Locked In?

The Enforceability of Covenants Not to Compete and the Careers of High-Tech Workers

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ABSTRACT

We study the relationship between the enforceability of covenants not to compete (CNCs) and employee mobility and wages. We exploit a 2015 CNC ban for technology workers in Hawaii and find that this ban increased mobility by 11 percent and new-hire wages by 4 percent. We supplement the Hawaii evaluation with a cross-state analysis using matched employer–employee data. We find that eight years after starting a job in an average-enforceability state, technology workers have about 8 percent fewer jobs and 4.6 percent lower cumulative earnings relative to equivalent workers starting in a nonenforcing state. These results are consistent with CNC enforceability increasing monopsony power.

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> New practices have emerged to facilitate employer collusion, such as noncompete clauses and no-raid pacts, but the basic insights are the same: employers often implicitly, and sometimes explicitly, act to prevent the forces of competition from enabling workers to earn what a competitive market would dictate, and from working where they would prefer to work.
>
> —Alan Krueger, "The Rigged Labor Market" (April 2017)

I. Introduction

In an op-ed, Furman and Krueger (2016) proposed that monopsony power is holding back wage growth, economic dynamism, and innovation.[1] This provocative thesis builds on recent scholarship examining frictions that reduce labor market competition (Manning 2011; Ashenfelter, Farber, and Ransom 2010; Boal and Ransom 1997). One such friction that has received significant public policy and media attention in recent years is the use of covenants not to compete (CNCs), which are employment provisions that prohibit a worker from leaving to join or start a competing firm.[2] Indeed, dozens of new reforms have sought to ban CNCs, and such proposals were on the list of labor reforms of almost all 2020 presidential candidates.[3]

The link between CNCs and monopsony power follows from the similarity of CNCs to moving costs. In particular, it has long been argued (for example, Manning 2003) that, in general, moving costs give firms monopsony power by restricting worker mobility. Similarly, because they increase the costs of moving to the firms most likely to value worker human capital (competitors), CNCs could facilitate monopsony power. Yet, the fact that CNCs are voluntarily agreed upon complicates this analogy—wouldn't workers only agree to significant mobility restrictions if they received some benefits in exchange? And wouldn't firms use them only because they need to protect their investments or valuable information? That is, couldn't CNCs actually encourage training and human capital investment, which could enhance higher worker productivity, and hence result in higher wages, such that banning them would make workers worse off?

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1. See https://www.wsj.com/articles/why-arent-americans-getting-raises-blame-the-monopsony-1478215983
2. Consistent with Furman and Krueger's hypothesis, many have previously argued that California's ban on CNCs led to the rapid growth of Silicon Valley (Gilson 1999; Hyde 2003; Fallick, Fleischman, and Rebitzer 2006).
3. See https://faircompetitionlaw.com/the-changing-landscape-of-trade-secrets-laws-and-noncompeteCNC -laws/.

Existing studies do not provide a clear answer to the question of whether workers are indeed hurt by CNC enforceability. An early literature considered how CNC enforceability might reduce job-to-job mobility (Fallick, Fleischman, and Rebitzer 2006; Marx, Strumsky, and Fleming 2009; Marx, Singh, and Fleming 2015), but given the voluntary nature of these provisions, the key question is not how CNC enforceability affects worker mobility per se, but whether workers will be better off as a result. A budding literature does examine wage differentials associated with CNC enforceability, but there are three shortcomings. First, the results are mixed in terms of their main effects.[4] Second, while the policy proposals consider a complete ban of CNCs, none of the studies examine the wage effects of a full CNC ban. Third, no research examines the cumulative career effects of CNCs, which is important because CNCs necessarily entail dynamic, intertemporal trade-offs. CNCs imply post-employment mobility restrictions that could suppress wage growth over time, so that average population effects may hide interesting heterogeneity over a worker's career or job spell.[5]

In this study, we address the aforementioned gaps using two complementary approaches. First, we exploit an unusual natural experiment in which Hawaii banned CNCs in 2015 for technology workers *only*. Using Quarterly Workforce Indicators (QWI, Appendix Table 1) data (which aggregates all quarterly unemployment–insurance records to the industry–state–quarter level), we find that new-hire monthly earnings for technology workers rose following the July 2015 CNC ban relative to other industries in Hawaii, relative to technology workers in other states, and in a triple difference specification that controls for industry–quarter, state–industry, and state–quarter effects. In our preferred specifications, we find that Hawaii's 2015 CNC ban increased new-hire monthly earnings by 4.2 percent, while overall (that is, all worker average) monthly earnings rose 0.7 percent. The difference between new-hire and overall earnings is important because the 2015 Hawaii CNC ban only applied to new contracts. We also study worker mobility using data from QWI and find Hawaii's CNC ban increased mobility (measured in terms of separation rates) for technology workers by 12.5 percent (relative to tech workers in other states).

While the Hawaii CNC ban for technology workers provides a unique opportunity to study the effects of an outright ban, two concerns prompt us to complement this analysis. First, it is based on only one state, raising potential concerns about generalizability. Second, the recency of the Hawaii ban and data constraints preclude examination of outcomes over workers' careers. Accordingly, we supplement this analysis using quarterly employer–employee matched data for the universe of employees in 30 U.S. states between 1991 and 2008, a time period in which CNC policies were relatively stable. Our empirical approach is similar to Starr (2019) in that we examine how within-state differences between workers more and less likely to be bound by CNCs vary with cross-state measures of CNC enforceability. We focus on technology workers as the treatment group for three reasons. In addition to aligning with the focus on high-tech workers in the Hawaii case-study, high-tech workers are the most likely to sign CNCs

4. For example, Lavetti, Simon, and White (2020) find that CNC bound physicians earn more in states that more vigorously enforce them, and Kini, Williams, and Yin (2021) find the same for CEOs. In contrast, Garmaise (2011) finds negative wage effects for CEOs, and Starr (2019) finds negative wage effects for those most likely to be bound by a CNC.

5. In Online Appendix E we provide a simple model that documents this tension.

among all occupations (Starr, Prescott, and Bishara 2021), and they are also likely to have access to intellectual property that would theoretically precipitate the potential benefits of CNCs.[6]

Under the key identifying assumption that the group of workers we identify as "technology" workers are more strongly affected by CNC enforceability, and assuming that all other state-specific mobility and wage drivers that may be correlated with enforceability impacts workers in the technology sector in a similar way as other (control) industries, the coefficient on the interaction of technology sector with our CNC enforceability index measure provides an estimate of the differential effect of CNC enforceability on technology workers.[7] We first document that, despite more stringent identifying assumptions, this pseudo difference-in-differences empirical approach corroborates the findings from the Hawaii experiment: compared to nontech workers, technology workers have, on average, 6 percent longer job durations but 2–2.8 percent lower quarterly earnings in an average enforceability state relative to states that do not enforce CNCs (compared to the same differential for nontech workers). We subsequently expand our analysis to account for the cumulative mobility and earnings effects across eight years of a worker's career. We find that eight years after starting a job in an average enforceability state, high-tech workers have 4.6 percent lower cumulative earnings and 8 percent fewer jobs relative to observably equivalent high-tech workers in a nonenforcing state. We also study cross-state and cross-industry transitions. We find that workers whose first jobs are in technology in higher enforceability states are more likely to move across states without switching industries, potentially as a way to circumvent the CNC while retaining industry-specific capital.

Both sets of empirical analyses are robust to a variety of alternative specifications and subsample analyses, which are described in the robustness checks section and appendixes.[8]

Taken together, our results strongly suggest that CNC enforceability is associated with "job-lock" (similar to the findings of Gruber and Madrian 1994) and reduced bargaining power for the average technical worker (as discussed in Arnow-Richman 2001, 2006). In particular, our results suggest that CNC enforceability serves as a barrier to workers switching jobs and contributes to lower labor dynamism and wage stagnation. Together, these results contribute to the budding literature on CNCs and earnings by exploiting an unusual natural experiment and by studying cumulative career effects of CNC policies. The fact that the average tech worker does not appear to be compensated more for the CNC-related reductions in job-to-job mobility (for example, in

6. In addition, studying high-tech workers allows us to corroborate our results with the prior literature, which focused largely on the mobility of high-tech workers (Fallick, Fleischman, and Rebitzer 2006; Marx, Strumsky, and Fleming 2009; Marx, Singh, and Fleming 2015).

7. Because the estimated effect is the differential impact of CNC enforceability on technology workers, if there is also a similar effect for other sectors, our estimate would be a lower bound of the total effect on technology workers. Starr, Prescott, and Bishara (2021) and anecdotal evidence from CNC litigation (LaVan 2000) suggest that CNCs impacts other sectors as well.

8. Among others, for the cross-sectional results using the LEHD, we check and confirm robustness using the effects for workers with in the top 2 percent of start-of-spell wages as the focal group, which affords a triple difference test that also controls for state-by-industry fixed effects. For the Hawaii-specific results, we confirm robustness to permutation tests (Hess 2017), creating synthetic controls groups (Abadie, Diamond, and Hainmueller 2010), and various subsample checks.

any of the first eight years after starting a job) is also directly relevant for the state and federal policymakers who are actively debating changes to CNC policy (Office of Economic Policy, U.S. Department of the Treasury 2016; Burke 2016).

More broadly, our work contributes to the literature on monopsony in labor markets. Our finding of wage differences being correlated with CNC enforceability suggests a deviation from the "law of one wage" and contributes to work documenting wage dispersion (for example, Katz and Krueger 1992; see also the review by Bhasker, Manning, and To 2002). As in Naidu (2010), our results suggest that CNC enforceability slows down and redirects worker movement, reducing the returns to tenure and experience more broadly. Our findings are also consistent with Cahuc, Postel-Vinay, and Robin (2006), who find that between-firm competition is quantitatively more important than wage bargaining in raising wages above workers' reservation wages.

II. Examining Hawaii's 2015 CNC Ban for Tech Workers

A. Legal Background and Empirical Design

Given the policy and theoretical interest in identifying the causal effects of banning CNCs on wages, we begin with a rare but recent natural experiment: Hawaii, which previously enforced "reasonable" CNCs (Malsberger et al. 2010), banned CNCs for technology workers *only*, effective July 1, 2015. Specifically, the text of the Hawaii bill reads:

> …it shall be prohibited to include a noncompete clause or a nonsolicit clause in any employment contract relating to an employee of a technology business. The clause shall be void and of no force and effect.
> (HB 1090 H.D 2 S.D.2 C.D.1)

The stated reason for this ban was to promote growth of technology businesses, protect jobs, and encourage the establishment of new technology businesses by tech employees.[9]

We use data from the Quarterly Workforce Indicators (QWI) to examine mobility and wage patterns before and after July 2015 in Hawaii. The QWI is a public use database provided by the U.S. Census Bureau and derived from the universe of unemployment insurance records in the state.[10] The data include a set of quarterly "economic indicators,"

9. Section 1 states: "The legislature finds that restrictive employment covenants impede the development of technology businesses within the State by driving skilled workers to other jurisdictions and by requiring local technology businesses to solicit skilled workers from out of the State.…Because the geographic area of Hawaii is unique and limited, noncompete agreements unduly restrict future employment opportunities for technology workers and have a chilling effect on the creation of new technology businesses within the State by innovative employees.…[A] noncompete atmosphere hinders innovation, creates a restrictive work environment for technology employees in the State, and forces spin-offs of existing technology companies to choose places other than Hawaii to establish their businesses." The law was indeed not intended to be retroactive, as reflected in the phrasing "it shall be prohibited to include", and this was further clarified in a brief (one sentence) Section 4, which stated "This Act does not affect rights and duties that matured, penalties that were incurred, and proceedings that were begun before its effective date."

10. The QWI data were extracted using the online LED extraction tool: https://ledextract.ces.census.gov/static/data.html

including employment, job creation, earnings, and other measures of employment flows at the state–industry–quarter level. Relevant for our purposes, the QWI includes variables that track both the monthly earnings and mobility of workers.

Given that the ban on CNCs is specific to Hawaii and specific to the technology industry, we leverage both the state-specific dimension and the industry-specific dimension to perform three types of difference-in-differences (DID) analyses. In each design, firms and workers in the "technology business" per the Hawaii statute are the "treated" entities, but what varies across these designs are the control groups. In the first analysis, "Within-Hawaii, Cross-Industry" (hereafter "Within-Hawaii"), the control group is other industries in Hawaii. In the second analysis, "Cross-State, Within-Tech" (hereafter "Cross-State"), the tech sectors in the other 50 states form the control group. In the triple-difference analysis, we exploit the fact that our shock is both state- and industry-specific, allowing us to identify the Hawaii × Tech × Post 2015 interaction while controlling for industry–quarter, state–industry and state–quarter fixed effects. We undertake Fisher Permutation test (randomization inference) checks for all of the approaches (Hess 2017; Rosenbaum 2002), and also a synthetic control check of the "Cross-State, Within-Tech" DID results (Abadie, Diamond, and Hainmueller 2010).

Section 2 (d) of the Hawaii HB1090 bill defines a "technology business" to mean "a trade or business that derives the majority of its gross income from the sale or license of products or services resulting from its software development or information technology development, or both." Further, it states that "'Software development' means the creation of coded computer instructions", and that "'Information technology development' means the design, integration, deployment, or support services for software." Thus, we define "Tech" as NAICS four-digit subsectors related to software development, design, and related services.[11]

To examine how tech wages change following the CNC ban, we examine two QWI wage variables—average monthly earnings of workers in full quarter employment ("EarnS"), and average monthly earnings of all *hires* into full-quarter employment ("EarnHirAS"). While the ban on CNCs can be expected to improve the bargaining power of workers, because wage adjustment/renegotiation for existing workers is infrequent (and typically at year-end), larger effects should be expected for the latter, as wages for hires should reflect the changed bargaining position immediately after the ban. To obtain comparable length of pre- and post-ban trends, we examine data from 2013Q2–2017Q1 (the latest available data). To examine mobility responses, we construct a separation rate measure defined as the ratio of all separations of workers from employers in the quarter (variable named "Sep" in QWI) to total employment count (variable named "EmpTotal" in QWI). As an alternative, we use a defined separation rate variable available in the QWI—Beginning-of-Quarter Separation rate ("SepBegR") which is Beginning-of-Quarter Separations divided by average employment.[12]

11. The detailed list is provided in Online Appendix Table OA8. Note that the definition of "Tech" per the Hawaii statute is narrower than the definition we used in the cross-sectional analysis using LEHD data in Section III below.

