MOBILITY CONSTRAINT EXTERNALITIES*

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June 2018

ABSTRACT

Covenants not to compete are often included in employment agreements between firms and employees, justified by each party's voluntary "freedom to contract." However, noncompetes may also generate externalities for all individuals in the market, including those who have not signed such agreements. We theorize that enforceable noncompetes increase frictions in the labor market by increasing uncertainty and recruitment costs, and by curtailing entrepreneurship. We find that in state-industry combinations with a higher incidence and enforceability of noncompetes, workers – including those unconstrained by noncompetes – receive relatively fewer job offers, have reduced mobility, and experience lower wages. The results offer policymakers a reason to restrict noncompetes beyond axiomatic appeals to a worker's "freedom of contract" and highlight labor market frictions that may impact firm-level human capital strategies.

* All authors contributed equally, and the names are in reverse alphabetical order. We thank the editor Chris Rider, three anonymous reviewers, JJ Prescott, David Tan, and the participants of the 2016 Tuck Summer Camp, 2016 Academy of Management Annual Meeting, 2017 NYU Economics of Strategy Workshop, 2017 Harvard Business School WIGE Mini-Conference, 2017 SMS Conference on Strategic Human Capital, 2017 Strategy Research Forum, the UMD Summer Strategy Research Workshop. All errors are our own.

Turnover allows workers to find more productive job matches, which can lead to greater economic productivity (Jovanovic, 1979; Mortensen & Pissarides, 1999). But, turnover can also represent loss of human capital and competitive advantage for the focal firm (Coff, 1997; Lippman & Rumelt, 1982). Mobility constraints, such as noncompete agreements and intellectual property enforcement, are used by firms to reduce employee turnover and prevent inter-firm knowledge diffusion (Campbell, Coff, & Kryscynski, 2012a; Ganco, Ziedonis, & Agarwal, 2015; Marx, Strumsky, & Fleming, 2009; Png & Samila, 2015). Recent research has examined the effects of mobility constraints on the *focal* worker, but no attention has been paid to potential market frictions they generate, and whether they affect *other* workers not party to the contractual constraint. Given variation across labor markets in both the incidence (fraction of workers constrained) and enforceability (the extent to which courts will enforce the constraints) of noncompetes, we lack an understanding of how increases in the incidence of enforceable noncompetes creates externalities for others in the market. Addressing this important gap has significant research, policy, and managerial implications regarding turnover-related productivity and concomitant individual or social welfare.

Theoretically, we address this gap by building on two related insights. First, we rely on classic work on labor market frictions (Diamond, 1982) to theorize that increases in the incidence of enforceable noncompetes result in increased uncertainty and asymmetric information. These frictions create trading externalities through increases in recruitment and search costs for hiring firms and workers respectively. Second, enforceable noncompetes increase barriers to entrepreneurship (Starr, Balasubramanian, & Sakakibara, 2017), thus curtailing an important engine for job creation (Decker, Haltiwanger, & Jarmin, 2014). Accordingly, we hypothesize that a greater incidence of enforceable noncompetes will be associated with reduced job offer rates, reduced mobility, and lower wages, even for the unconstrained.

We test these hypotheses using novel data from the 2014 Noncompete Survey Project (Prescott, Bishara, & Starr, 2016). This nationally representative sample of over 11,500 U.S. labor force participants contains rich information on individual and job attributes, including whether

noncompetes have been signed by the focal individual. Our empirical approach first examines the labor market effects of higher incidence of enforceable noncompetes on the entire labor market (the constrained and the unconstrained) using the NSP data. We then isolate the pure externality effects of increased incidence of enforceable noncompetes on unconstrained individuals alone in the NSP data. The results indicate that job offers, mobility rates, and wages are all lower in state-industries where the use and enforceability of noncompetes are higher. We also find what matters is not the incidence of noncompetes per se, but its interaction with enforceability; in states with greater noncompete enforceability, an increase in the proportion of constrained individuals reduces the relative rate of job offers, mobility, and wages, even for the unconstrained. These results are robust to the inclusion of many potentially confounding variables, and diagnostic tests (Oster, 2017) suggest selection on unobservables would need to be uncannily strong to overturn our results. We also replicate our market-level wage and mobility results using data from the Current Population Survey.

We contribute to several literature streams. While existing research has documented the impediments of noncompetes on the focal worker's mobility options (Garmaise, 2009; Marx et al., 2009; Starr et al., 2018b), we show noncompete agreements indirectly constrain mobility in two ways. The same workers who voluntarily enter into noncompetes are also receivers of externalities produced by others who entered into these agreements, and, moreover, these market externalities constrain the mobility options of those who are *unconstrained* by these agreements. In addition, while existing work has documented how the mobility of one worker may impact others in the form of team mobility (Campbell, Ganco, Franco, & Agarwal, 2012b; Groysberg & Lee, 2009; Marx & Timmermans, 2017), we show how even unrelated workers may be impacted due to these market frictions.

To the strategic human capital literature, we integrate a market frictions approach (Diamond, 1982; Mahoney and Qian, 2013) within research on mobility constraints. While it may seem beneficial to utilize mobility constraints for greater retention of human capital and valuable information (Agarwal, Ganco, & Ziedonis, 2009; Ganco et al., 2015), the aggregate result may be a reduction in local labor market efficiencies (Gilson, 1999; Saxenian, 1996), with potential losses in productivity and

innovation due to less optimal employee-firm matches (Jovanovic, 1979). Our results thus suggest the use of enforceable mobility constraints may form a microfoundation for market-level frictions.

In light of these results, managers contemplating the use of mobility constraints or making location decisions may consider state-level enforceability regimes not only for their own use of such constraints, but also for the effects on labor markets within which they operate. To legal and policy scholars, our work provides novel insights regarding trading externalities of noncompetes on even those not bound by them. While noncompetes are defended on the voluntary "freedom of contract" basis (Callahan, 1985), our results highlight the non-trivial costs borne by others in the labor market.

THEORY DEVELOPMENT

Determinants and Implications of Turnover for Human Capital Markets

The beneficial implications of turnover include higher employee earnings (Topel & Ward, 1992), increases in firm status (Tan and Rider, 2017; Bidwell et al., 2015), knowledge diffusion between firms (Almeida & Kogut, 1999; Møen, 2005), and increased economic productivity (Jovanovic, 2015). At root, turnover occurs because a job is both a "search" and an "experience" good, so information about the fit between a worker and the employer is revealed through initial search and screening (Burdett & Mortensen, 1998), and later through experience on the job (Jovanovic, 1979). Accordingly, turnover may result from new information about alternative prospective matches, or from learning about the focal match's quality, relative to other potential options. While early models of turnover focused on the worker and poaching firm as key players (Burdett & Mortensen, 1998), later models incorporate the current employer's ability to counteroffer in a three-party bargain (Bagger, Fontaine, Postel-Vinay, & Robin, 2004; Postel-Vinay & Robin, 2002). Thus, mobility and wage outcomes are determined by preferences, the expected productivity of the potential match, and rent-sharing by firms with workers (Postel-Vinay & Robin, 2002). Moreover, the fruition of these benefits relies on well-functioning markets, absent frictions resulting from limited information or uncertainty among other factors (Diamond, 1982; Mahoney and Qian, 2013).

Meanwhile, strategic human capital scholars highlight the considerable costs related to turnover from the current employer's perspective. Turnover represents a loss of human assets, an

important source of competitive advantage (Coff, 1997; Lippman & Rumelt, 1982), and leakage of valuable information of the firm's routines, capabilities, and even other employees (Campbell et al., 2012b). Firms also incur recruiting costs to fill the vacancy left by the departed employee (Tziner & Birati, 1996). Finally, the threat of mobility may cause firms to underinvest in their employees' human capital, resulting in dynamic losses in productivity for the firm and the worker (Schultz, 1961; Starr, 2018). Given these consequences, scholars have identified various strategies firms use to deter mobility. Strategies directed at incentive alignment include efficiency wages and compensation dispersion, which reduce mobility by permitting greater rent sharing (Carnahan, Agarwal, & Campbell, 2012; Coff, 1997; Salop, 1979). Strategies imposing mobility constraints include reputations for patent litigation (Ganco et al., 2015), use of trade secrecy laws (Png & Samila, 2015), and noncompete covenants in employment contracts (Garmaise, 2009; Marx et al., 2009; Starr et al., 2018b).

The two literature streams have developed in parallel, resulting in a research gap worthy of deeper examination. Specifically, the above strategies have different implications for turnover causes and consequences. While rent sharing strategies may be incorporated into the offer-counteroffer process in three-party bargaining turnover models, strategies imposing mobility constraints may create market frictions whose effects may extend beyond the three focal parties (Diamond, 1982). We focus on noncompetes as one such constraint and next turn to a brief review of this literature.

Noncompete Agreements as Constraints to Mobility

To become a binding constraint on an individual's mobility, noncompetes must be agreed upon contractually, and these agreements must be held up legally (i.e. they should be enforceable). To the latter point, legal scholars and courts have long debated noncompete enforceability in relation to the worker's freedom to trade vs. the worker's freedom of contract. The "freedom to trade" argument privileges workers' ability to move across employers and is rooted in the notion that noncompetes are akin to indenture (Blake, 1960). Scholars note noncompete agreements are unfair to workers who have limited bargaining power and those 'forced' to sign noncompete agreements after accepting the job offer (Greenhouse, 2014; Marx, 2011; Starr, Prescott, & Bishara, 2018a). The "freedom of contract" argument puts primacy on voluntary agreements between parties when value creation requires

investments that yield returns later or over a longer period. Scholars find evidence for both perspectives (Starr, 2018; Starr et al., 2018a). In the face of these contrasting perspectives, courts have developed very different enforcement policies. California and North Dakota do not enforce them.[1] Among the rest of the states that enforce noncompetes, enforceability is contingent on a number of unobserved job-specific factors including the employee's geographic location, the employee's job duties, the reason for separation (e.g., fire or quit), the consideration (e.g., wages or training) given to the worker in exchange for signing the noncompete, when the noncompete was signed, the breadth of the the noncompete, etc. (Bishara, 2011).

For those bound by noncompetes, the recourse of a legally enforceable mobility constraint increases the employer firm's bargaining power in three-party negotiations (Postel-Vinay & Robin, 2002; 2004). Here, what matters is not only the use of noncompetes, but also their expected enforceability. On the one extreme, unenforceable noncompetes may afford little leverage to the employer firm for using them. But, with a higher likelihood of enforceability, each party's decision must incorporate the expected costs of noncompetes, which include the employer firm's ability to seek compensation, should it choose to release the worker from the contractual obligation, and litigation-related uncertainty and costs, should the move occur in spite of the noncompete agreement (Balasubramanian et al., 2017; Rauch & Watson, 2015). While unenforceable noncompetes may be less effective, enforceable noncompetes increase recruiting costs—time, monetary, psychic investments, and potential litigation costs—for all negotiating parties. In the case of entrepreneurship, the entrepreneurial employee bears these costs. Accordingly, noncompete consequences include lower levels of employee mobility (Balasubramanian et al., 2017; Garmaise, 2009; Lavetti, Simon, & White, 2014; Marx et al., 2009; Starr et al., 2018b), increased "career detours" by workers (Marx, 2011), and reduced entrepreneurship and startup size (Starr et al., 2017; Stuart & Sorenson, 2003).

A salient feature of the existing noncompete literature is its focus on only the parties involved in the specific contract (the worker, the employer, and the poaching firm). Left unexamined are the

[1] Total or partial bans have recently been proposed in Hawaii, Missouri, New Jersey, Maryland, Massachusetts, Michigan, and Washington.

effects enforceable noncompetes have on the overall market, including those who have not signed them. Likely, this gap may either be due to the theoretical focus on first-order effects of noncompetes on involved parties (Lavetti et al., 2014; Marx, 2011; Starr et al., 2018a), or because previous studies were unable to empirically distinguish between signers and non-signers (Garmaise, 2009; Marx et al., 2009; Starr, 2018; Stuart & Sorenson, 2003). Investigating the effects of enforceable noncompete agreements on the market as a whole and on those who have not signed them is important for several reasons. First, it enables more effective policy design regarding noncompete enforceability by taking into account potential positive or negative externality effects. Second, managers making location decisions may want to be aware of these effects to either avoid or embrace them.

Market Frictions and Externality Effects of Noncompetes

The human capital market search process is time intensive and costly for firms and prospective employees (Bidwell and Keller, 2014), with job offers often made sequentially (Rubinstein & Wolinsky, 1985). Noncompetes increase overall human capital market frictions in this process in two important ways. One, when used under higher expectation of enforceability, there is increased uncertainty and recruiting costs for the *constrained* workers. Two, noncompetes add a source of limited/asymmetric information because *who* is constrained is not transparent or public knowledge, resulting in additional recruitment and screening costs for firms, and search costs for prospective employees.[2] These frictions, we argue below, create negative externalities in the market, for constrained and unconstrained workers alike.

