Three Essays in Employee Mobility

by

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TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

LIST OF APPENDICES

APPENDIX

ABSTRACT

This dissertation studies employer and employee outcomes that are related to employee mobility. In particular, it explores the consequences of employee mobility on employer performance, and how constraints on employee mobility can affect employee welfare. The first two chapters investigate how quitting, and firing, of employees are associated with the employer's future growth. The third chapter examines how mobility constraints in the form of the enforceability of covenants-not-to-compete (CNCs) affect employee mobility and wages.

In the first chapter, I examine how the effect of worker quits on future establishment growth varies by the quitting worker's productivity level. I argue that when there are labor market frictions related to search and training, the difficulty of replacing a worker with another of equivalent productivity increases with the productivity level of the quitting worker, and this in turn leads to greater detrimental effect of quits by higher productivity workers. I provide novel empirical evidence that quits of high productivity workers lower future growth, whereas quits of low productivity workers do not. In particular, a one standard deviation increase in quit rate of high productivity workers is associated with a 1.2 percentage point decline in future establishment employment growth rate. The detrimental effect of high productivity worker quits is stronger for establishments with higher replacement costs: establishments that are more likely to have unfilled vacancies, that are more likely to cover the training costs of their employees, that have a more knowledge-intensive workforce, that have more complex operations, and that have worker representatives participating in hiring decisions. I exploit the richness of the German employer-employee linked data to distinguish quitting workers from fired workers, and construct

instrumental variables to alleviate potential endogeneity concerns. This study contributes to the literature by exploring the role of replacement-related frictions in mediating the relation between worker quits and establishment performance.

The second chapter examines how firing as a management practice can enhance worker-firm idiosyncratic match quality, and thereby foster growth of small firms. I view match quality as a component of a worker's productivity contribution to the firm, determined by the fit between the worker and the firm. Utilizing German employer-employee linked data, I propose a measure of match quality and examine how firing, match quality, and growth are related at the firm-level. I hypothesize that firms can enhance its match quality with its workforce by trying out different workers, and find that firing is positively associated with enhanced match quality and stronger future growth. This relation is stronger for firing workers with fewer number of past jobs.

In the third chapter, we examine the relationship between the enforceability of covenants-not-to-compete (CNCs), and employee mobility and wages. Using matched employer-employee data, we find that workers starting a job in an average-enforceability state experience longer job spells and lower wages such that after 8 years they have about 8% fewer jobs and 5% lower cumulative earnings relative to equivalent workers in a non-enforcing state. We then examine the 2015 CNC ban for tech workers in Hawaii and find that this ban increased mobility by 11% and new-hire wages by 4%. These results are consistent with CNC enforceability increasing monopsony power.

CHAPTER I

When Does Quitting Matter? Evidence on Voluntary Worker Turnover and Establishment Growth from Employer-Employee Linked Data

1. Introduction

Human capital is a vital component of firm productivity. A distinguishing feature of human capital is that it is tied to the worker; when workers move, productivity embedded in their human capital moves with them.

Because human capital is tied to the worker, worker quits result in human capital loss to the source firm and thereby can hurt the firm's performance, unless replacement workers of equivalent productivity can be found relatively quickly. Specifically, a firm's potential for growth can be linked to human capital of the firm, just as national economic growth has been shown to be linked to national levels of human capital (Mincer 1984, Barro 1991, Jones 2003).

In this paper, I contribute to the literature by studying how human capital loss due to voluntary worker turnover (i.e., worker quits) affects the source establishment's future growth. In particular, I examine how the impact of worker quits are related to costs in replacing a quitting worker, by analyzing how the impact of worker quits varies by worker characteristics and establishment characteristics that are likely to be correlated with worker replacement costs that rise from labor market frictions.[1] I find that quits of high productivity workers (which I argue below to incur higher replacement costs than quits of low productivity workers) have detrimental effects on the establishment's future growth, while the quits of low productivity workers do not. Furthermore, I find stronger detrimental effects of high productivity worker quits for establishments with characteristics indicating higher replacement costs.

I present these novel empirical findings by using the German employer-employee linked data (hereafter LIAB) that allows me to overcome two major empirical challenges in tackling this research question. First, the richness of the LIAB allows me to address the concern of potential negative bias arising from endogenous quits: i.e., poor establishment quality may be positively correlated with worker quits and negatively correlated with future establishment growth. Using rich establishment-level information on size, pay level, and past growth, I non-parametrically control for a number of measures of observed establishment quality. Moreover, the LIAB allows me to track the worker's job transition with detailed information on the establishment's geography, industry, and on the worker's occupation. I exploit this rich job- (i.e., combination of worker-establishment) level information to construct establishment-level instruments for worker quits that allows me to address potential bias from unobserved establishment quality. In particular, I construct a measure of local labor market worker inflows, by adding up the worker inflows to establishments that are in the same local labor market as the focal establishment, *excluding worker inflows to and from the focal establishment*. This instrument captures the shift in local labor market demand that "pulls" the workers to different jobs, independent of the source establishment's quality or future prospects. The instrument's

[1] Although not explicitly discussed in the framework of labor market frictions, costs of employee turnover, which are conceptually similar to the difficulty in replacing a worker of this paper, has been discussed in Ton and Huckman (2008). The authors discuss the costs of recruitment and training of new employees, and operational disruption, as sources of the negative impact of employee turnover.

statistical power is drawn from the local labor market being defined at a highly disaggregated industry-occupation-geography level, and by exploiting variation in occupation shares across establishments within the same industry and geographical location.[2]

Second, worker quits, which arise from a worker's choice to leave the establishment, could have very different effects on establishment growth, compared to the establishment's choice to fire workers. In particular, establishments may fire poor quality workers (e.g., in terms of match quality or ability) to enhance performance. However, the two different types of worker turnover are very hard, if not impossible, to distinguish in typically available datasets.[3] I am able to separate out voluntary worker turnover (i.e., worker quits) from involuntary worker turnover (i.e., worker firing) at the job-level, utilizing the German Unemployment Insurance (UI) system that imposes sanction periods on receiving unemployment benefits when the separation was voluntary (i.e., the worker's quit). The LIAB allows me to precisely observe the day of the separation and the first day of receiving benefits, so that I can detect the sanction period imposed on worker quits. Indeed, my results (reported in Table I.1 and Appendix 1 Table A1.2) confirm the importance of distinguishing the two types of worker turnover - the coefficient estimates (for the effect of worker turnover on future establishment growth) are negative and statistically significant for high productivity workers' quits, while those for fired workers are smaller and statistically insignificant. This suggests that misclassification of firing into quits can lead to a positive bias when examining the effect of worker quits on growth.

[2] Thus, if establishment A has a different occupation structure from establishment B (which is in the same industry and geographical location), and occupations with greater share of employment in firm A have experienced growth in the local labor market, we can expect greater voluntary worker outflows from establishment A relative to establishment B. The instrument picks up this "external demand pull" variation.

[3] Distinguishing the types of worker turnover is often not feasible even with other employer-employee linked datasets, e.g., the LEHD at the US Census Bureau.

As noted earlier, the establishment would not be negatively impacted by worker quits if it can easily and quickly find replacement workers with similar productivity. That is, worker quits are costly to the establishment when labor market frictions, or establishment-specificity in the workers' human capital, make searching for and training a replacement worker costly (Manning, 2011).[4] Consistent with this expectation, replacement costs are prominently highlighted by practitioners in the media and non-academic articles about worker turnover.[5]

I argue that replacement costs – referring to costs incurred in the process of finding and training replacement workers to recover the productivity loss from worker quits – can be captured by specific worker and establishment characteristics. In particular, at the job-level (i.e., worker-establishment combination), workers' marginal product (measured by their wage) is likely to be positively correlated with replacement costs. Workers with higher marginal product, relative to others who are in the *same industry* and have the *same occupation*, are likely to be more difficult to replace for two reasons. First, the external labor market would be thinner for high productivity workers as they bear higher moving costs due to the higher establishment-specificity of their human capital (Jovanovic, 1979b; Coff, 1997).[6] Therefore, the search costs in finding the replacement worker to recover the lost productivity would be higher when high productivity workers quit. Second, a high productivity worker's replacement is likely to require

[4] The underlying premise of this paper is that the labor market is not perfectly competitive and labor market frictions exist, which induces replacement costs in the form of search and training costs, and lets firms enjoy economic rent from employment. In a frictionless perfectly competitive labor market, the worker's wage would be determined at her marginal revenue product and the firm could find a replacement for a quitting worker immediately at the labor market.

[5] For example, in a Harvard Business Review article, Daniels (2010) notes that "more employees quit their jobs than were terminated […] This is […] bad for individual businesses: when jobs become more plentiful, the first to exit are often the business's most ambitious employees […]. The cost of replacing an employee is estimated at up to 250 percent of annual salary." See also, Garland (2016).

[6] Summary statistics in Table A1.1 show that the worker quit rate at the establishment is smaller for high productivity workers than low productivity workers, consistent with the assertion that the labor market is thinner for high productivity workers.

higher training costs that the establishment bears, as high productivity workers are more likely to receive establishment-specific human capital investment. The higher search and training costs for replacing a high productivity worker implies that recovering the lost productivity due to worker quits is more difficult when high productivity workers quit.

I exploit the availability of detailed occupation codes in the LIAB (absent in other employer-employee linked datasets such as the US LEHD dataset), to measure the relative productivity of the worker by wage within industry-occupation cells, and use it as a proxy correlated with replacement costs at the job-level. Controlling for detailed occupation levels is vital because wages can vary significantly by occupation, so that relative wages within industry could simply reflect heterogeneity in occupations (e.g., across data entry operators and architects within the software industry).

My empirical findings are strongly consistent with the expectation that replacement costs significantly impact the relation between worker quits and establishment growth. I find novel evidence for an asymmetry in the effect of worker quits on the establishment's future growth by the leaving worker's productivity level. Quits of high productivity workers have detrimental effects on the establishment's future growth – a one standard deviation increase in quit rate of high productivity workers is associated with a 1.2 percentage point decline in future establishment employment growth rate – while quits of low productivity workers do not. This asymmetry holds consistently under various empirical specifications, including fixed effects estimation (that non-parametrically controls for establishment characteristics) and two-stage least squares estimation, and across various subsamples discussed below.

In particular, I expect this detrimental effect of high productivity worker quits to be stronger for establishments with high replacement costs. The establishment's replacement costs can be captured using establishment- or industry-level heterogeneity in a number of ways: (1) the industry-level likelihood of having unfilled vacant positions as a proxy for search difficulty, (2) the industry-level likelihood of the establishment covering their employees' training costs as a proxy for the establishment-specificity of the skills required (Becker, 1962), (3) the knowledge-intensity of the industry workforce, as search and training costs are expected to be higher for knowledge-workers, (4) industry-level complexity of operations, measured using the prevailing number of unique occupations at the establishment, that is likely to impact training costs, (5) the existence of works council, a unique institutional feature in Germany through which workers represents their interests to the management, that is likely to impact hiring costs.[7]

I find that quits of high productivity workers have a stronger detrimental effect for establishments in industries with higher likelihood of having unfilled vacancies, establishments in industries with higher likelihood of the establishment covering their employees' training costs, establishments in industries with higher proportion of knowledge-workers, establishments in industries with more complex operations, and for establishments with a works council. On the other hand, low productivity worker quits do not have detrimental effects, for both high and low replacement costs establishments. This set of results reinforces the importance of replacement costs in the relation between worker quits and establishment growth.

Studies in the strategic management literature have found interesting competition-related detrimental effects of worker turnover, focusing on transfers of knowledge or routines to rivals,

[7] The existence of works council is decided by the workers at the establishment, and therefore leads to potential endogeneity concerns. The empirical findings on the relation between works council presence and firm performance are mixed (Addison, Schnabel, Wagner, 2004). Addison, Bellmann, Schnabel, Wagner (2004) suggest that there is no statistically significant effect of works council on the establishment's employment growth. Using the LIAB data, they test the difference between establishments that newly introduced works council after the 2001 reform, and their matched establishments that did not change the works council status. In any case, because I am examining the moderating influence of works council on the effect of worker quits on establishment growth (by sub-sampling the data by the existence of works council), even a direct correlation between works council and establishment growth do not necessarily call into question my interpretation of the sub-sampling analysis.

facilitated by the moving workers (Campbell, Ganco, Franco, Agarwal (2012), Aime, Johnson, Ridge, and Hill (2010), Somaya, Williamson, Lorinkova (2008), and Wezel, Cattani, Pennings (2006)). Motivated by this extant literature, I examine whether the worker turnover effect through replacement costs may be larger if the worker turnover affects the source establishment's competitive position (e.g., by transferring valuable knowledge to a rival establishment). In particular, I compare the worker quit effects of moves to rival establishments to the worker quit effects of moves to non-rival establishments.[8] I find that while the magnitude of the detrimental effect is indeed somewhat larger for quits to rival establishments, the difference in the effects of the two types of quits are not statistically significant. Interestingly, I also find that the detrimental effect of quits to rival establishments is statistically significant only when the source establishment is characterized as having high replacement costs by the replacement costs proxies discussed above.

This paper contributes to the literature in several ways. First, this paper contributes to the broad literature of labor mobility and productivity, being the first paper to provide rich empirical evidence that supports the importance of replacement costs in explaining the consequences of worker turnover. Past studies in economics have examined how labor mobility is related to knowledge spillover and productivity gains (Greenstone, Hornbeck, Moretti (2010), Moretti (2004), Parrotta and Pozzoli (2012), Eriksson and Rodriguez-Pose (2017)). Replacement costs (adjustment costs) has been studied in the labor demand literature (Hamermesh (1989), Nickell (1986), Hamermesh (1995)), but its implications for firm performance has been under-explored. The strategic management literature has studied how transfer of human capital and managerial

[8] A similar approach was taken in Phillips (2002), in which the author tests how the overlap in the product market of the parent firm with that of the spinout firm is associated with the failure of the parent firm in the law firm industry. Spinouts are very sparsely sampled in the LIAB, and therefore are not studied in this paper.

routines in the event of worker moves can affect the performance of the source firm and the destination firm (e.g., Campbell, Ganco, Franco, Agarwal (2012), Groysberg, Lee, Nanda (2008)).[9]

This paper is the first to systematically examine how replacement costs that rise from labor market frictions modulate the impact of worker quits on establishment performance. My empirical findings strongly suggest that factors that increase the cost of replacing a worker, over and above the transfer of resources and knowledge that previous studies focused on, hurt the employer in the event of worker quits. As often noted in the practitioner's arguments, managers should be aware of the replacement costs to recover the lost productivity due to worker quits, and the findings of this paper pose labor market frictions as an essential aspect to consider in the management of human capital.