12. To provide further information on the variables used, per QWI documentation: (i) EarnS is the average monthly earnings of employees with stable jobs (that is, worked with the same firm throughout the quarter). This is obtained by adding all quarterly earnings at j in t for all i who are full-quarter employees, dividing this by the number of full-quarter employees at j, then dividing that by three (number of months in a quarter). (ii) EarnHirAS is the Average monthly earnings for workers who started a job that turned into a job lasting a full

Our basic specifications are:
DID Cross-Industry, Within-Hawaii Analysis:

$$(1)\quad Y_{jt} = \alpha + \beta Post_t * I\{Tech\}_j + \Sigma_t + \omega_j + \varepsilon_{jt}$$

DID Cross-State, Within-Tech Analysis:

$$(2)\quad Y_{kt} = \alpha + \beta Post_t * I\{Hawaii\}_k + \Sigma_t + \omega_k + \varepsilon_{kt}$$

Triple Difference (DDD) Analysis:

$$(3)\quad Y_{jkt} = \alpha + \beta Post_t * I\{Tech\}_j * I\{Hawaii\}_k + \Sigma_{jt} + \Sigma_{kt} + \omega_{jk} + \varepsilon_{jkt}$$

where the subscripts *j, k,* and *t* are for industry, state, and quarter, respectively; Σ_t are year-by-quarter fixed effects; and ω_j are a set of industry fixed effects (NAICS four-digit codes). For the Cross-State analysis, we also allow for industry-by-year–quarter and state-by-industry fixed effects. In the triple difference analysis, ω_{jk} are a set of state–industry fixed effects, Σ_{jt} are industry $\times$ year-by-month (industry $\times$ year-by-quarter in QWI) fixed effects, and Σ_{kt} are state $\times$ year-by-quarter fixed effects.[13] Standard errors are clustered at the industry level in the Within-Hawaii analysis, and at the state level in the other two.

Within each of the alternative DID approaches and for the triple difference analysis, we check robustness to using alternative comparison groups. In particular, for the Within-Hawaii analysis, we use a smaller comparison group of the four-digit industries, which belong to the same two-digit level definition of the tech sector ("Tech 2d sector"), as well as a comparison group of all nontech four-digit sectors. Similarly, for the Cross-State and DDD analyses, we use a comparison group that excludes outlier CNC enforceability states by limiting the sample to the 40 states with CNC enforceability index scores (discussed in more detail in Section III.B below) closest (in absolute terms) to Hawaii in the pre-ban period, as well as a comparison to group of all other states.

B. Hawaii CNC Ban Results

We first discuss the wage effects. Figure 1 presents the time trends of the wage variables from QWI. In the top panel, the pre-ban trends in the wage variables are similar across

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quarter. This is obtained by adding all quarterly earnings at j in *t* for all *i* who are hires (all) to full-quarter status employees, then dividing this by the number of hires (all) to full-quarter status at *j* in *t*, and then dividing that by three. (iii) EmpTotal is technically termed "Employment - Reference Quarter: Counts" and is a count of people employed in a firm at any time during the quarter. (iv) Beginning-of-Quarter Separations (SepBegR) is the estimated number of workers whose job in the previous quarter continued and ended in the given quarter. A worker *i* is defined as a beginning-of-quarter separation from employer *j* in *t* if the worker has positive earnings at *j* in $t-1$ and *t* but no earnings from *j* in $t+1$. (iii) All Separations (Sep) is the estimated number of workers whose job with a given employer ended in the specified quarter. A worker *i* is defined as separating from employer *j* in *t* if the worker has positive earnings at *j* in *t* but no earnings from *j* in $t+1$. (iv) Average employment used in the definition of Beginning of Quarter Separation rate uses the variable "Emp" which is technically termed "Beginning-of-Quarter Employment: Counts". A worker *i* is beginning-of-quarter employed with employer *j* in *t* if worker has positive earnings at *j* in *t-1* and *t*.

13. Note that the double interactions in the DDD analysis (and the dummies themselves in both the DID and DDD analysis) get absorbed by the included fixed effects, so these specifications are fully saturated.

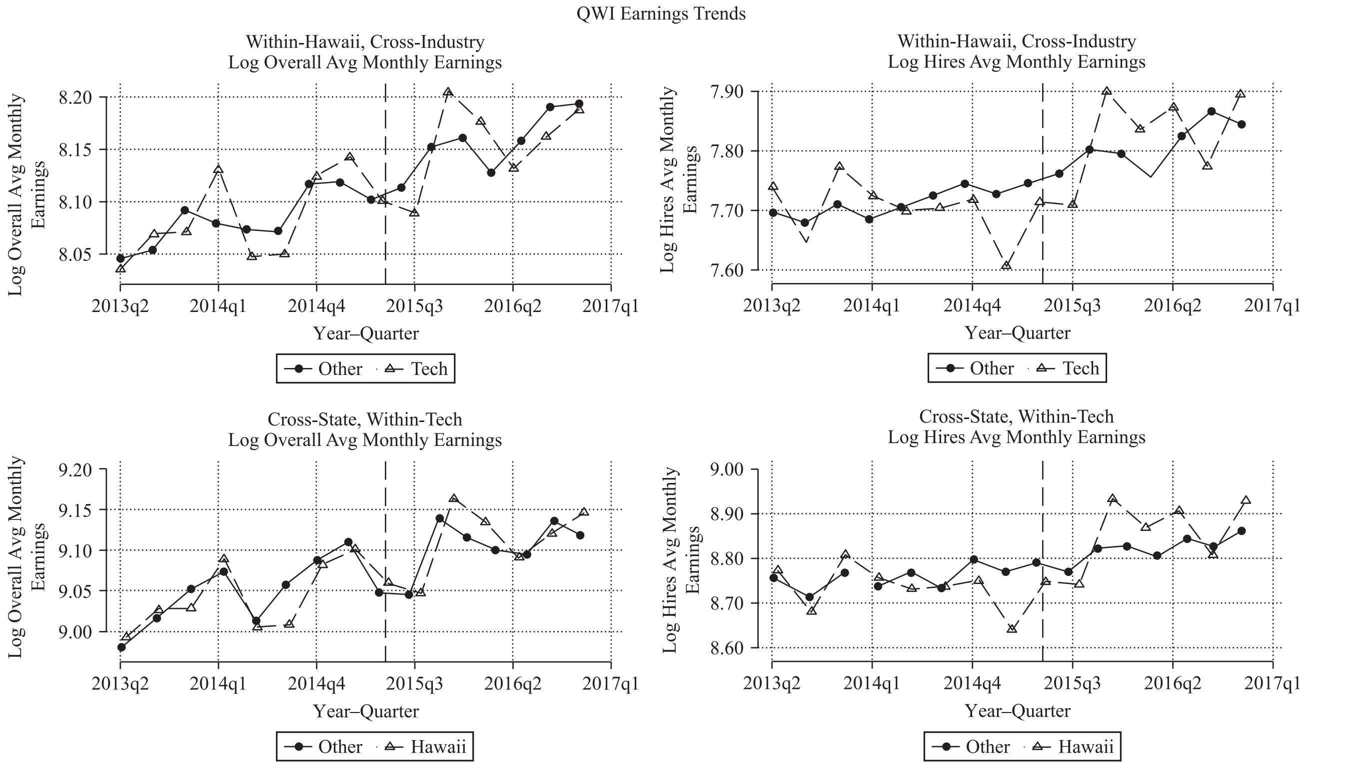

Figure 1

Hawaii CNC Ban and Wage Variables from QWI

Notes: This figure presents period-specific means (controlling for industry fixed effects in the "Within-Hawaii, Cross-Industry" graphs and for state fixed effects in the "Cross-State, Within-Tech" graphs). Data are limited to the state of Hawaii in the "Within-Hawaii, Cross-Industry" graphs (top panel) and to "Tech" industries in the "Cross-State, Within-Tech" graphs (bottom panel). "Tech" is defined as QWI four-digit industry classifications that cover software design, development, and services, to concord with the definition of "technology business" in the Hawaii statute. Log Overall Average Monthly Earnings is the log of group average of overall Average Monthly Earnings (Full Quarter Employment) (that is, log EarnS). Log Hires Average Monthly Earnings is the log of Average Monthly Earnings of All Hires into Full Quarter Employment (that is, log EarnHirAS). The group average means are weighted means, with industry–period Beginning of Quarter Employment (Emp) as (analytical) weights. Data are from the QWI, 2013Q2–2017Q1.

tech and other sectors within Hawaii. After the ban, the tech sector overall log wages show a short-run upward spike (left figure), as do log hiring wages (right figure). In the longer run, the other industry wages appear to climb upwards, so the trend changes catch up with those for tech. In the bottom panel, the short-run increase in log overall wage appears partly to be a reflection of a similar jump for tech wages in other states (left figure). However, in the bottom-right figure, the increase in hiring wages for the tech sector in Hawaii appears to be systematically greater than for tech hiring wages in other states.

These patterns are largely confirmed by the regression results in Table 1.[14] Panel A presents Cross-Industry, Within-Hawaii specifications, and Panel B presents Cross-State, Within-Tech results. Columns 3, 4, 7, and 8 present full-sample results, while Columns 1, 2, 5, and 6 present comparisons to narrower groups (other industries within two-digit technology sector in Panel A, and the 40 states closest to Hawaii in terms of CNC enforceability in Panel B). While the overall post-ban change is small and not statistically significant for overall wages in the Within-Hawaii (Panel A) analysis, there appears to be a notable increase in hiring wages (significant relative to all other industries in Column 7), with the effect driven by a large short-run increase. The Cross-State comparison in Panel B shows a systematic increase in the wages for tech workers in Hawaii relative to tech workers in other states, and these effects (with additional industry-by-quarter and state-by-industry fixed effects) appear to be more precise than suggested by the trends in Figure 1. The magnitude of the cross-state effects is largest for the hiring wage, which increases by 0.078 log points in Column 5 (or 0.071 in Column 7). This is more than 50 percent of the standard deviation in the pre-ban log wage rate for tech workers in Hawaii (0.14 log points). As discussed earlier, because overall average wages reflect the contracted wage for *all* workers, including those that do not have or are not seeking outside opportunities, and including workers who may have pre-existing contracts with CNC clauses (which were not affected by the ban), the lower magnitude of effects on overall average wage is not surprising. Overall, the results suggest modest effects on overall wages, but stronger increases in hiring wages.

These DID analyses are confirmed by the DDD results in Panel A of Table 2. Our preferred specifications show an increase in overall (that is, all worker average) monthly earnings of 0.7 percent (Panel A, Column 3) and a 4.2 percent increase in new-hire wages after the ban (Panel A, Column 7). These results are reassuring, showing that the increases in wages are robust to Hawaii-specific and tech-specific shocks that may have coincided with the ban. In particular, the results for wages are consistently larger and more significant than in the Cross-Industry, Within-Hawaii analysis.

The mobility results are shown in Figure 2, which displays the time trends of the mobility variables from QWI. The top panel shows the Within-Hawaii trends and compares the Hawaii tech sector to other industries in Hawaii, controlling for industry fixed effects. The tech sector in Hawaii shows a distinct increase in the short run following the ban, though there appears to be a reversal and greater volatility in the post-ban trends. In contrast, the trends for other industries in Hawaii don't show a significant jump just after the ban, but exhibit a slow increase in mobility, so that towards the end of

14. While the figures provide period means which are volatile, the regression tables capture the (smoothed average) DID effect which is the difference in means of the two trend lines in the pre-law change period relative to the difference in the two trend lines in the post-law change period.

Table 1
QWI Wage Variables Analysis from the Hawaii Natural Experiment—Difference-in-Differences Results

	Log Overall Average Monthly Earnings				Log Hires Average Monthly Earnings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Cross-Industry, Within-Hawaii								
Post × Tech	–0.000726		–0.00506		0.0389		0.0259***	
	(0.0245)		(0.0147)		(0.0348)		(0.00973)	
SR_Post × Tech		0.0190		0.00269		0.0943***		0.0440***
		(0.0187)		(0.0115)		(0.0278)		(0.0125)
LR_Post × Tech		–0.0414		–0.0212		–0.0751		–0.0115
		(0.0381)		(0.0216)		(0.0683)		(0.0139)
Observations	453	453	3,428	3,428	423	423	3,335	3,335
R-squared	0.962	0.962	0.979	0.979	0.906	0.907	0.924	0.924
Sample	Tech 2d	Tech 2d	All	All	Tech 2d	Tech 2d	All	All
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(*continued*)

Table 1 (*continued*)

	Log Overall Average Monthly Earnings				Log Hires Average Monthly Earnings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Cross-State, Within-Tech								
Post × HI	0.0223***		0.0178***		0.0778***		0.0711***	
	(0.00287)		(0.00430)		(0.00541)		(0.00617)	
SR_Post × HI		0.0217***		0.0180***		0.0825***		0.0776***
		(0.00313)		(0.00362)		(0.00602)		(0.00607)
LR_Post × HI		0.0237***		0.0175**		0.0681***		0.0575***
		(0.00548)		(0.00700)		(0.00584)		(0.00739)
Observations	3,721	3,721	4,753	4,753	3,668	3,668	4,690	4,690
R-squared	0.902	0.902	0.926	0.926	0.888	0.888	0.896	0.896
Sample	40 states	40 states	All	All	40 states	40 states	All	All
Ind × Year–Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: $^{*}p<0.1$, $^{**}p<0.05$, $^{***}p<0.01$. Robust standard errors in parentheses are clustered at the industry level in Panel A and state level in Panel B. Data are from the QWI, 2013Q2–2017Q1. Data are limited to the state of Hawaii in Panel A and to "Tech" industries in Panel B. "Tech" is defined as QWI four-digit industry classifications that cover software design, development, and services, to concord with the definition of "technology business" in the Hawaii statute. The dependent variable in Columns 1–4 is the log of overall Average Monthly Earnings (Full Quarter Employment) (that is, log EarnS). The dependent variable in Columns 5–8 is the log of the Average Monthly Earnings of All Hires into Full Quarter Employment (that is, log EarnHirAS). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3–2016Q2, and LR_Post is 2016Q3–2017Q1. In Panel A, Columns 1–2 and 5–6 are limited to four-digit industries within the two-digit industries that contain the tech industries, while other columns include all industries. In Panel B, Columns 1–2 and 5–6 are limited to the 40 states closest to Hawaii in the CNC score in absolute terms, while other columns include all states. All specifications use Beginning of Quarter Employment (Emp) as (analytical) weights. Number of observations adjusts for weights and singleton cells, that is, drops zero weights and singleton-cells (when fixed effects are added). The mean (SD) for tech industries in the pre-July 2015 of Log Overall Average Monthly Earnings period is 8.788 (0.084) and of Log Hires Average Monthly Earnings is 8.640 (0.140).