First, however, we acknowledge the potential of positive externalities for the unconstrained workers. For positive externalities to accrue to the unconstrained, not only should there be transparency about who is constrained vs. unconstrained, but either firms should redirect their recruitment towards the unbound, or constrained workers should self-select out of the market. We

[2] Here, we assume *ceteris paribus* conditions on all worker attributes other than whether they have signed noncompetes. i.e. we are interested in comparing similar workers in terms of education, age, occupation, and industry experience but different only in the dimension of whether they are in markets with varying levels of noncompete incidence and enforceability. The empirical evidence regarding same workers flowing in and out of jobs using noncompetes (Starr et al., 2018a), and lack of knowledge about whether workers will be asked to sign noncompetes (Marx, 2011; Starr et al., 2018a) is consistent with this assumption.

describe each of these in turn. Unconstrained workers may benefit from being in a labor market with many constrained individuals if most, if not all, labor demand is directed towards them (Rogerson, Shimer, & Wright, 2005), which would occur if hiring firms perceive the expected costs of recruiting constrained workers to be higher than expected benefits. This requires the recruiting firm to ex-ante assess the expected costs associated with bargaining or litigating for release from noncompetes to be higher than the expected productivity gains from the next best alternative match. Second, positive externalities may occur if constrained workers are more likely to opt-out of the market by not applying for job openings, given their "stuck" status. This requires (1) constrained employees know they have signed a noncompete agreement, (2) constrained workers estimate the agreements are strong enough to make mobility unlikely, (3) there is minimal "head-hunting" initiated by the recruiting firm. Under such assumptions, only unconstrained workers will apply for open vacancies and constrained workers will select out of the market. Taken together, these conditions (transparency, directed search by firms, lack of search by constrained workers) will result, ceteris paribus, in positive externalities for the unconstrained through higher offer rates, mobility and wages.

Empirical evidence suggest these conditions do not exist. Information on who is constrained is neither easily available or divulged: Starr et al. (2018a) show nearly 30 percent of employees themselves do not know if they have signed a noncompete agreement, and Prescott and Starr (2018) report that 60% of the unconstrained workers do not know if the state they are in enforces noncompetes. Firms also do not seem to engage in directed search towards unconstrained workers: Starr et al. (2018b) find no evidence that constrained workers receive fewer offers than the unconstrained. Finally, constrained workers do not completely opt-out of the labor market; noncompetes were a factor in about 40 percent of the job offers turned down by constrained employees (Starr et al. 2018b). Therefore, positive externalities for the unconstrained are unlikely.[3]

[3] A related positive externality relates to new venture creation: if noncompetes constrain some individuals to start new businesses, then the unconstrained may find greater product market opportunities for entry. However, given the high use of enforceable noncompetes may limit the focal new firm's ability to hire, it is again an empirical question whether unconstrained are able to exploit such opportunities. Evidence suggests that reduction in formation of firms due to noncompete enforceability is not accompanied by a concomitant increase in the formation of new firms of founders

We thus posit that labor markets with a higher incidence of enforceable noncompetes will have negative externalities because they limit the number of potential trading partners in the market, causing delays from both demand and supply side actors.[4] We suggest this occurs for two primary reasons, both of which result in reduced rates of job-offers, which in turn reduce mobility and wages for the unconstrained workers. We note any one of the following manifestations of the trading externality is sufficient for generating the results, and no one reason is necessary.

First, imperfect information and increased recruiting costs will cause delays and even reductions in job offers. On the demand side, distinguishing between constrained and unconstrained workers is time-consuming for hiring firms. Similarly, on the supply side, workers may not be aware they are constrained, resulting in additional costs as constrained individuals entertain offers they would otherwise turn down were they aware of their noncompete (Starr et al., 2018b). Moreover, when poaching firms and constrained workers enter into three-party negotiations to seek release from employer firms in spite of noncompete constraints, they have to invest time and effort to estimate the expected costs and benefits of hiring constrained workers. Accordingly, the presence of constrained workers increases uncertainty and slows down the recruiting process for hiring firms, decreases the net expected value of recruiting and thus delays or reduces the number of offers eventually made.

On the supply side, if workers value being unbound by an enforceable noncompete, but know they are in a market characterized by their frequent use, then assessing cost-benefit tradeoffs of potential noncompete clauses in new employment contracts in light of other compensating features (Starr et al., 2018a) adds cognitive effort and time, thus increasing expected search costs. To the extent that workers prefer to be unbound, they may experience higher job satisfaction at their current (unconstrained) position, which may cause them to search less if they are in labor markets with a high incidence of enforceable noncompetes.

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from other industries (Starr et al. 2017). Furthermore, new firms created in high enforceability states also tend to be start smaller, suggesting that hiring challenges are prevalent.

[4] To the best of our knowledge, Diamond (1982) was the first to highlight this trading externality, noting increases "in the number of potential trading partners makes trade easier" (pp.882). Shimer (2001) discusses this trading externality in the context of young and old workers, highlighting that "hiring is easier when a larger proportion of the labor force is willing to accept a job" (pp. 972). In our case, noncompete related market frictions create the opposite effect, resulting in limiting potential trading partners, creating negative trading externalities.

Second, new firms contribute disproportionately to job growth (Decker et al., 2014). Scholars have documented that noncompete enforceability leads to fewer and smaller new firms (Starr et al., 2017; Stuart & Sorenson, 2003). We expect the noncompete-related decline in the rate of entrepreneurial firm-formations to reduce labor market options, for constrained and unconstrained workers alike. This decline is because, in areas with a greater prevalence of enforceable noncompetes, constrained workers may have difficulty being released from their noncompetes to found firms in the first place, which results in a loss of opportunities for job creation. Even if they are able to secure release, they may perceive startup costs associated with recruiting employees to be prohibitively high due to the prevalence of enforceable noncompetes in the labor market, resulting in fewer job openings.

In summary, noncompete trading externalities may be low when they are either not used, or used in non-enforcing states (with lower credible constraints on mobility). As noncompetes become more enforceable and their incidence increases, they will create trading externalities that manifest in increased recruiting costs and reduced new job creation from the firm perspective, as well as potentially reduced job search by the unconstrained.[5] Job offers are a revealed indicator of these underlying factors, because they represent a culmination of the recruitment, search, and bargaining processes. All factors lead to fewer job offers, and so we posit:

H1: As the incidence of noncompetes rises, workers will have relatively fewer job offers in states that more vigorously enforce noncompetes.

The logical outcome of reduced job offers, it follows, is reduced mobility and lower wages. In turnover models, the rate of job-offer generation is critical to matching efficiencies (Burdett &

[5] Vacancy chains (White, 1970) may amplify the proposed demand side effects. Vacancy chains occur because of sequential replacement hiring. An initial move (e.g., retirement, opening of a new job) creates an open vacancy, which, when subsequently filled by an employed worker, leads to the creation of another vacancy. This process of filling and opening vacancies continues to create a "chain" of vacancies linked by moves of individuals who are currently employed. The vacancy chain logic implies a magnification of costs associated with greater incidence and enforceability of noncompetes. Congestion in filling just one vacancy will naturally cause delays in filling all subsequent vacancies in the chain. Moreover, if each vacancy in the chain experiences delays from the presence of constrained workers, then delays are further magnified, resulting in every subsequent offer within the chain being further postponed. Finally, if an entrepreneurial firm is not created, or an existing firm closes its search altogether due to noncompete-related increases in expected recruitment costs, then the chain will not initiate or it will stop entirely, with no subsequent options for mobility for those in the counterfactual chain.

Mortensen, 1998; Mortensen & Pissarides, 1999). Indeed, three-party bargaining models predict even small increases in the rate of outside job offers puts strong upward pressure on wages,[6] with mobility increasing or staying the same, depending on preferences and whether individuals and employer firms renegotiate the terms of the employment contract (Cahuc, Postel-Vinay, & Robin, 2006). In our case, a higher incidence of enforceable noncompetes decreases the number of prospective employment options, and thus works in the opposite direction. Fewer job offers received per period imply individuals have a reduced ability to seek or receive options which may increase overall productivity (Jovanovic, 1979), thus reducing their ability to move or increase their wages through the offer-counteroffer process (Postel-Vinay & Robin, 2002). Accordingly:

H2: As the incidence of noncompetes rises, workers will have relatively higher tenure (lower mobility) in states that more vigorously enforce noncompetes.

H3: As the incidence of noncompetes rises, workers will receive relatively lower wages in states that more vigorously enforce noncompetes.

DATA AND EMPIRICAL APPROACH

Our theory is about how an important labor market characteristic—the use and enforceability of noncompetes—creates externality effects on all individuals in the market, even those who are unconstrained by noncompete agreements. Accordingly, our empirical work must measure the independent variable of interest at the level at which these differences matter and exist. Noncompetes generally restrict mobility within an industry and geography, and are enforced at the state level. Consistent with our interest in whom noncompetes impact, our unit of analysis is at the worker level. We seek to compare otherwise identical workers who vary only in their exposure to noncompete incidence and enforceability. We address these factors in our data description and study design below.

Data Sources and Sample

Most studies of noncompetes do not use data which distinguish between whether a worker has signed a noncompete, relying instead on differences in enforceability within and across states (Conti, 2014; Fallick, Fleischman, & Rebitzer, 2006; Garmaise, 2009; Marx et al., 2009). To test our

[6] Infinitely high job-offer rates naturally lead to a perfectly competitive market (Burdett and Mortensen 1998).

hypotheses, we need a dataset that includes information on whether an individual is bound by a noncompete, in addition to detailed characteristics of workers across nationally representative labor market pools, local labor market characteristics, and our dependent variables of interest. The 2014 Noncompete Survey Project (Prescott et al., 2016), hereafter called NSP, represents such a unique dataset, drawn from a nationally representative data of over 11,500 labor force participants. We highlight salient aspects of the survey and rigorous cleaning process for the data here, and refer interested readers to the extensive description in Prescott et al. (2016).

The NSP is specifically designed to study the use and effects of noncompete covenants across the U.S. Labor Force. It surveys U.S. labor force participants aged between age 18 to 75, in the private (for-profit and non-profit) sector and public healthcare system, and was administered to verified respondents via Qualtrics. Survey responses that did not pass attention checks or gave answers that appeared intentionally noncompliant were excluded from the data, as were responses from the same IP address to avoid duplicate survey responses from the same person. The NSP data were manually cleaned and validated for occupation and industry codes by numerous sets of RAs, who independently hand coded information from written descriptions of employer characteristics, worker job titles and self-described job duties. Prescott et al. (2016) document the robustness of the survey to sample selection concerns through an extensive investigation of the possible sources of sample bias. After ascertaining the data to be as clean and free of sample selection concerns as possible, Prescott et al. (2016) apply various weighting schemes to ensure the data are reflective of the population of labor force participants in 2014. To accomplish this, they merged the NSP data with data from the 2014 American Community Survey, a nationally representative sample that encompasses over a million respondents and used to determine many federal thresholds. We weight the observations using an iterative proportional fitting.[7]

[7] Prescott et al. (2016) tested several weighting schemes, including iterative proportional fitting, inverse probability weighting, and post-stratification. After testing the various limits of these weighting schemes, Prescott et al. (2016) concluded iterative proportional fitting performs the best.

While the NSP includes detailed information on worker and job characteristics, we also need data on local market characteristics. To obtain these, we merge the NSP with data from County Level Business Patterns to include measures of local competition by industry, data on noncompete enforceability from Starr (2018), and with data from the Current Population Survey to validate our main estimates in a second, nationally representative database.

From the full sample, we also extract the unconstrained sample from the NSP by restricting respondents who are not bound by a noncompete. However, an important challenge in doing so is the treatment of the 30% of individuals who answered "*maybe*" to the question of whether they signed a noncompete. Over 83% of the "maybes" are coded as such because they had never heard of a noncompete, which suggests that they have likely not signed a noncompete but is not conclusive. We thus conservatively adopt the multiple imputation approach used by Prescott et al. (2016), who generate 25 different datasets in which the *maybes* are assigned a *yes* or *no* depending on the statistical similarity between those who have and have not signed. The benefit of this approach is that all estimates account for the uncertainty in whether the *maybes* have signed by averaging the 25 estimates across the 25 datasets and correcting the standard errors for the additional uncertainty. Because of multiple imputation, the unconstrained sample varies between 7,718 and 7,762 across the 25 imputed datasets, with a median of 7,742.[8]

Dependent Variables

We use two measures to capture the rate of job offers. Our first measure, *Job Offer in Last Year,* is an indicator variable set to 1 if the worker has received a job offer in the last year, and zero otherwise. Our second measure, *Job Offer During Current Employment,* is a similar indicator variable, set to one if the worker has received a job offer since their most recent change in employment, and zero otherwise. Our use of the two measures accommodates for differences in job offer flows and tests for robustness, as one year may be too short a time frame to observe all job offers received, and offers received over all years in current employment is conflated with tenure. Our cross-sectional data limits observation of mobility events over time, so we use observed *Tenure* at the current firm (in years) as our measure

[8] To avoid confusion, we omit number of observations in the tables for the unconstrained sample.

of mobility. Tenure reflects a *lack* of prior mobility (i.e., if a worker has stayed a long time at their job it is because they have not had a mobility event).[9] Given the limitations of this measure, we conduct additional robustness checks, described in more detail later, using actual mobility events in the CPS data. Note we use the CPS data as a robustness check, as opposed to the main analysis, because it does not provide the full set of controls available in the NSP, and because it does not contain information on whether the focal individual is constrained. Finally, we use the worker's logged hourly *Wages* as a dependent variable in wage regressions. In the CPS robustness checks, we use logged weekly wages.