Second, this paper contributes to the empirical literature estimating the detrimental effect of worker turnover, by being one of the first to distinguish worker quits from worker firing, and by utilizing non-parametric controls and instruments to alleviate the concerns about potential endogeneity bias. Third, the economy-wide analysis allows me to draw generalizable implications of worker turnover across various sectors in the economy, and allows me to show how establishment and industry characteristics of the source establishment impact the worker turnover effect.

2. Framework and Empirical Predictions

2.1. Worker Quits, Loss in Productivity, and Establishment Growth

[9] Also see Aime, Johnson, Ridge, and Hill (2010), Somaya, Williamson, Lorinkova (2008), and Wezel, Cattani, Pennings (2006), Franco and Filson (2006), Agarwal, Echambadi, Franco, Sarkar (2004), Song, Almeida, Wu (2003), Rao and Drazin (2002). Relatedly, the strategic management literature has also studied what drives worker mobility (Ganco (2013), Campbell, Coff, Kryscynski (2012)).

I present a simple framework that illustrates how worker quits affect the establishment's human capital stock and establishment growth. The framework also motivates the empirical specification discussed in Section 3.

The idea of the framework is that worker quits decrease the human capital stock at the establishment, which cannot be recovered immediately due to labor market frictions, and therefore hurt the establishment's future growth. For instance, even if the establishment finds a replacing worker, it takes time and effort to learn the worker's type and train the worker, let alone the difficulty in hiring the replacing worker in the first place.

I assume that the establishment's production can be described by the following Cobb-Douglas production function:

$$Y = \theta \phi^{\gamma} E^{\alpha}$$

where Y is production output, θ is technology not related to worker human capital, ϕ is average human capital stock at the establishment, and E is employment size at the establishment.

The establishment has two types of workers: high productivity workers with marginal human capital ϕ_H, and low productivity workers with marginal human capital ϕ_L. Then the establishment's average human capital stock ϕ is given as the geometric mean of the workers' human capital:

$$\phi = \phi_H^{\frac{W_H}{W_H + W_L}} \phi_L^{\frac{W_L}{W_H + W_L}}$$

where W_H and W_L are generic weights that reflect the contribution of high human capital and low human capital to the establishment's overall human capital stock.[10]

[10] Note that average human capital stock at the establishment for high (low) productivity workers is equal to the marginal human capital of the high (low) productivity workers ϕ_H (ϕ_L), in a discrete setting as given.

The establishment's worker quit rate is τ; $0 < \tau < 1$. Note that E is independent from τ; the level of employment does not tell anything about how many workers leave (or join) the establishment. Worker quit rate for high productivity workers and low productivity workers are denoted as τ_H and τ_L, respectively. I assume that worker quits are exogenous with regards to the technology term θ. For instance, as discussed further in the empirical methodology section, workers do not quit because they think the establishment is a poor-quality establishment, but because they seek better employment opportunities at the external local labor market.[11]

Worker quits leads to decrease in human capital stock, as the worker's human capital is attached to the worker. I assume that the establishment's average human capital stock at time t (ϕ_t) is determined by past turnover rate τ_{t-1}, past average human capital stock ϕ_{t-1}, and cost in replacing the quitting worker, referred to as replacement costs.[12] Replacement costs (denoted as ρ; $0 < \rho < 1$) rise from labor market frictions that make searching for, and training, a replacement worker costly.

I assume that it is more difficult to replace high productivity workers than low productivity workers. In other words, replacement costs for high productivity workers (ρ_H) is higher than replacement costs for low productivity workers (ρ_L): $\rho_H > \rho_L$. The next subsection discusses this assumption in further detail. This leads to the following equations for average human capital stock, quit rate, and replacement costs[13]:

$$\phi_{H,t} = \phi_{H,t-1}\exp(-\rho_H \tau_{H,t-1})$$

[11] I also assume that, in this framework section, all worker turnover is quits, and abstract away from workers being fired.

[12] I abstract away from increase in human capital stock due to new hires in this framework.

[13] Note that the difference in ρ in this equation can also reflect the difference in the lost human capital itself between quits of high productivity workers and quits of low productivity workers: by definition, loss in human capital by unit rate of quits is larger for quits of high productivity workers.

$$\phi_{L,t} = \phi_{L,t-1}\exp(-\rho_L \tau_{L,t-1})$$

Note that in the absence of replacement costs ($\rho = 0$), then worker quits do not change the human capital stock at the establishment. Change in average high human capital stock and average low human capital stock is:

$$dln\phi_H = -\rho_H \tau_{H,t-1}$$

$$dln\phi_L = -\rho_L \tau_{L,t-1}$$

The establishment's profit is:

$$\pi = pY - wE$$

where w denotes average wage at the establishment, and p denotes average price of the establishment's production output. Then the optimal employment size by the first-order condition is:

$$lnE^* = \frac{\gamma}{1-\alpha}\left[ln\phi_H^{\frac{W_H}{W_H+W_L}}\phi_L^{\frac{W_L}{W_H+W_L}}\right] + \frac{1}{1-\alpha}[ln\theta + lnp - lnw + ln\alpha]$$

Differentiating the above equation,

$$dlnE^* = \frac{\gamma}{1-\alpha}\left[\frac{W_H}{W_H+W_L}dln\phi_H + \frac{W_L}{W_H+W_L}dln\phi_L\right] + \frac{1}{1-\alpha}d[ln\theta + lnp - lnw + ln\alpha]$$

$$= \frac{\gamma}{1-\alpha}\left[\frac{W_H}{W_H+W_L}(-\rho_H\tau_{H,t-1}) + \frac{W_L}{W_H+W_L}(-\rho_L\tau_{L,t-1})\right] + \frac{1}{1-\alpha}d[ln\theta + lnp - lnw + ln\alpha]$$

$$= \beta_H(-\rho_H\tau_{H,t-1}) + \beta_L(-\rho_L\tau_{L,t-1}) + X\Gamma$$

where $\beta_H = -\frac{\gamma}{1-\alpha}\frac{W_H}{W_H+W_L} < 0$, $\beta_L = \frac{\gamma}{1-\alpha}\frac{W_L}{W_H+W_L} < 0$ and X denotes a vector of establishment technology, output price, input price, and output elasticity of labor. Thus, the marginal effect of worker quits on establishment growth is:

$$\frac{\partial growth}{\partial \tau_{H,t-1}} = \beta_H\rho_H < 0;\ \frac{\partial growth}{\partial \tau_{L,t-1}} = \beta_L\rho_L < 0$$

Several observations can be made from the above expressions. First, worker quits have detrimental effects on establishment growth, and the detrimental effect increases with replacement costs. If the labor market is without friction and the establishment can immediately replace a quitting worker (i.e., $\rho = 0$), the establishment's human capital stock is unaffected by the worker's quit, and therefore worker quits do not affect the establishment's growth. Second, the detrimental effect of worker quits is larger when high productivity workers quit, assuming that replacement costs are higher for quits of high productivity workers ($\rho_H > \rho_L$). As discussed further in the next subsection, the search and training costs for high productivity workers are assumed to be higher (i.e., labor market frictions are larger in the labor market of high productivity workers). Third, establishments can grow faster when they have better technology (θ), or can enjoy higher price (p) or lower wage (w) when having market power at the product market or the labor market.[14]

The set of assumptions and results derived using this framework provides the main prediction of this paper:

Prediction 1: Worker quits hurt establishment growth, when there exists replacement costs. The detrimental effect of worker quits is stronger for the separation of high productivity workers, compared to that of low productivity workers.

Quits of high productivity workers have larger detrimental effect on the establishment's growth because the loss in productivity is larger, and more importantly, because it is costlier to recover the lost productivity, for the quits of high productivity workers.

[14] As discussed below, I control for X using non-parametric controls for establishment quality (defined as a combination of the establishment's pay level, size, and past growth), and state-year-industry fixed effects.

2.2. Costs to Replace Workers

In this subsection, I discuss various forms of labor market frictions that can lead to higher costs in replacing high productivity workers. Replacement costs can be in the form of search costs in finding a worker at the labor market who can replace the leaving worker with similar productivity, and in the form of training costs for on-the-job training to raise the replacement worker's productivity level to that of the worker who left. I first discuss the search cost aspect, assuming thinner labor markets for high productivity workers; this assumption is drawn from the literatures of human capital theory (Becker, 1964) and imperfect labor markets (Manning, 2011). I then turn later to the training cost aspect.

What makes a higher productivity worker more difficult to search for? One source of heterogeneity is the relative scarcity of high productivity workers in the labor market due to higher moving costs (i.e., the labor market of high productivity workers is thinner), in the form of establishment-specific human capital. Higher productivity workers are more likely to have received larger amounts of establishment-specific training. Establishment-specific training increases the workers' relationship specificity with their establishment, and decreases their mobility: the worker's productivity is strongly tied to her current establishment, and thus, her productivity contribution at an outside establishment can be lower than her contribution at her current establishment (Jovanovic, 1979b; Coff, 1997). The productivity gap functions as a moving cost for the worker, as an outside employer is unable to match the wage offered by the incumbent employer.[15] Thus, high productivity workers have higher moving cost than low

[15] The higher moving cost from specificity of human capital induces the employer to exercise greater monopsony power, labor supply to be more inelastic, and the economic rent extracted from employment to be larger, for high productivity workers (Boal and Ransom,1997; Manning, 2011).

productivity workers. More generally, high moving cost for high productivity workers can be also due to greater general training if the general skill is complementary to establishment-specific skills (Acemoglu and Pischke, 1999).

This implies that labor markets of high productivity workers are likely to be thinner than those of low productivity workers.[16] Assuming that the establishment would search at the labor market of high (low) productivity workers when a high (low) productivity worker quits (e.g., by posting a wage offer at a similar level to the wage of the quitting worker), the number of potential candidates that the establishment can consider as a potential replacement would be smaller for replacing high productivity workers.

A smaller pool of candidates implies greater difficulty in finding a replacement worker equipped with the productivity level of the quitting worker. For instance, when the worker's productivity at the establishment has a worker-establishment match-specific component (Jovanovic, 1979a; Jovanovic, 1979b), the probability of finding a good match is lower, and therefore the expected time in finding a good match would be longer, when the labor market is thinner. Also, in the presence of asymmetric information on the worker's type, the firm's screening is less likely to be effective when there are a smaller number of candidates to consider, and employer learning (Farber and Gibbons, 1996; Gibbons, Katz, Lemieux, and Parent, 2005) after hiring the worker would be more important.[17] In either case, thinner high productivity worker labor market would induce longer delay and higher cost (i.e., larger ρ) to recover the lost human capital (ϕ) when high productivity workers quit.

[16] I assume that the labor market is defined locally by the combination of industry, occupation, and worker's productivity level throughout this paper.

[17] Information asymmetry can be another reason why the labor market of high productivity workers would be thinner (Akerlof, 1970; Coff, 1997).

The difficulty in replacing a high productivity worker is not limited to higher search costs, however. As discussed earlier, high productivity workers are more likely to have received larger amount of establishment-specific training by the time of their quits. This means that the establishment's investment in the replacement worker, to recover the productivity of the worker who left, would be larger for high productivity workers. As an illustration, consider the case where the establishment searches for a replacement worker always at the labor market of low productivity workers, regardless of the productivity level of the worker who left (for reasons including the labor market being potentially thinner for high productivity workers), and invest in training the replacement worker to recover the lost productivity. Then the training costs incurred to recover the lost productivity would be larger when a high productivity worker quits than when a low productivity worker quits. Cost of training is not limited to direct monetary costs, but can also be in the form of opportunity costs of the time it takes to reach the required level of skill.[18]

2.3. Employer Characteristics that Reflect Replacement Costs

In addition to worker characteristics (such as worker productivity discussed in Subsection 2B), the cost in replacing a worker is also likely to vary by industry or establishment characteristics. High productivity worker's quit would have an even stronger detrimental effect on the establishment's future growth for establishments with higher replacement costs. In this subsection, I discuss these employer characteristics that can reflect replacement costs.

I first explore industry-level heterogeneity in the difficulty of filling a vacant position, which I assume to reflect industry heterogeneity in search costs. Having unfilled vacant positions that establishments are willing to, but unable to, fill in would be an immediate indicator of the

[18] Further, the fact that it takes more time to train high productivity workers means there exists time compression diseconomies (Dierickx and Cool, 1989; Hatch and Dyer, 2004).

establishment having higher search costs. Thus, establishments in industries that more often have unfilled vacancies are more likely to have difficulty in finding a replacement worker equipped with the equivalent human capital level of the quitting worker, and therefore they would suffer more from worker quits. Recovering the lost productivity due to worker quits would be more time consuming and costly for these establishments. This leads to the following empirical prediction.

Prediction 2a: Establishments in industries where there is a higher likelihood of vacant positions being unfilled would suffer more from high productivity worker quits.