Table 2
QWI Wage and Mobility Analysis from the Hawaii Natural Experiment—Triple Difference Results

	Log Overall Average Monthly Earnings				Log Hires Average Monthly Earnings			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: QWI Wage Variables								
Post × HI × Tech	0.00964***		0.00712**		0.0441***		0.0424***	
	(0.00282)		(0.00270)		(0.00457)		(0.00361)	
SR_Post × HI × Tech		0.0121***		0.0100***		0.0558***		0.0548***
		(0.00276)		(0.00238)		(0.00479)		(0.00352)
LR_Post × HI × Tech		0.00451		0.00104		0.0198***		0.0166***
		(0.00653)		(0.00546)		(0.00625)		(0.00593)
Observations	166,529	166,529	208,728	208,728	164,140	164,140	205,828	205,828
R-squared	0.992	0.992	0.993	0.993	0.975	0.975	0.975	0.975

(*continued*)

Table 2 (*continued*)

	Overall Separation Rate				Beginning-of-Quarter Separation Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: QWI Mobility Variables								
Post × HI × Tech	0.0104***		0.00979***		0.0104***		0.00960***	
	(0.00180)		(0.00130)		(0.00108)		(0.000912)	
SR_Post × HI × Tech		0.0114***		0.0112***		0.0129***		0.0126***
		(0.00177)		(0.00125)		(0.000840)		(0.000644)
LR_Post × HI × Tech		0.00836***		0.00676***		0.00533**		0.00337*
		(0.00214)		(0.00173)		(0.00198)		(0.00174)
Observations	163,965	163,965	205,608	205,608	166,450	166,450	208,632	208,632
R-squared	0.945	0.945	0.945	0.945	0.902	0.902	0.899	0.899
Sample	40 States	40 States	All	All	40 States	40 States	All	All
Ind × Year–Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Year–Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$. Robust standard errors in parentheses are clustered at the state level. Data are from the QWI, 2013Q2–2017Q1. "Tech" is defined as QWI four-digit industry classifications that cover software design, development, and services, to concord with the definition of "technology business" in the Hawaii statute. In Panel A, the dependent variable in Columns 1–4 is the Overall Separation Rate defined as All Separations (that is, Sep) divided by Employment in the Reference Quarter (that is, EmpTotal), and in Columns 5–8 is the Beginning-of-Quarter separation rate (that is, SepBegR). In Panel B, the dependent variable in Columns 1–4 is the log of overall Average Monthly Earnings (Full Quarter Employment) (that is, log EarnS), and in Columns 5–8 is the log of the Average Monthly Earnings of All Hires into Full Quarter Employment (that is, log EarnHirAS). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3–2016Q2, and LR_Post is 2016Q3–2017Q1. Columns 1–2 and 5–6 are limited to the 40 states closest to Hawaii in the CNC score in absolute terms, while other columns include all states. All specifications use Beginning-of-Quarter Employment (Emp) as (analytical) weights. Number of observations adjusts for weights and singleton cells, that is, drops zero weights and singleton-cells (when fixed effects are added). The mean (SD) for tech industries in the pre-July 2015 of Log Overall Average Monthly Earnings period is 8.788 (0.084) and of Log Hires Average Monthly Earnings is 8.640 (0.140). The mean (SD) of the Overall Separation Rate for Tech industries in the pre-July 2015 period is 0.091 (0.020) and for Beginning-of-Quarter Separation Rate is 0.085 (0.025).

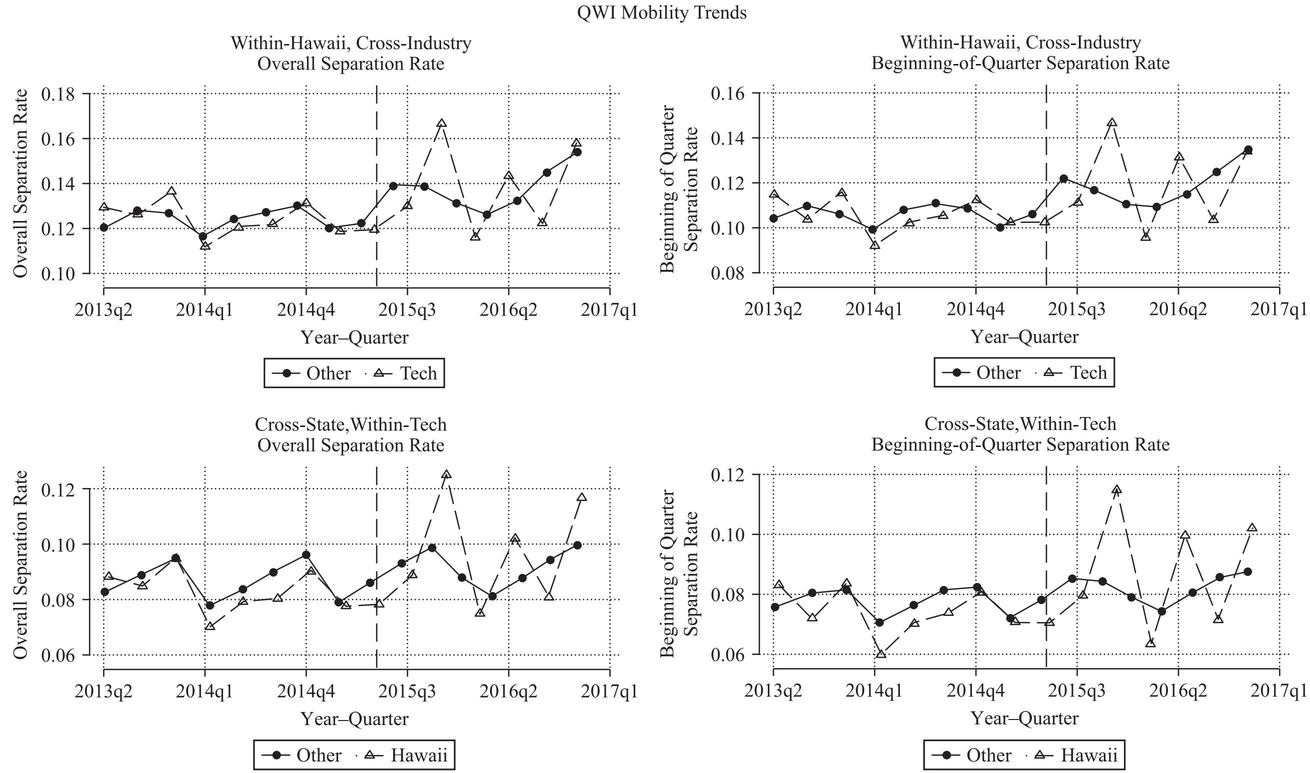

Figure 2

Hawaii CNC Ban and Mobility Variables from QWI

Noes: This figure presents period-specific means (controlling for industry fixed effects in the "Within-Hawaii, Cross-Industry" graphs and for state fixed effects in the "Cross-State, Within-Tech" graphs). Data are limited to the state of Hawaii in the "Within-Hawaii, Cross-Industry" graphs (top panel) and to "Tech" industries in the "Cross-State, Within-Tech" graphs (bottom panel). "Tech" is defined as QWI four-digit industry classifications that cover software design, development, and services, to concord with the definition of "technology business" in the Hawaii statute. The Overall Separation Rate is defined as All Separations (Sep) divided by Employment in the Reference Quarter (EmpTotal). The "Beginning-of-Quarter Separation rate" is beginning of quarter separation rate (SepBegr). The aggregated means are weighted means, with industry–period Beginning of Quarter Employment (Emp) as (analytical) weights. Data are from the QWI, 2013Q2–2017Q1.

our window, the mobility levels are similar for other industries in Hawaii. The bottom panel of Figure 2 shows the cross-state trends and compares the tech sector in Hawaii to tech sectors in other states, controlling for state fixed effects. The tech sectors in other states do not show any surge in mobility after July 2015, and the increased mobility levels in the tech sector in Hawaii are generally higher than for the other states. Overall, the surge in mobility in Hawaii after the ban is consistent with a release of mobility restrictions, and suggests facilitation of reallocation by the CNC ban.

These graphical results for QWI mobility variables are confirmed by regression results in Table 3. We find that there is an increase in the separation rate in the Within-Hawaii analysis (Panel A), particularly relative to other industries in the Tech 2d sector (Columns 1 and 5), and this is most pronounced in the short run (first four quarters) after the passage of the ban (Columns 2 and 6). The overall increase of 1.25 percent relative to other tech sectors in Column 1 translates to a 13.7 percent increase in mobility when compared to the mean separation rate of 9.1 percent for the tech sector in the pre-ban period. The point estimate is smaller in Column 3 (0.272 percent, or 3 percent of pre-ban mean separation rate), though this overall effect is a combination of a surge in the short run (0.753 percent or 8.3 percent of pre-ban mean separation rate) and a reversal in the long run. In Panel B, relative to other states, the tech sector in Hawaii shows systematically higher mobility in both the short and long run, and across both QWI mobility variables. The magnitude in Column 1 and Column 5 are very similar and is about 12.5 percent of the pre-ban mean separation rate. Results for the alternative mobility variable in Columns 5–8 are similar to the overall separation rate variable in Columns 1–4, for both panels. We note that the mobility patterns may have been affected by anticipation of the passage of the statute, and in this context it is reassuring that the magnitude of the effect we find from the ban (about 12.5 percent, comparing tech sector across states) is similar to the 15.4 percent effect found for inventor mobility from the increase in enforceability for Michigan stemming from a state supreme court decision (which may have been less anticipated) in Marx, Strumsky, and Fleming (2009). Panel B of Table 2 examines a triple-difference specification for mobility variables, and we get consistently significant effects, with magnitudes in line with that in the cross-state within-tech results. In particular, we find a 10.75 percent increase in overall separation rate (0.00979/0.091, in Column 3) and a 11.3 percent (0.0096/0.085, in Column 7) increase in beginning-of-quarter separation rate.[15]

One challenge associated with studying the case study of a change for one state is that the traditional DID inference approach may have shortcomings. In particular, as discussed in Buchmueller, DiNardo, and Valletta (2011), who study the effects of Hawaii's Prepaid Healthcare Act which mandated employer health insurance, standard inference may be insufficiently conservative and clustering approaches may have shortcomings because only one state experiences the policy change. We check and confirm statistical significance of our estimates using two prominent alternative approaches used in the literature in contexts similar to ours: (i) inference using a synthetic control group and (ii)

15. In Online Appendix B, we undertake a similar mobility analysis using data from the Current Population Survey (CPS). While we are cautious about emphasizing these results given the small sample size of individual-level data for technology workers targeted by the CNC ban in Hawaii, it is reassuring that these results show a significant increase in mobility for targeted tech workers in Hawaii after the CNC ban, consistent with the results using the QWI data.

Table 3
QWI Mobility Analysis from the Hawaii Natural Experiment—Difference-in-Differences Results

	Overall Separation Rate				Quarter Beginning Separation Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Cross-Industry, Within-Hawaii								
Post × Tech	0.0125*		0.00272		0.0109		0.00373	
	(0.00701)		(0.00219)		(0.00737)		(0.00282)	
SR_Post × Tech		0.0187***		0.00753***		0.0219***		0.00974***
		(0.00487)		(0.00206)		(0.00653)		(0.00286)
LR_Post × Tech		–0.000164		–0.00728**		–0.0118		–0.00876**
		(0.0185)		(0.00369)		(0.0211)		(0.00417)
Observations	413	413	3,321	3,321	452	452	3,423	3,423
R-squared	0.735	0.736	0.803	0.803	0.282	0.284	0.664	0.664
Sample	Tech 2d	Tech 2d	All	All	Tech 2d	Tech 2d	All	All
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year–Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

(*continued*)

Table 3 (*continued*)

	Overall Separation Rate				Quarter Beginning Separation Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Cross-State, Within-Tech								
Post × HI	0.0114***		0.0115***		0.0113***		0.0114***	
	(0.00119)		(0.000875)		(0.000966)		(0.000716)	
SR_Post × HI		0.0135***		0.0136***		0.0145***		0.0144***
		(0.00128)		(0.000930)		(0.000567)		(0.000504)
LR_Post × HI		0.00706***		0.00718***		0.00479*		0.00514***
		(0.00237)		(0.00170)		(0.00241)		(0.00173)
Observations	3,651	3,651	4,653	4,653	3,720	3,720	4,752	4,752
R-squared	0.822	0.822	0.835	0.835	0.760	0.760	0.782	0.782
Sample	40 States	40 states	All	All	40 states	40 states	All	All
Ind × Year–Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State × Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: $*p<0.1$, $**p<0.05$, $***p<0.01$. Robust standard errors in parentheses are clustered at the industry level in Panel A and state level in Panel B. Data are from the QWI, 2013Q2–2017Q1. Data are limited to the state of Hawaii in Panel A, and to "Tech" industries in Panel B. "Tech" is defined as QWI four-digit industry classifications that cover software design, development and services, to concord with the definition of "technology business" in the Hawaii statute. The dependent variable in Columns 1–4 is the Overall Separation Rate defined as All Separations (that is, Sep) divided by Employment in the Reference Quarter (that is, EmpTotal). The dependent variable in Columns 5–8 is the Beginning-of-Quarter separation rate (that is, SepBegR). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3–2016Q2, and LR_Post is 2016Q3–2016Q4 (latest date for which separations data was available). In Panel A, Columns 1–2 and 5–6 are limited to four-digit industries within the two-digit industries that contain the tech industries, while other columns include all industries. In Panel B, Columns 1–2 and 5–6 are limited to the 40 states closest to Hawaii in the CNC score in absolute terms, while other columns include all states. All specifications use Beginning-of-Quarter Employment (Emp) as (analytical) weights. Number of observations adjusts for weights and singleton cells, that is, drops zero weights and singleton-cells (when fixed effects are added). The mean (SD) of the Overall Separation Rate for Tech industries in the pre-July 2015 period is 0.091 (0.020) and for Beginning-of-Quarter Separation Rate is 0.085 (0.025).

inference using permutation tests, also used by Buchmueller, DiNardo, and Valletta (2011). In Online Appendix C, we check robustness of the Cross-State, Within-Tech analysis, using a synthetic control group approach proposed by Abadie, Diamond, and Hainmueller (2010). In this approach, a synthetic matched control is constructed based on a weighted average of states, with optimal weights constructed so that the predicted (from a factor model) dependent variable of interest for the synthetic control closely fits the pre-treatment trend in the treated state. Inference of statistical significance uses a variation of the Fisher permutation test, and goodness of fit (using a mean square prediction error measure) relative to the synthetic control in the post-treatment vs. pre-treatment for the "treated" synthetic state relative to placebo runs involving other states (as in Figure 8 in Abadie, Diamond, and Hainmueller 2010). We undertook a synthetic matched control analysis comparing the tech sector in Hawaii to the synthetic control composite of the tech sector in others states.[16] We obtain a good fit for pre-trends relative to the synthetic control for all variables and find significance relative to synthetic control at 7 percent for average hiring wage and and at 2 percent for mobility variables (but lower at 19 percent for average wages).[17]

In Online Appendix D, we verify the robustness of our results to a randomization inference (or permutation test) approach (Hess 2017). The randomization inference approach, analogous to Fisher permutation tests, collects estimates of Equation 2 across 500 replications that randomly allocate the tech indicator across four-digit sectors (in the Within-Hawaii analysis) and across states (in the Cross-State analysis), allowing us to examine how our point estimates compare with the distribution of potential point estimates using similarly sized alternative subsets of industries and states. We find results that are qualitatively consistent with those in the baseline analysis (Tables 1 and 2). In particular, for wage variables, we find stronger significance for increase in average hiring wage after the ban, and generally stronger significance when comparing tech sector in Hawaii to the tech sector in other states. We also find stronger cross-state, within-tech effects for mobility variables, with larger effects in the short-run period after the ban.