Independent Variables

Our key independent variable of interest—*Incidence of enforceable noncompetes*—is the interaction term of two underlying variables: the incidence of noncompetes and the degree of enforceability of noncompetes across state-industry combinations. Recall this is because the use of noncompetes alone may not impose recruiting costs if they are not perceived to be credible threats. Similarly, the enforceability of noncompetes will impose no recruiting costs if firms do not use them. *Incidence* of noncompetes is measured as the proportion of individuals bound by a noncompete in each state-industry. Figure 1 presents a heat map of the incidence of noncompetes across state-industry combinations.[10] To measure the *Enforceability* of noncompete agreements, we utilize the index in Starr (2018), which builds upon the underlying research by Bishara (2011). The initial research by Bishara (2011) quantified each state's position in 2009[11] (whether by statute or case law) on seven dimensions of noncompete enforceability between 0 and 10, with 10 being the highest level of enforceability. These dimensions include, for example, whether the state will enforce a noncompete if the worker is fired, whether the court will modify an unreasonable contract and then enforce it, and whether additional consideration beyond continued employment is required. The Starr (2018) measures incorporate the underlying Bishara scores and weights them using factor analysis. Figure A1 provides

[9] Although somewhat non-standard, the use of tenure to measure mobility stems from a tight mapping between tenure and likelihood of a move: e.g. a 10% probability of move in a given year results in expected tenure of 10 (=1/.10) years.
[10] Only state-industries with at least 10 individuals in the survey are included in the sample, to ensure representativeness. See Table A1 in the online appendix for evidence that our results are robust to other cutoff values. The precise numbers from Figure 1 are available in an Excel file included in the online appendix.
[11] We are not aware of any new noncompete laws passed between 2009 and 2014, despite a number of recent changes in 2015 and 2016. We thus view the 2009 measures as a good proxy for the level of enforceability in 2014.

this index, with states ordered from low to high enforceability. This index is normalized to have a mean of zero and a standard deviation of one from a sample where each state is given a weight of one.

[Insert Figure 1 here]

Empirical Approach

In an ideal research design, we would observe the mobility, job offers, and wages of workers who are randomly assigned to areas with varying levels of noncompete usage and enforceability. However, as Starr et al., (2018a) document, noncompetes are not randomly assigned.[12] Thus, our empirical approach must overcome two related endogeneity concerns. First, why are noncompetes more prevalent in some state-industries, and why are noncompetes more enforceable in some states? Second, are there systematic differences between workers who do not sign noncompetes across low and high incidence*enforceability state-industries? If answers to these questions are related to any of our dependent variables, then our estimates will be biased absent controls for such omitted variables. To address these concerns, we either need an instrument that exogenously affects the use and enforceability of noncompetes without affecting wage and mobility outcomes through any other mechanism, or we need to employ a rich set of controls to capture potential unobserved variables and conduct additional tests to address potential alternative explanations that pose a threat to inference.

We are unable to use the first approach,[13] and rely on the second to address potential endogeneity concerns by using the inductive-statistical approach to assess the improbability of alternative explanations (Hempel, 1962; Salmon, 1989). We do so in the following ways. First, we

[12] Starr et al. (2018a) show 18.1% of U.S. labor force participants are noncompete bound, and high skilled, higher earning individuals are most likely to sign noncompetes, though noncompetes are also prevalent among low-earners.
[13] A likely instrument for the use of noncompetes would appear to be the laws that regulate the enforceability of noncompetes. Perhaps surprisingly, as shown in Starr, Bishara, and Prescott (2018), the use of noncompetes is only partially correlated with their enforceability, resulting in a limited first stage. Other than enforceability, one can think of a number of other potential instruments, such as shocks to the local competitive environment or the diffusion of these practices through the mobility of managers. However, each of these potential instruments are unlikely to satisfy the exclusion restriction. For example, changes to the local competitive structure (e.g., a sharp increase in the number of competitors) may cause firms to be more likely to use noncompetes, but they will also likely change other wage and mobility patterns as well (Haveman & Cohen, 1994; Olson, 2017). Similarly, the diffusion of managers from high use industries to low use industries will violate the exclusion restriction if such managers bring with them other practices outside of the noncompete which also affect wages and mobility patterns. Given these concerns, we are left without a viable instrument for the use of noncompetes.

exploit an extensive and unique set of controls within our data, to account for the effects of the most serious set of explanatory variables. These include, for example, poaching rates to and from the firm, indicators for the type of valuable information possessed by the worker, and the existence of a number of other post-employment restrictions or HR benefits. The extent of saturation in the model is substantial, with the R-squared in cross-sectional wage regressions reaching 0.57.

Second, in addition to conducting a variety of robustness tests to examine alternative explanations, we utilize the diagnostic test developed in Oster (2017), which extends the method in Altonji et al. (2005) to examine the sensitivity of the results to possible selection on unobservables.[14] The intuition behind this method relates to how the coefficient of our variable of interest and the R-squared change between a less saturated (omitting many controls) and more saturated model (including such controls) in the model. If (i) the coefficient of interest stays the same as the additional controls are added to the model *and* the R-squared increases substantially, then there is less variation that could overturn the estimate and we might be relatively confident in the *direction* of the results. If, however, (ii) the R-squared barely moves and the coefficient of interest does not change, then this does not tell us anything about how confident we should be about selection on unobservables because we have not explained any more variation in our dependent variable. Alternatively, (iii) if the R-squared rises, but our coefficient of interest falls dramatically, then we should be less confident that our results are robust to omitted variables. Oster (2017) develops a simple formula that integrates outcomes (i), (ii), and (iii) to produce a parameter δ that reflects how strong selection on unobservables would have to be, relative to selection on observables, to drive the estimated treatment effect to zero. In this test, $\delta=1$ suggests selection on unobservables would have to be equally strong as selection on observables to overturn the results. If $\delta>1$, then selection on unobservables would have to be stronger than selection on observables. We note the Oster (2017) method does not provide an estimate of bias;

[14] The test can be administered through the Stata command, *psacalc*. The inputs to the program are the maximum R-squared that could possibly be expected in explaining variation in the particular dependent variable, the baseline set of controls, and the advanced set of controls. Oster (2017) recommends the maximum R-squared be the minimum of 1 or 1.3 times the R-squared from the most saturated model.

instead it provides information on how confident one can be in the directionality of the estimate. Oster (2017) recommends a cutoff value of $\delta=1$ is appropriate if one can control for all of the first order determinants of the dependent variable.[15]

In our main analysis, we employ the following empirical specification:

$$Y_{iojs}=a_0+a_1*Incidence_{sj} + a_2*Incidence_{sj}*Enforceability_s+\theta_s+ \alpha_{oj} +BX+e_{iojs}$$

Where Y_{iojs} are our outcomes of interest, and θ_s are state fixed effects, which subsume the main effect of enforceability. The terms α_{oj} are occupation (2-digit SOC) by industry (2-digit NAICS) fixed effects. The use of both state-level and occupation by industry fixed effects are important because they ensure neither state-level policies or demographic differences, nor comparisons across different jobs will drive our results. The basic set of controls, *X*, in the initial specification are a third-degree polynomial in age, education, hours and weeks worked and their interaction, gender, sector of the worker (e.g., for-profit), a dummy for multi-unit firm, and firm size.[16] We describe two sets of advanced controls in the robustness section below. The standard errors are clustered at the state level. The job offer regressions are estimated using linear probability models given ease of calculation and interpretations of marginal effects for interaction terms as percentage point changes (Zelner, 2009); Appendix Table A4 shows results are robust in logit specifications.

RESULTS

We report descriptive statistics in Table 1. Panel A reports summary statistics for the full sample and Panel B reports them for the unconstrained sample. For all variables, the means and standard deviations are very similar in the two samples. The full sample has slightly longer mean tenures, which we should expect given the full sample includes workers who have signed

[15] As a result, the $\delta>1$ threshold is based on the assumption that the controls included in the most saturated models are the most important "next" set of controls that are not included in the less saturated model. To examine the robustness of our results to our choice of the "next best" controls, we report in Table A2 the δ term for a comparison between a less-saturated model with all controls except for the occupation by industry fixed effects and state fixed effects and a more saturated model that includes these fixed effects.

[16] While worker tenure is often included in wage regressions, we do not do so because tenure is an intermediate outcome of incidence*enforceability—making it what is called a "bad control," which therefore biases results. Nevertheless, Appendix Table A3 shows robustness of results when controlling for tenure.

noncompetes. In what follows, we present our analyses first for the full sample, and then for the unconstrained sample.

[Insert Tables 1 & 2 here]

Table 2 shows the full sample effects of the interaction of the incidence and enforceability of noncompetes on each dependent variables of interest; Job offers are predicted in Panel (A) and (B), while Tenure is predicted in Panel (C) and Wages are predicted in Panel (D). Each specification builds up from the simple interaction, adding state fixed effects, then occupation by industry fixed effects, and finally adding basic controls. We note the main effect of *Enforceability* is absorbed by state fixed effects. The results in column (4) of Panel A suggest that for every percentage point increase in the incidence of noncompetes, the probability of a job offer rises by 0.17 percentage points in an average enforceability state (recall enforceability is mean centered to zero). But, a one standard deviation increase in noncompete enforceability reduces the probability of receiving a job offer in the last year by an additional 0.084 percentage points (i.e., the marginal effect of noncompete incidence is 0.17-0.084*Enforceability). This interaction effect is more negative in column (4) of Panel B, -0.135, for the probability of receiving a job offer during the current employment spell. To appreciate the size of these interaction estimates, consider how a 10 percentage point increase in the use of noncompetes differentially changes the likelihood of receiving a job offer in the last year in an average enforcing state versus a nonenforcing state (a difference of 4 standard deviations). A 10 percentage point increase in the use of noncompetes reduces the likelihood of receiving a job offer in the last year by 3.36 (0.084*10*4) percentage points for an individual in an average enforcing state relative to a non-enforcing state (a 21% increase relative to the mean). The same comparison for job offers during tenure reveals a relative 5.4 (0.135*10*4) percentage point decrease, 16% of the mean rate.

Column (4) of Panel C suggests that for every one percentage point increase in the incidence of noncompetes, tenure rises by 0.004 years in an average enforcing state, but that a one standard deviation increase in enforceability increases tenure by an additional 0.02 years. This interaction effect translates into a 0.8 (0.02*10*4) year difference when comparing a 10 percentage point increase in the

use of noncompetes between an average enforcing state and a state that does not enforce noncompetes, a 12% effect relative to mean tenure. Column (4) of Panel D suggests that the interaction of noncompete enforceability and the incidence of noncompetes is associated with lower wages, though the difference is statistically insignificant at canonical levels.[17]

We turn now to the unconstrained sample, which allows us to starkly isolate the externality effects, given that such workers are free to leave for a competitor. Table 3 is similar to Table 2 and builds to the main specification in each of the four set of regression results for unconstrained workers.[18] As seen in Panel A and B, both job offer results (Column (4) of Panel A; Column (8) of Panel B) show the same patterns: the *Incidence* of noncompetes in a state-industry in an average enforcing state is not associated with statistically significant changes in offer rates, while the interaction term of *Incidence*Enforceability* is negative and statistically significant. That is, greater incidence of noncompetes reduces the rate of job offers in higher enforceability states relative to lower enforceability states. To interpret the interaction coefficient in column (4) of Panel A, an increase in the use of noncompetes in a state-industry by 10 percentage points results in a 3.6 (0.09*4*10) percentage point lower probability of receiving a job offer in the last year in an average enforcing state versus a non-enforcing state, which is 24% of the mean. Similar calculations from coefficients in column (8) of Panel B indicate a 10 percentage point increase in noncompete incidence is associated with a 5.04 (0.126*4*10) percentage point differential in the probability of receiving a job offer in the current employment spell when comparing an average enforcing state and a nonenforcing state, which is 16% of the mean. Therefore, taking Table 2 and 3 together, hypothesis 1 is supported in the overall market and also in the unconstrained sample.

[Insert Table 3 here]

[17] We also run similar models controlling for whether the focal worker has signed a noncompete with their current employer, to ensure that our results are not driven by the direct effect of a noncompete. The results, presented in Appendix Table A5, are consistent both in terms of magnitude and statistical significance.