Standard human capital theory argues that employers would bear the cost of their employee's human capital investment in part or in whole, if the employee's human capital is firm-specific (Becker, 1962). Thus, whether the establishment covers the training costs (in part or in whole) of their employees would well reflect the establishment-specificity of the workers' skills at the establishment. The more establishment-specific the skills that are required at the establishment, the more costly it would be to recover the lost productivity due to the worker's quit. The replacement worker acquired from the labor market would not have the productivity level that is equivalent to that of the quitting worker, and the establishment would need to bear the cost in establishment-specific human capital investment to recover the lost productivity. In sum, recovering productivity would be costlier for establishments in industries where establishments more often cover their employees' training costs. This leads to the second empirical prediction.

Prediction 2b: Establishments in industries where there is a higher likelihood of the establishment covering the training expenses of their employees would suffer more from high productivity worker quits.

Beyond these primary proxies that are directly related to search costs and training costs, I further explore establishment characteristics that can well-reflect the difference in replacement costs and that can yield interesting managerial implications. The third establishment characteristic I explore is the heterogeneity in workforce composition across industries. In parallel to the assumption that the search costs and training costs for high productivity workers would be larger than those for low productivity workers, I assume that establishments in industries where knowledge-workers constitute a substantial proportion of the workforce would be in a labor market with larger search and training costs than establishments in other industries. That is, the labor market is thinner for knowledge-workers than other workers, because they are more likely to have establishment-specific skills (compared to other workers) and therefore are more likely to have higher moving costs. For example, one can assume that it takes more time and effort to find a replacement and recover the lost productivity for an R&D worker with a college degree, compared to a warehouse worker with a high school degree. In this regard, search costs would be larger for establishments in industries where a substantial proportion of their workforce is knowledge-workers. Moreover, training costs would be larger for these establishments, as well. Establishment-specific skills are more likely to be important in the operations of establishments in knowledge-intensive industries, and one can expect that there would be more on-the-job training for an R&D worker than a warehouse worker. This leads to the third empirical prediction.

Prediction 2c: Establishments in industries where knowledge-workers are important in their operation would suffer more from high productivity worker quits.

Another establishment characteristic that can impact the difficulty in replacing a worker can be the complexity of operations. That is, I postulate that establishments in industries that have more complex operations would have higher costs of replacing a worker. One reason is that training costs for establishments with more complex operations would likely be higher. If there is a fixed cost component in the training cost for each operation, the total training cost would be larger for establishments with larger number of operations, and therefore recovering the lost productivity through worker training would be costlier in terms of time and money. For instance, setting up a routine in training is one way to mitigate the negative turnover effect on establishment growth (Ton and Huckman, 2008). However, establishments that have more complex operations may well have a larger fixed cost in setting up the routine.

Prediction 2d: The detrimental effect of high productivity worker quits is stronger for establishments in industries with more complex operations.

Institutional arrangements at the establishment that distort the labor market can also increase the cost of replacing a worker. The German context provides an opportunity to examine how worker representation to the management may impact the detrimental effect of worker quits. Works council is a form of worker representation in Germany through which workers participate in establishment management. In particular, establishments with works council need to get approval of the worker representatives when hiring a worker. If the works council objects to the hiring decision of the establishment, the establishment needs to appeal to a court to hire the worker (Muehlemann and Pfeifer, 2016). Thus, existence of works council by itself would increases the hiring cost, as it would take more time and effort to hire a new worker. The potential legal cost increases the hiring cost, even if the hire is not subject to court appeal.[19] Thus, establishments with works council would have higher search costs, and recovering the lost productivity due to worker quits would be more costly.

Prediction 2e: The detrimental effect of high productivity worker quits would be stronger for establishments with works council.

3. Data and Empirical Methodology

3.1. Overview of the Dataset Construction

The LIAB is an Unemployment Insurance (UI) based administrative data that holds daily level information on the timing of accession and separation, and the timing of receiving unemployment benefits, of every paid worker employed at the sampled establishments from 1999 to 2009. I exploit the detailed information on the workers' employment history and unemployment history to construct measures of worker quits at the worker-time level. Another strength of the LIAB is that it holds rich information on job characteristics, such as wage, occupation, and industry.[20] I utilize this information in constructing measures of relative worker productivity within industry-occupation combinations at the worker-time level. Combining the

[19] Firms with works council generally have lower turnover (Hirsch, Schnabel, and Schnabel, 2010), and therefore are more likely to have stronger incentives for on-the-job-training investments. In this regard, existence of works council can indirectly increase the expected training cost for the replacing worker.

[20] All wage values are CPI adjusted to 2013 EUROs.

worker quit measures and the worker productivity measures, and aggregating up to the establishment-year level, I construct measures of worker quits by worker productivity level cells for each establishment-year. The estimation sample is an establishment-year level dataset of establishments with more than 10 full-time employees and trainees in West Germany for the period of 2002-2008, which covers about 1.3 million worker-establishment-year observations.[21]

3.2. Measuring Relative Worker Productivity Level within Industry-Occupation

I assume that workers with different combinations of industry and occupation are in different labor markets.[22] Further, I also assume that labor market frictions (and therefore the difficulty in replacing a worker) differ by the worker's relative productivity level within industry-occupation combinations. In effect, I define a labor market by industry, occupation, and worker productivity level.

The worker's relative productivity is measured using workers' wage. Past studies on wage and productivity provide support for using wage as the measure for productivity. For instance, Hellerstein, Neumark, and Troske (1999) show that workers' wage is strongly correlated with their marginal product. Also, Fox and Smeets (2011) show that most of the variation in Total Factor Productivity (TFP) explained by various dimensions of human capital can be explained by wage bill information.

[21] The estimation sample period is determined by the definition of the dependent variable and the regressor that are in averages across multiple years.

[22] Occupation may well be a sufficient level in defining the labor market. However, I consider industry as well, because the sampling in the LIAB is done at the establishment-level, under a stratified sampling scheme by establishment size, industry, and state. The workers included in the sample are those who were at one point employed at the sampled establishments. Including industry in defining the labor market becomes even more relevant when constructing the instruments discussed later.

In addition to the wage variable that is given in the LIAB, I construct two variations of it as relative productivity measures. First, wage information in the LIAB is top-coded, and I use Tobit models to impute the non-censored wage, following past studies (Card, Heining, Kline (2013), Dustmann, Ludsteck, Schönberg (2009)). I denote this wage as Imputed Wage, hereafter, and use it as the primary measure for the worker's relative productivity in the empirical analysis. The second measure takes into consideration establishment-specific policies that can affect the wage of all employees within the establishment. That is, wage is determined not only by the worker's productivity, but also by establishment-specific factors, such as rent-sharing, efficiency wage premium, or strategic wage posting. I use the decomposition of Abowd, Kramarz, Margolis (1999), hereafter AKM decomposition, to difference out this establishment factor from wage, and denote it as Worker Human Capital.[23] The original wage measure in the LIAB is denoted as Raw Wage, hereafter.

Using these three wage variables, I construct measures of the worker's relative productivity, for each job-year (i.e., worker-establishment-year) combination, by comparing across wages of jobs of same occupation in the same industry. That is, my measure of worker productivity is defined for each job-year combination, using quantiles of wages within the wage distribution of job-years sharing three-digit industry codes and occupation codes.[24] This approach of using buckets of wage, rather than the wage value itself, alleviates the concern that wage can be a noisy measure of worker productivity, to the extent that ranks of worker

[23] I follow Card et al. (2013) for the AKM decomposition. AKM decomposition decomposes wage by the fixed worker component, observed worker characteristics (education and age), and fixed firm component using the equation:

$$\ln(wage_{ijt}) = \theta_i + \psi_{j(i,t)} + X'_{ijt}\beta + \varepsilon_{ijt}; \; i\text{=worker, } j\text{=establishment, } t\text{=year}$$

My worker human capital variable is wage after differencing out the fixed firm component ψ.

[24] The occupation code in the LIAB has about 300 unique values.

productivity are not systematically overturned when using ranks of wages. Also, it is important to construct the worker productivity measures within industry-occupation cells because the analysis is conducted across the German economy, and we expect wage levels to be heterogeneous across industries and occupations.[25] The primary measure of the worker's relative productivity at the job-year is constructed using the median in the distribution of wages of job-years whose establishment is in the same industry, and whose occupations are the same. If the worker's wage at the job-year is above the median in that distribution, then the worker is categorized as a high productivity worker for that specific job in that specific year. Otherwise, she is categorized to be a low productivity worker for that specific job in that specific year.

3.3. Distinguishing Worker Quits from Worker Firing

There are two types of worker turnover. One is voluntary worker turnover, which refers to the worker quitting the establishment on her decision, and the other is involuntary worker turnover, which refers to the establishment firing the worker. In this subsection, I discuss why misclassification of firing into quits is a valid concern for the empirical analysis of the relation between worker quits and firm performance. I then discuss how this paper makes improvement in the measure of quits, taking advantage of the richness of the LIAB and utilizing the institutional features of the German UI system.

The reason why misclassification is a concern is because involuntary worker turnover (i.e., firing) is an endogenous choice of the establishment, while voluntary worker turnover (i.e., quitting) is an endogenous choice of the worker. Assuming that the establishment makes the

[25] Wage level may well be heterogeneous by geography. This heterogeneity, however, would be absorbed in the establishment component of wage in an AKM decomposition discussed above. Analysis using the Worker Human Capital measure (which I construct using AKM decomposition and differencing out the establishment component in wage) yields results that are materially similar.

decision to optimize its profitability in its firing decision, the propensity of worker firing would be relatively larger for low quality workers.[26] This potential correlation could induce a positive bias in the estimate of the effect of worker quits on establishment performance, when firing is misclassified into quitting.

Setting aside the cases of downsizing due to negative product market shocks (i.e., mass-layoff events), establishments are likely to fire those with the lowest productivity or the poorest match quality first.[27] It is even plausible that establishments that use firing to find better workers can enhance their productivity through involuntary worker turnover. Furthermore, there is another factor that intensifies this relation between firing and worker quality: cost of firing and hiring. Firing, and hiring a replacement worker, can often be a costly process. The cost of firing and finding a new worker is endogenized in the firing decision, and the establishment would fire the worker only if the expected net benefit is positive. Positive net benefit of firing is likely to exist for firing low productivity workers than firing high productivity workers, because the search and training costs for low productivity workers are likely to be smaller, which is supported by the empirical findings of this paper. On the other hand, worker leaving is not expected by the establishment, and the cost of finding the replacement worker hurts the establishment.

To summarize, firing workers may well have a positive impact on establishment growth, whereas worker quits is expected to have a negative impact on establishment growth.[28] Thus,

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[26] Likewise, when the worker makes the termination decision (i.e., the turnover is voluntary), it may well be that the establishment is of low-quality (e.g., the worker anticipates that the establishment will perform poorly in the future). This potential correlation, and the endogeneity bias due to the correlation, is discussed in more detail below.

[27] Low productivity workers are likely to be those that contribute less to the establishment's profitability (or economic rent), assuming that labor supply is more elastic for low productivity workers.

[28] I am setting aside the case where worker quits is a firing in disguise. For example, the establishment can make the worker unhappy and make them quit. This is a relevant concern in particular in the German context, where firing

misclassification of firing into quitting can potentially yield a positive bias when studying the relation between worker quits and firm performance, when distinguishing quitting from firing is often not very clear to the researcher when using conventional firm-level data.[29]

In the German labor market context, workers who quit suffer from a twelve-week sanction period in receiving benefits. Because the LIAB allows me to precisely observe the day of the separation and the first day of receiving benefits, I can detect the sanction period imposed on worker quits at the job-level. Specifically, I define a worker separation to be voluntary if I observe an interval longer than twelve weeks between the timing of unemployment and the receipt of benefits, or, if there is no unemployment spell between jobs. I refine this measure of worker quits by change in wages between jobs. Among the worker turnover observations that have been coded as worker quits by the two criteria above, those with more than 10% decrease in wage are re-coded as fired events.

3.4. Decomposing Worker Quits by Volume

I decompose voluntary worker turnover (i.e., worker quits) into mass voluntary turnover and non-mass voluntary turnover, by the volume of workers quitting the establishment within the same month: if more than 30% of employment quit in the same month, it is defined as mass voluntary turnover. Other worker quits are defined as non-mass voluntary turnover. These two measures are supplementary measures of the overall voluntary turnover measure, each with

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cost is significant. The instruments discussed below alleviate this concern, as it uses the variation in the local labor market demand, which is independent from the establishment's decision.

[29] Relatedly, voluntary worker turnover and involuntary worker turnover can have different managerial implications. That is, the channel through which worker turnover affects performance is different, and therefore managerial implications are also different, across the types of turnover. For involuntary turnover, the managerial implication is on sorting out who is the worker to fire (and perhaps replace) to improve the productivity of the workforce. For voluntary turnover, it is about preventing the workers from leaving the establishment, as it would be costly to replace them, and as the leaving workers can join or found a rival establishment.

strengths and weaknesses. Examining the turnover effect of mass worker quits can capture the complementarity of labor which may lead to stronger detrimental effect when a large group of workers leave in the same month (i.e., the detrimental effect can be non-linear). Comparing the turnover effect estimates using this measure to those using the non-mass voluntary turnover measure can provide estimates of the effect of losing a large group of workers compared to losing workers individually.[30] However, it is plausible that worker quits anticipating bad future performance of the establishment (i.e., endogenous quits discussed later in this section) is more likely to be in the form of mass voluntary turnover than non-mass voluntary turnover.[31]

3.5. The Key Regressors, the Dependent Variable, and the Baseline Specification

I aggregate the number of workers quitting by worker productivity cells to the establishment-year level, and scale it by total employment at the establishment, to construct the turnover rate variables at the establishment-year level.[32] The dependent variable is future employment growth rate of the establishment.

Below is the empirical specification for the baseline regression of this study:

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[30] Higher replacement costs for (and therefore stronger detrimental effect of) mass worker quits is consistent with the convexity of adjustment costs studied in the dynamic labor demand literature (Nickell, 1986).

[31] In the empirical model below, I non-parametrically control for establishment quality to alleviate the concern for the anticipation effect. Instrumental variables are also used to mitigate the endogeneity concern.

