vacancies*

Prior work supports our assumption that strict NCA enforceability reduces offer arrival rates for all employed workers in a labor market. Using survey data, Starr, Frake, and Agarwal (2019) find that workers located in states with strict enforceability and working in state industries with high NCA use report receiving relatively fewer job offers, 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.<sup>41</sup>

We corroborate this prediction that strict NCA enforceability reduces offer arrival rates using data on job openings. Vacancy rates measure the existence of potential jobs (Bagger et al. 2022) for workers who are and are not bound by NCAs. Our primary proxy for offer arrival rates is the number of unemployed individuals per job opening, a metric used by the BLS that reflects how tight or slack the labor market is. A higher ratio

<sup>41</sup> Other factors, however, could push this relationship the other way: in theory, NCAs could encourage recruitment by providing more flexible contracting structures. See Potter, Hobijn, and Kurmann (2024) for the implications that follow from that assumption.

TABLE 6  
EFFECTS OF NCA ENFORCEABILITY ON JOB OPENINGS

	Unemployed Individuals per Job Opening (1)	Job Openings (2)
NCA enforceability score	1.783* (1.045)	-.225 (.233)
Observations	8,568	8,568
R <sup>2</sup>	.922	.9308
Estimation methodology	OLS	Poisson

NOTE.—Estimates are ordinary least squares (OLS) or Poisson pseudo likelihood coefficients from a model using BLS Job Openings and Labor Turnover Survey data from 2001 to 2014. Each observation is a state  $\times$  year  $\times$  month cell. All regressions include fixed state and census division  $\times$  year  $\times$  month effects. Regressions are weighted by employment, and standard errors (in parentheses) are clustered by state.

\*  $p < .10$ .

indicates that it would take longer for the average worker to receive a job offer. We also consider the number of job openings to ensure that any effects are not solely driven by changes in the number of unemployed individuals. Both of these measures are available at the state  $\times$  year level starting in 2001 from the Job Openings and Labor Turnover Survey conducted by the BLS.<sup>42</sup>

In table 6, we present estimates of the impact of NCA enforceability on these measures of job offer arrival rates. We estimate an analog of equation (2) at the state-time level, 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 25th to the 75th percentile of enforceability leads to a reduction in that rate of 0.27 ( $p = .094$ ), 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 25th to the 75th percentile of enforceability leads to a reduction in job openings of 3.4%. These results imply that NCA enforceability reduces offer arrival rates to all workers, even for those not bound by NCAs.

*B. Spillover Effects across State Borders*

Having provided empirical support for our model’s assumption that NCA enforceability affects offer arrival rates for all workers, we now turn to the

<sup>42</sup> We use monthly data aggregated across industries (total nonfarm) at the state level, seasonally adjusted, which is the most granular level available (see <https://www.bls.gov/jlt/data.htm>).

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 a donor state affect workers who share a local labor market with that state but work in a different state. Our goal is to 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 follow many prior studies (e.g., Autor, Dorn, and Hanson 2013) and measure local labor markets as CZs, which are clusters of counties with strong commuting ties. We identify CZs that straddle state borders to capture labor markets that include business establishments in two states and are therefore subject to two different NCA enforcement regimes. We remove eight CZs that contain counties in more than two states to ensure clarity in defining the donor state. These restrictions leave us with 137 CZs and 742 counties in them. In our main analysis, we focus on the 545 counties in these CZs that themselves lie directly on state borders; with this restriction, we avoid counties such as Los Angeles County, which shares a CZ with counties in Arizona but is nearly 200 miles driving distance from anywhere in Arizona.

We use the QWI data, which (as described in sec. III) include quarterly earnings and employment flows at the county level by various worker demographics. Each observation 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 in equation (2) to estimate the impact of a change in NCA enforceability across a state border among workers employed in a CZ 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 as follows:

$$Y_{clga} = \phi_0 + \phi_1 \times Enforce_{cl} + \phi_2 \times BorderEnforce_{cl} + \phi_3 \times Female_g + \psi_a + \zeta_c + \Omega_{d(c)t} + \varepsilon_{clga}, \quad (5)$$

where  $c, t, g, a$ , and  $d(c)$  index county, year-quarter, sex, age group, and county  $c$ 's census division, respectively.  $\psi_a$  and  $\zeta_c$  are fixed age group and county effects, respectively.  $\Omega_{d(c)t}$  is a census division  $\times$  year-quarter fixed effect. The primary coefficient of interest is  $\phi_2$ , which captures the spillover effect on workers in county  $c$  of enforceability in the state that borders county  $c$ 's CZ.  $\phi_1$  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 CZ.

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 ( $p < .01$ ). 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  $-0.181$  ( $p < .01$ ), while the coefficient on donor state NCA score reveals evidence of meaningful spillover effects: the coefficient is  $-0.137$  ( $p = .059$ ), which equals 76% of the own state effect.