[18] Table A6 examines heterogeneous effects of Incidence*Enforceability between the unconstrained and constrained using a triple interaction model (including the full set of controls that we introduce in Table 5), which adds a third interaction based on whether the worker is constrained by a noncompete agreement. These results show that higher Incidence*Enforceability is associated with lower job offer rates, longer tenure effects, and lower wages for the baseline unconstrained segment. The triple interaction term finds no statistically significant differences for the constrained.

The results in Panel C of Table 3, column (1) shows a strong positive effect of *Incidence* of noncompetes on the *Tenure* of the unconstrained, which dissipates as occupation by industry fixed effects are included in column (3). In the fully specified model in column (4), we observe a positive but statistically insignificant main effect, while the *Incidence*Enforceability* interaction is strongly positive and statistically significant. To interpret the coefficients from column (4) of Panel C, an increase in incidence of noncompetes in a state-industry of 10 percentage points results in an increase in tenure among the unconstrained of approximately 0.04 years, or 14 days, in an average enforcing state. But, for every additional standard deviation increase in enforceability, tenure increases by an additional 0.22 years, or 80 days. Thus, the differential between an average enforcing state and a non-enforcing state would be 0.88 (0.22*4) years, which is 13% of mean tenure. Accordingly, both Table 2 (overall market) and 3 (unconstrained sample) provide support for Hypothesis 2.

Columns (5) and (6) of Panel D show a positive and significantly strong effect of *Incidence* on wages, which dissipates in column (7) once we include occupation by industry fixed effects. In Column (8) of Panel D, we see a greater incidence of noncompetes in a state-industry is associated with a positive and statistically insignificant main effect, and the interaction with noncompete enforceability is negative and marginally statistically significant. Therefore, hypothesis 3 is supported in the unconstrained sample, though less strongly than the other hypotheses.[19] Interpreting the results from column (8), we find a 10 percentage point increase in the use of noncompetes in a state-industry is associated with a 0.51% (=0.051*10) increase in hourly wages in an average enforcing state, while a standard deviation increase in enforceability reduces wages by an additional 1.53% (=-0.153*10*1). Thus, a 10% rise in the incidence of noncompete is associated with approximately 6.12% (1.53*4) lower wages in an average enforcing state relative to a non-enforcing state.

These patterns are also depicted in Figure 2 as binned scatterplots to help interpret the interaction coefficients identified in Table 3, where the continuous enforceability measure is

[19] One potential reason the wage results are weaker is large interaction effect on tenure; longer tenures are also associated with higher wages. In Appendix Table A3, we see stronger negative wage effects when controlling for tenure.

dichotomized into high (weakly greater than average) and low (less than average) enforceability.[20] In addition to visualization of the extent of nonlinearity in this relationship, the binned scatterplots show the raw data and best fit lines after incorporating the set of controls in Table 3.

[Insert Figure 2 here]

Additional Analyses: Selection, Mechanisms and Robustness

In this section, we report on our inductive-statistical approach to assess the improbability of alternative explanations and conduct a series of robustness tests to assess sensitivity of results to choices of samples, variable definitions, and model specifications. As noted above, our empirical approach must overcome two serious concerns: (1) Why is there a higher incidence of noncompetes in some state-industries and why are noncompetes more enforceable in some states? (2) Are there systematic differences between workers who do not sign noncompetes across low and high incidence*enforceability state-industries? Table 4 presents a summary of potential concerns and results from robustness tests.

[Insert Table 4 here]

Panel A of Table 4 lists threats to inference stemming from labor market differences across states and state-industries, some of which manifest in differences in incidence and enforceability. Our baseline specification included in Tables 2 and 3 already reflects three sets of controls to deal with this issue. State fixed effects account for unobserved fixed state-level differences (e.g., policies or demographic characteristics); occupation-by-industry fixed effects ensure our effects are identified only through a comparison of workers in similar jobs (and facing similar labor market competition), and the logged number of firms in the respondent's county-industry (2 digit NAICS) capture extent of local labor market competition. Additional concerns relate to the fact that local labor markets for high-use*enforceability state-industries might generally exhibit less competition for workers, leading

[20] Appendix Figure A2 provides this Figure for overall sample. Both Figure 2 and Figure A2's results are based on the 20th set of imputed data, which was selected randomly. Identical plots for the other 24 datasets are available upon request. Also, given state fixed effects, the reported interaction in Table 2 and 3 reflect the relative differences in the slopes: in some cases this is as a result of high enforceability states having a steeper or flatter slope, than the low enforceability states.

to such that lower competition may result in lower job offer rates, longer tenure, and lower wages. In Table 5, Panel A, we include an expanded list of controls to address these concerns. Specifically, we replicate the most saturated models from Table 3 (Columns 4 and 8), but add additional controls for the reported industry flows between competitors (in addition to a variety of other variables described below), based on the respondents' perceived rate of job hopping between competitors in the focal industry. Appendix Table A7 shows robustness of results in the full sample.

[Insert Table 5 here]

Panel B of Table 4 addresses potential concerns regarding unobserved differences between *unconstrained* individuals in high and low incidence*enforceability areas. There are firm-level and individual-specific reasons for such selection effects, and the results from the expanded list of controls suggest these reasons do not impact our main findings. It may be the case that the focal worker is employed at a firm that does not use noncompetes because nobody poaches from them. Such a worker would naturally receive fewer offers, and exhibit lower mobility and wages. To account for this concern, we include a unique set of indicators in the regression based on the worker's answer to questions about how frequently workers are poached from, and by, their focal firm. Alternatively, it may be the case that the focal worker has not signed a noncompete because of lower baseline levels of mobility. To address this concern, we control for the number of employers the worker has had in the last five years. Another individual-specific concern relates to the value of the worker's knowledge; this may impact the worker's offers, tenure, and wages. To account for this issue, we include controls for the information the worker possesses, including trade secrets, whether they work directly with clients, or have client-specific information (as well as the interaction of each of these categories).

In high-incidence and high enforceability state-industries, firms and workers may also agree to alternative binding constraints or benefits (Kryscynski & Starr, 2017) instead of noncompetes. If so, then these alternative constraints may cause the negative effects we attribute to enforceable noncompetes. We account for this by including a unique set of control variables for both contractual post-employment restrictive covenants and various benefit-type constraints. The contractual, post-

employment restrictive covenants include indicators for whether the worker is bound by a nondisclosure agreement, a nonsolicitation-of-clients agreement, a nonpoaching-of-coworkers agreement, an IP pre-assignment agreement, and an arbitration agreement. The various benefits controls include indicators for worker as stock options, deferred compensation, retirement plan, health insurance, paid vacation, sick leave, life insurance, or bonuses. The even-numbered columns in Panel A of Table 5 show robustness of our main results to the inclusion of these controls, and Table A7 confirms similar results for the full sample.

We next report on the diagnostic tests (Oster, 2017) to assess the extent to which additional unobservable variables may be exerting undue influence on our results. In Table 5, we report the parameter, δ, from Oster's method for the interaction of *Incidence*Enforceability* in the even-numbered columns of Panel A and columns (4) and (8) of Panel B. The δ term for each dependent variable analysis is considerably larger than one. Based on this diagnostic statistic, to reduce the estimated effect to zero, selection on unobservables would need to be 8.75 and 15.94 times stronger than the selection on observables in the respective job offer regressions, and 3.88 and 1.62 times stronger in the tenure and wage regressions, respectively. Overall, these results strongly suggest any such unobserved variables are unlikely to overturn the directionality of our main estimates. Appendix Table A2 report similar results when Oster's delta includes state and occupation by industry fixed effects.

As summarized in Panel C of Table 4, we address other concerns related to sample selection, variable measurement, and to use of the NSP dataset. Our main sample did not include observations from state-industries in which fewer than ten people responded to our survey. To test the sensitivity of our findings to this decision, we test whether our results are robust to other cutoff values. Table A1 of the online appendix demonstrates that our results are robust to cutoff values of 5, 15, and 20.

Figure A1 reveals significant variation across states in the enforceability index. To address concerns regarding outlier effects, we replace the cardinal differences with an ordinal ranking of states between 1 and 51 (with high numbers reflecting higher enforceability) and standardize the ordinal enforceability index distribution to have a mean of zero with a standard deviation of one (to ensure

comparability with our main results). The results in Table A8 of the online appendix are similar to Table 5, suggesting cardinal differences in the enforceability measure are not driving our results.

Tests of Some Underlying Mechanisms

While we have provided evidence on our key mechanism—reductions in the rate of job offers—lack of data preclude direct tests of the recruitment/search costs, or new venture creation mechanisms. We are able to test some of the other potential mechanisms at play. One possibility is that unconstrained individuals in high incidence and high enforceability areas may be less likely to move or receive job offers because they are more satisfied with their jobs (despite the observed lower wages). For example, it may be the case that unconstrained individuals, particularly in state-industries with high noncompete incidence and enforceability have merely found the best jobs for themselves, or because they value non-pecuniary benefits, including the freedom to leave. If so, this may reflect in higher job satisfaction. We examine this mechanism in columns (1) through (4) of Panel B in Table 5 by reporting results on a new dependent variable, *Job Satisfaction,* measured on a 5 point Likert scale where 5 reflects high satisfaction. The results are inconsistent with this story, and are more consistent with the negative wage effects observed earlier —workers in state-industries with high incidence and enforceability are relatively *less satisfied* in their job.

Another possibility is that workers in high incidence and enforceability areas expend less effort in searching for a job. If unconstrained workers predict moving jobs is either more difficult, or will likely result in them needing to sign an enforceable noncompete agreement, then they may be less likely to engage in job search. To examine this mechanism, we test the interaction effect of noncompete incidence and enforceability on a new dependent variable*, Job Search*, equal to one if the respondent sent out one or more job applications in the last year and zero otherwise. The point estimates in Table 5, Panel B, columns (5) through (8) show that increased incidence and enforceability

of noncompetes is *not* associated with a negative effect on the job search effort of the unconstrained, suggesting that this mechanism is not operative.[21]

Replication in the CPS

The full sample results reported in Table 2, A5, and A6 already confirm the importance of market frictions for both constrained and unconstrained workers. These aggregate labor market analyses resolve an empirical concern too. In our analysis of the unconstrained, the nature of our empirical strategy created a power problem: As the proportion of constrained workers increases, the proportion of unconstrained workers diminishes. In the extreme case, if 100% of the workers in a state-industry signed noncompetes, there would not be an unconstrained sample. Thus, by definition, higher incidence state-industries will have a smaller sample of unconstrained than those with lower incidence. The fact that our results hold in the full sample implies they are not subject to power issues.

We also address two additional concerns arising from the use of NSP data. First, in spite of the NSP's nationally representative design, there may be unresolved sample selection issues. Second, there is potentially common source bias, given our dependent variables and a key component of our explanatory variable—*Incidence*—draws from the same survey. To address these issues, we replicate our analysis by using the Current Population Survey (CPS). The CPS is a monthly survey administered by the Census Bureau and the Bureau of Labor Statistics to over 60,000 households and is the standard dataset used by the government to measure labor force participation, unemployment, and other labor force characteristics across the US. Households are randomly selected to be in the survey, and participants are surveyed for two periods of four consecutive months, with a break in-between for eight consecutive months. We note the CPS does not distinguish between who is and is not constrained, and thus this sample allows us to estimate only the average effects across the market. Also, the CPS does not provide any information on offers received by individuals, so we are unable

[21] In part, the lack of evidence for reduced job search may be reflective of the knowledge burden imposed on workers for it to be operative. Reduced job search by unconstrained workers in state-industries with high incidence and enforceability requires they (1) know that many are bound, (2) know the constraints are enforceable, (3) don't want to be asked to sign a noncompete in a subsequent job. As discussed earlier, the empirical evidence suggests these conditions are not often met (Prescott and Starr; 2018; Starr et al., 2018a).

to replicate our results for offers in this sample. Nonetheless, the CPS provides data on individual mobility patterns within each 4-month period of interviews, and weekly wages from the interview at the end of the 4-months of interviews, from which one may make inferences for the underlying job offer mechanisms (Cahuc et al., 2006).

To construct the CPS sample for our analysis of mobility and wages, we utilize the information in the CPS for location, industry, and employment status of each worker to merge our measures of noncompete incidence (by state-industry) and noncompete enforceability (by state). We retain employed workers aged 18-70, in the private for-profit or private not-for-profit sectors, who had one job (so we can tie the occupation and industry to wages and mobility), and who responded to the CPS survey themselves (to reduce noise induced by proxy respondents who may not know, for example, the worker's earnings in the last week). We include data from 2012 to 2014 since our noncompete incidence measure was created in 2014.[22] While the CPS does not have the very rich set of controls available in NSP, the available data permits at least the inclusion of controls similar to our baseline specification (in Table 2). In particular, we can account for cubic age terms, quadratic terms for hours worked last week, indicators for whether the job is full time or part time, state fixed effects, occupation (3-digit Census code) by industry (3-digit Census code) fixed effects, year dummies, a gender dummy, and indicators for education level.