[32] Turnover rate is defined as the number of high (low) productivity worker quits at the establishment scaled by total employment at the establishment. An alternative approach can be to scale by the number of high (low) productivity workers in measuring the turnover rate of high (low) productivity workers. Note that the relation between current average human capital stock, past average human capital stock, and quit rate discussed in section 2.1 does not dictate a particular scaling measure in defining turnover rates. The turnover rate measure is used to capture the loss in average human capital stock, and the way in which scaling is done would reflect how quits of high productivity workers and quits of low productivity workers are weighted in constructing the average. The framework developed in section 2.1 allows flexibility in choosing the scales. That is, the differences in scaling can be embraced by adjusting the weights W_H and W_L in defining the aggregate ϕ. Also, unreported analysis that controls for the establishment's workforce composition by productivity level (i.e., the ratio of high productivity worker employment to total employment) at the establishment-year level yields very similar results for the baseline analysis.

$$
\begin{aligned}
&\overline{growth}_{j,t+1,t+2} \\
&\quad = \alpha + \beta_H \overline{turnover\ rate}_{j,H,t-1,t-2} + \beta_L \overline{turnover\ rate}_{j,L,t-1,t-2} \\
&\quad + I\{estab\ size * median\ wage * growth\}_{j,t} + I\{state * industry * year\}_{j,t} \\
&\quad + I\{SU\}_{j,t} + \varepsilon_{j,t} \qquad (1)
\end{aligned}
$$

where *j* and *t* denote establishment and year, respectively. *H* refers to workers of high productivity level at the job-year (i.e., above the median in the distribution of wage of workers with the same industry and occupation codes), and *L* refers to workers of low productivity level at the job-year (i.e., below the median in the distribution of wage of workers with the same industry and occupation codes). The bar above *growth* and *turnover rate* denotes averaging across the subscript years. I smooth the dependent variable and the regressor because job flows are temporally volatile (Davis and Haltiwanger, 1999), and regression-to-the mean effects are known to be pervasive in growth measures (Davis, Haltiwanger, Schuh (1996)).[33]

Establishment quality can be a primary source of endogeneity in the relation between turnover rate and growth. First, workers may voluntarily leave the establishment anticipating negative future establishment growth. Second, there can be assortative matching between the worker and the establishment (i.e., matching between high-paying establishments and high-quality workers), which can be correlated with both future growth and the volume of worker quits (Brown and Medoff, 1989). High-paying establishments will often be matched with high-quality workers, and these establishments could experience high growth and low quitting rates. In either case, the unobserved correlations pose a potential threat of negative endogeneity bias in the coefficient estimates of βs. I alleviate these concerns about potential endogeneity bias by

[33] Estimation results for the baseline specification are robust to using single year values of the regressor and the dependent variable, as shown in Appendix Table A1.5 and Appendix Table A1.6.

non-parametrically controlling for establishment quality at the establishment-year level. *I{estab size*median wage*growth}* denotes the set of dummy variables (twenty bins in total) defined by the interaction terms of establishment employment size, median wage level at the establishment, and past establishment growth.

*I{state*industry*year}* denotes the fixed effects bins defined by the interaction terms of state dummies, two-digit industry code dummies, and year dummies used to control for local product market shocks.[34] *I{SU}* denotes single unit establishment dummy, as single unit and multi-unit firms grow in different ways. Standard errors are clustered by establishment.

3.6. Instruments for Voluntary Worker Turnover: Local Labor Market Demand

As discussed above, an endogeneity concern about the worker turnover rate variables is that it may be capturing quits that is due to poor establishment quality, which reflects "push" turnover. Ideally, the variation in quits that would be impervious to the endogeneity concern is that due to other job opportunities attractive to the worker. To capture this "pull" turnover variation, I propose to construct a set of local labor market demand instruments, using hiring intensity at the local labor market.

The idea is to exploit the variation in hiring intensity for each occupation in the local labor market, which affects the firms' quitting rate differently by each firm's occupation composition. For each of the turnover rate variables of high productivity workers and of low productivity workers, I construct a Bartik-style instrument (Bartik, 1991; Goldsmith-Pinkham, Sorkin, and Swift, 2017) by using the interaction of occupation shares at the establishment and hiring intensity for each occupation at the local labor market. The richness of the LIAB allows

[34] "State" refers to the German federal state (Bundesland). The term "County" in this paper is referring to the district authority (Kreis).

me to measure inflow (i.e., hiring) of workers at local labor markets defined at a very fine level (by the combination of occupation, worker productivity level, industry, geography, and year), which provides statistical power to the instrument. Constructing the instruments as an interaction of occupation shares at the establishment and hiring intensity at the local labor market, the instrument exploits the variation in external local labor market demand for each occupation that affects each establishment differently by the establishment's occupational composition. For the exogeneity of the instrument, I remove worker inflow or worker outflow (i.e., hiring, firing, quitting) occurring at the focal establishment when constructing the hiring intensity at the local labor market. This allows the instrument to be independent of focal establishment growth and characteristics.

Moreover, the instruments can also alleviate the concerns on the positive bias due to misclassification of firing into quitting, which may still exist despite the improved measure of quits I use in this study. That is, there can be cases where the establishment makes the worker quit when they want to fire her, for instance by taking steps that make the worker dissatisfied or unhappy. This type of misclassification (or firing in disguise) is a valid concern in the context of the German economy in particular, given the high legal cost of firing. The set of instruments also alleviates the positive bias concern, as it uses the variation that is independent from the focal establishment's decision.

In constructing the instrument, I first define a local labor market by workers who are in the same occupation group (o;13 groups), with the same worker productivity level (h; ($h \in \{H, L\}$)), and by establishments in the same three-digit industry codes (i), and county codes (g), for each year (t). Then I aggregate the number of inflows (i.e., hiring) of workers at establishments in the local labor market. This aggregate includes both employment-to-employment transitions and unemployment-to-employment transitions. I exclude worker inflows (i.e., hiring) to the focal establishment (j), and worker outflows from the focal establishment (i.e., fired or quitting workers) to the local market, so that the instruments would not be confounded with the focal establishment's growth. The aggregated number of worker inflows is denoted as $inflow_{ohigt}$. I scale the local labor market worker inflow by the size of the local labor market ($employment_{ohigt}$), which is an aggregate of the number of employment at the local labor market.[35]

Occupation ratio for each occupation at the establishment j, year t, is constructed as the number of workers for each occupation group *o*, scaled by the total employment at the establishment j, year t, for each worker productivity level *h*. Finally, I construct an *external pull instrumental variable* for each worker productivity level h, as the weighted average of the worker inflow rate (worker inflow aggregate scaled by the size of the local labor market), weighted by the occupation ratios at the establishment j in year t:

$$External\ pull\ IV_{hjt} = \sum_{o} \frac{employment_{ohjt}}{employment_{hjt}} * \frac{inflow_{ohigt}}{employment_{ohigt}}$$

[35] Industry classification of the establishment is included in defining the local labor market, because the sampling of the LIAB is done at the establishment-level, under a stratified sampling scheme by establishment size, industry, and state. As the worker flows are calculated using the employment history records of the workers who were employed at the sampled establishments, concern about the worker inflow rate measure due to sampling errors becomes more severe if industry is not included in the local labor market definition. Regarding the occupation codes, I use 13 occupation groups instead of the raw 300 values, as measurement error due to sampling error can be larger when using a finer market definition. The definition of the 13 occupation groups follow those used at the IAB (Institute for Employment Research; the German entity that manages the LIAB), based on Blossfeld (1987). The instrumental variable estimations using the 13 occupation groups and the 300 raw occupation values yield results that are materially the same.

Then I conduct a 2SLS analysis. Estimation of the first stage uses equation (1), after replacing the dependent variable with the turnover-rate variables, and having the instruments as the regressors.[36]

3.7. Proxies for Replacement Costs at the Source Establishment

The baseline estimation examines how difference in the level of worker productivity can yield different turnover effects, under the postulation that workers of higher productivity are more difficult to replace. As discussed in Section 2, the heterogeneity in the cost of replacing a worker can also be captured by industry or establishment characteristics. The detrimental effect of high productivity worker quits would be even stronger for establishments with higher costs of replacing a quitting worker. I examine this heterogeneity by subsampling the data by proxies of replacement costs defined at the industry-level or establishment-level.

The first proxy is a measure of search costs at the three-digit industry level, constructed using an indicator variable for the establishment having unfilled vacancies that it was willing to fill in (*prediction 2a*). The second proxy is a measure of training costs at the three-digit industry level, constructed using an indicator variable for the establishment having released its employees to receive training, and having covered the training expense in part or in whole (*prediction 2b*). These two indicator variables are available in the IABBP dataset, which is an establishment-year-level survey dataset that can be linked to the LIAB. The third and fourth proxies capture the difference in replacement costs by workforce composition at the three-digit industry level, constructed using the share of workers of college or higher educational attainment at the

[36] The local labor market used for constructing the instruments is defined at a finer geographical (county), industry (three-digit industry) classification than the state-industry (two-digit industry)-year fixed effects that are used in both the first-stage and the second-stage estimation.

establishment, and share of R&D workers at the establishment (*prediction 2c*). These variables are available through the BHP dataset, which is an establishment-year level administrative dataset that is constructed using the workers' employment history records of the IAB (Institute for Employment Research).[37] The fifth proxy captures the complexity of operation at the three-digit industry level, where complexity is measured as the number of unique occupations within the establishment (*prediction 2d*). This proxy is constructed using the occupation codes available in the LIAB. The sixth proxy is the existence of works council at the establishment (*prediction 2e*), which is available through the IABBP dataset.

Other than the works council proxy, I construct the replacement costs proxies as the three-digit industry mean of the establishment-level variables discussed above. That is, the first proxy (search cost proxy) is a measure of industry-level propensity of having unfilled vacancies, and the second proxy (training cost proxy) is a measure of industry-level propensity of the establishment covering their employees' training costs (in part or in whole). The third proxy and the fourth proxy (the workforce composition proxy) are the industry-level average share of workers with college or higher educational attainment, and the industry-level average share of R&D workers, in the workforce. The fifth proxy (the complexity proxy) is the industry-level average number of unique occupations. I subsample the estimation sample using these proxies of replacement costs, and comparing the three-digit industry means to their median. That is, establishments are subsampled into high or low replacement costs groups, by the median of the three-digit industry means.[38]

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[37] These employment history records are also the main data source of the workers' employment history covered in the LIAB.

[38] The subsampling by industry is done at a finer level (three-digit industry classification) than the fixed effects (two-digit industry classification).

As for the works council proxy, it is the establishment-level dummy variable for the existence of works council, and I subsample the estimation sample by the dummy variable.

4. Baseline Estimation Results

I first estimate the effects of different types of worker turnover on future establishment growth, regardless of the worker productivity level of the separating workers. Table I.1 reports the estimation results using equation (1) without the separate terms for worker productivity levels, *H* and *L*. That is, each variable listed on the second row is the sole regressor, while the control variables in equation (1) are all included. The turnover variables used for each column are: (1) any type of worker turnover (i.e., voluntary or involuntary turnover) for column 1; (2) voluntary turnover (i.e., worker quits) for column 2; (3) involuntary turnover (i.e., firing) for column 3; (4) mass voluntary turnover for column 4; (5) non-mass voluntary turnover for column 5.

There are three points to note. First, although none of the coefficient estimates are statistically significant, their magnitudes suggest that misclassification of firing into quitting may be a source of positive bias in the estimate. The estimates in columns (1) - (3) suggest that including involuntary turnover in turnover measures (or misclassification of involuntary turnover as voluntary turnover) can lead to underestimates of the negative relation between worker quits and establishment growth.[39] Second, it shows us that when worker turnover is measured as an overall measure (i.e., turnover without conditioning on worker productivity level), the coefficient estimates are small in magnitude and not statistically significant. This holds not only for involuntary turnover, but also for voluntary turnover measures as well, and suggests that

[39] Smaller magnitudes of the estimates for involuntary turnover are also observed when decomposing the turnover measures by worker productivity level, as reported in Appendix Table A1.2.

turnover effects (that may be heterogeneous by the separating worker's productivity) can be obscured when examined all together. Third, the coefficient estimates of column (4) and column (5) suggest that establishments suffer more when workers leave simultaneously than individually. One reason for this asymmetry can be complementarity of labor among the employees. If a worker's productivity contribution is independent from another worker's productivity contribution, then workers leaving together or workers leaving individually would not have different effects.

I next examine how the impacts of worker quits on future establishment growth are different by the quitting workers' level of productivity using equation (1). Table I.2 reports the estimation results, for voluntary turnover (i.e., worker quits) in columns (1) - (3), mass voluntary turnover in columns (4) - (6), and non-mass voluntary turnover in columns (7) - (9). Three different variations of the wage variable are used as measures of worker productivity. Columns (1), (4), (7) report results using the imputed wage using Tobit models, columns (2), (5), (8) report results using the raw wage value given in the LIAB, and columns (3), (6), (9) report results using the imputed wage after differencing out the establishment component by AKM decomposition (denoted as Worker Human Capital). High productivity worker denotes workers who are above the median in the distribution of workers' wages for each industry-occupation-year cell. Low productivity worker denotes workers who are below the median in the distribution.

Overall, we observe asymmetric effects of worker quits: worker quits of high productivity workers has statistically significant negative effects on future growth, while that of low productivity workers has statistically insignificant (positive) effect. The asymmetry suggests that it is important to take into account the productivity level of the worker leaving, when studying the impact of worker quits on firm performance. The pattern of asymmetry between high productivity worker turnover and low productivity worker turnover continues to hold when dividing the worker productivity level into quartiles, instead of by the median, as shown in Appendix 1 Table A1.3. The stronger negative effect of high productivity workers' turnover is in accordance with the assumption that high productivity workers are more difficult to replace.

The point estimate of -0.292 in column (1) can be translated into about 1.2 percentage point decline in future establishment growth rate when the worker quit rate of high productivity workers increases by 1 standard deviation, which is about 7.7% of the standard deviation in the future establishment growth variable (Appendix 1 Table A1.1).

