Human capital accumulation by workers is a key source of productivity growth, both at the aggregate (e.g., Romer, 1989) as well as at the firm-level (e.g., Bartel, 1994). Workers accumulate human capital at their jobs through their own effort and through training and related investments made by their employers. Some skills are not entirely firm-specific and may be valuable to other firms (Becker, 1962; Cooper et al., 1994; Lazear, 2009). Because employees can quit at-will (Autor, Donahue, and Schwab, 2006), employers facing the possibility of a worker leaving their employment after they have made investments in the worker’s human capital development might be discouraged from making such investments in the first place (Rajan and Zingales, 2001). Employers might be especially discouraged from providing employees with valuable training or information if they are concerned that such investments will be used directly against them in the case that the employee leaves for a competitor.

One of the policy measures to restore the firm’s incentives to make these investments is to allow employers to restrict the ability of employees to depart for competitors through the enforcement of covenants not to compete (CNCs) (Rubin and Shedd, 1981). CNCs are post-employment restraints which prohibit employees from either joining competitors or starting a competing firm for a specified amount of time (typically between 6 months and two years (Gilson, 1999)) and in a specified geographic region.

While enforcing agreed-upon contracts that prevent the flow of workers to competitors provides firms with additional incentives to invest in employee human capital and the development of commercially valuable information, it also prevents the free flow of employees and ideas across firms, potentially reducing innovation and entrepreneurship (Gilson, 1999; Stuart and Sorenson, 2003; Samila and Sorenson, 2011; Starr, Balasubramanian, and Sakakibara, 2015; Fallick, Fleischman, and Rebitzer, 2006; Marx, Strumsky, and Fleming, 2009). Though some liken the enforcement of CNCs to impinging on sacrosanct personal freedom or enslaving
workers (Oman, 2009; Bishara and Westermann-Behaylo, 2012), different states have taken starkly contrasting perspectives, leading to interesting variation in the degree of CNC enforceability across states (Bishara, 2006; Bishara, 2012).

In this paper, we use quarterly employment records for the universe of employees in 30 U.S. states to study how workers’ mobility and wage are related to CNC enforceability across the job spell. While most models predict a reduction in mobility as a consequence of increased CNC enforceability (e.g., Garmaise, 2009), the effect on wages is ambiguous. If CNC enforceability stimulates human capital investments and the gains from this investment are shared between employer and employee, this could take the form of higher wages at some point in the job spell. Alternatively, if wages are uniformly lower in higher enforceability states, but workers are staying longer in their jobs, then this would be strong evidence that workers in higher enforceability states are systematically disadvantaged or “locked-in” by the restrictions on their mobility.

We focus on workers in technology industries because of the high observed incidence of CNCs and because the protection of incentives for investment in human capital is likely to be very important for such workers. For the same reason, prior work on CNC effects has also focused on high-tech workers, using data on inventors available in patent grant datasets (e.g., Marx et al., 2009; Marx et al., 2015). Indeed, recent work has found greater prevalence of CNC contracts for higher skilled and higher-technology related work. Starr, Bishara, and Prescott (2015) report that while the national signing rate is 18.1%, college-educated workers are more than 10 percentage points more likely to be bound (14.3% vs. 25.0%) and those with a graduate degree are almost 15 percentage points more likely to be subject to CNCs (30.0%). In other studies, the incidence of CNCs was found to be 70-80% for CEOs (Bishara, 2012; Garmaise, 2011), 45% for physicians (Lavetti et al., 2013), 40% for engineers (Marx, 2011), and 70% for firms with VC contracts (Kaplan and Stromberg, 2003).

The key contribution of this paper is to trace out the effect of CNC enforceability on both the length of job spells and the trajectory of wages over that job spell. In doing so, we exploit very rich data on workers, which allows us to better address significant econometric identification challenges posed by the setting. In particular, significant challenges arise from the selection of different types of workers and/or firms into states with different enforceability, potentially severely biasing any analysis that is not able to carefully control for these factors. In our analysis, we are able to use very detailed, non-parametric controls for firm size and industry, as well as individual characteristics such as gender and age, and starting wages, which allow us to control for important sources of bias.