One caveat to keep in mind when interpreting our results is that the Hawaii statute included a prohibition of "nonsolicit" clauses (intended to prevent ex-employees from soliciting clients). Because these clauses are also broadly intended to discourage competition from ex-employees, this could be viewed as another form of restriction on competition; nevertheless, when considering implications of our results for policy interventions, it should be noted that the effects we find arise from a prohibition of both noncompete and nonsolicit agreements.[18]

16. We use the Abadie, Diamond, and Hainmueller (2010) Stata procedure "synth."

17. We also checked robustness of the DDD QWI results to including industry–year–quarter log employment as a control. This addresses a potential concern that coincidental labor supply shocks could drive workers out of the tech sector in Hawaii; while such a supply shift (say facilitating exit for Hawaii workers) could be induced by the ban on CNCs and so may not necessarily indicate the presence of some other shocks, results presented in Online Appendix Table OA9 show that the DDD results are not materially affected by inclusion of log employment as a control. Thus, changes in employment induced by other shocks are not spuriously leading to the observed DDD estimates.

18. We want to note that we are unaware of research examining the effect of nonsolicit clauses on mobility or wages. Further, media stories and discussions in the press about the statute mainly discussed noncompete provisions, and the preamble (Section 1) of the statute (quoted in Footnote 9 above) repeatedly emphasize the harms of noncompete clauses.

III. Examining Cross-State Variation in CNC Policies with Matched Employer–Employee Data

While the Hawaii analysis is a rare, policy-relevant opportunity to identify how banning CNCs influences worker wages and job-to-job transitions, the analysis has a few important limitations. First, Hawaii's labor market is geographically isolated, which raises concerns about the potential generalizability of the findings. Second, because the changes are recent, we lack individual-level data examining how the CNC ban may change long-term dynamics related to the evolution of wages within and across jobs. Third, the ban on CNCs simultaneously banned nonsolicitation agreements, suggesting that the effects we identified could be driven by the combination of the two provisions being banned, rather than just CNCs. Motivated by these limitations, in this section we bolster and extend the findings from the Hawaii experiment using employer–employee matched data covering all workers from 30 states for 1991–2008.[19]

The data come from the Longitudinal Employer-Household Dynamics (LEHD) database at the U.S. Census Bureau and are derived from the universe of unemployment insurance records of employees (the same source as the QWI data used above). There are two advantages of the LEHD. First, the LEHD provides employment history data for individual workers over a long horizon for a full spectrum of industries across a large number of states that vary in CNC enforceability levels. Second, the quarterly administrative data on all firms provides a clear measure of job transfer, mobility, and wage at a high frequency, largely free from selection issues that may arise in studies that use patent or listed-firm executive employment data (Marx, Strumsky, and Fleming 2009; Garmaise 2011).

Despite these benefits, the main downside of the LEHD is that it does not cover a time period with many major exogenous changes in the enforceability of CNCs to use within-state variation to identify the causal effects. Garmaise (2011), for example, identifies several such changes, but only one of those changes occurs during our timeframe, and Census disclosure rules require at least three states to have the same policy shock to disclose the estimated coefficient.[20] Given these limitations, the only feasible identification strategy is to exploit cross-state heterogeneity in the enforceability of CNCs, which requires stronger identifying assumptions. Fortunately, cross-state variation in

19. The states in the LEHD (in order from weakest to strongest CNC enforceability) are California, Oklahoma, West Virginia, Arkansas, Montana, Rhode Island, Virginia, Texas, South Carolina, Hawaii, Wisconsin, Georgia, Nevada, Oregon, North Carolina, Washington, Colorado, Maine, Tennessee, Louisiana, Maryland, Vermont, Indiana, New Mexico, Idaho, New Jersey, Illinois, Utah, Iowa, and Florida. Vilhuber and McKinney (2011) provide detailed documentation for LEHD data.

20. In particular, while we find very strong correlation in CNC enforceability index ranks across 2009 and 1991 (both for our Starr index, as well as the Bishara 2011 index), Garmaise (2011) argues for changes in CNC enforceability for three states—Texas (in 1994), Louisiana (briefly from 2001 to 2003), and Florida in 1996. Of these, only Louisiana has data in our LEHD sample before and after the identified change as Texas joins the LEHD data only in 1995 and Florida only in 1998. Two other changes identified recently in Ewens and Marx (2018) fall within our sample window—Ohio (in 2004) and Vermont (in 2005). However, Ohio is not among the 30 LEHD states available in our sample. This leaves only two states (Louisiana and Vermont) with potential changes within our sample period. Therefore, per the Census disclosure requirement that at least three states with common policy changes be included in any analysis, we are unable to examine these two within-state changes using the LEHD data.

the enforceability of CNCs is large, and several recent studies have created indexes that seek to measure these differences (Bishara 2011; Starr 2019; Lavetti, Simon, and White 2020).

A central challenge to using only cross-state variation in enforceability is the concern that other omitted state-level variables may be correlated with our outcomes of interest, as well as with CNC enforceability. We describe our identification strategy and assumptions in detail in Section III.C below. The key idea is that we identify one group of workers—specifically workers employed in the "technology" sector (as defined below in Section III.C.1)—as those for whom CNC is likely to have a greater impact, and make the assumption that only CNC enforceability (and not other omitted state level variables) has a differential impact for this group of workers.

A. LEHD Data and Sample Construction

We construct two samples with the LEHD: a job-level (that is, worker–firm combination) data set and a worker-level data set that tracks workers within and across jobs (that is, their employment history).[21] In both data sets, we drop all left-censored jobs (that is, individual worker jobs spells that started before the beginning of our panel period). We do so because we do not know the lengths of the latent spells for these jobs (as we do not know the start date of these spells) or the characteristics of these jobs at the beginning of the spell, which we use to construct our job-level fixed effects (as described below). Further, dropping these jobs avoids bias from stock sampling. We also drop workers whose first-year annual income in the LEHD is less than \$35,000 in 2008 dollars (close to the 10th percentile of 2008 earnings in the tech sector per the 2008 Occupational Employment Statistics), to focus on high-earning workers who are the most likely to sign CNCs (Starr, Prescott, and Bishara 2021). Secondary jobs (defined by the share of that job's earnings to the worker's total earnings) whose spell is continuing in parallel to another job for the same worker are also dropped.[22]

In terms of our key earnings outcomes, in the job-level analysis we measure log quarterly earnings at the 1st, 4th, 8th… 32nd quarters of the job spell, CPI-adjusted to 2008 dollars.[23] We also examine log cumulative earnings within the job, and within-job wage growth relative to the starting wage at the 4th, 8th… 32nd quarters of the job spell. In our analysis across the worker's employment history, which tracks wages regardless

21. We allow job spells of the previous job and the new job to overlap in a year–quarter in which the job transition occurs. Moreover, because the firm identifiers from each state's database are within-state identifiers, we use the national-level firm identifier (ALPHA) available in the Business Register Bridge (BRB) for defining the job. "ALPHA" is a cleaned longitudinal firm identifier. Using ALPHA ensures we do not wrongly capture within-firm, interstate, or intrastate transfers as worker movements out of a firm.

22. Specifically, because we wish to focus on full-time jobs, we imposed a conservative earnings share threshold to define the full time primary job. We noted that a job's earnings share can be low when there is job transition in the quarter (because earnings in the LEHD are quarterly aggregates). Taking this into account, we dropped job records whose earnings share from the job is less than 75 percent of total quarterly earnings for two consecutive quarters. For job spells that lasted for only a single quarter, we dropped the spell job when its earnings share was less than 75 percent for that quarter.

23. The LEHD has quarterly earnings data. When the job spell ends at Quarter $4t$, we take the quarterly earnings at quarter $4t-1$, because the earnings in Quarter $4t$ would not fully reflect the worker's per quarter wage level (for example, if the spell ends in the 28th quarter, then we use the quarterly wage aggregate from the 27th quarter instead of the 28th quarter).

of whether the worker switches jobs,[24] we use as a dependent variable, the cumulative earnings of the worker at the 4th, 8th, …, 32nd quarters since the worker started their employment history.

As before, we supplement the wage results with analyses of worker mobility. In the job-level analysis, we measure the length of the job spell defined as the log number of quarters the worker was employed at the firm. To mitigate concerns about right-censoring, we restrict our sample to jobs whose spell started in 2000 or earlier for this analysis, so that we have a minimum of eight years of observations for each worker.[25] The second approach avoids the right-censoring issue entirely by using a set of dummy variables for the job spell surviving a given length of time as the dependent variables: a dummy variable with a value of one if the job spell survives until the 4th, 8th, …, 32nd quarter of its spell (or eight years). These job-level analyses miss, however, the cumulative effect on job switches across multiple jobs. Accordingly, we also examine the cumulative number of jobs taken (in logs) and the number of state or industry switches (in logs) over the course of the worker's eight year employment history (estimated at the 4th, 8th… 32nd quarter).[26]

B. Index Measuring Cross-State Differences in CNC Enforceability

A central component of our empirical approach with the LEHD data is to measure the extent of CNC enforceability across states. We adopt the index derived in Starr (2019), but before describing the specifics of the index, we begin with a brief overview of the law and the ideal measure of CNC enforceability. States regulate CNCs, and states have historically come to very different positions related to whether and the conditions under which CNCs should be enforced (Bishara 2011). A handful of states (for example, California, North Dakota) will not enforce CNCs related to job-to-job mobility (exceptions are made in the context of CNCs ancillary to the sale of a business). Prior work on the California policy, which was adopted in 1872, has linked it to the growth of Silicon Valley (Gilson 1999; Fallick, Fleischman, Rebitzer 2006).

All other states enforce CNCs that pass a "reasonableness" test, though the contours of that test differs across states—what one state deems reasonable, another may not. Various editions of a treatise on CNC enforcement across states edited by Malsberger and coauthors (for example, Malsberger et al. 2010) provide detailed descriptions of the conditions under which each state will enforce a CNC. All prior measures of CNC enforceability review this treatise and score states on various dimensions of CNC enforceability (Stuart and Sorenson 2003; Bishara 2011; Garmaise 2011). These dimensions include, for example, whether the state will enforce CNCs even if a worker is fired, whether continued employment is sufficient consideration for the enforcement of a CNC, and whether the state courts can rewrite or "blue-pencil" overbroad provisions. A full list of the components in the index in Bishara (2011), which forms the basis for the Starr (2019) index used in this paper, is provided in Appendix Table 2.

24. Because the LEHD covers only 30 states, examining worker-level outcomes (unlike job-level outcomes) potentially carries measurement error due to movement of workers into noncovered states. See Section III.G.4 for evidence on the lack of correlation between the missing states and enforceability.

25. Nonlinear duration model estimations (for example, cox proportional hazard models) were not computationally feasible alternatives for our analysis, because our identification strategy utilizes high-dimensional fixed effects.

26. For left-censored workers, the 4th, 8th, 32nd quarters of the worker's employment history are measured starting from the first job that is not left-censored.

Existing indexes score several of these dimensions, with higher scores reflecting that it is easier to enforce CNCs, and then aggregate these dimensions to obtain a state-specific score. Garmaise (2011) reviews 12 dimensions of enforceability and gives each dimension a score of one or zero. Bishara (2011) similarly scores seven dimensions from zero to ten (again with higher meaning easier to enforce). Furthermore, Bishara—a former practicing attorney who litigated CNCs—attempts to weight each dimension of CNC enforceability on the basis of his subjective assessment of its importance, while the Garmaise (2011) measure gives each dimension equal weight. This weighting is theoretically important. If we would like to measure the enforceability of CNCs in a given state (defined as the probability of enforcement for a random violator of a CNC), we would have to know not only the situations under which a state would enforce a CNC (as captured in the Malsberger treatises), but also the probability with which those conditions occur. For example, if a state enforces CNCs when a worker is fired, but such an event is either rare or nonexistent, then such a policy will not have any real effect on the probability of CNC enforcement.

We use the measure of CNC enforceability derived in Starr (2019), which uses factor analysis of the seven underlying dimensions across all states of CNC enforceability initially scored in Bishara (2011) for both 1991 and 2009, to back out an estimate of a (latent) enforceability index for these years. The resulting index is normalized to have a mean of zero and a standard deviation of one in a sample where states are given equal weight; the index ranges from –4.23 for North Dakota to 1.60 for Florida. The scores for 1991 and 2009 have a correlation of 0.94, reflecting that during this time there is very little change in how states enforced CNCs. We use the 2009 measure for our analysis. Figure 3 presents the enforceability index scores by state. California and North Dakota have the lowest scores, while the highest enforcing states are Florida and Connecticut.