The signs of the coefficients are consistent regardless of the wage measure used. Also, consistent with the patterns in Table I.1, the magnitudes of the effect are pronounced for mass voluntary turnover, compared to non-mass voluntary turnover. This suggests the possibilities of complementarity of labor that leads to larger detrimental effect when workers quit at the same time.[40]

Table I.3 reports 2SLS estimation results for worker quits using instrumental variables – the high productivity worker hiring intensity and the low productivity worker hiring intensity at the local labor market. The first two columns report the first-stage estimation results, having the high productivity worker turnover rate as the dependent variable in column (1) and the low

[40] I also examine future labor productivity growth defined as future growth in sales per employee or future growth in value added per employee. This analysis shows the channel through which worker quits affect establishment employment growth, as discussed in the framework section. That is, worker quits lead to decrease in average human capital stock at the establishment, which leads to decreased labor productivity, and thereby hurts the establishment's future employment growth. Table A1.4 repeats Table I.2, after replacing the dependent variable with measures of future labor productivity growth: columns (1) – (3) use future two-year average growth in sales per employment, and columns (4) – (6) use future two-year average growth in value added per employment, as the measure of establishment growth. We see that the estimates follow a similar pattern to those in Table I.2, but the statistical significance is weaker. This may be attributable to the fact that the sales measure and the value-added measure are missing for a substantial proportion of the estimation sample. This is due to sales and costs information originating from a linked establishment-year level survey data (the IABBP dataset), for which the sample is re-drawn every year.

productivity worker turnover rate as the dependent variable in column (2). The third column reports the second stage estimation result. The first stage estimates show expected signs, as we would expect larger volume of worker quits when there is larger labor demand at the local labor market. The F-stats for the first stage estimations are larger than 30, for both columns (1) and (2). The second stage results show coefficient estimates that are consistent with the baseline estimation results in Table I.2. Furthermore, the quits of low productivity workers have statistically significant positive effects, suggesting that worker quits can even be beneficial to the establishment when the replacement costs are low: the benefit of having new workers (e.g., better quality workers) can be larger than the cost of replacing a worker.

The estimation results discussed so far support the prediction that loss of high productivity workers has a relatively stronger detrimental effect on establishment growth. As an extension, I examine whether the loss of workers with productivity due to establishment-specific human capital, has even more severe consequences on the establishment's growth. The underlying hypothesis is that worker productivity due to establishment-specific human capital would be even more costly to replace, compared to worker productivity due to general human capital, because establishment-specific human capital is accumulated by on-the-job training that the establishment pays for (Becker, 1962). Moreover, the larger establishment specificity due to the larger establishment-specific human capital implies that the establishment would have extracted larger rent in the employment relationship utilizing the worker's mobility constraint (Manning, 2011; Campbell, Coff, Kryscynski, 2012).

I use the worker's job tenure at the establishment as a proxy for the accumulation of establishment-specific human capital (Topel, 1991; Buchinsky et al., 2010). In the same way as in measuring the workers' productivity level, long (short) tenure workers at the job for the year are those above (below) the median in the distribution of tenure of workers at jobs with the same industry and occupation codes. Then I examine whether the loss of workers with productivity due to establishment-specific human capital is costlier to the establishment, by decomposing the turnover rate variables in equation (1) by the length of job tenure at the time of worker departure. That is, high (low) productivity worker turnover rate in equation (1) is decomposed into turnover rates of high (low) productivity workers with long tenure at the establishment, and turnover rates of high (low) productivity workers with short tenure at the establishment. I then estimate equation (1) with the four turnover rate variables.

The estimation results are reported in Table I.4. Columns (1), (2), (3) report the estimation results using imputed wage, raw wage, and worker human capital, respectively. The coefficient estimates once again show the asymmetry in the turnover effect between high productivity workers and low productivity workers. Also, note that turnover of high productivity workers with long tenure has statistically significant, negative effect on future growth. On the other hand, the coefficient estimates of high productivity workers with short tenure are smaller in magnitude and not statistically significant, although the signs are negative.[41] These results suggest that losing workers with high establishment-specific human capital is even worse to the establishment.

In summary, I find evidence for an asymmetry between high productivity worker quits and low productivity worker quits, in their effects on establishment growth. However, the detrimental effect of worker quits overall is not statistically significant (with a smaller coefficient estimate), as the effect of low productivity worker quits mutes the effect of high productivity worker quits. The asymmetry can be explained by the difference in the cost of replacing a

[41] These patterns in the coefficient estimates are also observed when job tenure is divided into quartiles, instead of divided by the median (Appendix Table A1.7).

leaving worker. High productivity workers are likely to be more difficult to replace than low productivity workers, and therefore the separation of high productivity workers is costlier.

5. Employer Characteristics and the Turnover Effect

To test predictions 2a – 2e, I repeat the baseline analysis in Section 4 using equation (1), on a subsample of establishments with high replacement costs and on another subsample of establishments with low replacement costs, using the replacement costs proxies discussed in Section 3.

Estimation results are presented in Table I.5A and Table I.5B, where the odd numbered columns are results on subsamples with high replacement costs, and the even numbered columns are results on subsamples with low replacement costs. Table I.5A shows estimates using the search costs proxy (unfilled vacancies) in column (1) and column (2), and the estimates using the training costs proxy (the establishment covering the training costs) in column (3) and column (4). Table I.5B shows estimates using other employer characteristics that can reflect replacement costs: high educational attainment share (columns (1), (2)), R&D worker share (columns (3), (4)), complexity of operation (columns (5), (6)), and the existence of works council (columns (7), (8)).

The estimation results suggest that the detrimental effect of high productivity worker quits is indeed stronger for establishments with high replacement costs: we observe stronger detrimental effects for establishments in industries where there is a higher likelihood of vacancies being unfilled, in industries where there is a higher likelihood of establishments covering their employees' training costs, in industries where the workforce is largely composed of knowledge-workers, in industries where operations are more complex, and for establishments with works council. Also, the asymmetry in the effect between high productivity worker turnover and low productivity worker turnover observed in the baseline analyses holds in Table I.5A and Table I.5B as well: quits of high productivity workers at high replacement costs establishments has statistically significant negative effects on future growth, whereas quits of low productivity workers do not.

In Table I.6A and Table I.6B, I repeat Table I.5A and Table I.5B, using the local hiring intensity instruments. For brevity, I report only the second-stage estimation results and the F-statistics of the first-stage estimation. The full estimation results are reported in Appendix 1 Table A1.8. Other than the additional first-stage F-statistics results, the formatting of the tables is identical to that of Table I.5A and Table I.5B. The estimation results once again support the predictions that the detrimental effect of high productivity worker turnover is stronger for establishments with high replacement costs than for establishments with low replacement costs.

Overall, these results reinforce the baseline results, and support the insight that replacement costs is an important factor modulating the detrimental effect of worker quits.

6. Extension: Industrial (Competitive) Distance of the Job Transition and the Turnover Effect

The empirical results thus far provide evidence that supports the significance of replacement costs in the impact of worker quits on the establishment's future growth, as the establishment loses productivity when the worker quits, and it is costly for the establishment to recover the lost productivity. However, the worker's job transition can also be a productivity addition to the destination establishment. The addition of worker productivity to the destination establishment can be detrimental to the source establishment, when the source establishment and the destination establishment compete in the same market (i.e., the destination establishment is a rival establishment). The mobile worker at the destination establishment can change the product market competition, and thereby hurt the source establishment's growth.

The detrimental effect of worker turnover that a literature in strategic management has studied is related to this channel, examining how well the separated worker uses her knowledge and other assets she brings into the destination establishment or how well routines are replicated (Campbell, Ganco, Franco, Agarwal (2012); Aime, Johnson, Ridge, and Hill (2010); Somaya, Williamson, Lorinkova (2008); Wezel, Cattani, Pennings (2006)). In this section, I check if my baseline results vary in ways related to these extant studies. In particular, I examine whether mobile workers moving to a competing establishment (i.e., rival establishment) and mobile workers moving to a non-competing establishment (i.e., non-rival establishment) have different turnover effects on source establishment growth. I then explore how these turnover effects may be different for establishments with characteristics that reflect high replacement costs, compared to other establishments.

I use the three-digit industry codes in defining the product market competition. That is, if the source establishment and the destination establishment have the same three-digit industry code, then the job transition is a transition to a rival establishment. Otherwise, it is a transition to a non-rival establishment. I then decompose the turnover rates of each productivity level workers, to those that lead to job transitions to a rival establishment and those that lead to job transitions to a non-rival establishment. This results in four turnover rates, as in the tenure effect analysis. Other than having four turnover rates instead of two turnover rates, the specification remains the same as in equation (1). I repeat the baseline estimations of Table I.2 and the subsampling estimations of Table I.5A and Table I.5B.

The estimation results are reported in Table I.7. The first four rows are the four turnover rates – quitting rate of high productivity workers going to a rival establishment, quitting rate of low productivity workers going to a rival establishment, quitting rate of high productivity workers going to a non-rival establishment, and quitting rate of low productivity workers going to a non-rival establishment. Column (1) reports the full-sample estimates, and columns (2) - (7) report the subsample estimates.

The estimation result in column (1) presents three interesting observations. The first is that, the magnitude of the coefficient estimate of quits of high productivity workers moving to rival establishments is larger than that of quits of high productivity workers moving to non-rival establishments. Workers moving to rival establishments have stronger detrimental effects on the source establishment's growth. Second, the above two coefficient estimates are not statistically significantly different, as indicated by the p-value of the difference in the coefficients. The third observation is that both inter-industry moves of high productivity workers and intra-industry moves of high productivity workers have statistically significant negative effects at the 10% level. That is, even if the worker does not change the product market competition by her job transition, the establishment suffers from her quit. This set of results suggests that worker moves to competitors change the product market competition, and thereby hurt the source establishment's growth, but it also suggests that worker loss itself hurts the establishment as it is costly to replace the quitting worker.

The split-sample estimation results reinforce the significance of replacement costs in the effect of worker quits on future establishment growth. In columns (2) - (7), I report the estimation results of subsampling analyses using the replacement costs proxies used in Section 5. Columns (2) – (3) use the propensity of having unfilled vacancies, columns (4) – (5) use high educational attainment share, columns (6) – (7) use R&D worker share as the replacement costs proxy. The even numbered columns report the results for the subsamples of high replacement costs establishments, and the odd numbered columns report the results for the subsamples of low replacement costs establishments.

The coefficient estimates display contrasting effects of high productivity worker leaving to a rival establishment, between high replacement costs establishments and low replacements costs establishments. For the high replacement costs subsamples, turnover of high productivity workers moving to rival establishments has statistically significant and negative effects. On the other hand, turnover effects at low replacement costs establishments are not statistically significantly negative. These results suggest that worker moves to a rival establishment hurt the source establishment only when the source establishment suffers from high replacement costs, and the moving worker is a high productivity worker. Overall, the set of results reported in Table I.7 supports the idea that difficulty in replacing a worker is an important factor to consider when studying the relation between worker turnover and firm performance.

7. Conclusion

This paper studies the effect of voluntary worker turnover on source establishment growth, exploring how the effect varies by worker and establishment characteristics that reflect the difficulty in replacing a quitting worker. I find that losing higher productivity workers has a stronger detrimental effect on the establishment's future growth, and that establishments with characteristics that reflect high replacement costs suffer more from quits of high productivity workers. Worker quits can affect the source establishment's growth also by changing the product market competition, as the quitting worker contributes to the productivity of the destination establishment. The findings of this paper suggest that, while worker moves to rival establishments have a stronger detrimental effect to the source establishment's growth (than worker moves to non-rival establishments), establishments suffer from worker quits even when this alternative channel of the turnover effect is muted, and that this alternative channel is notable only when the source establishment has high replacement costs.

Overall, this study documents the centrality of the difficulty in replacing a worker in the relation between worker quits and firm performance. It suggests that managers should be concerned about quits of high productivity workers who are more difficult to replace, especially if the firm often has unfilled vacancies that it is willing to fill in, if the firm covers the training costs of their employees, if the firm's workforce is largely composed of knowledge-workers, if the firm's operations are complex, or if the firm has high legal cost of hiring. In studying the managerial implications of labor market frictions, past studies have focused on how it can be used to constrain worker mobility and increase the firm's competitive advantage (Coff 1997; Campbell, Coff, Kryscynski, 2012). This paper studies labor market frictions from a different angle, and suggests that managers should be concerned about labor market frictions present in replacing the worker who left. Using various measures that reflect heterogeneity in the impact of labor market frictions, this paper underscores the significance of labor market frictions as a source of variation that can affect the firm's value (Mahoney and Qian, 2013).

This paper discusses the difficulty in replacing a worker in terms of search and training costs. Although they are conceptually separable, selection in worker's tenure and training by the worker's quality can generate a correlation between the two costs. To be more specific, higher ability workers are more likely to receive the firm's human capital investment, because the firm's return from the investment is expected to be larger, and the firm can benefit from the mobility constraint due to the firm-specific human capital, and thereby enjoy larger rent. Thus, when the firm loses a high productivity worker, it needs to find a replacement for the quitting worker at a thinner labor market because potential high-productivity job candidates at other firms are less likely to quit their current firm, and it needs to repeat the larger human capital investment to recover the lost productivity.

The potential correlation in the search and training costs poses an empirical challenge to the researcher, in teasing out the search cost effect and the training cost effect from each other, while the decomposition is expected to yield interesting managerial implications. This paper provides empirical evidence that suggests that both the search costs and the training costs hurt the firm when high productivity workers leave, by exploiting employer characteristics that are more likely to capture search costs than training costs (i.e., the unfilled vacancy proxy and the works council proxy), and employer characteristics that are more likely to capture training costs than search costs (i.e., the training costs coverage proxy and the complexity proxy). However, establishments characterized to have high search costs (training costs) by the proxies used in this paper may well have high training costs (search costs). While this paper contributes to the literature by providing evidence for the importance of replacement costs in an economy-wide scope, exploring the detailed components of the replacement costs, in specific contexts that provide interesting identification opportunities, would be an exciting venue for future research.