**EFFECTS OF CNC ENFORCEABILITY ON THE DURATION OF EMPLOYMENT AND WAGE**

We draw on insights from search and matching models to understand how CNC enforceability affects the length of job spells and the pattern of wages (Burdett and Mortensen, 1998; Manning, 2003; Postel-Vinay and Robin, 2002 and 2004). Fundamentally, enforceable CNCs drive a legal wedge between a departing employee and competing firms, raising the threshold wage a competitor must offer to successfully poach an employee. For a given distribution of potential wage offers, a greater competitor-specific reservation wage in turn reduces the set of feasible offers, increasing job-spell length in expectation.
As discussed above, the predicted effect of CNC enforceability on wages is ambiguous. Although CNC enforceability would reduce the labor market competition for the worker, and thereby suppress wages, CNC enforceability can also stimulate human capital investments made by the firm and the employee, and therefore can potentially offset the reduced wage growth. Indeed, Starr (2015) shows that CNC enforceability encourages firms to invest more in their employees, finding little evidence of reduced self-investment (in contrast to findings in Garmaise, 2011, and Lobel and Amir, 2014). If such human capital investments increase productivity and increases in productivity are partially tied to wages, then such investments will also increase wages.

If we observe workers in higher enforceability states staying longer but being paid less than very similar counterparts in states potentially due to reduced labor market competition, we could conclude that such individuals are “locked” into their jobs in a disadvantageous way. If, however, we observe that individuals in high CNC enforceability states have higher wages, potentially due to increased human capital investments, then the higher wages further increase the competitor-specific reservation wage, resulting in longer job-spells all else equal. Such individuals are, however, not “locked-in”, in the sense that they are underpaid and stuck — they are earning more than their counterparts in low enforceability states.

If workers are fully informed, and have no restrictions or extra costs on their mobility across states, they may be able to negotiate upfront to ensure there are no wage penalties associated with working for a firm in a high enforceability state. Despite initial claims that workers would be likely to negotiate over these contracts (Callahan, 1985; Sterk, 1993), recent evidence shows that negotiation is rare: Starr, Bishara, and Prescott (2015) report that only 10% of employees negotiate over their CNCs, and in subsequent work (Starr, Bishara, and Prescott, 2016) show that workers are almost entirely uninformed about their state’s enforceability policy. Combined with the fact that firms frequently delay the offering of the CNC until after the employee has accepted the job (Marx, 2011; Starr, Bishara, and Prescott, 2015), in which case it is very difficult for the employee to negotiate over it ex-ante, we do not expect Coasean-style negotiations to be important here.

Nevertheless, whether CNC enforceability locks-in employees is ultimately an empirical question.

DATA AND EMPIRICAL METHODOLOGY

We construct a job-level repeated cross-sectional dataset using the Longitudinal Employer-Household Dynamics (LEHD) database at the U.S. Census Bureau. The LEHD is a composite linked employer-employee dataset at the worker-firm-year-quarter level comprising 30 state-level databases (AR, CA, CO, FL, GA, HI, IA, ID, IL, IN, LA, MD, ME, MT, NC, NJ, NM, NV, OK, OR, RI, SC, TN, TX, UT, VA, VT, WA, WI, and WV). There are a number of unique advantages to using the LEHD for this study. First, the LEHD provides us employment history data for individual workers over a long horizon for a full spectrum of industries in the U.S. economy across a large number of states that vary in CNC enforceability levels. Second, being quarterly administrative data on all firms, the LEHD provides clear measures of job transfer, mobility, and wage at a high frequency level, free from selection issues that survey data often suffer from.

Jobs are defined by changes in the firm identifier within the worker’s employment history. For each job defined, we construct two dependent variables for examining the effect of CNC
enforceability on worker’s mobility. The first is a set of dummy variables for continuation of job spells: $D_q$ are dummy variables with value 1 if the job spell lasts at least $q$ quarters (0 otherwise), with $q$ varying from 4 to 32 quarters, in 4 quarter increments. The second is the length of the job spell as the number of quarters the worker was employed at the firm, in logs. To examine the effect of CNC enforceability on wages across the job tenure profile, we use the log of workers’ wages at the $4^{th}$, $8^{th}$, …, $32^{nd}$ quarter of each job spell. All wages are CPI-adjusted to 2008 dollars. The data covers the years 1991-2008.