The results, presented in Table 6, mirror those from the NSP data in Tables 2. The interaction of *Incidence*Enforceability* points in the same direction as in Table 2 and maintains canonical levels of statistical significance: those in state-industries with a high incidence of enforceable noncompetes are relatively more likely to stay with the same employer and also earn relatively lower weekly wages.

[Insert Table 6 here]

DISCUSSION AND CONCLUSION

[22] For the mobility specifications, we keep workers who are present in each of the four months of the interview period (approximately 111 thousand workers), and measure *Stayed at same employer in 4 consecutive months* as an indicator equal to one if the worker *did not* switch employers over the four months in the sample. In the mobility sample, 95.4% of workers stayed at their employers over four consecutive months. For the wage specifications, we draw on the sample with wage information (approximately 160k observations), which are those in the outgoing rotation groups (i.e., the fourth and eighth interview), and measure *Wages* as the log of the worker's weekly earnings in the week prior to their interview.

The use of noncompete agreements is one among many strategies used to retain valuable human capital and protect competitive advantage, and scholars have studied the effects of such mobility constraints on individuals who are bound, firms, and industry/regional knowledge diffusion (Agarwal et al., 2009; Ganco et al., 2015; Marx et al., 2015; Marx et al., 2009; Png & Samila, 2015; Samila & Sorenson, 2011; Starr et al., 2018b; Starr et al., 2017). Within this literature, our study examines the externality effects of enforceable mobility constraints for others in the labor market, including individuals *not* bound by such constraints. We add an important and thus far missing perspective to the fundamental debate over tradeoffs between an individual's "freedom of contract" (supporting their voluntary agreement to reduce mobility options) and "freedom to trade" (supporting their ability to move at will; Blake, 1960). Our results—increases in incidence of enforceable noncompetes reduces the relative rate of job offers, wages, mobility, and job satisfaction in the overall market, and even for the unconstrained—suggest one person's "freedom of contract" may have costly externalities for another who did not sign away their own "freedom to trade."

Before turning to our study's implications and contributions, we note several limitations that may also spark future research. First, while the NSP data are the only available source to permit an in-depth examination of our research question, its cross-sectional nature precludes a longitudinal research design. Ideally, scholars need a dataset to follow individuals over their career lifecycle and examine the changes over time in their job offers, mobility, and wages as they interface within an external environment with longitudinal differences in the incidence and enforceability of mobility constraints such as noncompete agreements. We hope that researchers and statistical agencies (e.g. US Census; Bureau of Labor Statistics) will develop such longitudinal datasets to gather information on mobility constraints encountered by individuals.

Second, our theorizing relies on several underlying factors associated with increased market frictions. While we were able to test and rule out some reasons, data limitations preclude the testing of others. To directly test whether noncompetes increase recruiting costs and delays in the labor market, scholars could collect detailed data on the number of interviews conducted by firms and the

effort allocated to fill a vacancy. Such data could test predictions, consistent with our theory, that firms in high incidence and enforceability areas should take longer and/or interview more people to fill a single vacancy. Similarly, surveys and interviews of workers, both constrained and unconstrained, may provide insights on their underlying preferences, and decision-making calculus as they weigh different pecuniary and non-pecuniary benefits of employment contracts and workplace environments, and whether both preferences and choices change over career life cycles. Here, an experimental research design employing conjoint analysis may enable the examination of how workers evaluate noncompete agreements in light of the different features of their employment contract and workplace environments. Also, we relied on prior work showing noncompete enforceability reduces new venture formation (Starr et al., 2017; Stuart & Sorenson, 2003). A logical extension of our work would be to investigate whether such effects manifest largely because the constrained have lower propensity to create new firms, or because of indirect effects on the ability of the unconstrained to form new ventures, through increases in recruiting costs.

Relatedly, because we do not have firm-level data, we cannot observe internal versus external mobility events, which may play different roles in career advancement (Bidwell and Mollick 2015). One plausible direction for future work therefore is to examine whether enforceable noncompetes within the firm reduce the within-firm, versus the cross-firm, advancement of others. Additionally, we did not discuss theoretically, or examine empirically, factors that may enhance or attenuate effects of noncompete labor market frictions. These include underlying additional heterogeneity in terms of attributes of individuals (e.g. high/low performing; white/blue collar; more/less human capital fungibility, etc.) or firms (small vs. large; single product or diversified; good vs. bad HR cultures, etc.). These issues represent additional avenues for future research.

Another limitation of our study is that our interaction results are interpreted by comparing how a unit increase in incidence has different effects in a non-enforcing state and an average enforcing state. Yet we have little understanding of why the incidence varies in non-enforcing states, given that such provisions are unenforceable. We encourage future work to address this important gap.

A final limitation is that our results relate to one mobility constraint, albeit an important one. It is an empirical question as to whether these results will hold with regard to alternative constraints. However, our theory here can provide a guide for examining alternative constraints. For example, if constraints are easily observable, we would expect positive externalities onto the unconstrained, at least if the negative effects on job creation are not overwhelming. However, if such constraints are less transparent to hiring firms, then our theory predicts that when such constraints are enforceable and used *en masse*, they will induce additional frictions in the labor market by delaying or abating the receipt of job offers by individuals.

Limitations notwithstanding, our study makes important contributions to the literature streams we draw upon. Within the labor economics turnover and job search literature (Diamond, 1982; Jovanovic, 1979; Mortensen & Pissarides, 1999; Postel-Vinay & Robin, 2002; 2004; Shimer, 2001), as well as the sociology literature on mobility (Rider and Tan 2015, Haveman & Cohen, 1994; White, 1970), our study contributes by highlighting how noncompete usage and enforcement can result in trading externalities. Future modeling of the three-party bargaining process could benefit from better integration of when and where firms may strategically exercise constraints on mobility. Though this literature has pointed out the willingness of market participants to trade influences the efficiency of the market (Diamond, 1982; Shimer, 2001), our study suggests even private contracts between a worker and a firm can have significant externalities. While prior work has documented the effects of these frictions on the constrained, we document they also "shackle" others in the market, even the unconstrained, through reductions job offer rates, mobility, wages, and job satisfaction.

Within the knowledge diffusion literature (Almeida & Kogut, 1999; Fallick et al., 2006; Gilson, 1999; Saxenian, 1996), scholars have highlighted the dampening effects of mobility constraints among source and recipient firms when focal workers cannot move (Agarwal et al., 2009). Thus, the firm's choice to use enforceable noncompetes may be a prisoner's dilemma: private benefits to restricting mobility from one's firm may be rational, even though collectively all would be better off if workers were free to move (Gilson, 1999). We add to this literature by showing that everybody in the market—

even the unbound—are affected due to resulting constraints on their movement and wages. Future work may examine implications for knowledge diffusion: our results suggest diffusion should be reduced when enforceable noncompetes are used in high degree.

On a related note, this work contributes to two active streams in strategic human capital: First, strategic human capital scholars have highlighted how firms may retain their human capital by strategies that bind either the focal worker (Ganco et al., 2015; Starr et al., 2018b), or by direct linkages within workers as in team relationships (Marx & Timmermans, 2017, Agarwal et al, 2016, Ganco, 2013) or reductions in toxic colleagues (Minor & Housman, 2015). Second, the strategy literature has recently highlighted the important role of market frictions as a fundamental building block to strategic management (Mahoney and Qian, 2013). Our work links these literatures by highlighting that the usage of mobility constraints may be a double-edged sword: while firms are able to retain even workers who are not contractually constrained in regions of high incidence and enforceability, they may find these very strategies backfiring because it reduces their ability to hire needed human capital. In this sense, our paper identifies how mobility constraints form a microfoundation for market frictions.

Our study also has important managerial and policy implications. Our findings suggest that there are costs and benefits to locating in industries and states with high levels of use and enforceability of noncompetes. The costs of locating in such areas stem from increased recruiting costs. Relatedly, firms in high incidence/enforceability areas may benefit from the ability to offer lower wages and to retain more easily those workers who have not signed noncompete agreements. The decreased threat of employee departure in high incidence and enforceability areas may allow employers to invest in their human capital of unconstrained, as well as constrained, workers because they face lower risks of employee departure (Starr, 2018). Managers in these areas may focus on internal promotion in an effort to minimize the costs of recruiting in the external labor market. Lastly, though firms may not be able to materially impact the use of noncompetes by their labor market competitors, they may reduce the market externalities by lobbying state and federal legislatures to change the enforceability regime of noncompete agreements.

For policymakers, our study provides evidence of externality effects of noncompetes in labor markets, adding to the mix of complex issues regarding the freedom to contract vs. trade for the focal individual. For example, while banning noncompetes enables better matching of individuals and firms for enhanced individual and social welfare, it also may reduce inter-temporal gains through reduction in firm-sponsored training (Starr, 2018). Given our strong evidence that such externality effects result from higher enforceability and incidence, the consequences of policy changes are all the more critical for overall mobility, wages, and job-satisfaction of all workers, in addition to economic and social welfare stemming from knowledge diffusion and productivity.

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Tables and Figures

Figure 1. The Incidence of Noncompetes Across State-Industry Combinations

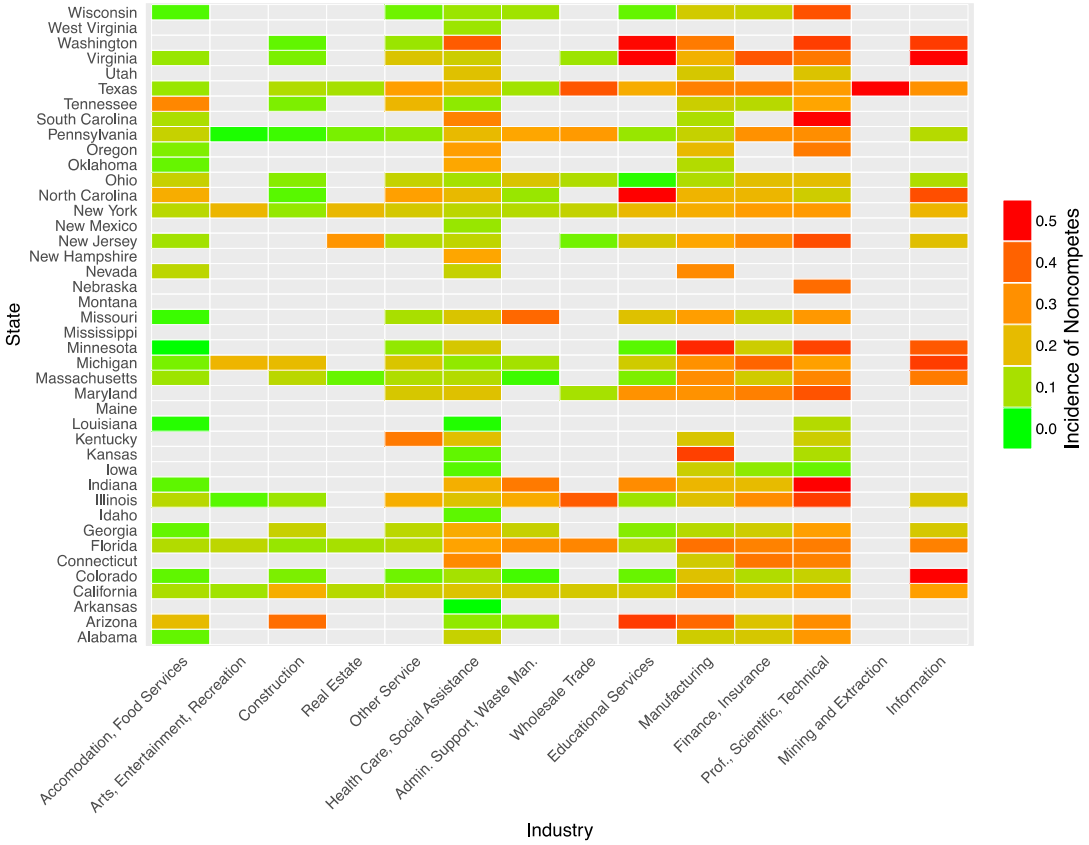
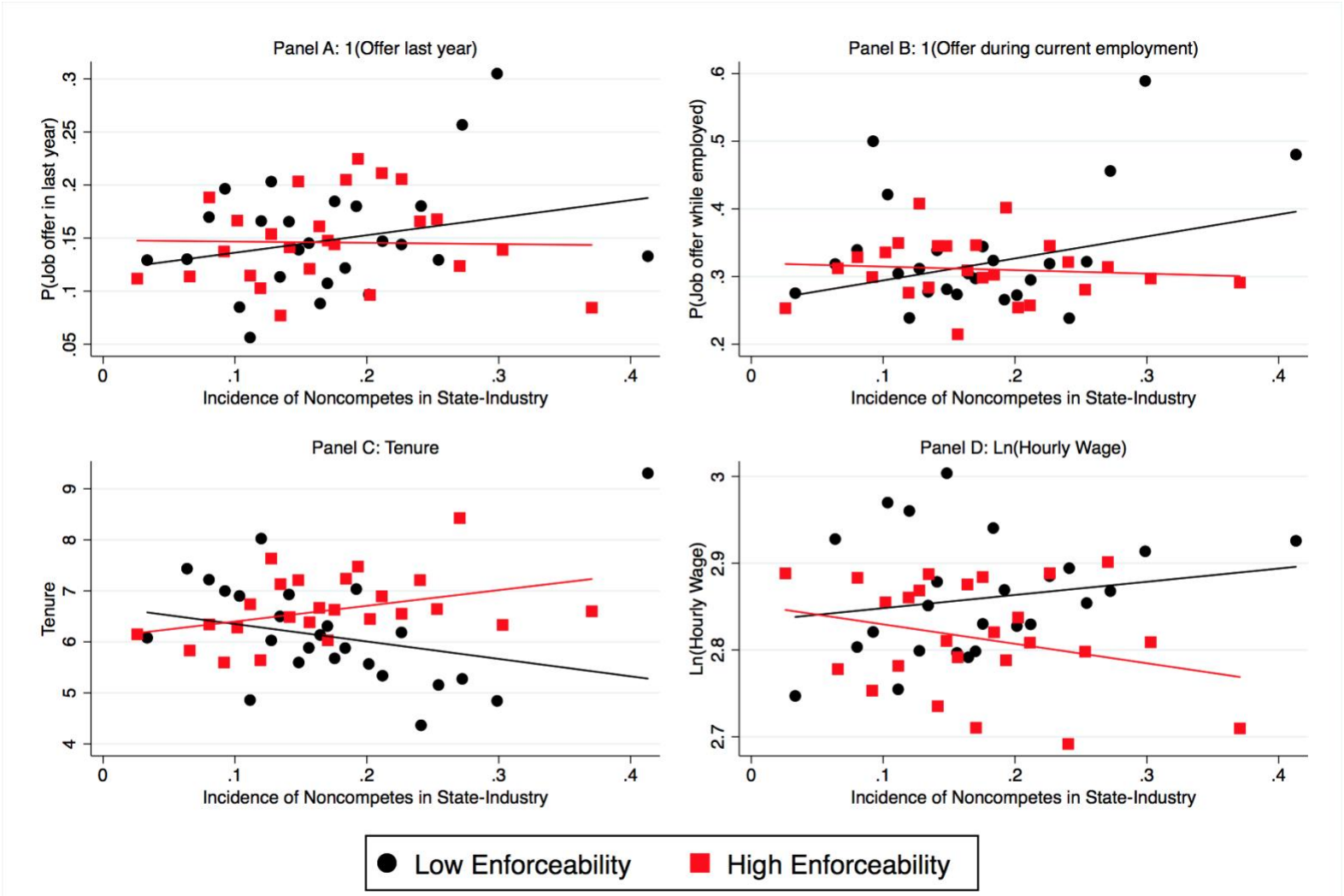