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Table I.1. Effect of Overall Worker Turnover on Future Growth

This table reports the effect of worker turnover on future establishment employment growth, using different measures of turnover rates. The column heads denote the measure of turnover rates used for each regression. For column (1), numbers of both voluntary turnover (i.e., quits) and involuntary turnover (i.e., firing) are counted in calculating the turnover rate measure. For column (2) and column (3), only voluntary turnover and involuntary turnover is counted, respectively. For column (4) and column (5), voluntary turnover is decomposed into mass voluntary turnover and non-mass voluntary turnover, by the volume of workers quitting in the same month. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Future Establishment Employment Growth				
Coefficient	-0.017	-0.069	-0.011	-0.252	-0.039
Std. Error	(0.024)	(0.061)	(0.029)	(0.182)	(0.060)
Regressor: Turnover by Type	All Turnover	Voluntary Turnover	Involuntary Turnover	Mass Voluntary Turnover	Non-mass Voluntary Turnover
Estab. Quality Dummies	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	5241	5241	5241	5241	5241
R2	0.263	0.263	0.262	0.264	0.263

Table I.2. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using equation (1). Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Different wage measures are used in constructing the wage distribution, as denoted in the fourth row. "Imputed Wage" denotes imputed wage using Tobit models to recover the non-censored wage values, "Raw Wage" denotes the wage as given in the raw data, and "Worker Human Capital" denotes wage after differencing out the establishment component of the worker's wage using the decomposition of Abowd, Kramarz, Margolis (1999). For columns (4) - (9), voluntary turnover (i.e., worker quits) is decomposed into mass voluntary turnover and non-mass voluntary turnover, by the volume of workers quitting in the same month. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable	Future Establishment Employment Growth								
Turnover Rate:	-0.292***	-0.293***	-0.185*	-0.631**	-0.631**	-0.531***	-0.236**	-0.237**	-0.146
High Productivity Worker	(0.099)	(0.099)	(0.100)	(0.265)	(0.264)	(0.189)	(0.099)	(0.099)	(0.113)
Turnover Rate:	0.106	0.107	0.053	0.191	0.195	0.028	0.100	0.101	0.072
Low Productivity Worker	(0.109)	(0.108)	(0.105)	(0.389)	(0.391)	(0.368)	(0.100)	(0.100)	(0.096)
Turnover Type	Voluntary Turnover			Mass Voluntary Turnover			Non-mass Voluntary Turnover		
Marginal Productivity Measure	Imputed Wage	Raw Wage	Worker Human Capital	Imputed Wage	Raw Wage	Worker Human Capital	Imputed Wage	Raw Wage	Worker Human Capital
Estab. Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5241	5241	5241	5241	5241	5241	5241	5241	5241
R2	0.266	0.266	0.264	0.265	0.265	0.264	0.264	0.264	0.263
P-Value (H≠L)	0.0212	0.0204	0.143	0.147	0.147	0.228	0.0397	0.0381	0.193

Table I.3. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Instrumental Variables

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using hiring intensity at the focal firm's local labor market as instrumental variables. Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. Columns (1) and (2) report the first-stage estimation results, and column (3) reports the second-stage estimation results. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	First Stage		Second Stage
	(1)	(2)	(3)
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Establishment Employment Growth
Inflow Rate: High Productivity Worker	0.203***	0.130***	
	(0.025)	(0.027)	
Inflow Rate: Low Productivity Worker	0.029***	0.088***	
	(0.009)	(0.010)	
Turnover Rate: High Productivity Worker			-1.107**
			(0.466)
Turnover Rate: Low Productivity Worker			1.239***
			(0.450)
Estab Quality Dummies	Yes	Yes	Yes
State-Ind (two-digit)-Year FE	Yes	Yes	Yes
First Stage F-stat	33.36	55.77	
N	5051	5051	5051
R2	0.488	0.550	0.199
P-Value (H≠L)			0.008

Table I.4. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: The Tenure Effect

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level and by the quitting worker's tenure at the establishment. Productivity level (tenure) is defined to be high if the worker's wage (tenure at the establishment) at the job-year is above the median in the distribution of wages (tenures) of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Different wage measures are used in constructing the wage distribution, as denoted in the fifth row. "Imputed Wage" denotes imputed wage using Tobit models to recover the non-censored wage values, "Raw Wage" denotes the wage as given in the raw data, and "Worker Human Capital" denotes wage after differencing out the establishment component of the worker's wage using the decomposition of Abowd, Kramarz, Margolis (1999). For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)
Dependent Variable	Future Establishment Employment Growth		
Turnover Rate: Long Tenure-High Productivity Worker	-0.680***	-0.681***	-0.514**
	(0.258)	(0.259)	(0.230)
Turnover Rate: Long Tenure-Low Productivity Worker	0.304	0.310	0.107
	(0.543)	(0.546)	(0.582)
Turnover Rate: Short Tenure-High Productivity Worker	-0.102	-0.105	-0.048
	(0.120)	(0.120)	(0.139)
Turnover Rate: Short Tenure-Low Productivity Worker	0.066	0.068	0.049
	(0.097)	(0.096)	(0.091)
Marginal Productivity Measure	Imputed Wage	Raw Wage	Worker Human Capital
Estab Quality Dummies	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes
N	5241	5241	5241
R2	0.268	0.268	0.265
P-Value (Long Tenure H≠ Short Tenure H)	0.0673	0.0692	0.129

Table I.5A. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Search Costs and Training Costs

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using subsamples of establishments with high replacement costs and subsamples of establishments with low replacement costs. Replacement costs proxies used for subsampling the data are as denoted in the third row: columns (1) and (2) use industry-level propensity of having unfilled vacancies, and columns (3) and (4) use industry-level propensity of the establishment covering their employees' training costs (in part or in whole). Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable	Future Establishment Employment Growth			
Turnover Rate: High Productivity Worker	-0.318***	-0.117	-0.434***	-0.279**
	(0.117)	(0.211)	(0.142)	(0.140)
Turnover Rate: Low Productivity Worker	0.116	0.104	0.096	0.091
	(0.132)	(0.200)	(0.168)	(0.122)
Replacement Costs Proxy	Propensity of Having Unfilled Vacancies		Propensity of the Establishment Covering Training Costs	
Subsample	High	Low	High	Low
Estab Quality Dummies	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes
N	3571	1480	3178	1812
R2	0.268	0.330	0.263	0.359

Table I.5B. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Other Characteristics that Reflect Replacement Costs

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using subsamples of establishments with high replacement costs and subsamples of establishments with low replacement costs. Replacement costs proxies used for subsampling the data are as denoted in the third row: columns (1) and (2) use the industry-level average share of workers with college or higher educational attainment in the workforce, columns (3) and (4) use the industry-level average share of R&D workers in the workforce, columns (5) and (6) use the industry-level average complexity in operation measured by the number of unique occupations, and columns (7) and (8) use the existence of works council. Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Future Establishment Employment Growth							
Turnover Rate: High Productivity Worker	-0.460***	-0.244*	-0.545***	-0.120	-0.340*	-0.222*	-0.495***	-0.210
	(0.168)	(0.126)	(0.165)	(0.116)	(0.191)	(0.123)	(0.175)	(0.145)
Turnover Rate: Low Productivity Worker	0.077	0.148	-0.187	0.143	-0.064	0.146	0.349	-0.058
	(0.192)	(0.091)	(0.213)	(0.098)	(0.167)	(0.109)	(0.246)	(0.105)
Replacement Costs Proxy	High Educational Attainment Share		R&D Worker Share		Complexity		Works Council Exists	
Subsample	High	Low	High	Low	High	Low	Yes	No
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3025	1920	2695	2258	2358	2660	3063	1673
R2	0.272	0.351	0.323	0.319	0.303	0.315	0.289	0.388

Table I.6A. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Search Costs and Training Costs using Instrumental Variables

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using two-stage least squares estimation with hiring intensity at the focal firm's local labor market as instrumental variables, for subsamples of establishments with high replacement costs and for subsamples of establishments with low replacement costs. Replacement costs proxies used for subsampling the data are as denoted in the third row: columns (1) and (2) use industry-level propensity of having unfilled vacancies, and columns (3) and (4) use industry-level propensity of the establishment covering their employees' training costs (in part or in whole). Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. First Stage F-stat refers to the first-stage F-statistics. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)	(2)	(3)	(4)
Dependent Variable	Future Establishment Employment Growth			
Turnover Rate: High Productivity Worker	-0.834*	-1.255	-1.323*	-0.656
	(0.467)	(1.584)	(0.687)	(0.527)
Turnover Rate: Low Productivity Worker	0.853*	2.094	0.955*	1.213**
	(0.471)	(1.507)	(0.573)	(0.589)
Replacement Costs Proxy	Propensity of Having Unfilled Vacancies		Propensity of the Establishment Covering Training Costs	
Subsample	High	Low	High	Low
Estab Quality Dummies	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes
First Stage F-stat: High Productivity Worker	23.48	34.54	24.88	46.02
First Stage F-stat: Low Productivity Worker	36.41	27.07	28.99	34.87
N	3474	1395	3097	1707
R2	0.247	0.202	0.227	0.308

Table I.6B. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Other Characteristics that Reflect Replacement Costs using Instrumental Variables

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level, using two-stage least squares estimation with hiring intensity at the focal firm's local labor market as instrumental variables, for subsamples of establishments with high replacement costs and for subsamples of establishments with low replacement costs. Replacement costs proxies used for subsampling the data are as denoted in the third row: columns (1) and (2) use the industry-level average share of workers with college or higher educational attainment in the workforce, columns (3) and (4) use the industry-level average share of R&D workers in the workforce, columns (5) and (6) use the industry-level average complexity in operation measured by the number of unique occupations, and columns (7) and (8) use the existence of works council. Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. First Stage F-stat refers to the first-stage F-statistics. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Table I.6B. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Other Characteristics that Reflect Replacement Costs using Instrumental Variables (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Future Establishment Employment Growth							
Turnover Rate: High Productivity Worker	-1.432**	-1.189	-1.694*	-0.539	-1.754**	-0.813	-1.309*	-0.414
	(0.703)	(0.871)	(0.949)	(0.421)	(0.872)	(0.575)	(0.694)	(0.648)
Turnover Rate: Low Productivity Worker	0.830	1.572**	1.487	0.709*	1.491*	1.279**	1.804**	0.525
	(0.570)	(0.796)	(0.965)	(0.363)	(0.793)	(0.621)	(0.814)	(0.503)
Replacement Costs Proxy	High Educational Attainment Share		R&D Worker Share		Complexity		Works Council Exists	
Subsample	High	Low	High	Low	High	Low	Yes	No
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat: High Productivity Worker	31.70	15.37	27.81	23.31	10.90	62.88	20.31	41.91
First Stage F-stat: Low Productivity Worker	28.98	26.75	40.12	28.92	28.76	33.60	41.90	26.80
N	2930	1825	2608	2157	2325	2509	3020	1544
R2	0.253	0.217	0.237	0.301	0.191	0.245	0.246	0.355

Table I.7. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Industrial Distance of Job Transition

This table reports the effect of voluntary worker turnover (i.e., worker quits) on future establishment employment growth by the quitting worker's productivity level and by the industrial distance between the quitting worker's source establishment and destination establishment (i.e., whether the destination establishments is a rival or non-rival establishment). Productivity level is defined to be high if the worker's wage at the job-year is above the median in the distribution of wages of workers at jobs with the same occupation code and industry code in the same year, and is defined to be low otherwise. Imputed wage using Tobit models to recover the non-censored wage values is used as the wage measure in constructing the wage distribution. The destination establishment is defined as a rival establishment if its three-digit industry code is the same as that of the source establishment, and is defined as a non-rival establishment otherwise. Column (1) reports estimation results using the full sample. Columns (2) – (7) are estimation results for the subsamples of establishments with high replacement costs and for the subsamples of establishments with low replacement costs. Columns (2) and (3) use industry-level propensity of having unfilled vacancies, columns (4) and (5) use the industry-level average share of workers with college or higher educational attainment in the workforce, and columns (6) and (7) use the industry-level average share of R&D workers in the workforce as the replacement costs proxies for subsampling the data. For all columns, non-parametric controls for establishment quality and state-industry-year fixed effects are used. Standard errors are clustered by establishment. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Table I.7. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Industrial Distance of Job Transition (*continued*)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable	Future Establishment Employment Growth						
Turnover Rate: To rival establishment - High Productivity Worker	-0.337*	-0.467**	0.154	-0.614**	-0.069	-0.763**	-0.019
	(0.187)	(0.234)	(0.219)	(0.298)	(0.201)	(0.303)	(0.183)
Turnover Rate: To rival establishment - Low Productivity Worker	0.301	0.397	-0.061	0.420	-0.031	-0.034	0.237
	(0.270)	(0.330)	(0.303)	(0.377)	(0.223)	(0.284)	(0.234)
Turnover Rate: To non-rival establishment - High Productivity Worker	-0.281*	-0.218	-0.367	-0.328	-0.351	-0.394*	-0.248
	(0.156)	(0.186)	(0.364)	(0.218)	(0.240)	(0.225)	(0.236)
Turnover Rate: To non-rival establishment - Low Productivity Worker	-0.019	-0.091	0.178	-0.223	0.229*	-0.269	0.081
	(0.136)	(0.188)	(0.251)	(0.212)	(0.118)	(0.250)	(0.140)
Replacement Costs Proxy	N/A	Propensity of Having Unfilled Vacancies		High Educational Attainment Share		R&D Worker Share	
Subsample	Full Sample	High	Low	High	Low	High	Low
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	5241	3571	1480	3025	1920	2695	2258
R2	0.267	0.270	0.332	0.275	0.352	0.324	0.320
P-Value (To rival establishment High Productivity ≠ To non-rival establishment High Productivity)	0.840	0.470	0.234	0.472	0.444	0.376	0.510

CHAPTER II

Trying Out Different Workers: Worker Firing, Match Quality, and Growth of Small Firms in Germany

1. Introduction

As small and young firms are the main vehicles of job creation (Haltiwanger, Jarmin, and Miranda, 2013), understanding the underpinnings of their productivity and growth is critical for nurturing the sources of employment growth in the economy. Managerial practices have drawn attention of economists as an important factor that drive heterogeneity in firm productivity (Syverson, 2011; Bloom, Sadun, Van Reenen, 2016). Managerial practices of small firms are of particular interest, as their effect on enhancing firm performance, is likely to stimulate job creation. Despite the widespread recognition of the importance of human resource management by business practitioners, there have yet been few studies that systematically explores how trying out different workers can affect the firm's performance. The current study aims to fill in this gap in the literature by exploring the link between firing practices, worker-firm idiosyncratic match quality, and firm growth of small firms.