For the CNC enforceability measure, we use the 2009 enforceability index score developed in Starr (2015). Bishara (2011) quantifies the various dimensions of CNC enforceability for each state in seven dimensions, which Starr (2015) modifies by performing factor analysis to re-weight the seven dimensions of enforceability. Compared to indices used in past studies, this index captures CNC enforceability with finer granularity along a spectrum of weak to strong enforceability. The enforceability index scores are normalized to have mean 0 and standard deviation of 1 in a sample where each state is given equal weight.

We estimate the effect of CNC enforceability on mobility and wage across job tenure as its differential effect on high-tech jobs compared to other jobs. As discussed earlier, high-tech jobs are most likely to be affected by CNC enforceability because CNC is intended to protect the firm’s knowledge capital from its competitors. High-tech jobs are defined using the three-digit NAICS industry code of the firm at which the worker is employed, and we follow the definition of “Technology Industries” by Paytas and Berglund (2004), which classifies the NAICS industries into technology industries by employment of occupations that are science and engineering intensive based on the occupation-NAICS employment concordance provided by the BLS. We refer to jobs in “Technology Industries” as “tech jobs” and those not in “Technology Industries” as “non-tech jobs” hereafter.

In all of our analysis, we use very rich job-level fixed effects, using the job characteristics at the time in which the job spell starts. Each fixed effect defines a group of jobs that are common in terms of their NAICS3 codes, starting year, firm size group, starting wage group, starting age group of the worker, and gender of the worker. A shortcoming of the LEHD data is that it does not contain detailed occupation or education data for the workers, so that there could be legitimate concerns about unobserved heterogeneity related to differences on these characteristics. To mitigate potential bias from this source, we use starting wages as a proxy for the level of general human capital of the worker, using a categorical variable defined within the jobs with the same NAICS3 codes; to the extent that workers with the same age/gender starting at the same job have very different educational backgrounds or occupations, this should be reflected in the starting wage. Starting year fixed effects are used to control for cohort specific initial period shocks.

We then estimate the differential effect of CNC enforceability by exploiting the significant inter-state variation in the enforceability index scores using equation (1) below:

$$ Y_j = \alpha + \delta C_s * I^T_j + \Sigma_s + FE_j + \gamma fb_j + \epsilon_j $$

where subscripts $j, s$ are for job and state, respectively. This semi-parametric regression specification is fully saturated, as the job-level fixed effects absorb the dummy variables that are not included above. $Y_j$ is the dependent variable explained earlier. $C_s$ is the 2009 CNC enforceability index score of the state. $I^T_j$ denotes the dummy variable for the job being a tech job. $FE_j$ denotes the job-level fixed effects. $fb_j$ denotes whether the worker was foreign-born; this
allows for foreign-born employees to be subject to employment eligibility (visa-related) constraints that may affect their mobility. Our coefficient of interest is $\delta$, which estimates the differential effect of CNC enforceability on mobility and wage.

$\mathcal{L}_s$ denotes state fixed effects dummy variables. By using non-tech jobs as a control group, specification (1) implements a pseudo difference-in-differences methodology. In particular, we rely on the identification assumption that the effect of CNC enforceability would be higher for tech rather than non-tech jobs, leading to a relative difference between tech and non-tech in high enforceability vs low-enforceability states. This allows us to include state fixed effects, which fully control for (fixed) state-level unobservables such as state policies, economic characteristics, and demographic characteristics that are likely to be correlated with CNC enforceability, mobility, and wages, and hence could be potential sources of significant bias. (The cost is that we identify only the differential effect on high-tech jobs relative to non-tech jobs, so that baseline effects on non-tech jobs get subsumed by the state fixed effects).

Despite controlling for state-level fixed factors, the above specification may still be subject to bias, if there are state-specific omitted industry factors. E.g., if the technology sector has differences in characteristics across states in ways correlated with CNC enforceability, that could be a source of bias in (1) as we cannot recover the parameter of interest if state-industry effects are included in that specification. E.g., if the thickness of the labor market for tech (relative to non-tech) is much higher in the state with low enforceability score (e.g. California) than in other states with high enforceability score (e.g. Florida), then observed differences in mobility and wages may be reflecting differences in the thickness of labor markets, rather than the differential effect of CNC enforceability.