A potential concern about the CNC enforceability index arises from the fact that the measures for California and North Dakota are markedly lower than for other states. Administrative constraints related to use of LEHD data preclude a straightforward robustness check in the form of excluding these two states. As an alternative, we undertake and confirm robustness of all of our key results to using the rank of the indexes, which significantly mutes the differences in levels (see discussion in Section III.G).

A summary of states classified into three groups of high, medium, and low enforceability, as well as a brief description of enforceability provisions in a typical state in each group, is provided in Appendix Table 3. The index we use is not very different from, and hence highly correlated with, the Bishara (2011) index used in the literature (for example, Lavetti, Simon, and White 2020), as, rather surprisingly, the factor weights generated from the factor analysis by Starr (2019) closely match the subjective weights used by Bishara (2011).[27] The means for the dummy variable measure used in Stuart and Sorenson (2003) by different enforceability groups are provided in Column 5 of Appendix Table 3 and show that our measure is broadly consistent with their measure as well.

Enforceability of CNCs is not geographically clustered in a systematic way, as there is significant variation within regions of the United States (Figure OA8 in the Online Appendix provides a geographic heat map of the index). To get a sense of what types

27. The R-squared in a univariate regression of the Starr index on the Bishara index is 0.976, and for a regression of the ranks of the measure on each other is 0.937. The scatter plots showing correlation (in raw scores and ranks) between the Starr (2019) and the Bishara (2011) indexes, as well as both the indexes for each of the states are available in Figure OA7 in the Online Appendix.

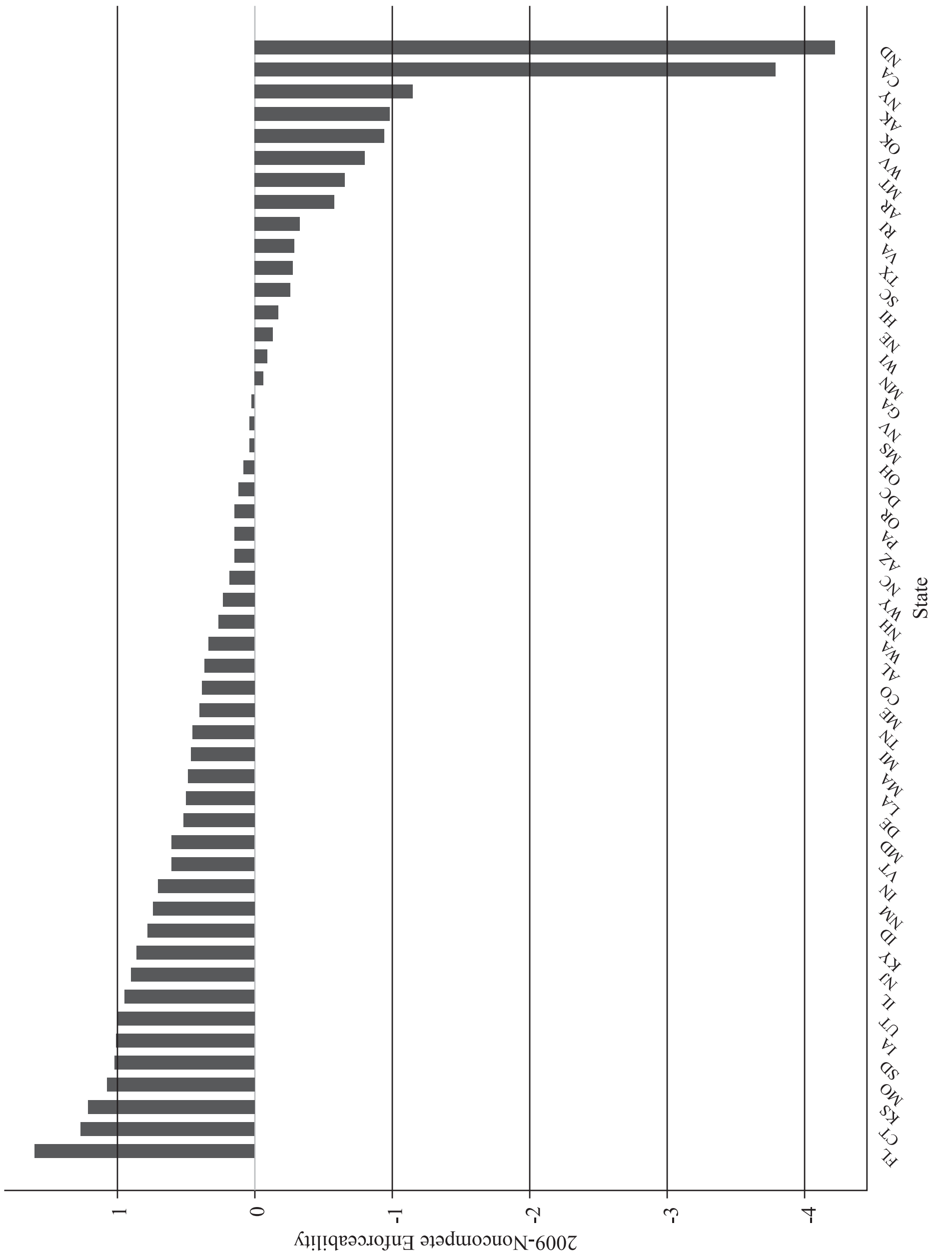

Figure 3
Factor Analysis CNC Enforceability Index Scores for 2009 from Starr (2019)

of characteristics may be associated with this measure of state CNC enforceability, we examine how this measure was correlated with a range of state-level variables that could plausibly affect worker wages or mobility, around the midpoint of our data period (year 2000). Our analysis shows that our enforceability index has very little correlation with state per capita income, state corporate tax rates, union density, right to work law adoption, and different exceptions to the employment-at-will doctrine (taken from Autor, Donohue, and Schwab 2006), which could impact worker firing and hence mobility), with the R-squared from a regression including all seven state-level variables equal to 0.122.[28]

C. Empirical Approach with LEHD Data

1. Identification assumptions and definition of "tech" workers

The key identifying assumptions for the cross-state LEHD analysis are the following: (A1) the group of workers identified by us as "high technology" (high-tech or tech) workers are more strongly impacted by CNC enforceability than other workers, and (A2) other omitted state-level variables that may be incidentally correlated with CNC enforceability have a similar impact on high-tech and non-high-tech workers.

Specifically, we assume that variable Y_{ks} that measures the outcome in a job spell (for example, wage at end of Quarter 4 in a job spell) in state s and in sector k has the following form:

$$Y_{ks} = \alpha + \beta R_s + \gamma CNC_s + \delta CNC_s \times I\{Tech\}_k + e_{ks}$$

where R_s represents a vector of (omitted) state-level regulatory or other characteristics that can affect the outcome variable, CNC_s represents our enforceability measure, and $I\{Tech\}_k$ is a dummy indicator variable for if the sector k belongs in high-tech (the definition of high-technology sector is explained in detail below). In general, the omitted R_s variables could be correlated with CNC_s, which makes it impractical to recover an unbiased estimate of parameter γ from cross-state regressions of the type we use. However, because by our assumptions CNC (but not other omitted state-level variables) has an additional impact on outcomes for job spells in the technology sector, we can recover an unbiased estimate of δ by including state fixed effects that condition out the effect of R_s. We want to note that under our assumptions, the estimated parameter δ yields only the differential effect of CNC enforceability on the group of workers we define as "high-tech" workers and hence must be interpreted carefully.[29]

28. See Online Appendix, Figures OA9 and Table OA13, for results examining correlation of the CNC measure to other state-level variables.

29. In particular, we want to stress three points. One, even with our maintained assumptions, a null estimate for δ only means that the differential effect of CNC enforceability for the job spells in the group of industries we define as high-tech are not different from that of other industries, and it does not tell us about the overall effect of enforceability on all sectors. Two, if the group of interest is a different subset of "technology" workers than who we define here, our estimate would need to be suitably adjusted. For example, if we assume that our estimate is entirely driven by a subset of "true" technology workers (say with actual science or engineering college degrees), and if they form say 50 percent of the high-tech-sector job spells in our sample, then our estimates would need to be adjusted by a factor of two to obtain the effect for those workers. (Note that, as discussed above, our sample is restricted to workers with wage above $35,000 in the first year they are in the LEHD panel, so the share of workers with college or advanced degrees is likely to be higher in our sample than in the overall

The plausibility of our identifying assumptions rests importantly on our definition of high-technology workers. We define a worker, or more accurately a worker–firm pair job spell, as belonging to "high-tech" if the industry of the firm (as defined in the LEHD at the start of the job spell) belongs to a "Technology Employer" industry NAICS code (aggregated to three-digit level) per the classification in Paytas and Berglund (2004). This definition uses a categorization of "science and engineering intensive occupations" based on U.S. Bureau of Labor Statistics Occupational Employment Statistics (OES) codes, developed by Chapple (2004), and an occupation–NAICS employment concordance provided by the Bureau of Labor Statistics to obtain the share of employment in science and engineering intensive occupations for each NAICS code. Paytas and Berglund then define "Technology Employers" as those NAICS industries where the share of employment in science and engineering intensive occupations exceeds three times the national average (that is, a cutoff of 9.98 percent, as the national average was 3.33 percent in 2002). We aggregate this definition to the NAICS three-digit level.[30] Job spells in these "Technology Employers" industries form our treatment group, hereafter referred to as "high-tech" or "tech" jobs, whereas jobs in "Other Industries" are referred to as "nontech jobs."

Given our definition of "high-tech" jobs, we believe Assumption A1 is plausible for two reasons. First, using nationally representative survey data, Starr, Prescott, and Bishara (2021) conclude that "the use of noncompetes is highest for technical occupations (computer, mathematical, engineering, architecture) in the manufacturing and information industries." Because our definition of high-tech workers relies on share of science and technology occupations within the sector, it is likely these workers are more subject to noncompetes and hence more likely to be impacted by CNC enforceability. To provide a sense of the differential in the use of CNCs across sectors, we note that Starr, Prescott, and Bishara (2021) find that 26.3 percent of workers in Information, Mining and Extraction, Manufacturing, or Professional, Scientific, and Technical Services sign CNCs, compared to 15.6 percent in other industries. Second, the sectors we define as high-tech are more intellectual property–intensive, as reflected in the commonly used (albeit imperfect) measure of patent counts. Specifically, our estimation using USPTO and QWI national

economy.) Three, while a secondary motivation for our choice of high-tech industry is to examine whether high-tech workers as a group are indeed differentially affected by CNC and hence worthy of targeted policy interventions (such as for Hawaii), even if the differential effect on wages and mobility for high-tech are not significant, our analysis would not rule out the impact of enforceability on other welfare-relevant outcomes, such as extent of knowledge spillovers, new venture creation, or innovation being higher for the high-tech sector (justifying policy targeted to easing CNC enforceability for these sectors). In addition to interest from policymakers (for example, as in Hawaii), prior literature (for example, papers using data on inventors) on CNCs has treated technology sector as the distinctive group of interest, and others argue that mobility of technology workers is uniquely important for the development of high-technology clusters (Gilson 1999; Hyde 2003; Fallick, Fleischmann, and Rebitzer 2006).

30. That is, every three-digit sector where any four-digit subsector belongs to the Paytas–Berglund list is classified as high-tech for our analysis. The list of these technology employer industries (Table 1 in Paytas and Berglund 2004) is provided in the Online Appendix (Table OA10), and we provide summary statistics on workforce characteristics (average log wage, separation rates, and demographics) by technology sector and enforceability in Online Appendix Table OA11a, using the QWI publicly available data workers (albeit not limited to those with >$35,000 annual wages that we use in our LEHD regression analysis). Tech-sector workers have higher wage, lower separation rates, are more male, smaller share of young workers and larger fraction in large firms, with this pattern similar across states with different enforceability levels. Separation rates and employment share of young are larger, and employment share of large firms are smaller, in low enforceability states (both for tech and nontech sectors). As discussed in Section III.C.2, our fixed effects controls for (interacted) firm size, gender, and age effects.

data for the period 2000–2004 (where QWI national coverage is more complete than prior years) shows that these sectors accounted for 62.1 percent of all patents granted in this period, while accounting for only 14.5 percent of total employment.[31]

Assumption A2 is an important one that we believe is plausible for several potential sources of omitted variable bias. For example, other labor regulations (such as exceptions to employment-at-will examined in Autor, Donohue, and Schwab 2006)[32] could be expected to impact firing (and hence mobility and wages) for a broad set of industries, not just the high-technology industries. However, any characteristics correlated with wages and CNC enforceability that have a differential relationship with tech or non-high-tech workers would lead to a violation of A2 and bias our estimates. In a key robustness check, we relax ASSUMPTION A2 by making a weaker assumption that the subgroup of more highly paid workers within the high-tech sector are more strongly impacted by CNC enforceability and that other omitted factors do not impact this specific subgroup differently. This approach is discussed briefly in Section III.G and in more detail in the Online Appendix A.

One particular weakness of Assumption A2 is that other state-level characteristics could influence selection of certain types of (for example, larger) firms or (for example, younger or more male) workers into a state, and these firms or workers may be associated with different levels of mobility or wages specifically for the high-tech sector. For example, if young male workers in high-tech are highly mobile, and young workers are attracted to low CNC locations (for example, California) because of other factors potentially correlated with enforceability, then we might find higher mobility associated with low CNC enforceability induced by this selection. Fortunately the richness of the LEHD data allows us to include a very rich set of interacted {sector by firm size by worker age by gender by initial wage by job-spell starting year} fixed effects that flexibly condition out biases from this type of sector–worker–firm–cohort selection effects.