C. *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.

Our first test is whether the magnitude of spillover effects varies in proportion to the relative size of the labor force. Intuitively, in a CZ 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

TABLE 7  
EXTERNAL EFFECTS OF NCA ENFORCEABILITY ON EARNINGS

	(1)	(2)	(3)
Own state NCA score	-.160*** (.058)	-.181*** (.066)	-.161** (.069)
Donor state NCA score		-.137* (.071)	-.167** (.075)
Own county employment/CZ employment $\times$ own state NCA score			-.110 (.150)
Own county employment/CZ employment $\times$ donor state NCA score			.157*** (.054)
Observations	615,191	615,191	613,762
R <sup>2</sup>	.899	.899	.902

NOTE.—The dependent variable is log earnings. The sample is the QWI from 1991 to 2014 restricted to counties directly on state borders in CZs that straddle a state border. An observation is a county  $\times$  sex  $\times$  age group  $\times$  quarter. All regressions include controls for sex and age group as well as division  $\times$  year  $\times$  quarter and county fixed effects. Own county employment/CZ employment is the ratio of sex- and age-group-specific employment in own county divided by sex- and age-group-specific employment in the entire CZ. Standard errors (in parentheses) are clustered by own state in col. 1 and two-way clustered by own state and CZ in cols. 2 and 3.

\*  $p < .10$ .  
\*\*  $p < .05$ .  
\*\*\*  $p < .01$ .



total employment in the CZ. Conversely, if the donor state is the primary location of employers in the CZ, 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 CZ labor force that is employed on the own state side of the border. We calculate these shares at the demographic group (age-sex combinations) level.<sup>43</sup> 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 0% of its CZ's employment (and thus where the donor state comprises essentially all of the CZ's total employment)—is negative ( $-0.167$ ,  $p = .032$ ). However, the spillover effect is substantially smaller in counties that account for a large share of employment in their CZ. In the extreme case in which a county contains 100% of CZ employment, the estimated spillover effect is close to zero ( $-0.009 = -0.167 + 0.157$ ) and statistically insignificant ( $p = .891$ ).<sup>44</sup>

Our second test of the interpretability of these estimates draws from the intuition that the magnitude of spillovers should attenuate with distance to the state border; if they did not, one might worry that our spillover estimates are driven by spurious common shocks. Whereas the results in table 7 focus on adjacent pairs of counties bisected by state borders, in table B.7 we present estimates from samples that include (1) interior counties that are neither in CZs 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 nearest neighbor NCA score that corresponds to the state geographically closest to that county.<sup>45</sup> Reassuringly,

<sup>43</sup> We also include the main effect of this ratio but do not report its coefficient in the table.

<sup>44</sup> Unlike the analysis with the QWI data reported in table 3 and fig. 4, we leave the regressions in table 7 unweighted. We do this for two reasons. First, we weight the nationwide 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, a model that weights observations by employment would likely reveal minimal average impact of donor state NCA score. We report a weighted version of table 7 in table B.6, which indeed shows an attenuated average effect; however, the heterogeneity based on employment shares persists (col. 3).

<sup>45</sup> 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

the point estimate on nearest neighbor NCA score is substantially attenuated in each of these three subsamples, with coefficients  $-0.059$ ,  $-0.027$ , and  $-0.036$ , respectively. 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. Our model implies that negative spillovers arise because strict NCA enforceability slows job offer arrival rates, but other explanations are possible. For example, workers may direct job search across state lines if their own state increases NCA enforceability, leading to an outward shift in labor supply in border states and causing the market-clearing wage to decline. We find no evidence, however, that such behavior can explain the spillover effect on earnings. In table B.8, 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 number of hires and separations from the QWI in columns 1–3 and 4–6, 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 in section IV.E using the J2J dataset. The spillover effects (reported in cols. 2, 5) are imprecisely estimated, though they are negative and of a magnitude that is 53%–66% as large as the direct effect. Thus, there is no evidence that an increase in NCA enforceability in one state leads to an influx of workers into border counties of neighboring states; if anything, these estimates suggest that strict NCA enforceability reduces hiring in these border counties.

Collectively, these results bolster our evidence that NCA enforceability reduces earnings and labor market churn, even across state borders.

#### *D. 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 II, the effect of enforceability on average earnings depends not just on spillovers but also on the average difference in earnings between constrained workers bound by an enforceable NCA and unconstrained workers not bound by one ( $\bar{w}^C - \bar{w}^F$ ). This term can be positive or negative and is what makes the overall effect on average earnings indeterminate.

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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 tables B.7 and 7.

Along with this theoretical ambiguity, it is not obvious that one could empirically identify the causal effect of signing an NCA. The decision by workers and firms to use NCAs is likely correlated with unobserved characteristics, such as intangible capital and opportunities for investments, causing endogenous selection into employment contracts with NCAs. Some correlational studies find that workers who are bound by NCAs have 5%–6% higher earnings than observationally similar workers not bound by one (Starr, Prescott, and Bishara 2021; Starr and Rothstein 2022). However, these comparisons likely suffer from omitted variable bias; Balasubramanian, Starr, and Yamaguchi (2023) estimate a negative earnings effect of signing an NCA when accounting for plausible selection effects.