Finding the right worker for the right task is a costly, yet important, aspect of human resource management in maximizing the growth potential of the firm. The right worker would be the worker who can contribute the maximum amount of marginal product to the firm, given the marginal cost of employing her. Labor economists have studied the worker's productivity contribution in terms of her human capital, which can be determined as a combination of many different dimensions, such as her innate ability, education, and past experience (e.g., Becker, 1962; Topel, 1991; Buchinsky et al., 2010; Hellerstein, Neumark, Troske, 1999; Fox and Smeets, 2011).

However, human capital is not the only source of variation for workers' productivity at the firm. Another potential factor that can affect the workers' productivity at the firm is the idiosyncratic match quality between the firm and the worker (Jovanovic, 1979a; Mortensen and Pissarides, 1994).[42] That is, even if the worker is highly qualified in terms of her human capital, she might not be the best fit for the firm. In an extreme case, "there are no 'good' workers and 'good' employers, but only good matches" (Jovanovic, 1979b). To say the least, given the quality of human capital, the firm confronts another problem of finding the best person that fits well with the firm (e.g., in the context of the firm's culture, organizational structure, compensation schemes).

Idiosyncratic match quality between firms and workers has been studied as a factor driving turnover (Jovanovic, 1979a), as a factor determining workers' wages (Woodcock, 2011; Card, Heining, and Kline, 2013), and also as an unobservable in wages when studying the return to tenure (Abraham and Farber, 1987; Topel, 1991).[43] However, to my knowledge, the importance of idiosyncratic match quality in firm performance has been largely underexplored.

[42] Match quality in this proposal is referring to the degree to which the firm and the worker are idiosyncratically well-combined, and is not referring to the degree of assortative matching between firms and workers (i.e., the matching between high paying firms and high quality workers).

[43] Woodcock (2011) estimates that 16% of variation in log earnings is explained by match quality, using the Longitudinal Employer-Household Dynamics (LEHD) database.

Given that past studies examine the positive correlation between match quality, turnover, and wages of workers, and given that wages are positively correlated with worker productivity (Hellerstein et al., 1999), one can hypothesize positive correlation between match quality, worker turnover as a firm choice to improve match quality, and firm growth. Match quality can be a factor for firm growth when it is a component of the firm's labor productivity.

Match quality is, in an extreme case, an experience good (Jovanovic, 1979a) that can only be revealed ex-post of hiring, and is not known ex-ante, to both the firm and the worker. That is, firms can learn the match quality only by hiring the worker, and seek a better match quality only by firing a low match quality worker and hiring a new worker. In that regard, the firm's firing of workers can be viewed as a management practice of employer learning to improve match quality.[44]

This paper explores how firing at small firms in Germany is related to changes in its match quality with its workforce and growth. In particular, I examine how firing practices of small firms are associated with their future within-firm match quality dispersion and their future employment growth, using the German employer-employee linked data ("Linked Employer-Employee Data from the IAB", hereafter referred to as the LIAB). Assuming that the worker's match quality with the firm is unknown to the firm ex-ante of hiring, I predict that small firms that use firing more intensively will enhance their match quality, and therefore will experience stronger growth in employment in future periods, as firms with better idiosyncratic match quality should be more productive.

[44] An alternative for the firm can be to adjust the wage after learning the worker's match quality. However, wages are known to be downwardly rigid, and the hypothesis development section illustrates how the idiosyncratic nature of match quality can preclude wage reduction.

I further assume that uncertainty in the worker's match quality is even larger for workers with few past job experiences (e.g., the firm would be more efficient in knowing their match quality with a worker who has previous job experiences, as her previous job experience could reveal the worker's preference that would help predict the match quality ex-ante of hiring; moreover, the worker could also learn which type of firms would have a better fit with her through experiencing different firms, and therefore later jobs in her career can be expected to have higher match quality), and thereby predict that firing of workers with few past experiences has a stronger relation to the firm's future match quality and growth.

I test these predictions on small firms in Germany, as the German context is well suited to study the relation between worker firing and growth of small firms: firms with 10 or less employees are excluded from the Protection Against Dismissal Act (hereafter denoted as PADA) in Germany from 2004.[45] That is, firing is largely a firm choice for those firms, whereas government regulation may distort firing decisions for larger firms, and therefore, the implications of firing for small firms as a means to improve match quality would be less prone to be influenced by job security regulations.[46]

The richness of the LIAB allows me to overcome three empirical challenges in empirically testing these predictions: 1) estimating the match quality at the worker-firm level, and constructing a firm-level measure of match quality – I estimate the component of each

[45] I examine the relation between current period firing intensity and future period growth, and thereby not censor firm growth in my estimation sample by the employment threshold of 10 employees.

[46] There is another benefit of studying small firms for examining match quality and firm growth. That is, I expect that the significance of idiosyncratic match quality for firm performance would be greater for small firms than large firms. The assortative matching between firms and workers (i.e., the matching between high paying firms and high quality workers (Brown and Medoff, 1989), which is a different kind of matching from idiosyncratic matching), shown to be present in the German economy in Card et al. (2013), suggests that, in general, workforce in large firms is of high human capital level, and that of small firms is of low human capital level. Thus, when decomposing worker productivity into the human capital component and the idiosyncratic match quality component, the match quality component is likely to be relatively more important in the human resource management of small firms, as its relative contribution to firm productivity would be larger for small firms.

worker's wage that can be attributed to the match quality between the worker and the firm, and construct a measure of dispersion of this estimate within the firm; 2) constructing a measure of firing that is not contaminated by worker quits, and that is not driven by product market shocks; 3) measuring the human capital of the fired workers and controlling for it, as it could confound the empirical analysis. Unique features of the LIAB, discussed in detail in the data and empirical methodology section, allows me to overcome each of these challenges.

I find that more intensive firing is associated with smaller dispersion in the firm's future match quality, and that this association is stronger for firing workers with few past job experiences. This relation holds for both firing of high human capital workers and firing of low human capital workers. The match quality improvement by firing is reflected in the firm's future growth. I find that firms that use firing more intensively experience stronger growth in the future. I also find that the positive relation between firing and growth is stronger for firms in industries where match quality is relatively more important (compared to the worker's human capital), and for firms in industries where trying out different workers is less costly.

By providing novel evidence on the relation between worker firing, match quality, and the firm's growth, this study contributes to the literature of management practices and firm productivity (Ichniowski, Shaw, and Prennushi, 1997; Black and Lynch, 2001; Haltiwanger, Lane, and Spletzer, 2007; Bloom, Sadun, Van Reenen, 2016). While past studies have investigated various dimensions of human resource management, the current study is the first to examine firing as a means to improve match quality. This paper is also related to the literature on job creation driven by small and young firms (Haltiwanger, Jarmin, and Miranda, 2013), and sheds light on the firm-level dynamics that underlies the relation between labor reallocation and employment growth (Davis and Haltiwanger, 2014). It also contributes to the relatively thin literature that explores the implications of employer learning about workers for the firm (Lazear, 1995; Burgess, Lane, and Stevens, 1998), compared to the larger body of literature on its implications for workers (Farber and Gibbons, 1996; Altonji and Pierret, 2001; Lange, 2007).

2. Hypothesis Development

I assume that a worker's productivity at a firm is determined by the worker's human capital and her idiosyncratic match quality with the firm. The worker's human capital is her productivity contribution to the firm due to her ability, experience, and education. The worker's match quality is her productivity contribution to the firm due to her fit with the firm, that cannot be explained by her human capital. I also assume that the worker's productivity is well reflected in her wage, and decompose the worker's wage into the human capital component and the match quality component, as the following AKM decomposition (Abowd, Kramarz, and Margolis, 1999; Woodcock, 2011; Card, Heining, Kline, 2013).

$$\ln\left(wage_{ijt}\right) = \theta_i + X'_{ijt}\beta + \psi_{j(i,t)} + \phi_{ij} + \varepsilon_{ijt} \quad (1)$$

where subscripts *i, j, t* is for worker, establishment, and year, respectively. θ denotes indicator variable for the worker, X denotes the worker's education and age, ψ denotes indicator variable for the firm, and ϕ denotes indicator variable for the worker-firm combination. In other words, the first two terms ($\theta + X\beta$) reflects the worker's human capital, the third term (ψ) is the wage component that is attributable to the firm (e.g., reflecting the firm's unique compensation policy), and the fourth term (ϕ) reflects the match quality between the worker and the firm.

While the worker's human capital can be screened by, and signaled to, the firm before hiring, the match quality between the worker and the firm is largely unknown to both the worker and the firm – it can only be predicted to the extent that workers and firms can be categorized into several types, and that particular combinations of worker types and firm types can be predicted to have a better match quality compared to other combinations.[47]

The firm hires a new worker with unknown match quality, and sets her match quality component of wage (ϕ) at 0 (which is the normalized mean at the labor market). Then the firm learns the match quality as it experiences the worker. When it is revealed that the match quality is high, the firm can increase ϕ to reflect the high match quality.[48] By doing so, the worker would also be less likely to move to a different firm and experiment further. When it is revealed that the match quality is low, on the other hand, then the firm can either lower ϕ to reflect her lower productivity contribution, keep her at $\phi = 0$, or fire her and try out a new worker. However, because the worker can move to a different firm and receive $\phi = 0$, the firm cannot effectively lower her wage. Moreover, hiring a new worker that has a possibility of high match quality provides option value of hiring a new worker (as in Lazear, 1995). In that regard, firms are worse off by keeping her at $\phi = 0$, compared to firing the worker, because the firm is giving up the option value of the potential new worker or because the firm would be paying the worker more than her productivity.[49]

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[47] There may exist heterogeneity in efficiency in screening match quality across firms. However, at firms with good matches due to better screening device (or, even by chance), workers would be less likely to be fired, while the firms would enjoy good employment growth and homogenously good match quality. On the other hand, if poor screening ability is associated with poor match quality, then more firing would indicate poor productivity and hence would be associated with weaker firm growth. Thus, the existence of screening heterogeneity would understate the overall firing, match quality, and growth relation I find in this paper. Furthermore, the empirical methodology of this paper controls for firm quality using firm size, pay level, and growth level, which is likely to be correlated with screening ability.

[48] This increase in wage would be smaller than the worker's additional productivity contribution due to the match quality, because otherwise there is no benefit of increasing the wage and keeping the worker (instead of hiring a new worker at the labor market) to the firm. This market power exists because ϕ is reset at zero for a newly hired worker at the labor market.

[49] There are several potential institutional factors and organizational factors that can distort this wage-bargaining process. For instance, minimum wage can differentially affect the firms and industries whose workforce's wage is bound by minimum wage. However, minimum wage was introduced to the German economy in January 1, 2015, which is out of range of the time period this paper studies. Second, collective bargaining can be another factor that can distort the wage bargaining process (44% of the observations in the estimation sample is subject to collective

Thus, the firm can improve its match quality, which leads to higher firm productivity, and thereby attain stronger future growth, by firing workers with poor match quality while keeping the workers with good match quality. This leads to the first hypothesis of the paper:

Hypothesis 1: Firms that use firing more intensely attain a workforce with better match quality and enjoy stronger growth in future periods.

While I assume that match quality can be learned by experiencing the worker after hiring, there can be certain type of firms that fit well with certain type of workers. For instance, some workers would fit well with firms that have flexible working hours, while other workers may be more productive at firms where working hours are fixed. Thus, past working experience at other firms can be a good screening or signaling device in predicting the fit between the worker and the firm, and the uncertainty in the match quality outcome would be larger for workers with few or no past working experiences.[50] Thus, one could expect that early career worker-firm

bargaining). Wage binding due to collective bargaining would suppress ϕ_{ij} while suppression in ϕ does not preclude the existence of good fit and bad fit in the employer-employee relationship. That is, there still will be good fit workers and bad fit workers, but this difference would not be reflected in their wages. Because these firms cannot use wage to adjust the goodness of the fit in match quality, they are more likely to keep bad fit workers, and lose good fit workers, *via quitting,* and firing itself would not be affected due to the suppression in ϕ and the wage bargaining process. If firing is constrained by collective bargaining in ways other than wage bargaining (e.g., in case that the collective bargaining overrides the legal exemption from dismissal protection for small firms), then firing would occur less for these firms (i.e., the firms would not be able to fire the bad fits), and would result in the coefficient estimates being biased towards zero. Furthermore, because collective bargaining in Germany is done at the industry-level, the empirical methodology of this paper using industry fixed effects at the two-digit level alleviates the concerns. Third, works council, a form of employee representation to management in the German economy, can be another factor that can distort the wage bargaining process. However, works council activities are often legally protected for firms with more than 20 employees, and only 1.7% of the observations in the estimation sample have works council. Finally, wage of family members at family firms may not necessarily reflect match quality. However, family members would not be subject to firing, and firing, match quality, and growth would have no relation for family members. Thus, the presence of family firms (which is not identifiable using the current data) in the estimation sample would bias the coefficient estimates towards zero.