2. *Empirical specification*

Equation 4 displays our estimating equation for the job-level analysis, which captures the differential relationship between CNC enforceability and outcomes for tech versus nontech workers, with additional subscripts to capture additional details in our actual estimation:

$$(4) \quad Y_{ijkls} = \alpha + \delta CNC_s \times I\{Tech\}_k + \Sigma_s + FE(i, j, k, l) + \gamma FB_i + \varepsilon_{ijkls}$$

Similar to the discussion above, Y_{ijkts} denotes a job-spell-level outcome variable of interest (for example, wage at end of Quarter 1 of the job spell). The subscripts i, j, k, l, and s denote individual, job spell, industry, firm, and state, respectively. Our coefficient of interest is δ, which estimates the differential association between CNC enforceability

31. We thank Nathan Goldschlag for sharing USPTO patent count data by NAICS four-digit sectors, which he and coauthors put together in connection with the work on Goldschlag, Lybbert, and Zolas (2019). The summary table is provided in the Online Appendix (Table OA11b).

32. As discussed above, we verify that the different exceptions to the employment-at-will doctrine studied by Autor, Donohue, and Schwab (2006) have low correlation with our enforceability measure (Online Appendix Table OA13).

and the dependent variable for high-tech jobs relative to nontech jobs. CNC_s is the 2009 CNC enforceability index measure (defined as discussed in Section III.B above) of the state where the job spell is observed. $I\{Tech\}_k$ is one if the firm of the worker–firm pair is in one of the "Technology Industries" (defined as discussed in Section III.C.1 above). Σ_s denotes state fixed effects, which subsumes the main effect of CNC enforceability and controls for the direct effect of omitted state-level variables that affect wages and mobility. In addition to these basic controls, we include the aforementioned fixed effects in $FE(i,j,k,l)$. Each joint fixed effect defines a group of jobs that are common (that is, fully interacted) in terms of their three-digit NAICS codes, job-spell starting year, firm-size group, job-spell starting-wage group, job-spell starting-age group of the worker, and gender of the worker.[33] FB_i is a dummy that denotes whether the worker is foreign-born, which we include because foreign-born employees are subject to visa-related employment eligibility constraints that may affect their mobility (and wages).

For the worker-level analysis of career outcomes (in Section III.E), such as cumulative number of jobs taken, cumulative wages, and cumulative number of states (or industries), the CNC enforceability measure is that of the state in which the worker's first job is located. The job-level variables and the job-level fixed effects are based on the initial characteristics of the first observed job.

D. Job-Level Results

We first corroborate our baseline findings in the Hawaii analysis by examining the job-level results. Table 4 presents the mirror of our Hawaii results in the context of our LEHD Analysis, tracking the relative effect of a one standard deviation increase in CNC enforceability on tech versus nontech workers. We observe a persistent negative relation between enforceability and wages. The coefficient δ ranges from 0.5 percent to 0.7 percent for high-tech jobs compared with nontech jobs in a given quarter, and lower earnings of 2.5 percent in the quarter hired. To put these estimates in context, assuming a uniform causal effect over the distribution of enforceability scores, applying the average enforceability score to a nonenforcing state (a difference of four standard deviations) suggests that moving from a ban to average enforceability would be associated with 2–2.8 percent lower wages for the average technical worker in a given quarter. The coefficient estimates and the 95 percent confidence intervals, translated to reflect the effect of applying the average enforceability score to a nonenforcing state (a difference of four standard deviations), are plotted in Figure 4. The wage profile is relatively flat, so the wage penalty of CNCs is similar in log difference terms over the job tenure. Column 1 of Table 4 presents results on the initial start of job spell wage (this specification naturally excludes initial wage fixed effects)—consistent with the results for new-hire wages for Hawaii, we find that initial wages are lower in states with higher CNC enforceability. Thus, there is no evidence that lower wages later in the job spell are offset by higher initial wages. Overall, the results are consistent with a reduction in bargaining power starting early in the job tenure of tech workers.

33. Firm size is the maximum number of quarterly workers employed at the firm in the year when the job spell started, grouped in quartiles. Starting wage is defined by a categorical variable, with 11 categories along the distribution of starting wages of jobs within three-digit NAICS codes (this particular dimension is omitted in the analysis of starting wages, cumulative wages, and wage growth). Starting age is the worker's age in the job's first year in quartiles of the distribution of starting ages for all jobs.

Table 4
CNCs and High-Tech Workers' Wage across Job Tenure (LEHD)

	Dependent Variable: Log of Wage at *x*th Quarter								
	Initial Wage (1)	4th Qtr. (2)	8th Qtr. (3)	12th Qtr. (4)	16th Qtr. (5)	20th Qtr. (6)	24th Qtr. (7)	28th Qtr. (8)	32nd Qtr. (9)
Tech × CNC score	−0.0259*** (0.0019)	−0.0057*** (0.0006)	−0.0066*** (0.0006)	−0.0068*** (0.0007)	−0.0069*** (0.0008)	−0.0061*** (0.0008)	−0.0054*** (0.0009)	−0.0061*** (0.0012)	−0.0057*** (0.0016)
Observations	13,205,400	10,904,200	7,397,200	5,399,500	4,048,400	3,145,300	2,478,900	1,858,400	,1412,600
R-squared	0.1853	0.6726	0.6090	0.5764	0.5570	0.5429	0.5323	0.5237	0.5114
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Age – Sex]	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All continuing jobs in the quarter								

Notes: This table reports the differential effect of CNC enforceability on wage across job tenure by industry (high-tech jobs vs. nontech jobs). The dependent variables are the log of monthly earnings at 4th, …, 32nd quarter of the job spell. CNC score is measured as the 2009 CNC enforceability index scores. Data are from the LEHD (1991–2008). All standard errors are clustered by state. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

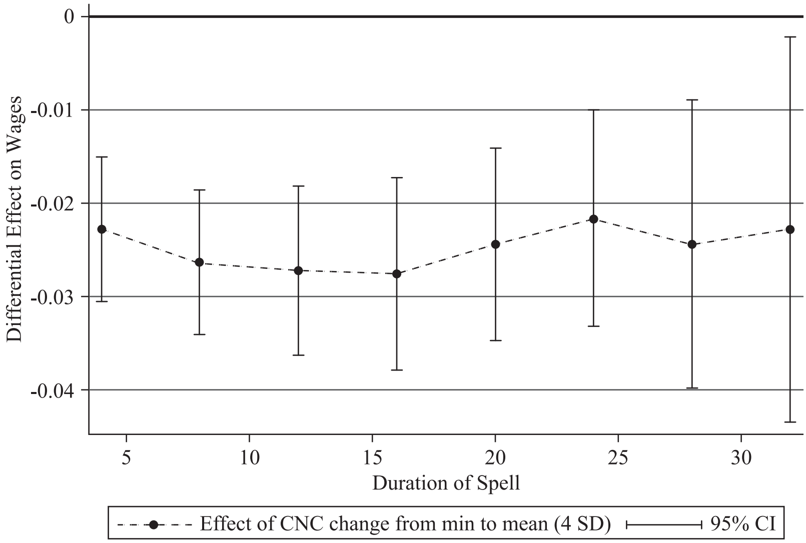

Figure 4

CNCs and High-Tech Workers' Wage across Job Tenure (LEHD)

Notes: This figure plots the coefficient estimates of the differential relation of CNC enforceability with wage, for high-tech jobs relative to nontech jobs, translated to reflect the effect of applying the average enforceability score to a nonenforcing state (a difference of four standard deviations) and the associated 95 percent confidence intervals. Wage is the log of quarterly wage at 4th, …, 32nd quarter of the job spell. Data are from the LEHD (1991–2008).

Next, in Table 5, we examine the relation between CNC enforceability and workers' cumulative wage and wage growth across job tenure. We measure wage growth as the difference between the log of monthly earnings at 4th, 8th, …, 32nd quarters of the job spell and the log of initial wage of the job. The results show that cumulative wages decline with CNC enforceability for tech workers, and the magnitudes of these declines increase over job tenure. For wage growth, the negative coefficient on CNC enforceability displays a U-shaped pattern. To provide context for the coefficient estimates, the results in Panel A suggest that applying the average enforceability score to a nonenforcing state would be associated with 4 percent lower cumulative earnings eight years into a job for the average tech worker relative to a nontech worker.

We next examine the duration of jobs. Table 6 presents the differential effect of CNC enforceability on mobility from estimating Equation 4. The column titles denote the dependent variables for each of the specifications. We find that an increase of one standard deviation in the enforceability score is associated with a 1.5 percent increase in the mean job-spell duration (Column 9). This is driven by rightward shifts in the job-spell distribution in higher enforceability states beginning in Year 2 (Column 2). An increase of one standard deviation in enforceability is associated with a 0.5 percentage points increase in the probability that a job spell lasts at least eight years (Column 8). Given that only 12.4 percent of all job spells last eight years, a one-standard-deviation increase in enforceability increases the likelihood that the job lasts

Table 5
CNCs and High-Tech Workers' Cumulative Wage and Wage Growth across Job Tenure (LEHD)

	Dependent Variable: Log of Cumulative Wage at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel A: Cumulative Wage								
Tech × CNC score	–0.0060***	–0.0072***	–0.0077***	–0.0079***	–0.0080***	–0.0084***	–0.0081***	–0.0094***
	(0.0008)	(0.0005)	(0.0006)	(0.0006)	(0.0007)	(0.0009)	(0.0012)	(0.0015)
Observations	10,904,000	7,397,000	5,399,000	4,048,000	3,145,000	2,479,000	1,858,000	1,413,000
R-squared	0.5902	0.6437	0.6708	0.6838	0.6891	0.6894	0.6887	0.6814
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All continuing jobs in the quarter							

	Dependent Variable: Log of Wage at xth Quarter – Log of Initial Wage							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel B: Wage Growth								
Tech × CNC score	–0.0054***	–0.0063***	–0.0065***	–0.0066***	–0.0057***	–0.0050***	–0.0057***	–0.0056***
	(0.0005)	(0.0006)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0012)	(0.0015)
Observations	10,904,000	7,397,000	5,399,000	4,048,000	3,145,000	2,479,000	1,858,000	1,413,000
R-squared	0.1455	0.1779	0.2047	0.2281	0.2504	0.2721	0.2946	0.3129
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All continuing jobs in the quarter							

Notes: This table reports the differential effect of CNC enforceability on cumulative wage and on wage growth from initial wage, across job tenure, by industry (high-tech jobs vs. nontech jobs). The dependent variables are the log of cumulative wage at 4th, 8th, …, 32nd quarter of the job spell for Panel A, and the difference between the log of monthly wages at 4th, 8th, …, 32nd quarter of the job spell and the log of initial wage for Panel B. CNC score is measured as the 2009 CNC enforceability index scores. Data are from the LEHD (1991–2008). All standard errors are clustered by state. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table 6
CNCs and High-Tech Workers' Job Duration (LEHD)

	Dependent Variable: Job-Spell Survival at								
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)	Ln(job-spell) (9)
Tech × CNC score	–0.0002	0.0033***	0.0040***	0.0046***	0.0051***	0.0057***	0.0046***	0.0052***	0.0152***
	(0.0008)	(0.0011)	(0.0009)	(0.0012)	(0.0009)	(0.0009)	(0.0008)	(0.0007)	(0.0027)
Observations	12,984,300	12,425,700	11,971,100	11,602,500	11,334,900	11,127,400	10,861,700	10,661,700	6,492,100
R-squared	0.2108	0.1741	0.1731	0.1768	0.1817	0.1836	0.1831	0.1885	0.2113
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]								
Sample	All jobs that are not right-censored by the quarter								Spell started 2000 or earlier

Notes: This table reports the differential effect of CNC enforceability on job duration by industry (high-tech jobs vs. nontech jobs). The dependent variables are dummy variables for the job spell surviving at 4th, …, 32nd quarter of the job spell for Columns 1–8, and the log of length of job spells in number of quarters for Column 9. CNC score is measured as the 2009 CNC enforceability index scores. Estimation samples are all jobs that are not right-censored by the quarter for Columns 1–8 and all jobs that started its spell in year 2000 or earlier for Column 9. Data are from the LEHD (1991–2008). All standard errors are clustered by state. *$p<0.10$, **$p<0.05$, ***$p<0.01$.

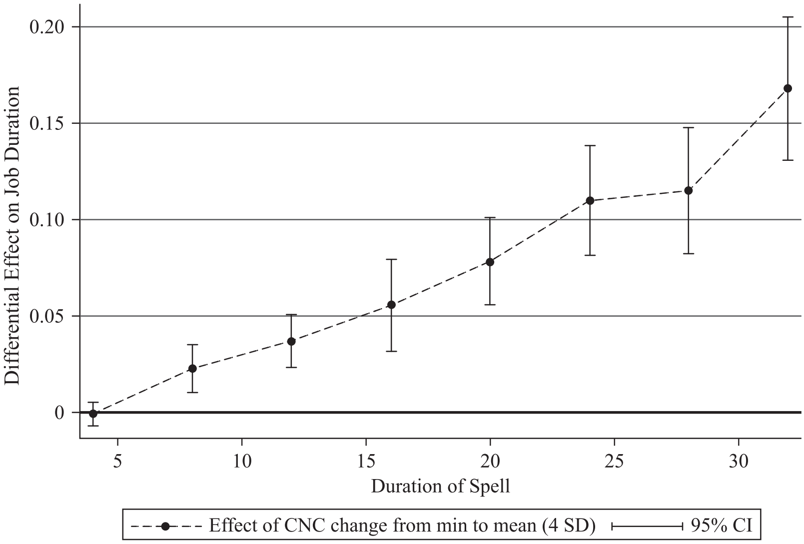

Figure 5

CNCs and High-Tech Workers' Job Duration across Job Tenure (LEHD)

Notes: This figure plots the coefficient estimates and the 95 percent confidence intervals of the differential relation of CNC enforceability with job duration, for high-tech jobs relative to nontech jobs (from Table 6), translated to reflect the effect of applying the average enforceability score to a nonenforcing state (a difference of four standard deviations), normalized by the mean probability of surviving to the end of that quarter. For example, the point estimate of 0.0.0052 for the 32nd quarter (column 8 of Table 6) translated for the effect of a four standard deviation change yields $4 \times 0.0052 = 0.0208$; this is normalized by the mean probability of surviving to Quarter 32 of 0.124 (from Online Appendix Table OA2) to yield the plotted point of $0.0208/0.124 = 0.1677$. Job duration is measured as the dummy variable for the spell surviving at 4th, …, 32nd quarter of the job spell. Data are from the QWI, 2013Q2–2017Q1.

at least eight years by 4 percent (0.5/12.4).[34] Applying the average enforceability score to a nonenforcing state would imply a 6 percent increase in the mean job-spell length (similar to the 8 percent observed in Marx, Strumsky, and Fleming 2009) and a 16 percent increase in the likelihood that jobs last at least eight years. Figure 5 presents a graphic illustration of the coefficient estimates and the 95 percent confidence intervals in Table 6, translated to reflect the effect of applying the average enforceability score to a nonenforcing state (a difference of four standard deviations) relative to the mean probability of surviving to that quarter. The increase in the effect of CNC enforceability on the relative mobility of tech workers over the tenure profile is consistent with employees gaining more intellectual capital and, hence, being more strongly targeted by firms for CNC enforcement.