That said, our spillover results provide some perspective on the magnitude of  $\bar{w}^C - \bar{w}^F$ . As shown in table 7, the spillover effect of NCA enforceability in a border state is roughly three-quarters of the magnitude of the effect in a worker's focal state, our empirical analog of  $d\bar{w}/d\theta$  from equation (1). If our estimate of spillovers is a perfect empirical analog of  $d\bar{w}^F/d\theta$ , this comparison suggests that  $\bar{w}^C - \bar{w}^F$  is negative (i.e., earnings for workers bound by NCAs are less than earnings for workers without NCAs). On the other hand, if our spillovers analysis underestimates  $d\bar{w}^F/d\theta$  (e.g., if true local labor markets are smaller than CZs), 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 bound by NCAs.<sup>46</sup>

The second implication is econometric. Our primary estimating equation (eq. [2]) relies on the stable unit treatment value assumption: that control states 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.9, which replicates column 5 of table 3 but restricts the sample to counties progressively farther away from a state border. Excluding counties near state borders increases the magnitude of the coefficient, consistent with spillovers attenuating our baseline estimate.

<sup>46</sup> Another reason why the spillover effect may be close in magnitude to the overall earnings effect is that the share of workers not bound by NCAs is larger than the share that is bound. Given evidence in Starr, Prescott, and Bishara (2021) that 18% of workers have NCAs (i.e.,  $\gamma = 0.18$  in eq. [1]), the spillover term gets more weight than the direct term in the overall effect in eq. (1).

## VI. Does NCA Enforceability Reduce Earnings by Worsening the Value of Outside Options?

According to our model and as discussed in prior literature, the key channel through which NCA enforceability lowers earnings is by slowing down the arrival rate of new job offers. For constrained workers, this slowdown is explicit, as NCAs by nature prevent workers from considering job offers that compete with their current employer. For unconstrained workers not bound by an NCA, this slowdown occurs if high enforceability leads employers to post fewer vacancies (which sec. V.A shows has empirical support). Fewer job offers mean that workers have less ability to use improvements in outside options to climb the job ladder to better-paying jobs and to negotiate raises.

We use two approaches to test whether this outside options channel explains the negative earnings effect of NCA enforceability. First, we show that NCA enforceability has a larger effect on earnings when it has a larger bite on workers' outside options. Second, we show that NCA enforceability disrupts workers' ability to take advantage of tight labor markets to raise earnings. In light of this evidence, we gauge the extent to which the reduction in (realized and potential) mobility can explain the overall earnings effect of NCA enforceability reported in section IV.

### A. *Heterogeneity Based on Workers' Outside Options*

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 outside options enforceability affects most (corollary A.6 in app. A).

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 across occupations. Because NCAs often restrict movement within a local geographic area (e.g., in service sectors where product markets are local), 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 also 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 2022). For workers who are outwardly occupationally mobile, such limitations will be less restrictive, since enforceable NCAs limit a smaller portion of potential job offers.

We measure variation in cross-state mobility at the industry level using the J2J data (described in sec. IV.E). J2J includes the share of job-to-job changes that are across state lines at the state  $\times$  industry  $\times$  year (where industry corresponds to two-digit North American Industry Classification

System [NAICS] code). We collapse this variable to the industry level by averaging across all states for the years 2000–2006.<sup>47</sup> This process gives us a measure of the share of job changes that are across state lines for each two-digit NAICS industry.<sup>48</sup>

We measure variation in cross-occupational mobility at the occupation level using data from Schubert, Stansbury, and Taska (2021). Schubert, Stansbury, and Taska (2021) use data from 16 million resumes compiled by Burning Glass Technologies over the period 2002–18 to construct the occupational leave share—the share of job transitions in which a worker switches occupations—at the six-digit Standard Occupational Classification (SOC) level.<sup>49</sup>

We first consider heterogeneity in the earnings effects of NCA enforceability across industries in the QWI dataset on the basis of the share of job changes in each industry that are across state lines (the cross-state leave share). Figure 5A displays a scatterplot in which the unit of observation is a two-digit NAICS industry: on the vertical axis is the earnings effect of NCA enforceability in that industry,<sup>50</sup> and on the horizontal axis is the industry's cross-state leave share. 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.10 displays corresponding regression results: a 1 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 ( $p = .052$ ), or roughly half of the main effect. Column 2 shows that this estimate is robust to also interacting NCA enforceability with an indicator that an industry's NCA use is above the median (high-NCA-use industry).

We next consider heterogeneity in the earnings effect across occupations in the CPS ASEC sample on the basis of the occupational leave share. Figure 5B displays a scatterplot in which the unit of observation is a six-digit SOC occupation: on the vertical axis is the earnings effect

<sup>47</sup> We choose this time window to avoid confounding effects from the 2007–9 Great Recession.

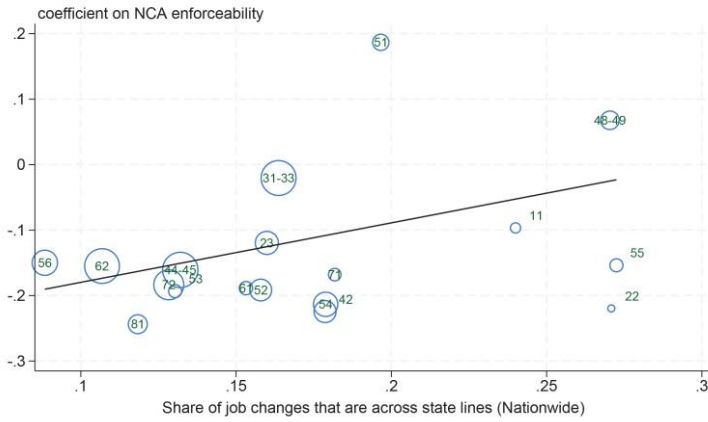
<sup>48</sup> One complication is that (as shown in table 5) the share of job changes across state lines is potentially endogenous to NCA enforceability. To partially address this issue, in some specifications we also control for each industry's incidence of NCA use as used in sec. IV.C.