To address this concern, we supplement our analysis by examining the differential effect of CNC enforceability for high wage jobs against low wage jobs, which exploits variation within industries, and hence allows for the use of state-industry fixed effects. High wage jobs are defined as jobs with starting wage above the 98th percentile in the distribution of starting wages of jobs that have the same NAICS3 codes (starting wage is defined as second quarter wage of each job, as first quarter wage is likely to be truncated). Under the reasonable assumption that jobs with higher starting wages reflect higher levels of human capital, we expect these jobs to be more significantly affected by CNC enforceability. We then estimate the differential effect using equation (2) below:

$$ Y_j = \alpha + \beta_1 C_s * I_{T}^W + \beta_2 C_s * I_{I}^W + \beta_3 C_s * I_{W}^W + \Sigma s_N_k + FE_j + \gamma fb_j + \epsilon_j $$

$I_{W}^j$ is a dummy variable for high wage job. $N_k$ indexes 2-digit NAICS industry fixed effects so $\Sigma s_N_k$ are state-industry fixed effects. This specification allows us to estimate the differential effect across different wage levels within tech jobs, while keeping the estimation sample as the same as in equation (1). The differential effect estimate of CNC enforceability for high wage jobs, compared to low wage jobs, within tech jobs is $\beta_1 + \beta_3$. Standard errors for all of the analyses are clustered by state to allow arbitrary correlation of the error term within each state (Bertrand, Duflo, and Mullainathan, 2004).

**RESULTS AND CONCLUSIONS**

We find that there is a persistent differential effect of CNC enforceability on mobility throughout job tenure, which leads to a relative effect of 1.5% longer expected spell for tech jobs
when the enforceability score rises by one standard deviation. That is, if a non-enforcing state such as California began enforcing CNCs to the same extent as the highest enforceability state (Florida), the average length of tech job spells would increase by 7.5% more, compared to the average length of spells of non-tech jobs.

We also observe a persistent wage suppressing differential effect throughout job tenure. We find that tech jobs earn 0.5%–0.7% lower wages compared to non-tech jobs across job tenure, when the enforceability score increases by 1 standard deviation. Thus, if a non-enforcing state were to increase its enforceability to the maximal level, earnings of tech jobs would be around 2.5%–3.5% lower at every point in the job-spell.

We find similar differential effects for high wage jobs compared to low wage jobs within the tech sector. In particular, there is a strong enforceability effect uniformly across job tenure, on both mobility and wage. Together, these results provide forceful evidence that CNC enforceability locks employees into their jobs, with little evidence for any potential human capital related productivity benefits being shared with the employees.

The lower wage observed throughout job tenure could reflect reduced competition for the worker or the firm’s reduced propensity to respond to outside offers. Direct evidence for this view can be found in the recent DOJ investigation into the illegal ‘Gentleman’s Agreements’ signed by major tech employers in Silicon Valley in which they agreed not to poach each other’s employees (Lindsay and Santon, 2012). Thus, in a bid to avoid labor market competition, these firms illegally instituted CNCs in a state where they are unenforceable, resulting in a class-action lawsuit that ultimately settled for $415 million. If CNCs were enforceable, they may not have had to collude.

Our results extend and complement prior studies on this topic in a few important ways. First, this study shows that there is persistent effect of CNC enforceability on mobility and wage throughout job tenure. Second, we show that CNC enforceability affects a broad spectrum of technology jobs, affecting hundreds of thousands of workers, rather than specific job groups that previous studies that used more limited data have focused on. Lastly, our results have managerial and policy implications. The results suggest that firms in higher enforceability states can maintain a competitive advantage by preventing their most informed and valuable employees from leaving by binding them with a CNC. For policymakers, our results suggest that the human capital investment justification for enforcing CNCs needs to be viewed with caution, to the extent that the argument rests on benefits being passed on to workers — we find no evidence for positive effects for workers, across their tenure profile. Combined with prior work finding negative effects of CNC enforceability on innovation, our work suggests that relaxing CNC enforceability may be worth careful consideration.

ENDNOTES

1. US Census Bureau Disclaimer: This research is being conducted at the Michigan Census Research Data Center. Research results in this article are those of the authors, and do not necessarily represent the views of the U.S. Census Bureau. The results presented here have been screened to ensure that no confidential data is revealed. We thank Clint Carter and others at CES for all their assistance.

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