Together, the results in this section corroborate the baseline patterns observed in the Hawaii analysis: CNC enforceability is associated with longer job spells and with lower wages both initially and throughout the job spells.

34. Summary statistics for all dependent variables are presented in Online Appendix Table OA2; the population mean for the dummy indicator of job spell surviving more than 32 quarters is 0.124.

E. Career Outcomes across Employment History

In this section, we exploit the richness of the LEHD data to extend the frame of analysis beyond single job spells (as in Section III.D above) to examine if and how the CNC enforceability level in a state where a worker started their initial job impacts cumulative earnings and job transitions across the span of the worker's career.

In Table 7, for cumulative earnings (Panel A) and mobility (Panel B), we observe persistent differentials associated with CNC enforceability. In Panel A, we find a gradually increasing and then decreasing wage suppression with CNC enforceability across employment history, such that eight years after starting a job in an average enforceability state a worker has on average 4.6 percent lower cumulative earnings relative to an equivalent worker who started a job at the same time in a nonenforcing state. For mobility, we find that the magnitude of the decline is gradually increasing across employment history, such that a one-standard-deviation increase in enforceability is associated with a 2.1 percent decrease in the number of jobs after eight years, which translates to an 8.2 percent differential (or about 0.22 additional jobs as mean log cumulative jobs after eight years is 0.939), when comparing an average enforceability state to a nonenforceability state.

One notable distinction between the cumulative earnings regressions at the career level (Table 7) versus at the job level (Table 5) is that the latter are conditional on the employee *staying in the same job* at the tenure examined (for example, the end of Quarter 24 analysis in Table 5 is conditional on workers staying in the job until Quarter 24). The mobility results (Table 6) suggest that high-tech workers in higher enforceability states have longer job spells than nontech workers. Thus, the estimated coefficients for the job-level wage regressions could be affected by a composition effect, with the direction of the effect depending on whether the workers that quit in low-enforceability states would have had higher or lower earnings had they stayed on than the average for those who did not quit. To the extent that the more productive workers are more likely to find job opportunities, the coefficients in the job-level analyses would be biased toward zero. The larger magnitudes of the coefficients in Panel A of Table 7 compared with those in Table 5 suggest this is indeed the case.

F. State- and Industry-Switching Behavior across Employment History

If variation in CNC enforceability were indeed material as our previous results suggest, one way to circumvent CNC enforceability would be to transfer to jobs outside the geographic scope of the CNCs (for example, the state) or to jobs in other industries. In this subsection, we examine the total number of state switches, industry switches, state but not industry switches, and industry but not state switches across the workers' employment histories. The analysis is conducted at the worker level, similar to the analysis in Section III.E, and we use the same specifications, except we replace the dependent variables with log (1 + cumulative number of state switches), log (1 + cumulative number of industry switches), and log (1 + cumulative number of state-but-not-industry-switches), and log (1 + cumulative number of industry-but-not-state-switches). We define state and industry switches as changes in state and the three-digit NAICS code of the worker's employer, respectively.

In Table 8, we observe a greater frequency of state switches for high-tech workers with initial employment in a high-enforceability jurisdiction, compared with nontech workers (Panel A). In contrast, greater enforceability is associated with a negative differential effect on the number of industry switches for workers in high-tech industries across their

Table 7
CNCs and High-Tech Workers' Career Outcomes across Employment History (LEHD)

	Dependent Variable: Log of Cumulative Earnings at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel A: Cumulative Earnings across Employment History								
Tech × CNC score	–0.0112***	–0.0118***	–0.0123***	–0.0128***	–0.0126***	–0.0125***	–0.0121***	–0.0115***
	(0.0028)	(0.0025)	(0.0022)	(0.0020)	(0.0017)	(0.0015)	(0.0012)	(0.0012)
Observations	7,517,000	6,389,000	5,594,000	4,973,000	4,485,000	4,057,000	3,671,000	3,229,000
R-squared	0.6245	0.6121	0.5951	0.5778	0.5603	0.5448	0.5291	0.5143
	Dependent Variable: Log of Cumulative Number of Jobs at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel B: Number of Jobs across Employment History								
Tech × CNC score	–0.0085	–0.0121*	–0.0142**	–0.0136*	–0.0156**	–0.0185**	–0.0197**	–0.0215**
	(0.0057)	(0.0062)	(0.0065)	(0.0073)	(0.0076)	(0.0074)	(0.0080)	(0.0079)
Observations	7,517,000	6,389,000	5,594,000	4,973,000	4,485,000	4,057,000	3,671,000	3,229,000
R-squared	0.3325	0.2892	0.2626	0.2477	0.2368	0.2330	0.2332	0.2352
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All employed workers in the quarter							

Notes: This table reports the differential effect of CNC enforceability on cumulative number of jobs taken across workers' employment history in Panel B and on cumulative earnings across workers' employment history in Panel A by industry (high-tech jobs vs. nontech jobs) of the worker's first job. The dependent variables are the log of cumulative number of jobs taken at 4th, …, 32nd quarter of the workers' employment history in Panel B and the log of cumulative earnings at 4th, …, 32nd quarter of the workers' employment history in Panel A. The high-tech job dummy variable is that of the first job of the worker. CNC score is measured as the 2009 CNC enforceability index scores of the state where the first job of the worker is geographically located. The job-level fixed effects controls for the job characteristics of the first job of the worker. Data are from the LEHD (1991–2008). All standard errors are clustered by state. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

employment history (Panel B). Panel C and Panel D show that what is driving these contrasting results is that greater enforceability is associated with workers switching states but not industries.[35]

These results suggest that while tech workers in high-enforceability states are more likely to switch states to avoid enforcement, they appear to have greater industry-specific human capital, so they are more likely to stay within the industry when they change jobs. This is consistent with greater investment in industry-specific human capital (or endogenous location of activities requiring industry-specific human capital) by firms in high-CNC locations (Marx 2011; Starr, Ganco, and Campbell 2018). Taken together with the baseline results of the significantly lower frequency of job changes by tech workers in high-enforceability jurisdictions, these results suggest that CNC enforceability places noticeable constraints on the frequency and direction of worker mobility across jobs.

G. Robustness Checks

We perform several checks to assess the robustness of the aforementioned results.

1. Triple difference between high and low-wage jobs within tech

As discussed in Section III.C.1, a key assumption (A2) in our baseline pseudo DID approach is that omitted state-level variables, unlike CNC enforceability, do not differentially impact tech-sector workers. We relax this assumption by making a narrower assumption that the mobility and wages of only the highest paid tech workers are differentially impacted by CNC enforceability. This affords a triple difference DDD approach that allows for state-by-industry fixed effects, where we compare differences in impacts for the highest paid (top 2 percent) tech workers relative to other tech workers net of the analogous difference between the highest paid and other workers in nontech industries. Online Appendix A provides a detailed discussion of this analysis, and the results presented there confirm that there is indeed a differential lowering of mobility and wages for the highest paid subsegment of tech workers in locations with higher CNC enforceability, as expected.

2. Local labor market thickness

One potentially important concern is that unobserved local labor market thickness may be (incidentally) negatively correlated with enforceability and also correlated with greater wages and mobility (as thicker markets could imply greater competition among labor-demanding firms, as in Cahuc, Postel-Vinay, and Robin 2006). We repeat our main analysis of job-spell duration and wages, controlling for local labor market

35. We also examined how CNC enforceability affects the worker's decision to switch state (or industry) at the point of job transition. For this analysis, we estimate Equation 4 having the outcome variables as the binary choice of switching state (or industry). Each observation in the estimation sample is the worker–job–year–quarter observation *at the quarter of job transition*. Thus, the regressions estimate the differential effect of CNC enforceability on the probability of switching state (or industry), *conditioning on job transition* and controlling for the job characteristics of the pre-transition job. The results, reported in Online Appendix Table OA4, show that workers in high-tech industries are more likely to switch state but not industry, and they are less likely to switch industry but not state, at job transitions in high-CNC-enforceability states. This set of results is consistent with the results in Table 8.

Table 8
CNCs and High-Tech Workers' Switching States or Industries (LEHD)

	Dependent Variable: Ln(1 + Cumulative # of State Switch) at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel A: Switch States								
Tech × CNC score	0.0003*	0.0008***	0.0012***	0.0014***	0.0012***	0.0013***	0.0013***	0.0013**
	(0.0001)	(0.0003)	(0.0003)	(0.0004)	(0.0003)	(0.0004)	(0.0005)	(0.0006)
R-squared	0.0746	0.0774	0.0855	0.0926	0.0987	0.104	0.1085	0.1138
	Dependent Variable: Ln(1 + Cumulative # of Industry Switch) at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel B: Switch Industry								
Tech × CNC score	–0.0018***	–0.0044***	–0.0067***	–0.0094***	–0.0119***	–0.0135***	–0.0162***	–0.0186***
	(0.0006)	(0.0012)	(0.0021)	(0.0027)	(0.0033)	(0.0038)	(0.0038)	(0.0037)
R-squared	0.1305	0.1394	0.1502	0.158	0.1633	0.1674	0.1722	0.1749
Observations	7,517,000	6,389,000	5,594,000	4,973,000	4,485,000	4,057,000	3,671,000	3,229,000
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All employed workers in the quarter							

(*continued*)

Table 8 (*continued*)

	Dependent Variable: Ln(1 + Cumulative # of State Switch without Industry Switch) at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel C: Switch State but not Industry								
Tech × CNC score	0.0001***	0.0003***	0.0006***	0.0007***	0.0007***	0.0008***	0.0008***	0.0009***
	(0)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
R-squared	0.043	0.0525	0.0611	0.0685	0.074	0.0779	0.0814	0.0858
	Dependent Variable: Ln(1 + Cumulative # of Industry Switch without State Switch) at							
	4th Qtr. (1)	8th Qtr. (2)	12th Qtr. (3)	16th Qtr. (4)	20th Qtr. (5)	24th Qtr. (6)	28th Qtr. (7)	32nd Qtr. (8)
Panel D: Switch Industry but not State								
Tech × CNC score	–0.0020**	–0.0050***	–0.0074***	–0.0101***	–0.0125***	–0.0141***	–0.0168***	–0.0193***
	(0.0007)	(0.0014)	(0.0022)	(0.0028)	(0.0033)	(0.0037)	(0.0038)	(0.0038)
R-squared	0.143	0.148	0.156	0.162	0.166	0.170	0.174	0.177
Observations	7,517,000	6,389,000	5,594,000	4,973,000	4,485,000	4,057,000	3,671,000	3,229,000
Fixed effects	State + [Industry – Starting Year – Firm Size – Starting Wage – Starting Age – Sex]							
Sample	All employed workers in the quarter							

Notes: This table reports the differential effect of CNC enforceability on cumulative number of state switches in Panel A, on cumulative number of industry switches in Panel B, on cumulative number of state-but-not-industry switches in Panel C, and on cumulative number of industry-but-not-state switches in Panel D, across workers' employment history, by industry (high-tech jobs vs. nontech jobs) of the first job. The dependent variables are log (1 + cumulative number of state switches) in Panel A, log (1 + cumulative number of three-digit NAICS code switches) in Panel B, log (1 + cumulative number of state-but-not-industry-switches) in Panel C, and log (1 + cumulative number of industry-but-not-state-switches) in Panel D, at 4th, …, 32nd quarter of the workers' employment history. CNC score is measured as the 2009 CNC enforceability index scores of the state in which the first job of the worker is geographically located in. The job-level fixed effects controls for the job characteristics of the first job of the worker. Data are from the LEHD (1991–2008). All standard errors are clustered by state. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

thickness proxied using total employment in state/three-digit NAICS code/year (in logs). The results (available in the Online Appendix Table OA3) are remarkably similar to the baseline results.

3. *Using CNC ranks instead of raw score*

Another potential concern is that California's large economy and near-complete CNC nonenforceability (an outlier among the CNC scores used in the baseline analysis) could be inordinately influencing the results. We therefore repeat our analyses in Section III.D, using ranks of the 2009 CNC enforceability index scores, which is free from extreme values by construction. The 2009 CNC enforceability index score ranks are assigned integer values of 1 to 50 across the 50 state values and are standardized (to have mean zero and standard deviation one). Larger values correspond to stronger CNC enforceability, and smaller values correspond to weaker CNC enforceability. In results presented in the Online Appendix (Table OA6 and Table OA7), we find that estimates using CNC ranks are very similar to those using CNC scores in terms of statistical significance and signs, and the magnitudes are similar as well (for a hypothetical change moving from California to Florida).

4. *Balance of enforceability measures across missing and available states*

Because the LEHD data we had access to does not cover all 50 U.S. states, there could be a bias as a result of workers' relocations to missing states. For example, when a worker is transferred to an establishment of the same firm that is located in a non-LEHD state, we lose track of the worker, potentially yielding a right-censoring in the spell measure. If firms relocate workers from low-enforceability states to high-enforceability states to protect their knowledge, and if the missing states have higher levels of CNC enforceability, there would be a positive bias in the estimated effect. However, a t-test for difference in the enforceability index scores between the states included in the LEHD and the states not included in the LEHD yields a p-value of 0.83, suggesting there is no significant difference in mean CNC enforceability scores across states in and out of the LEHD sample, which alleviates such a concern.

5. *Robustness of job-spell analysis to using alternative samples*

Finally, due to right-censoring, we restricted the sample to jobs that started in the year 2000 or earlier for our analysis of job-spell lengths. In an unreported analysis, we find that the results for the log of job-spells analysis are robust to the sample of *non-right-censored jobs* that started in the year 2000 or earlier. We also repeat the job survival analysis and the wage analysis on the sample of jobs that started in year 2000 or earlier and find that the results are robust. The estimation results are available upon request.