<sup>49</sup> We are 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.

In theory, this measure could also be endogenous to NCA enforceability, e.g., if workers bound by NCAs are more likely to switch occupations to escape their NCA (Marx 2011). We control for each (two-digit SOC) occupation's incidence of NCA use to account for this issue.

<sup>50</sup> 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  $\times$  quarter  $\times$  region, and we weight observations by employment.

## A Industry-level cross-state mobility [QWI]



## B Occupation-level cross-occupation mobility [CPS]

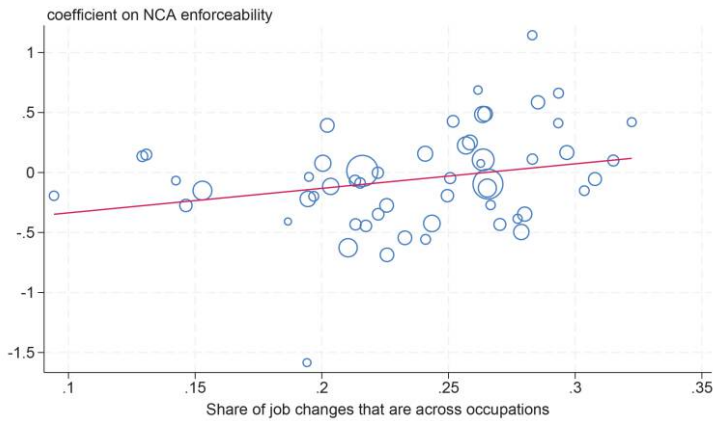


FIG. 5.—NCA enforceability has a larger effect on earnings when it has a bigger impact on workers’ outside options. Each scatterplot relates the earnings effect of NCA enforceability against the bite of enforceability on workers’ outside options using two dimensions of this bite. In *A*, the unit of observation is a two-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 (measured using the J2J dataset). In *B*, the unit of observation is a six-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 to different occupations (based on data from Schubert, Stansbury, and Taska 2021). See section VI.A for details.

of NCA enforceability in that occupation,<sup>51</sup> and on the horizontal axis is the occupation's share of job changes in which the worker switches occupations. The positive relationship demonstrates that NCA enforceability has a smaller effect on the earnings of workers who are more mobile across occupations. Corresponding regression results in column 3 of table B.10 show that a 1 standard deviation 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.<sup>52</sup>

A potential challenge to interpreting the occupation-level results is that occupations with low cross-occupational mobility may disproportionately comprise highly educated workers, and such workers may face earnings penalties from NCAs regardless of their ability to switch occupations. To account for this possibility, in column 5 we also interact NCA enforceability with the share of workers in each occupation who have a high school degree over our sample period. Our coefficient of interest is unchanged.

These analyses show consistent evidence that strict NCA enforceability affects earnings the most when it has the largest impact on workers' outside options.

### *B. NCA Enforceability Reduces Workers' Ability to Leverage Tight Labor Markets*

A second way that NCA enforceability could affect earnings via outside options is by reducing workers' ability to take advantage of tight labor markets.

<sup>51</sup> Using the CPS ASEC (which, unlike the QWI, 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 and year  $\times$  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.

<sup>52</sup> One might also expect NCA enforceability to have a larger earnings effect for workers with lower cross-industry mobility. Because the J2J data include information on job changes that are across (two-digit NAICS) industries, we can examine this heterogeneity by constructing industry leave shares analogous to the state leave shares we used above. However, there is not meaningful heterogeneity across two-digit NAICS industries in this leave share: in the J2J data, the average share of job changes between 2000–2006 that were across two-digit NAICS industries is 67%; the 25th percentile is 56%. Moreover, prior literature finds that occupation-specific human capital is much more important for earnings than industry-specific human capital (Kambourov and Manovskii 2009). For these reasons, it is not necessarily surprising that we find little to no relationship between across-industry mobility rates and the earnings impact of enforceability. The scatterplot shows a flat correlation (fig. B.5); the slope is 0.02 and statistically insignificant.



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. In Beaudry and DiNardo's (1991) model, firms insure workers against negative productivity shocks with implicit contracts. Improvements in labor market conditions enable workers to bargain for higher earnings that persist through their job spell but only if workers' mobility is costless (i.e., 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.