Figure 2. Binned Scatter Plots Showing Interactive Effects of Incidence and Enforceability



Data include only the unconstrained sample. Binned scatter plots include all controls in Table 2. Results are presented from the 20th imputation. High enfo is an indicator for a greater than average enforcing state.

Table 1. Summary Statistics

	Variable	Mean	SD	1	2	3	4	5	6	7	8	9	10	11	12	13
	Panel A. Full Sample															
1	State-Ind Incidence of Noncompetes	0.19	0.11	1.00												
2	Enforceability	-0.30	1.54	-0.02	1.00											
3	Tenure (Years)	6.68	7.25	0.07	0.02	1.00										
4	Ln(Hourly Wage)	2.90	0.73	0.24	-0.12	0.28	1.00									
5	**1**(Offer last year)	0.16	0.37	0.01	-0.02	-0.12	0.03	1.00								
6	**1**(Offer since employed)	0.34	0.47	0.05	-0.01	0.04	0.13	0.42	1.00							
7	Satisfied with job	3.76	1.13	0.02	0.03	0.09	0.13	-0.07	-0.04	1.00						
8	**1**(Sent job applications in last year)	0.30	0.45	-0.03	0.01	-0.24	-0.15	0.20	0.18	-0.35	1.00					
9	Age (Years)	40.40	13.71	0.03	-0.02	0.45	0.20	-0.16	-0.05	0.08	-0.24	1.00				
10	**1**(Male)	0.52	0.50	0.07	-0.05	0.13	0.27	0.02	0.07	0.02	-0.06	0.15	1.00			
11	Hours worked per week	37.82	13.22	0.14	-0.06	0.19	0.25	0.04	0.14	0.07	-0.12	0.09	0.29	1.00		
12	Weeks worked per year	48.27	8.19	0.03	-0.04	0.06	-0.08	-0.03	0.01	-0.02	-0.06	0.14	0.06	0.26	1.00	
13	Ln(Establishments in county-industry)	6.74	1.66	0.04	-0.34	0.00	0.17	0.03	0.03	-0.03	0.01	-0.02	0.04	0.05	0.05	1.00
	Panel B. Unconstrained Sample															
1	State-Ind Incidence of Noncompetes	0.17	0.10	1.00												
2	Enforceability	-0.30	1.53	-0.06	1.00											
3	Tenure (Years)	6.58	7.25	0.08	0.03	1.00										
4	Ln(Hourly Wage)	2.84	0.71	0.23	-0.10	0.28	1.00									
5	**1**(Offer last year)	0.15	0.35	-0.02	-0.02	-0.13	0.00	1.00								
6	**1**(Offer since employed)	0.32	0.46	0.02	0.00	0.02	0.12	0.42	1.00							
7	Satisfied with job	3.75	1.13	0.01	0.03	0.09	0.13	-0.07	-0.05	1.00						
8	**1**(Sent job applications in last year)	0.30	0.45	-0.04	0.00	-0.24	-0.14	0.20	0.20	-0.34	1.00					
9	Age (Years)	40.73	13.86	0.05	-0.02	0.45	0.18	-0.15	-0.05	0.07	-0.24	1.00				
10	**1**(Male)	0.51	0.50	0.05	-0.05	0.12	0.25	0.01	0.07	0.01	-0.05	0.14	1.00			
11	Hours worked per week	37.03	13.15	0.13	-0.06	0.18	0.23	0.02	0.12	0.06	-0.11	0.09	0.28	1.00		
12	Weeks worked per year	48.22	8.39	0.04	-0.04	0.06	-0.11	-0.04	0.01	-0.02	-0.05	0.15	0.06	0.27	1.00	
13	Ln(Establishments in county-industry)	6.71	1.66	0.07	-0.35	0.00	0.16	0.02	0.01	-0.03	0.02	-0.02	0.02	0.04	0.04	1.00

Note: Panel A includes only unconstrained individuals, while Panel B includes the full sample. The correlation matrix comes from the second set of imputed data. Only selected variables shown for brevity. The full correlation matrix is available from the authors upon request.

Table 2. Full Sample Results

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A				Panel B			
Dependent variable	***1****(Job offer in last year)*				***1****(Job offer in current employment)*			
State-Industry Noncompete Incidence	0.059	0.053	0.191**	0.171*	0.215***	0.137*	0.103	0.079
	(0.059)	(0.060)	(0.088)	(0.089)	(0.070)	(0.070)	(0.103)	(0.102)
Incidence*Enforceability	-0.069*	-0.068**	-0.087**	-0.084**	-0.116***	-0.134***	-0.146***	-0.135***
	(0.035)	(0.033)	(0.036)	(0.035)	(0.033)	(0.031)	(0.039)	(0.038)
Enforceability	0.008				0.020**			
	(0.008)				(0.007)			
Mean R-squared	0.001	0.012	0.082	0.111	0.003	0.019	0.100	0.118
	Panel C				Panel D			
Dependent variable	*Tenure*				*Ln(Hourly Wage)*			
State-Ind Noncompete Incidence	5.029***	4.964***	-0.285	0.402	1.507***	1.476***	0.167	0.111
	(1.215)	(1.235)	(1.042)	(0.869)	(0.210)	(0.237)	(0.161)	(0.157)
Incidence*Enforceability	-0.189	-0.528	2.248***	2.095***	-0.583***	-0.602***	-0.155*	-0.116
	(1.018)	(0.908)	(0.684)	(0.520)	(0.182)	(0.183)	(0.088)	(0.077)
Enforceability	0.123				0.064*			
	(0.199)				(0.033)			
Mean R-squared	0.006	0.016	0.120	0.306	0.069	0.099	0.368	0.478
Basic Controls	No	No	No	Yes	No	No	No	Yes
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occupation by Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Flow, Information Controls	No	No	No	No	No	No	No	No
Benefits, Contractual Controls	No	No	No	No	No	No	No	No
Observations	9,702	9,702	9,702	9,702	9,702	9,702	9,702	9,702

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes both those who have and have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects in the interacted model in column (4) and (8). Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry.

Table 3. Main Results for the Unconstrained Sample

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A				Panel B			
Dependent variable	***1****(Job offer in last year)*				***1****(Job offer in current employment)*			
State-Industry Noncompete Incidence	-0.037	-0.035	0.113	0.111	0.105	0.037	0.002	-0.004
	(0.054)	(0.052)	(0.081)	(0.086)	(0.078)	(0.072)	(0.107)	(0.110)
Incidence*Enforceability	-0.076*	-0.078**	-0.098**	-0.090**	-0.106**	-0.133***	-0.138***	-0.126**
	(0.039)	(0.037)	(0.039)	(0.035)	(0.041)	(0.041)	(0.046)	(0.048)
Enforceability	0.007				0.019**			
	(0.009)				(0.008)			
Mean R-squared	0.002	0.014	0.091	0.117	0.001	0.018	0.104	0.117
	Panel C				Panel D			
Dependent variable	*Tenure*				*Ln(Hourly Wage)*			
State-Ind Noncompete Incidence	6.193***	5.909***	0.618	0.386	1.473***	1.482***	0.124	0.051
	(1.488)	(1.494)	(1.473)	(1.148)	(0.213)	(0.240)	(0.192)	(0.182)
Incidence*Enforceability	0.276	-0.107	2.602***	2.198***	-0.641***	-0.644***	-0.169*	-0.153*
	(0.834)	(0.722)	(0.751)	(0.583)	(0.138)	(0.140)	(0.097)	(0.088)
Enforceability	0.099				0.078***			
	(0.180)				(0.027)			
Mean R-squared	0.008	0.021	0.130	0.306	0.069	0.111	0.395	0.505
Basic Controls	No	No	No	Yes	No	No	No	Yes
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Occupation by Industry FE	No	No	Yes	Yes	No	No	Yes	Yes
Flow, Information Controls	No	No	No	No	No	No	No	No
Benefits, Contractual Controls	No	No	No	No	No	No	No	No

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects in the interacted model in column (4) and (8). Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry.

Table 4. Summary of Additional Analyses, Robustness Checks, and Alternative Specifications

Concern	Robustness Check
Panel A: Why do state-industries differ in their incidence, and why do states differ in their enforceability of noncompetes?	
1. State policies may be correlated with noncompete policies, which could have positive or negative biases depending on the particular policy.	In Tables 2, 3, 5, and 6 we include state fixed effects to address state-level differences.
2. Labor markets exhibit different levels of competitiveness among firms, such that lower competitiveness results in fewer job offers, higher tenure and lower wages.	In Table 2 and 3, we include number of firms in county-industry as a control (in main specification already). In Table 5, we also control for reported industry flows between competitors, and the results are robust.
3. Labor markets have different average turnover rates, such that lower turnover rates result in fewer offers, longer tenure and lower wages.	In Table 5, results are robust to controls for reported industry flows between competitors.
Panel B. Are there systematic differences between the unconstrained in high incidence and high enforceability state-industries?	
4. Worker is at a firm that nobody poaches from, so receives fewer offers, and has longer tenure and lower wages.	In Table 5, results are robust to including controls for reported poaching flows to and from the focal respondent's firm.
5. Worker has lower levels of baseline mobility, so receives fewer offers, and has longer tenure and lower wages.	In Table 5, results are robust to control for number of employers the individual had in the last five years.
6. Worker knowledge is perceived as less valuable in the market, so receives fewer offers, and has longer tenure and lower wages.	In Table 5, the results are robust to controls for indicators for possessing trade secrets, working directly with clients, or having client-specific information.
7. Worker is bound by other related constraints/benefits, so receives fewer offers, and has longer tenure and lower wages.	In Table 5, the results are robust to controls for a variety of other postemployment restrictive covenants and HR benefits.
8. Worker is very satisfied at job, and willing to forgo mobility and higher wages.	In Table 5, Panel B, columns 1-4 show the interaction of incidence and enforceability is associated with lower job satisfaction.
9. Worker is less likely to search for new job in areas with high levels of noncompete incidence and enforceability.	In Table 5, Panel B, columns 5-8 show that the interaction of incidence and enforceability is not associated with lower search rates.
Panel C. Sample selection and other potential concerns	
10. The decision to only include state-industries with more than 10 observations may bias results.	In Table A1 of the online appendix, results are robust to cutoff values of 5, 10, 15, and 20.
11. Outliers in the enforceability index could be exerting undue influence on estimates.	In Table A8 of the online appendix, results are robust to using the standardized ordinal rank of enforceability instead of the cardinal index.
12. As incidence increases, there are fewer unconstrained, resulting in low power, high standard errors and imprecise estimates.	In Table 6, we utilize the CPS to replicate our results, and in Table 6 we find that our results are robust to the inclusion of those who have signed noncompetes
13. Same source bias may result in noncompete usage and wage/mobility outcomes being conflated.	In Table 6, we replicate the results in CPS, and the results are robust.
14. Noncompete externalities should impact the entire labor market, not just the unconstrained.	In Tables 2 and 6, the results are robust to the full sample of workers, which includes both the constrained and unconstrained workers. In Table A6, the results from a triple interaction model show finds no statistically significant differences between the effects of Incidence*Enforceability on the constrained and unconstrained.