IV. Discussion

This study uses a recent natural experiment in Hawaii and detailed matched employer–employee data from the U.S. Census Bureau to examine how CNC enforceability affects wages and employee mobility for workers in the technology sector. The longitudinal analysis of the recent ban on CNCs for tech workers in Hawaii suggests that the ban increased wages at hiring for tech workers, as well as mobility. In the cross-sectional analysis, higher CNC enforceability is associated with longer job spells, a greater likelihood of leaving the state, and a reduced propensity for cross-industry movement for tech workers. Most importantly, we also find that compared with their peers in low-enforceability states, tech workers in states with high enforceability receive reduced wages throughout a given job as well as over their career.

Our analysis of the Hawaii policy change is the first study of the impact of a complete ban on CNCs. Our cross-sectional analysis using the LEHD is the first to use comprehensive employer–employee matched data to examine the relative effect of CNC enforceability on tech workers, and it fills two important gaps related to: (i) variations in the effect of CNC enforceability over a worker's career and (ii) joint state–industry switching behavior to circumvent CNCs. Regarding the latter, we find nuanced results for job switching: there is increased propensity to move across states but decreased propensity to switch across industries, explained primarily by tech workers who leave the state but stay in the same industry. These results are consistent with greater investment in industry-specific human capital (or endogenous location of activities requiring industry-specific human capital) by firms in high-enforceability locations (Marx 2011; Starr, Ganco, and Campbell 2018).

Our finding of lower mobility of tech workers from CNC enforceability (both from the analysis of the Hawaii noncompete ban and from the cross-sectional analysis using LEHD data) is consistent with prior studies of CNC enforceability and mobility, such as Marx, Strumsky, and Fleming (2009); Marx, Singh, and Fleming (2015); and Garmaise (2011). The two former studies find that inventor mobility was reduced and redirected out of state following Michigan's reversal of its policy not to enforce CNCs. Based on within-state changes in enforceability, Garmaise (2011) finds reduced intra-industry mobility among top executives. Relative to these studies, our examination of job spells covers a significantly larger and less-selected sample that tracks mobility for all tech workers with greater accuracy. In particular, our data avoid four limitations of mobility measurement using inventor patent filings (discussed in detail in Agarwal, Frake, and Ganco 2018).[36]

Over the employee job spell for tech workers, we find that the potential impact of CNC enforceability on the duration of the job spell is lowest at short tenures, but rises at longer tenures. These results are consistent with CNCs being enforced only after workers gain or learn significant appropriable intellectual capital, indicating that CNC enforceability has a smaller effect early in the job tenure.[37] The higher impact at mid-tenure is consistent with Lazear and Gibbs (2014, p. 82–85), suggesting that the value of

36. One, a small percentage of firms patent (see for example, Balasubramanian and Sivadasan 2011), and within them only a small percentage of technical workers file patents. Two, even among patent filing workers, mobility can be tracked only for those workers that file multiple patents. Three, even for a worker with multiple filings, any completed job spells between the two consequent patent filings are missed. Four, even when there are no intervening spells, the timing of moves is difficult to pin down when the patenting is infrequent. We thank a reviewer for highlighting and suggesting inclusion of these points.

37. Further, in a Jovanovic-type learning–matching model, initial separations may be reflecting lack of fit between the worker and the firm, and such separations may in fact be mutually beneficial and therefore unrestricted by the firm, even when a CNC is enforceable.

a worker to a firm is highest for mid-career workers, making it more likely that firms enforce CNCs on such workers.

Lower mobility alone does not necessarily imply a negative effect on workers. Workers may trade off mobility in return for higher wages resulting from increased firm investments in their human capital. For instance, Lavetti, Simon, and White (2020) find that physicians who sign CNCs have higher earnings and earnings growth. In contrast, we find that stricter CNC *enforceability* is associated with lower wages, both initially and throughout the tech worker's tenure.[38] In this respect, our results are similar to those in Garmaise (2011) and Starr (2019). The wage analysis in Hawaii finds similarly sized short-run increase in wages for hires following the ban on CNCs for tech workers.

Our last finding examining outcomes over the career of a tech worker suggests that starting a job in an average enforceability state—regardless of whether the individual eventually leaves their initial job or state—is associated with reduced earnings of 4.6 percent eight years after starting the job compared to a tech worker in a nonenforcing state. This finding is important because, contrary to models in which early training or information sharing investments will increase productivity and thus worker pay in later periods (that is, Rubin and Shedd 1981), it suggests that the dominant effect of CNC enforceability for the average technical worker is to depress wages (relative to nontech workers).

Together, our results strongly suggest that CNC enforceability lowers worker welfare, consistent with CNC enforceability reducing workers' bargaining power relative to the firm and "locking" them into their jobs, as argued by Arnow-Richman (2001, 2006), and consistent with the lack of negotiation over CNCs observed in Starr, Prescott, and Bishara (2021). Our results are thus consistent with CNC enforceability creating monopsony power, leading to deviations from the law of one wage, reducing the elasticity of labor supply (Bhasker, Manning, and To 2002; Manning 2011; Ashenfelter, Farber, and Ransom 2010), and dampening labor market dynamism (Furman and Krueger 2016).

In line with Krueger's (2017) concerns about CNCs facilitating collusion quoted at the beginning of the paper, our findings also highlight a potential similarity between labor market collusion and the enforceability of CNCs. The "gentleman's agreements" signed by Apple, Google, and many other tech companies in California to not recruit each other's employees served to reduce both wage competition and mobility between competitors (Helft 2009).[39] Mukherjee and Vasconcelos (2012) model these agreements as alternate mechanisms for extracting surplus from productive workers, and our findings suggest that the outcomes for tech workers from high CNC enforceability may be similar to those due to labor market collusion under oligopsony (Krueger and Posner 2018).

38. A potential reason for the difference is that we examine effects of enforceability that impacts the institutional setting and hence the bargaining power of workers relative to employers, which is distinct from the potential effects of endogenous decisions by individual workers to sign CNC agreements. Workers may be able to negotiate higher wages and wage raises in return for signing a CNC agreement (Black and Loewenstein 1991).

39. In his deposition during the Department of Justice investigation into the Silicon Valley gentleman's agreements, George Lucas said "[We] could not get into a bidding war with other companies because we don't have the margins for that sort of thing."

Appendix

Appendix Table 1
QWI Data—Average Total Number of Employees, Hires, and Separations per Quarter

		Cross-Industry, Within-Hawaii			Cross-State, Within-Tech		
		Nontech (1)	Tech (2)	Total (3)	Non-HI (4)	HI (5)	Total (6)
Beginning of quarter employment	Pre-July 2015 ban	494,614	5,470	500,084	2,985,438	5,470	2,990,909
	Post-July 2015 ban	514,361	5,574	519,935	3,194,901	5,574	3,200,475
New hires	Pre-July 2015 ban	67,351	508	67,859	293,362	508	293,869
	Post-July 2015 ban	75,997	550	76,547	313,636	550	314,187
Separations (all)	Pre-July 2015 ban	74,218	553	74,770	288,647	553	289,200
	Post-July 2015 ban	74,073	569	74,642	281,760	569	282,329

Notes: This table presents the average of the total number of beginning of quarter employment, new hires, and separations in the pre- and post-ban period in the QWI data (2013Q2–2017Q1). For example, Column 2, Row 1 indicates that there were on average 5,470 employees in "tech" industries in Hawaii in the beginning of the quarter, in the period 2013Q2–2015Q2, where "Tech" is defined as QWI four-digit industry classifications that cover software design, development, and services, to concord with the definition of "technology business" in the Hawaii statute.

Appendix Table 2
Construction of Noncompete Enforceability Index

Question	Scoring	Bishara Weight	Starr Weight
Statute of enforceability: “Question 1: Is there a state statute of general application that governs the enforceability of covenants not to compete?”	Score of 10 to a state that has a statute that favors strong enforcement, 5 to a state that either did not have a statute or had a statute that was neutral in its approach to enforcement and 0 was given to a state that has a statute that disfavors enforcement.	0.10	0.09
Protectable interest: “Question 2: What is an employer’s protectable interest and how is that defined?”	Score of 10 to a state that has a broadly defined protectable interest, 5 to a state that has a balanced approach to defining a protectable interest, and 0 to a state that has a strictly defined limited protectable interest for the employer.	0.10	0.12
Plaintiff’s burden of proof: “Question 3: What must plaintiff be able to show to prove the existence of an enforceable covenant not to compete?”	Score of 10 to a state that places a weak burden of proof on the plaintiff employer, 5 to a state that has a balanced approach to the burden placed on the employer, and 0 to a state that places a strong burden of proof on the employer.	0.10	0.10
Consideration at inception: “Question 3a: Does the signing of a covenant not to compete at the inception of the employment relationship provide sufficient consideration to support the covenant?”	Score of 10 to a state where the start of employment is always sufficient to support a covenant not to compete, around 5 to a state where the start of employment is sometimes sufficient, and 0 to a state where the start of employment is never sufficient.	0.05	0.13
Consideration post-inception: “Question 3b & 3c: Will a change in the terms and conditions of employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun? Will continued employment provide sufficient consideration to support a covenant not to compete entered into after the employment relationship has begun?”	Score of 10 or near 10 to a state where continued employment is always sufficient to support a covenant not to compete, around 5 to a state where only a beneficial change in terms was sufficient, and 0 to a state where neither continued employment nor a beneficial change in terms would be sufficient consideration.	0.05	0.08

(*continued*)

Appendix Table 2 (*continued*)

Question	Scoring	Bishara Weight	Starr Weight
Overbroad contracts: "Question 4: If the restrictions in the covenant not to compete are unenforceable because they are overbroad, are the courts permitted to modify the covenant to make the restrictions more narrow and to make the covenant enforceable? If so, under what circumstances will the courts allow reduction and what form of reduction will the courts permit?"	Score of 10 to a state where judicial modification is allowed and there are broad circumstances where revision can be made and limited restrictions on maximum enforcement, score of 5 to a state where so-called "blue pencil" modifications were allowed as a way to reform the contract instead of disallowing it outright, showing a balanced approach to the allowable scope of restrictions and to accommodating the plaintiff's enforcement request, and low score, possibly 0, awarded to a state where neither "blue pencil" nor judicial modification was allowed.	0.05	0.04
Quit vs. Fire: "Question 8: If the employer terminates the employment relationship, is the covenant enforceable?"	Score of 10 to a state where a covenant is always enforceable if the employer terminates, 5 to a state where a covenant is enforceable only in some circumstances, and 0 where a covenant is not enforceable if the employer terminates.	0.10	0.09

Notes: The Starr (2019) index used in this paper is constructed using factor analysis to reweight the seven dimensions of CNC enforceability initially scored in Bishara (2011). Bishara selected seven questions from periodic, comprehensive, state-by-state surveys of noncompete enforcement policies undertaken by Brian Malsberger.[40] Bishara notes that these questions "were chosen because they directly address the legal issues relevant to measuring a given jurisdiction's intensity of noncompete enforcement" and adds that "these questions, in the aggregate, can flesh out a full picture of a state's policy on noncompetes, including if the state has contemplated its policy to the extent that it has enacted legislation on the topic." The seven questions and how they were treated for the construction of the CNC index by Bishara (2011) are described in the table below; raw scores on each of the seven dimensions for each of states are provided in Online Appendix Table OA12. See Bishara (2011) for a detailed discussion of the scoring.

40. The then current edition used by Bishara (2011) was Brian M. Malsberger, ed., *Covenants Not To Compete, A State-By-State Survey* (2008 and cum. suppl. 2009).

Appendix Table 3
Description of Difference between High vs. Low Enforceability States

Percentile of CNC Enforceability Index (2009)	Full Sample of 50 States (Ordered by Rank of Weakest to Strongest Enforceability within and across Categories)	LEHD Sample of 30 States (Ordered by Rank of Weakest to Strongest Enforceability within and across Categories)	Illustrative Samples of Enforceability Policies (from Bishara 2011 and Other Sources Indicated)	Mean of Stuart and Sorenson (2003) Dummy for Low Enforceability (LEHD Sample)
Bottom quintile (low enforceability)	North Dakota, California, New York, Alaska, Oklahoma, West Virginia, Montana, Arkansas, Rhode Island, Virginia	California, Oklahoma, West Virginia, Montana, Arkansas, Rhode Island, Virginia	California (and North Dakota) have anti-noncompete enforcement statutes without exceptions for any post-employment restrictions.	0.57
Middle 60% (moderate enforceability)	Texas, South Carolina, Hawaii, Nebraska, Wisconsin, Minnesota, Georgia, Nevada, Mississippi, Ohio, Oregon, Pennsylvania, Arizona, North Carolina, Wyoming, New Hampshire, Washington, Alabama, Colorado, Maine, Tennessee, Michigan, Massachusetts, Louisiana, Delaware, Maryland, Vermont, Indiana, New Mexico, Idaho	Texas, South Carolina, Hawaii, Wisconsin, Georgia, Nevada, Oregon, North Carolina, Washington, Colorado, Maine, Tennessee, Louisiana, Maryland, Vermont, Indiana, New Mexico, Idaho	Most have some legislation discussing noncompetes. For example, Colorado allows labor contracts that require an employee to repay training costs for employment that lasts less than two years, and recognizes noncompetes specifically for "e]xecutive and management personnel and officers and employees who constitute professional staff to executive and management personnel."	0.18

(*continued*)

Appendix Table 3 (*continued*)

Percentile of CNC Enforceability Index (2009)	Full Sample of 50 States (Ordered by Rank of Weakest to Strongest Enforceability within and across Categories)	LEHD Sample of 30 States (Ordered by Rank of Weakest to Strongest Enforceability within and across Categories)	Illustrative Samples of Enforceability Policies (from Bishara 2011 and Other Sources Indicated)	Mean of Stuart and Sorenson (2003) Dummy for Low Enforceability (LEHD Sample)
Top quintile (high enforceability)	Kentucky, New Jersey, Illinois, Utah, Iowa, South Dakota, Missouri, Kansas, Connecticut, Florida	New Jersey, Illinois, Utah, Iowa, Florida	Florida's noncompete law is viewed as strongly pro-employer; key provisions include one that prevents consideration of harm to the employee and another that encourages courts not to construe a restrictive covenant narrowly.	0

Notes: Stuart and Sorenson (2003) use a dummy indicator for whether the state has a statute that "precludes or severely limits" an employer's ability to enforce CNCs (drawing on the Malsberger treatise edition of 1996).

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