Beaudry and DiNardo (1991) 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 during the current job spell; if mobility is costly, earnings will be correlated with the initial market conditions at the start of the spell. Beaudry and DiNardo (1991) 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).<sup>53</sup>

More recently, Hagedorn and Manovskii (2013) propose a different explanation for why current earnings could be tied to prior labor market conditions. In contrast to Beaudry and DiNardo (1991), Hagedorn and Manovskii (2013) model workers' earnings as set in spot markets. 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, enabling workers to choose a job with a higher match quality. Hagedorn and Manovskii (2013) show that their model rationalizes the same reduced-form relationship between current earnings and history of unemployment rates, but they provide evidence to suggest that their model better explains this relationship than Beaudry and DiNardo's (1991).

While Beaudry and DiNardo (1991) and Hagedorn and Manovskii (2013) 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, strict NCA enforceability interrupts both channels through which tight labor markets translate

<sup>53</sup> Other papers in this literature have replicated this baseline result, using different datasets and time periods (e.g., Schmieder and Von Wachter 2010; Molloy et al. 2016).

to higher earnings by preventing them from climbing the job ladder (in the spirit of Hagedorn and Manovskii 2013) and by diminishing the threat of climbing the job ladder (in the spirit of Beaudry and DiNardo 1991). 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 Beaudry and DiNardo (1991) and Hagedorn and Manovskii (2013) that relates workers' 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 market conditions—relative to states where NCAs are less enforceable.

We begin by replicating the baseline analysis of Beaudry and DiNardo (1991) using the CPS JTS,<sup>54</sup> and we limit our analysis to full-time private sector workers for the years 1996–2014 (compared with Beaudry and DiNardo [1991], who used the years 1976–84).<sup>55</sup> We estimate the following model:

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

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 Beaudry and DiNardo (1991), in  $X_{i,t+j}$  we include race, Hispanic status, sex, marital status, age, age squared, tenure, tenure squared, education, and industry dummies. We also include dummy variables for metropolitan area status.  $C(t,j)$  is a vector of unemployment rates that, depending on the model, include initial unemployment rate (the unemployment rate at the beginning of the individual's job spell) and/or minimum unemployment rate (the lowest unemployment rate between the beginning of the job spell and the time of measurement of earnings). Following Beaudry and DiNardo (1991), we use annual national unemployment rates from the BLS.  $\rho_{s(i,t)}$  is a fixed effect for worker  $i$ 's state of residence.  $\delta_{d(i,t)t}$  is a fixed census division  $\times$  year effect.<sup>56</sup>

We report these results in table 8. Columns 1–3 replicate the main results of Beaudry and DiNardo (1991) for our sample period. In column 1, we include only the unemployment rate at the time of hire (initial unemployment rate): our estimated coefficient has a smaller magnitude ( $-0.008$ ) than that estimated in Beaudry and DiNardo (1991;  $-0.030$ ), but it is negative and statistically significant ( $p < .01$ ). Column 2 uses instead

<sup>54</sup> 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.

<sup>55</sup> Though Beaudry and DiNardo's (1991) sample period ends before 1996, the CPS JTS has been collected only since 1996.

<sup>56</sup> Beaudry and DiNardo (1991) do not use state fixed effects; we include them to use within-state variation in enforceability.

TABLE 8  
NCA ENFORCEABILITY CHANGES HOW PRIOR LABOR MARKET CONDITIONS AFFECT WAGES

	Log Earnings				
	(1)	(2)	(3)	(4)	(5)
Initial unemployment rate	−.008*** (.002)		−.002 (.003)	−.002 (.003)	.010** (.004)
Minimum unemployment rate		−.017*** (.003)	−.014*** (.005)	−.014*** (.005)	−.028*** (.006)
Initial NCA score				−.013 (.059)	−.033 (.074)
Initial NCA score × initial unemployment rate					−.017*** (.006)
Initial NCA score × minimum unemployment rate					.020** (.009)
Observations	76,350	76,350	76,350	76,350	76,350
R <sup>2</sup>	.364	.364	.364	.364	.364

NOTE.—The dependent variable is log weekly earnings. Initial unemployment rate is the unemployment rate at the beginning of the individual’s job spell, and minimum unemployment rate is the lowest unemployment rate between the beginning of the job spell and the time of measurement of earnings. All regressions include state, census division × 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. Standard errors (in parentheses) are clustered by state.

\*\*  $p < .05$ .  
\*\*\*  $p < .01$

the minimum unemployment rate over the course of the worker’s job spell (minimum unemployment rate); we find a negative and statistically significant effect. Column 3 mimics the main finding of Beaudry and DiNardo (1991): including both initial unemployment rate and minimum unemployment rate attenuates the coefficient on initial unemployment rate close to zero but leaves the coefficient on minimum unemployment rate negative and significant ( $p < .01$ ). In other words, on average, prior experience with tight labor markets leads to higher current earnings, consistent with either Beaudry and DiNardo’s (1991) model of implicit contracts with costless mobility or Hagedorn and Manovskii’s (2013) model in which match quality matters for earnings.