Table 5. Robustness and Mechanisms

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Robustness and Selection Unobservables								
Dependent Variable:	***1*** *(Job offer in last year)*		***1*** *(Job offer in current employment)*		*Tenure*		*Ln(Hourly Wage)*	
State-Industry Noncompete Incidence	0.127	0.106	-0.001	0.006	-0.054	0.525	0.015	0.067
	(0.086)	(0.087)	(0.114)	(0.115)	(1.123)	(1.107)	(0.179)	(0.176)
Incidence*Enforceability	-0.099***	-0.083**	-0.135**	-0.130**	1.954***	1.930***	-0.133	-0.151*
	(0.034)	(0.032)	(0.052)	(0.055)	(0.618)	(0.640)	(0.086)	(0.086)
Mean R-Squared	0.166	0.175	0.166	0.179	0.412	0.424	0.538	0.582
Oster's Delta		8.75		15.94		3.88		1.62
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel B. Mechanisms								
Dependent variable	*Satisfied with job (1=Strongly Disagree to 5= Strongly Agree)*				***1*** *(Sent Job Applications in Last Year)*			
State-Ind Noncompete Incidence	0.364	0.370	0.222	0.401	-0.050	-0.041	-0.009	-0.057
	(0.273)	(0.267)	(0.262)	(0.267)	(0.115)	(0.116)	(0.111)	(0.117)
Incidence*Enforceability	-0.328**	-0.322**	-0.289**	-0.193*	0.078	0.069*	0.064	0.060
	(0.122)	(0.127)	(0.133)	(0.112)	(0.049)	(0.040)	(0.039)	(0.036)
Mean R-squared	0.100	0.122	0.166	0.199	0.088	0.147	0.191	0.216
Oster's Delta				2.27				8.52
Basic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	No	No	Yes	Yes	No	No	Yes	Yes
Benefits, Contractual Controls	No	No	No	Yes	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects. Oster's $\boldsymbol{\delta}$ is shown for the interaction term, calculated by comparing the specification with state fixed effects, occupation by industry fixed effects and the basic controls to the most saturated specification which, in addition to these controls, also includes the flow and information controls and the benefits and contractual controls. Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry. Flow and information controls include indicators for how the worker is paid (hourly, salary, commission) number of employers in last five years, access to trade secrets, access to client lists, access to confidential information, poaching rates to and from the firm, and perceived mobility rates between competitors. Benefits and contractual controls include indicators for whether the respondent signed a nondisclosure agreement, a nonpoaching (of coworkers) agreement, a nonsolicitation (of clients) agreement, an IP assignment agreement, whether they were granted stock options, deferred compensation, retirement plan, health insurance, paid vacation, sick leave, or life insurance.

Table 6. Replication in CPS

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A				Panel B			
Dependent variable	***1****(Stay at employer over 4 consecutive months)*				*Ln(Weekly Earnings)*			
State-Industry Noncompete Incidence	0.029***	0.034***	0.001	0.000	1.646***	1.761***	0.024	0.030
	(0.007)	(0.009)	(0.010)	(0.010)	(0.107)	(0.123)	(0.037)	(0.027)
Incidence*Enforceability	-0.001	-0.001	0.012**	0.014**	-0.506***	-0.504***	-0.035	-0.035**
	(0.005)	(0.006)	(0.006)	(0.006)	(0.124)	(0.112)	(0.036)	(0.015)
Enforceability	0.000				0.081***			
	(0.001)				(0.023)			
Observations	105,390	105,390	99,523	99,523	159,278	159,278	152,833	152,833
R-squared	0.000	0.001	0.073	0.077	0.049	0.061	0.417	0.582
Basic Controls	No	No	No	Yes	No	No	No	Yes
State FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	No	No	Yes	Yes	No	No	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Data are from the CPS from 2012 to 2014. The dependent variable in Panel A is an indicator that the individual stayed with the same employer over 4 consecutive months. Panel B uses data from the CPS monthly outgoing rotation groups (which get asked earnings questions), covering the same time periods. Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education.

MOBILITY CONSTRAINT EXTERNALITIES
Online Appendix

This Online Appendix accompanies the research article on "Mobility Constraint Externalities" by providing additional Figures and Tables for analysis referred in the main paper, but not included due to space constraints. Additional information is available upon request from the authors

Figure A1. Noncompete Enforceability

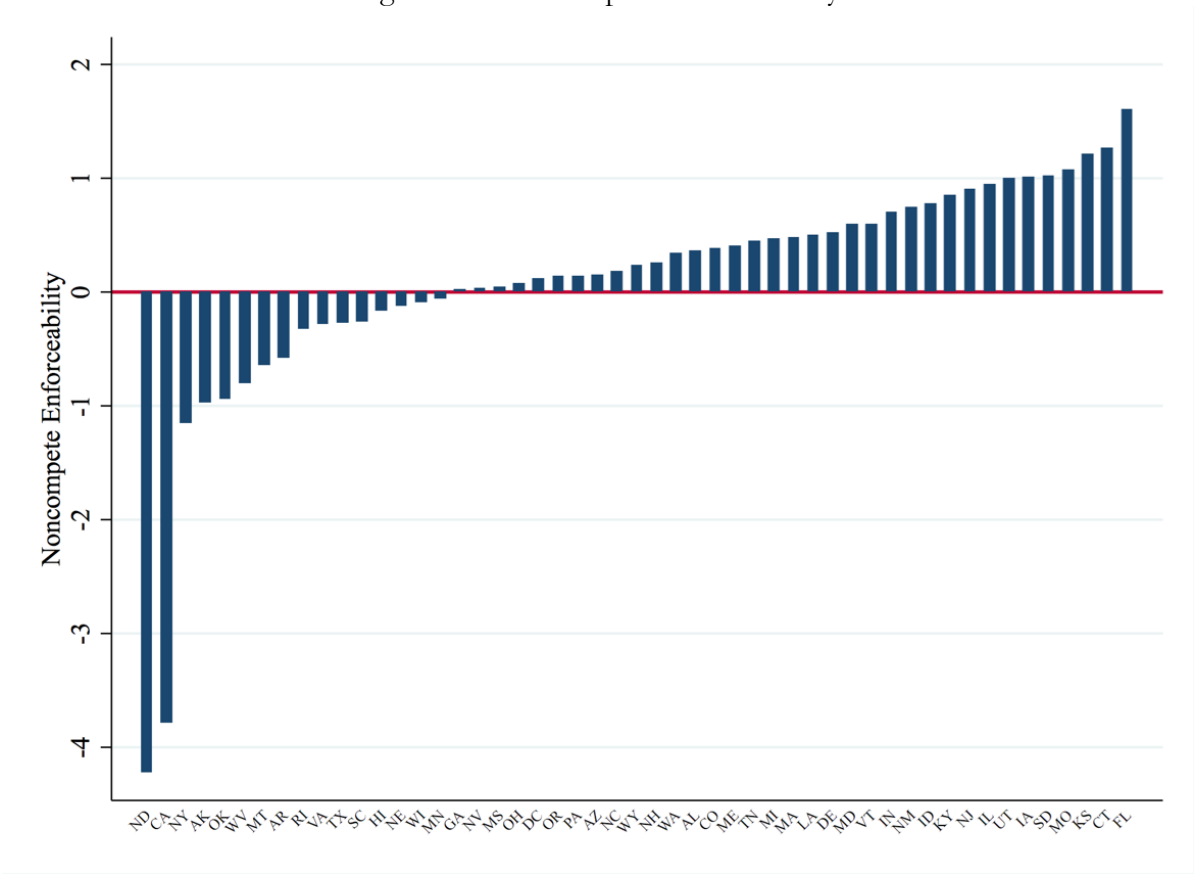
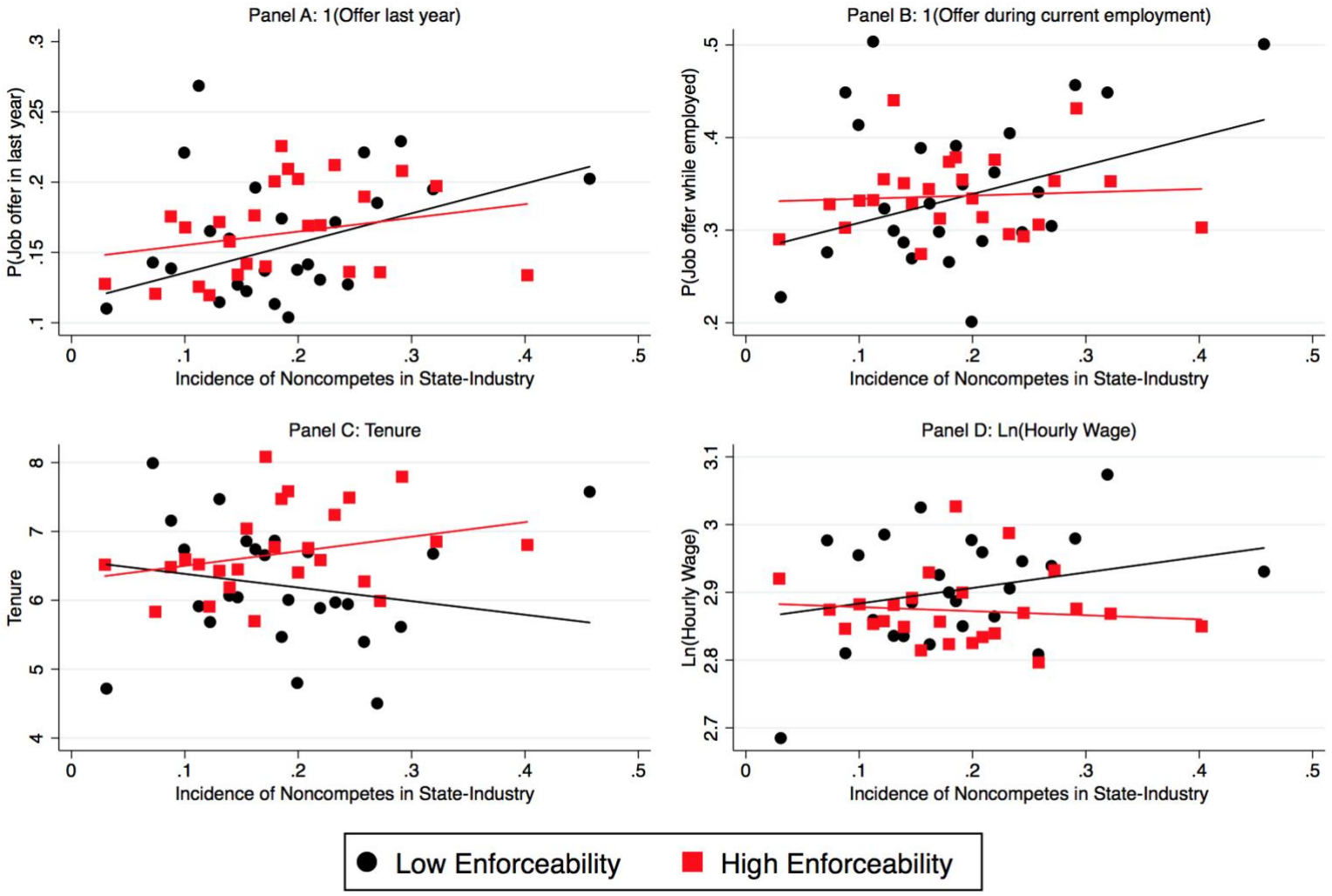


Figure A2. Binned Scatter Plots of Interaction Results in Full Sample



Data include the full sample. Binned scatter plots include all controls in Table 2. Results are presented from the 20th imputation. High enforceability is an indicator for a greater than average enforcing state.

Table A1 tests that the main results from Table 3 are robust to other sample cutoff values. Recall that the main results exclude observations from state-industries with fewer than 10 individuals. Here, we vary the cutoff threshold between five and 20, in increments of five. The results suggest our main results are robust to alterative cutoff values.