To test the hypothesis that NCA enforceability shuts down the ability of workers to leverage strong labor markets, we estimate the following model:

$$\ln w_{(i,t+j,l,s)} = \Omega_1 X_{i,t+j} + \Omega_2 C(t,j) + \Omega_3 \text{Enf}_{t,s} + \Omega_4 C(t,j) \times \text{Enf}_{t,s} + \varepsilon_{(i,t+j,l,s)}, \quad (7)$$

where  $\text{Enf}_{t,s}$  is the NCA enforceability score in state  $s$  at time  $t$ , the beginning of the worker’s job spell. If NCA enforceability affects the cost of mobility in an implicit contracts environment or if it prevents workers from attaining better match quality, we expect two effects. First, the

coefficient on  $Enf_{t,s} \times$  minimum unemployment rate will be positive, indicating that employees are less able to leverage favorable labor markets over their job spell when NCA enforceability is high. Second, the coefficient on  $Enf_{t,s} \times$  initial unemployment rate will 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 for  $Enf_{t,s}$ : encouragingly, the coefficients on initial unemployment rate and minimum unemployment rate 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. First, consider the main effects of initial unemployment rate and minimum unemployment rate, which indicate the effect of initial and most favorable labor market conditions, respectively, for a state with the lowest NCA enforceability. These coefficients mirror and amplify the findings from Beaudry and DiNardo (1991) and Hagedorn and Manovskii (2013): 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 unemployment rate indicates that a worker's earnings today are strongly correlated with 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 with costless mobility or, alternatively, reflect a greater ability of workers to find high-quality matches relative to the overall population.

Next, consider the interaction terms, indicating the differential effects of market conditions for a worker in the highest-enforcing state. The coefficient on  $Enf_{t,s} \times$  initial unemployment rate ( $-0.017$ ;  $p < .01$ ) shows that a higher unemployment rate at the time of hire significantly reduces current earnings when NCAs are more enforceable. The coefficient on the other interaction term,  $Enf_{t,s} \times$  minimum unemployment rate ( $0.020$ ;  $p < .05$ ), 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 unemployment rate with this interaction term, 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 ( $-0.028 + 0.020 = -0.008$ ,  $p = .19$ ). Or, using the policy range in line with our identifying variation, the effect of the minimum unemployment rate on the current wage is  $-0.0139$  ( $p < .01$ ) at the 25th enforceability percentile and  $-0.0108$  ( $p = .033$ ) at the 75th percentile.

We probe the robustness of these results in table B.11. Columns 1 and 2 show that the coefficients of interest (from col. 5 of table 8) are essentially unchanged if we restrict the sample to prime-age workers (ages 25–54) or

expand the sample to include part-time and government workers, respectively. In column 3, we use state-specific annual unemployment rates instead of national unemployment rates: the interaction of the initial NCA score with minimum unemployment rate remains positive and significant, and its interaction with initial unemployment rate remains negative but attenuates in magnitude and loses statistical significance. In columns 4 and 5, we examine heterogeneity by worker education to see whether NCA enforceability has the largest effect among workers most likely to sign NCAs. The interaction terms of interest are even larger in magnitude for workers with at least a high-school degree (col. 4) than for our overall sample; they are both tiny and insignificant for workers without a high school degree (col. 5), consistent with NCAs being less relevant for this group's labor market.

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.

*C. Effects of Lower Offer Arrival Rates: Contribution of Across- and Within-Job Wage Growth*

The results in sections VI.A and VI.B 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). But, quantitatively, how important is the reduction in (realized and potential) mobility in explaining NCA enforceability's effect on earnings?

One way a reduction in offer arrival rates reduces wages is by reducing workers' ability to move to higher-paying jobs. Our estimates imply that moving from the 25th to 75th percentile of NCA enforceability reduces the average worker's job mobility between 0.62% and 3.5%.<sup>57</sup> We use Tjaden and Wellschmied's (2014) baseline model estimate that job transitions are on average associated with a 7.1% earnings increase. In our CPS ASEC sample, the average worker is 38 years old and has 18 years of potential job experience. We assume that this average worker has

<sup>57</sup> We obtain 0.62% on the basis of the estimates in col. 2 of table 5 that the 25th–75th percentile difference in job-to-job mobility is 2.0% in high-use industries and that high-use industries comprise 31% of overall employment ( $0.020 \times 0.31 = 0.62\%$ ). We obtain 3.5% based on the estimates in col. 1 of table B.8; the coefficients imply that (in counties in CZs spanning state borders) a 25–75 difference in enforceability leads to a reduction in new hiring of  $\exp(-0.227 \times 0.15) - 1 = 3.5\%$ .

experienced a 6.4% quarterly rate of job-to-job transitions thus far in her career.<sup>58</sup> Given these parameters, we estimate that the reduction in realized mobility leads to 0.28%–1.59% differences in average earnings for a worker at the 75th versus 25th enforceability percentile.<sup>59</sup>

Our model highlights that lower job offer arrival can also reduce within-job wage growth by shutting down offer matching and thus workers' ability to negotiate for raises. Indeed, our results in table 8, viewed through the lens of Beaudry and DiNardo (1991), provide information about how NCA enforceability impacts a key element of within-job wage growth: workers' ability to leverage labor market improvements to bargain for a higher wage.<sup>60</sup> To quantify this effect, consider that in 1996–2016 (the sample period underlying table 8), the median worker's initial unemployment rate was 5.5, and the median worker's minimum unemployment rate was 4.6. The estimates in table 8 suggest that if this worker were employed in a state at the 25th percentile of NCA enforceability, her earnings would be 0.47% higher than if she were employed in a state at the 75th percentile of NCA enforceability.<sup>61</sup>

In table 3, we estimated that moving from the 75th to 25th enforceability percentile leads to a 1.6% wage increase for the average worker. If we take the implied magnitude of 0.47% from table 8 and we add it to the across-job component, the implied total earnings effect of changing NCA enforceability from the 75th to the 25th percentile is between  $(0.47\% + 0.28\% =) 0.75\%$  and  $(0.47\% + 1.59\% =) 2.06\%$ . The sum of these terms may overstate the total earnings effect if the results in table 8 include across-job earnings effects, or it may understate the total effect if the average gains from job changes most affected by NCA enforceability is above 7.1%.<sup>62</sup> Notably, this range,  $[0.75\%, 2.06\%]$ , contains our baseline estimate of the overall earnings effect (1.6%).

<sup>58</sup> Based on the US Census Bureau's "Job-to-Job Hires by Year/Quarter" series for workers aged 19–44 (<https://j2jexplorer.ces.census.gov/explore.html#1576911>).

<sup>59</sup> These calculations are

$$\begin{aligned} (1 + 0.071 \times 0.064 \times (1 + 0.020 \times 0.31))^{(4 \times 18)} - (1 + 0.071 \times 0.064)^{(4 \times 18)} &= 0.28\%, \\ (1 + 0.071 \times 0.064 \times (1.035))^{(4 \times 18)} - (1 + 0.071 \times 0.064)^{(4 \times 18)} &= 1.59\%. \end{aligned}$$

<sup>60</sup> Of course, as noted, the results in table 8 can be instead interpreted through Hagedorn and Manovskii's (2013) model of match quality, which is about across-job wage differences. We use the within-job interpretation in this calculation for expositional purposes.

<sup>61</sup>  $0.47\% = \exp(0.01 \times 5.5 - 0.028 \times 4.6 - 0.033 \times 0.81 - 0.017 \times 5.5 \times 0.81 + 0.02 \times 4.6 \times 0.81) - \exp(0.01 \times 5.5 - 0.028 \times 4.6 - 0.033 \times 0.66 - 0.017 \times 5.5 \times 0.66 + 0.02 \times 4.6 \times 0.66)$ .

<sup>62</sup> Specifically, the 7.1% average earnings gain includes the effects of involuntary moves to lower-paying jobs, which may not be impacted by NCA enforceability, depending on the state.

## VII. 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, women tend to be less willing than men to commute far distances for their job (Le Barbanchon, Rathelot, and Roulet 2021; Caldwell and Danieli 2024), 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 relatively less likely to migrate in response to earnings increases away from their hometown (Sprung-Keyser, Hendren, and Porter 2022). Together with our model, these differences predict that NCA enforceability will cause greater earnings penalties for women and racial minorities.

Figure 6 displays results from regressions that add demographic group indicators—alone and interacted with NCA score—to the regression reported in column 1 of table 3.<sup>63</sup> (Table B.12 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 score and indicators for college educated, high-NCA-use occupations, and high-NCA-use industries, alone and interacted with NCA score—to account for potential average differences in jobs and education levels across demographic groups.

Figure 6 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

<sup>63</sup> Unlike the models in table 3, we include part-time workers 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.



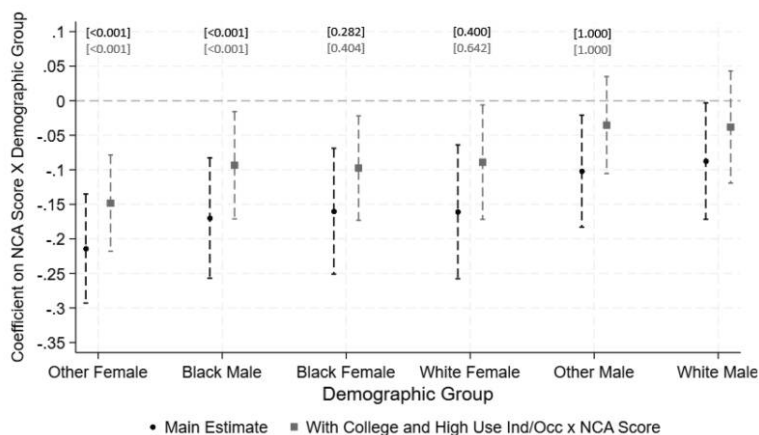


FIG. 6.—Heterogeneous effects of NCA enforceability on earnings by race and sex. The figure depicts coefficients from two regressions of earnings on NCA score interacted with demographic groups. The first regression builds on column 1 of table 3, adding indicators for each demographic group as well as interactions of those indicators with NCA score (the coefficients on which are depicted 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 square 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.

men.<sup>64</sup> These differences persist in the regression specification with additional controls. In both models, a test of equality of the earnings effects across all six groups is strongly rejected ( $p < .001$ ).<sup>65</sup>

These results suggest that strict NCA enforceability exacerbates existing disparities across demographic groups. Regression results in columns 1 and 2 of table B.12 imply that if a state that enforces NCAs at the 75th percentile of the distribution were to switch to enforcing NCAs at the 25th percentile of the distribution, the earnings gap between White men and each other demographic group would close by 1.5% for non-Black, non-White men; 1.9% for Black women; 2.3% for White women; 3.6% for Black men; and 3.8% for non-Black, non-White 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. We look

<sup>64</sup> These results suggest that the earnings penalties faced by non-White and female workers are not additive, consistent with other work on racial and gender earnings gaps (Paul, Zaw, and Darity 2022).