Table A1. Inclusion Cutoff Rule

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A				Panel B			
Dependent variable	***1****(Job offer in last year)*				***1****(Job offer in current employment)*			
State-Industry Noncompete Incidence	0.045	0.106	0.127	0.206	0.003	0.006	0.104	0.221
	(0.082)	(0.087)	(0.107)	(0.130)	(0.100)	(0.115)	(0.151)	(0.166)
Incidence*Enforceability	-0.068*	-0.083**	-0.061*	-0.048	-0.114**	-0.130**	-0.148***	-0.108*
	(0.035)	(0.032)	(0.033)	(0.030)	(0.055)	(0.055)	(0.051)	(0.053)
Mean R-squared	0.175	0.175	0.176	0.187	0.173	0.179	0.181	0.193
Inclusion Cutoff N	5	10	15	20	5	10	15	20
	Panel C				Panel D			
Dependent variable	*Tenure*				*Ln(Hourly Wage)*			
State-Ind Noncompete Incidence	0.412	0.525	0.733	-0.435	0.073	0.067	0.145	0.272
	(1.008)	(1.107)	(1.611)	(2.166)	(0.146)	(0.176)	(0.206)	(0.227)
Incidence*Enforceability	1.913**	1.930***	1.763***	1.880***	-0.171**	-0.151*	-0.155*	-0.173**
	(0.819)	(0.640)	(0.592)	(0.573)	(0.074)	(0.086)	(0.090)	(0.084)
Mean R-squared	0.421	0.424	0.432	0.435	0.571	0.582	0.590	0.604
Inclusion Cutoff N	5	10	15	20	5	10	15	20
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects. Each model is the fully saturated model, including all controls and fixed effects. Each column varies the inclusion cutoff for the number of individuals in a state-industry needed to be included in the estimation from 5 to 20 in increments of 5.

Table A2 reports Oster's Delta based on the inclusion of state and occupation by industry fixed effects, rather than on the benefits and contractual controls reported in Table 5.

Table A2. Alternative Approach to Oster Method

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A. Robustness and Selection Unobservables							
Dependent Variable:	***1*** *(Job offer in last year)*		***1*** *(Job offer in current*		*Tenure*		*Ln(Hourly Wage)*	
State-Industry Noncompete Incidence	-0.017	0.106	0.019	0.006	0.788	0.525	0.444***	0.067
	(0.059)	(0.087)	(0.072)	(0.115)	(1.129)	(1.107)	(0.148)	(0.176)
Enforceability	0.005		0.013		-0.080		0.041*	
	(0.009)		(0.009)		(0.134)		(0.021)	
Incidence*Enforceability	-0.057	-0.083**	-0.054	-0.130**	1.326*	1.930***	-0.279***	-0.151*
	(0.038)	(0.032)	(0.043)	(0.055)	(0.662)	(0.640)	(0.098)	(0.086)
Mean R-Squared	0.0991	0.175	0.0965	0.179	0.366	0.424	0.468	0.582
Oster's Delta		6.088		2.536		2.022		0.786
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes	No	Yes	No	Yes
Occupation by Industry FE	No	Yes	No	Yes	No	Yes	No	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects in even columns. Oster's $\boldsymbol{\delta}$ is shown for the interaction term, calculated by comparing the specification with all controls except state and occupation by industry fixed effects to the most saturated specification with those controls and the flow and information controls and the benefits and contractual controls. The maximum R-Squared is set to 30% higher than the most saturated model.

Table A3 demonstrates our (non-tenure) results from Table 3 are robust to controlling for tenure. However, the results should be interpreted with caution because tenure is an intermediate outcome of incidence*enforceability and, therefore, may bias these estimates.

Table A3. Including Tenure as a Control

Model: OLS	(1)	(2)	(3)	(4)	(5)
Dependent Variable	***1****(Job offer in last year)*	***1****(Job offer in current employment)*	*Ln(Hourly Wage)*	*Satisfied with job (1=Strongly Disagree to 5= Strongly Agree)*	***1****(Sent Job Applications in Last Year)*
Tenure (years)	-0.001	0.005***	0.012***	0.003	-0.004***
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)
State-Industry Noncompete Incidence	0.107	0.003	0.061	0.398	-0.055
	(0.087)	(0.115)	(0.178)	(0.268)	(0.118)
Incidence*Enforceability	-0.081**	-0.140**	-0.174**	-0.199*	0.067*
	(0.032)	(0.054)	(0.085)	(0.111)	(0.037)
Mean R-Squared	0.175	0.182	0.592	0.199	0.218
Basic Controls	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	Yes	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects. Each model is the fully saturated model, including all controls and fixed effects.

Table A4 examines robustness of the (dichotomous) job offer results from the unconstrained sample using a logit model.

Table A4. Logit Model for Offers

Dependent Variable	(1) *1(Job offer in last year)*	(2) *1(Job offer in current employment)*
State-Industry Noncompete Incidence	0.959	0.175
	(0.801)	(0.568)
Incidence*Enforceability	-0.677**	-0.550**
	(0.307)	(0.245)
Basic Controls	Yes	Yes
State FE	Yes	Yes
Occupation by Industry FE	Yes	Yes
Flow, Information Controls	Yes	Yes
Benefits, Contractual Controls	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Main effects of enforceability are subsumed by state fixed effects.

Table A5 examines the full sample results while controlling for the direct effect of a noncompete.

Table A5. Full Sample with All Controls and an Indicator for Noncompete Use

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	***1****(Job offer in last year)*		***1****(Job offer in current*		*Tenure*		*Ln(Hourly Wage)*	
State-Industry Noncompete Incidence	0.142	0.139	0.007	0.006	-0.580	-0.188	-0.014	0.076
	(0.089)	(0.089)	(0.111)	(0.116)	(0.760)	(0.736)	(0.145)	(0.145)
Incidence*Enforceability	-0.089**	-0.074**	-0.137***	-0.126***	1.824***	1.864***	-0.098	-0.125
	(0.035)	(0.036)	(0.040)	(0.042)	(0.520)	(0.575)	(0.077)	(0.079)
Noncompete	0.020	-0.000	0.045*	0.015	0.538*	0.650*	0.059*	0.032
	(0.019)	(0.021)	(0.025)	(0.027)	(0.282)	(0.347)	(0.030)	(0.031)
Mean R-Squared	0.156	0.163	0.163	0.173	0.411	0.421	0.556	0.598
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	No	Yes	No	Yes	No	Yes	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes both those who have and have not signed a noncompete. Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry. Flow and information controls include indicators for how the worker is paid (hourly, salary, commission) number of employers in last five years, access to trade secrets, access to client lists, access to confidential information, poaching rates to and from the firm, and perceived mobility rates between competitors. Benefits and contractual controls include indicators for whether the respondent signed a nondisclosure agreement, a nonpoaching (of coworkers) agreement, a nonsolicitation (of clients) agreement, an IP assignment agreement, whether they were granted stock options, deferred compensation, retirement plan, health insurance, paid vacation, sick leave, or life insurance.

Table A6 presents the results from a triple interaction model examining heterogeneous effects of Incidence*Enforceability between the unconstrained and constrained.

Table A6. Triple Interaction: Incidence, Enforceability, and Use

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	***1****(Job offer in last year)*	***1****(Job offer in current employment)*	*Tenure*	*Ln(Hourly Wage)*
State-Industry Noncompete Incidence	0.107	0.038	1.040	0.088
	(0.078)	(0.120)	(0.956)	(0.161)
Incidence*Enforceability	-0.085**	-0.132**	2.047***	-0.160*
	(0.037)	(0.049)	(0.592)	(0.082)
1(Constrained Individual)	-0.017	0.034	1.487**	0.031
	(0.039)	(0.047)	(0.557)	(0.076)
1(Constrained)* Incidence	0.094	-0.100	-4.032**	-0.017
	(0.133)	(0.184)	(1.885)	(0.245)
1(Constrained)*Enforceability	0.016	-0.026	-0.289	-0.056
	(0.027)	(0.027)	(0.314)	(0.049)
1(Constrained)*Enforceability*Incidence	-0.013	0.074	0.208	0.215
	(0.107)	(0.124)	(1.151)	(0.189)
Observations	9,092	9,092	9,092	9,092
Mean R-Squared	0.174	0.178	0.422	0.561
Basic Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes both those who have and have not signed a noncompete. Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry. Flow and information controls include indicators for how the worker is paid (hourly, salary, commission) number of employers in last five years, access to trade secrets, access to client lists, access to confidential information, poaching rates to and from the firm, and perceived mobility rates between competitors. Benefits and contractual controls include indicators for whether the respondent signed a nondisclosure agreement, a nonpoaching (of coworkers) agreement, a nonsolicitation (of clients) agreement, an IP assignment agreement, whether they were granted stock options, deferred compensation, retirement plan, health insurance, paid vacation, sick leave, or life insurance.

Table A7 demonstrates that the results from Table 2 (full sample results) are robust to the inclusion of the full set of flow, information, benefits, and contractual controls.

Table A7. Full Sample with All Controls

Model: OLS	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Including additional Controls								
Dependent Variable:	***1*** *(Job offer in last year)*		***1*** *(Job offer in current*		*Tenure*		*Ln(Hourly Wage)*	
State-Industry Noncompete Incidence	0.160*	0.139	0.047	0.017	-0.101	0.305	0.038	0.101
	(0.089)	(0.089)	(0.104)	(0.113)	(0.784)	(0.832)	(0.142)	(0.140)
Incidence*Enforceability	-0.088**	-0.074**	-0.136***	-0.126***	1.840***	1.881***	-0.097	-0.124
	(0.035)	(0.036)	(0.040)	(0.042)	(0.518)	(0.574)	(0.077)	(0.079)
Mean R-Squared	0.162	0.173	0.165	0.177	0.410	0.420	0.513	0.561
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	No	Yes	No	Yes	No	Yes	No	Yes
Panel B. Mechanisms								
Dependent Variable:	*Satisfied with job (1=Strongly Disagree to 5= Strongly Agree)*				***1*** *(Sent Job Applications in Last Year)*			
State-Ind Noncompete Incidence	0.215	0.266	0.121	0.231	0.041	0.022	0.052	-0.024
	(0.215)	(0.213)	(0.207)	(0.214)	(0.102)	(0.099)	(0.093)	(0.097)
Incidence*Enforceability	-0.187*	-0.178	-0.183*	-0.081	0.054	0.039	0.040	0.047
	(0.104)	(0.106)	(0.094)	(0.090)	(0.063)	(0.052)	(0.048)	(0.051)
Mean R-squared	0.093	0.115	0.161	0.211	0.077	0.134	0.176	0.202
Basic Controls	No	Yes	Yes	Yes	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Flow, Information Controls	No	No	Yes	Yes	No	No	Yes	Yes
Benefits, Contractual Controls	No	No	No	Yes	No	No	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses, clustered at the state level. Sample includes both those who have and have not signed a noncompete. Basic controls include a cubic in age, hours worked per week, weeks worked per year, hours worked per week*weeks worked per year, gender, class of the worker (e.g. private for profit), education, firm size indicators, a multi-unit firm dummy, and the log number of firms in the respondent's county-industry. Flow and information controls include indicators for how the worker is paid (hourly, salary, commission) number of employers in last five years, access to trade secrets, access to client lists, access to confidential information, poaching rates to and from the firm, and perceived mobility rates between competitors. Benefits and contractual controls include indicators for whether the respondent signed a nondisclosure agreement, a nonpoaching (of coworkers) agreement, a nonsolicitation (of clients) agreement, an IP assignment agreement, whether they were granted stock options, deferred compensation, retirement plan, health insurance, paid vacation, sick leave, or life insurance.

Table A8 demonstrates that the unconstrained results are robust to using an ordinal, rather than cardinal, measure of enforceability. The ordinal measure used in able A8 is standardized to have a mean of zero and a standard deviation of one.

Table A8. Using Standardized Rank of Enforceability Index

Model: OLS	(1)	(2)	(3)	(4)
Dependent Variable	***1****(Job offer in last year)*	***1****(Job offer in current employment)*	*Tenure*	*Ln(Hourly Wage)*
State-Industry Noncompete Incidence	0.095	0.052	1.015	0.092
	(0.096)	(0.120)	(1.194)	(0.181)
Incidence*Rank(Enforceability)	-0.107***	-0.150*	2.616**	-0.130
	(0.039)	(0.080)	(0.998)	(0.119)
Mean R-squared	0.179	0.178	0.427	0.582
Basic Controls	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Occupation by Industry FE	Yes	Yes	Yes	Yes
Flow, Information Controls	Yes	Yes	Yes	Yes
Benefits, Contractual Controls	Yes	Yes	Yes	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses, clustered at the state level. Sample includes only those who have not signed a noncompete. Each model is the fully saturated model, including all controls and fixed effects. The enforceability index is measured in ranks ranging from 1-51, and then standardized to have a mean of zero and a standard deviation of one, with higher scores signifying stronger enforceability.