<sup>65</sup> The  $p$ -values in fig. 6 are Bonferroni corrected to account for five pairwise comparisons.



forward to future research that more comprehensively examines how NCAs differentially affect workers of different demographic groups.

### VIII. How Generalizable Are the Earnings Effects of NCA Enforceability?

Our paper stands on the shoulders of prior work that has considered effects of NCA use and/or enforceability for specific subsets of workers or of law changes. 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 a null or a positive association with earnings (Lavetti, Simon, and White 2020; Starr, Prescott, and Bishara 2021; Starr and Rothstein 2022; Balasubramanian, Starr, and Yamaguchi 2023). In contrast, studies of enforceability of NCAs (including ours) tend to find negative impacts on earnings (Garmaise 2011; Balasubramanian et al. 2022; Lipsitz and Starr 2022).<sup>66</sup> 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 nonsigners (which we show theoretically and empirically is unambiguously negative).<sup>67</sup> Thus, our model explains why there could be positive/null earnings effects of use and negative earnings effects of enforceability.<sup>68</sup>

Second, our paper can rationalize heterogeneity in the estimated earnings impacts of NCA enforceability among existing studies. For example, Lipsitz and Starr (2022) find a 2%–3% earnings effect of a ban on

<sup>66</sup> An exception is Young (2021), who finds that an NCA ban in Austria for low-wage workers had a limited effect on earnings.

<sup>67</sup> This insight helps interpret results from Kini, Williams, and Yin (2021, their table 7), who estimate the interaction effect of NCA enforceability and NCA use on CEO earnings. They find a positive effect of this interaction (suggesting that CEOs with enforceable NCAs get an earnings premium) but a negative effect on the main effect of enforceability, which is consistent with negative spillovers.

<sup>68</sup> 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.

NCA 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. Low-wage workers are more mobile across industries than are high-wage workers (Lipsitz and Starr 2022) perhaps because of differences in the industry specificity of human capital; by comparison, high-tech workers may have particularly industry-specific skills, meaning that their outside options would be more affected by NCA use and enforceability.<sup>69</sup> 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 substantially lower outside-occupation mobility than other occupations (which the data from Schubert, Stansbury, and Taska [2021] show 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 fig. B.3), implying that the negative effects in prior case studies are not aberrations. At the same time, we show substantial heterogeneity in 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. We also provide evidence for a mechanism through which NCA enforceability affects earnings, namely, by restricting workers' outside options; this analysis extends prior work that has referenced the role of worker mobility but has not explicitly tested why lower mobility would translate to lower earnings. Our theoretical model highlights two channels through which such changes in outside options affect earnings—by affecting within- and across-job wage growth—and our empirical results show that both channels are meaningful. Our consideration of both of these channels may partially explain why our estimate that banning NCAs would raise wages by approximately 8% is larger than an analogous estimate from Gottfries and Jarosch (2023; 4%), as that paper's wage posting model considers only across-job wage growth.

<sup>69</sup> At the same time, high-tech workers might be more geographically mobile 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.'s [2022] definition) is 20% compared with 15% across all other sectors.

## IX. 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 extends across legal jurisdictions, illustrating that NCA enforceability has far-reaching consequences on labor market outcomes that likely extend beyond the subset of workers who sign NCAs. Multiple sources of evidence indicate that strict enforceability reduces earnings by dampening workers' outside options, shutting down a primary way that workers 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 long-standing 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. Incomplete markets might 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 human capital investments. In this case, NCA enforceability would exacerbate inequality between high- and low-wealth individuals. The earnings effects of NCA enforceability might also interact with other labor market institutions, like unions. Additionally, increases in NCA enforceability (or in NCA use) may have contributed to the decline in the labor share of income over the past several decades.

Finally, an important policy-relevant question is how our findings speak to the social welfare consequences of enforceable NCAs. Undoubtedly, some of the earnings effects that we document reflect a transfer of surplus from workers to firms. But there is also reason to believe that some of the earnings effect reflects losses to efficiency. To the extent that enforceable NCAs impede worker mobility, strict NCA enforceability might depress overall productivity by limiting worker reallocation to high-productivity firms and (in expectation) reducing match-specific productivity. Furthermore, though outside the scope of our theoretical model, enforceable NCAs could lead to inefficiently low employment if they endow firms with classical monopsony power. At the same time, enforceable NCAs could raise productivity by enhancing firms' incentives to invest in training and other intangible assets (in which case the earnings decline that we find would reflect an even bigger shift in bargaining power

between workers and firms). We believe that a direct examination of the relationship between NCA enforceability and productivity is an important avenue for future research.

### Data Availability

Code and data replicating the tables and figures in this article can be found in Johnson, Lipsitz, and Lavetti (2024) in the Harvard Dataverse, <https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/37A0L2>.

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