[50] Moreover, workers themselves would learn their type and the type of firms that would have a good fit for them through experimenting with different firms early in their career.

combinations would have a lower likelihood of being a good match. In that regard, the expected benefit of firing workers would be larger for firing workers with few past experiences, and firing these workers would have a stronger association with future match quality and future growth of the firm. This leads to the second prediction of the paper:

Hypothesis 2: The association between firing and match quality, and the association between firing and future growth, would be stronger for firing workers with few past job experiences.

3. Data and Empirical Methodology

The LIAB is an unemployment insurance (UI) record based, worker-establishment-day level data of a sample of establishments across the German economy.[51] Sampling is done at the establishment level, and the LIAB holds employment and unemployment history records of every paid-worker employed to the sampled establishments in the years from 1999 to 2009.[52] Because the LIAB is an administrative dataset constructed from unemployment insurance records, concerns on measurement errors are largely alleviated.[53] Moreover, the LIAB holds rich information on job characteristics and worker characteristics useful for this study, which other employer-employee linked datasets (such as the Longitudinal Employer-Household Dynamics

[51] To be more precise, each observation in the LIAB is an employment episode of the worker recorded at the day level. Whenever there is a change in the employment episode (e.g., unemployment-to-employment or job-to-job transition), a new observation is recorded for the worker, and the date of the change is provided. For more information on the LIAB, see Alda, Bender, and Gartner (2005).

[52] These employees' employment and unemployment history are covered from 1993 to 2010.

[53] Wage is censored at the social security maximum in the LIAB. Using the population data of workers in Germany, Card et al. (2013) notes that "10% to 12% of male wage observations and 1% to 3% of female wage observations are censored each year." I follow the procedures in Card et al. (2013) and use Tobit models to impute wages that are top-coded.

(LEHD) data of the United States) lack.[54] Utilizing the richness of the LIAB, I construct outcome variables measures of firm growth and match quality, and a measure of firing intensity that reflects the firm's effort in trying out different workers. The estimation sample consists of small West German firms with 10 or less employees in the years from 2004 to 2008.[55] I discuss below in detail how I take advantage of the richness of the LIAB in constructing the variables and the estimation sample, and developing the empirical strategy to alleviate the empirical concerns that rise in studying the research question.

3.1. Match Quality Outcome Variable: A Measure of Worker-Firm Match Quality

I construct a measure of worker-firm match quality using an extension of the wage decomposition of Abowd, Kramarz, and Margolis (1999), as in equation (1). As in Card et al. (2013), I estimate equation (1) using OLS estimation with dummy variables: the worker component θ is estimated using worker dummies, the firm component (ψ) is estimated using firm dummies, and the match component (ϕ) is estimated using dummies for firm-worker combination. The worker's observable characteristics (X) include education and age.[56] I use ϕ in constructing a measure of the firm's match quality, assuming that 1) wage well reflects the worker's marginal product, and that 2) there exists wage dispersion across workers within the firm, that can be uniquely attributable to the worker-firm combination, and that cannot be explained by the worker's human capital.

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[54] In this study, job is defined as the worker-firm combination in a continuing job spell.

[55] The size and the year thresholds are determined so that the firms in the estimation sample are not subject to regulation on protection against dismissal in Germany.

[56] In particular, I follow Card et al. (2013), and include quadratic and cubic terms in age interacted with education dummies, and year dummies interacted with education dummies. Also, male and female workers are estimated separately, as in Card et al. (2013).

The match component ϕ is identified only up to a normalization within the worker and the firm, and therefore the estimated term itself cannot be directly compared across firms or across workers. However, it is a residual wage term that is comparable, conditioning on θ, ψ, and X. Thus, its dispersion within the firm would reflect how the worker is paid differently compared to other workers of the same level of θ, ψ, and X.

I use this firm-level wage dispersion as the measure of the firm's match quality. Following the discussion in the hypothesis development section, the firm can improve its match quality by firing workers with poor match quality while keeping the workers with good match quality. Therefore, these firms would attain a workforce that is relatively homogenous in its match quality with its workforce. On the other hand, firms that keep the poor match quality workers (and at the same time keep the good match quality workers) would have a workforce that has a larger dispersion in match quality across their workers. That is, a firm that puts more effort in improving match quality is likely to have a workforce with smaller dispersion in the estimated match quality in future periods, compared to firms who put less effort. Assuming that the firm would fire workers who are of poor match quality to improve the match quality of its workforce, I use the firm's dispersion in ϕ among its low match quality workers as my primary measure of match quality dispersion. That is, I measure the distance between the firm's 1st percentile and the 25th percentile in ϕ constructed at the firm-year level.[57] In a firm with mean level of employment of the estimation samples, this would be the distance in match quality between the lowest match quality worker and the second-lowest match quality worker. The

[57] This measure, by definition, requires the firm to have at least five employees, which restricts our estimation sample to firms of relatively larger size. Thus, I supplement this measure using the within-firm standard deviation in ϕ constructed at the firm-year level. As can be seen in the estimation tables, the mean number of firm employment (denoted as "Sample Mean Employment" in the tables) of the estimation sample that examines the low match quality dispersion is larger (which is at around 6 employees in Table II.2) than the mean number of firm employment of the estimation sample that examines the standard deviation in ϕ (which is at around 4 employees in Table II.2).

outcome variable is the relative change in the dispersion measure during the following year, scaled by the mean of current year and following year dispersion, (i.e., it is growth in the dispersion measure):

$$match - improvement_{j,t+1} = \frac{dispersion(\phi)_{j,t+1} - dispersion(\phi)_{j,t}}{0.5 * (dispersion(\phi)_{j,t+1} + dispersion(\phi)_{j,t})}$$

3.2. Growth Outcome Variable: Future Employment Growth

I measure the firm's performance using the firm's employment growth. Following Davis, Haltiwanger, Schuh (1996*)*, employment growth is defined as change in employment in the following year compared to the current year, scaled by the average of the current and following year employment. That is,

$$employment - growth_{j,t+1} = \frac{E_{j,t+2} - E_{j,t+1}}{0.5 * (E_{j,t+2} + E_{j,t+1})}$$

where $E_{j,t}$refers to firm's employment as of January of the year *t*.[58]

3.3. Constructing the Independent Variable I: Distinguishing Firing Events from Quitting Events

It is difficult to construct a measure of the firm's firing intensity when using conventional firm-level data. Turnover measures (i.e., measure of workers quitting the firm and workers being fired from the firm) are often reported as an aggregate in conventional firm-level data. Because quitting is the worker's choice, whereas firing is the firm's choice, and because it is unclear how these two choices may or may not be correlated to each other, an aggregate measure of worker turnover would be inadequate in studying the research question of this paper. That is, the

[58] If the firm entered in the year *t*, then it E_t would refer to the employment as of the first month of the firm.

noisiness of the aggregate measure would be problematic because worker quitting does not reflect firm choice that aims to improve match quality.[59]

The LIAB provides a unique opportunity to overcome this measurement issue. In particular, it holds day-level information on the timing of accession and separation, and the timing of receiving benefits. In the German unemployment insurance system, there is a twelve-week sanction period for receiving benefits if the worker voluntarily separated (i.e., quitted) from her job. Because the LIAB holds employment and unemployment records on a daily basis, I can distinguish between voluntary turnover (i.e., worker quitting) and involuntary turnover (i.e., worker firing) for each worker at the time of separation. Specifically, I define a worker separation to be voluntary if I observe an interval longer than twelve weeks between the timing of unemployment and the receipt of benefits, or, if there is no unemployment spell between jobs. Otherwise, the separation event is defined as involuntary. I further refine this measure of worker firing by change in wages between jobs. Among the worker turnover observations that have been coded as worker quits by the two criteria above, those with more than 10% decrease in wage are re-coded as fired events.[60]

3.4. Constructing the Independent Variable II: Firing as Effort to Improve Match Quality vs Mass-Layoffs

The LIAB also allows me to alleviate the endogeneity concerns that rises from involuntary turnover being largely driven by product market shocks (and labor market shocks), which would be correlated with firm growth. In addition to using state-year-industry fixed effects

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[59] Hiring measures suffer from the same measurement issue, because the researcher cannot distinguish between hiring as a replacement of the worker who voluntarily left the firm and that as a replacement of a fired worker.

[60] I use the change-in-wage refinement in defining firing events, because the benefits sanction period also applies to workers who have been fired due to their behavior violating their employment contract.

to control for local product market and local labor market shocks, I introduce a novel aspect to this study noting that firing as a reaction to negative product market shocks are often in the forms of mass-layoffs. In particular, I construct a measure of firing intensity that would reflect the firm's effort to improve its match quality, by examining when mass-layoffs occur at the firm-month level, and excluding firing that occurred in those months for each firm. I define a month to be a mass-layoff month for a firm when more than 30% of employment of the firm are fired in that month.

3.5. Constructing the Independent Variable III: High Human Capital Workers and Low Human Capital

Wage dispersion due to match quality can be different between high human capital workers and low human capital workers. For instance, the labor market for low human capital workers may be competitive, which would not allow wage dispersion across workers with similar observable characteristics, and therefore match quality can be less significant to firm performance. On the other hand, the labor market for high human capital workers may not be perfectly competitive and be with more friction (e.g., Acemoglu and Pischke, 1999), where wider wage dispersion exists, and match quality could have a larger significance for firm performance. In this respect, firing high human capital workers and low human capital workers can have different associations to the firm's growth and match quality, and the worker's human capital level may be correlated with firing intensity. I therefore construct measures of firing of high human capital workers and firing of low human capital workers separately.

I first estimate the human capital of each job-year (i.e., worker-firm-year combination) using the wage decomposition of equation (1): for each job-year, it is the sum of the predicted value of the worker component and the predicted value of the observed worker characteristics ($\theta + X\beta$). Using this estimated worker human capital measure, I then determine whether each job-year is of high human capital or of low human capital, by comparing the estimated human capital at the job-year with the estimated human capital of other job-year combinations that are of the same occupation in the same industry. If the worker's human capital at the job -year is above the median in the distribution of human capital of job-years whose establishment has the same three-digit industry code and whose occupation has the same occupation code, then the worker is categorized as a high human capital worker for that specific job- year.[61] Otherwise, she is categorized to be a low human capital worker for that specific job-year.

This approach of using buckets of human capital, rather than the human capital value itself (i.e., the predicted value of the decomposed wage), alleviates the concern that wage can be a noisy measure of human capital, to the extent that ranks of human capital are not systematically overturned when using ranks of wages. Also, it is important to construct the worker's human capital measure within industry-occupation cells because the analysis is conducted across the German economy, and we expect wage levels to be heterogeneous across industries and occupations.

3.6. The Independent Variable: The Firm's Firing Intensity

Using the richness of the LIAB as described in the three subsections above, I construct a firm-year level measure of firing intensity of high human capital jobs and low human capital jobs that would reflect the firm's effort to improve its match quality with its workforce. For each human capital level, I count the workers who have been fired from the firm, in months when

[61] The occupation code in the LIAB has about 300 unique values.

mass-layoffs did not occur. I then scale these counts by the firm's average total employment in the past two years:

$$firing - intensity_{h,j,t} = \frac{non - mass - layoff\ fired\ workers_{h,j,t}}{0.5 * (E_{j,t} + E_{j,t-1})}$$

where subscripts h, j, t are for human capital, firm, and year, respectively.

3.7. The Early-Job Indicator Variable

As discussed in the hypothesis development section, I hypothesize that the benefit of firing would be larger for firing workers who are early in their career. Using the fact that the LIAB holds a complete employment and unemployment history record of the workers, I define a job to be an early job if it is the first or second job of the worker in her employment history, and construct an early job indicator variable accordingly.

Because the LIAB covers the workers' employment history from 1993, workers who have started their employment history before 1993 are left-censored. I define a worker to be left-censored if the worker's job tenure is continuing as of January 1, 1993. I then construct another early job indicator variable that is defined only for workers who are not left-censored: it is defined to be 1 if it is the first or second job of a non-left censored worker, and 0 if it is a third or later job of a non-left-censored worker.

3.8. The Estimation Sample and the Empirical Model

The estimation sample is constructed at the firm-year level, by collapsing the LIAB after measuring the events of firing, human capital, and match quality at the worker level.[62] It consists

[62] The worker level data preparation is done after restricting the sample to full-time jobs. I also drop secondary jobs when there are simultaneous job spells.

of the LIAB single-unit small firms (i.e., firms with 10 or less employees) located in West Germany from 2004 to 2008.[63]

As discussed in earlier subsections, the dependent variables are measured in post-year values because firing intensity is a measure of firm effort to improve match quality, and improvement in match quality and growth are expected to be attained in a future period when new workers are hired as replacements.

The baseline specification model is:

$$\begin{aligned} Y_{j,t+1} = \alpha + \beta_1 firing - intensity_{H,j,t} + \beta_2 firing - intensity_{L,j,t} \\ + I\{firm\ size * median\ wage * growth\}_{j,t} \\ + I\{state * industry\ (two - digit) * year\}_{j,t} + \varepsilon_{j,t+1} \qquad (2) \end{aligned}$$

where *j* refers to firm, *t* refers to year, *H* refers to high human capital level of the fired workers, and *L* refers to low human capital level of the fired workers.

Firm quality can be correlated with worker turnover and firm performance. For instance, there can be assortative matching between the worker and the firm (i.e., matching between high paying firms and high quality workers – Brown and Medoff, 1989), which can be correlated with future growth and firing intensity. Poor quality firms are more likely to be matched with poor quality workers, and thus are more likely to fire workers frequently and experience poor performance. I thus non-parametrically control for firm quality at the firm-year level. $I\{firm\ size * median\ wage * growth\}_{j,t}$ denotes the set of dummy variables for each bin determined by the combination of firm size (whether the firm has 5 or less employees), the

[63] Single-unit firms refer to firms with a single geographic location. Although the LIAB is an establishment-level dataset, and does not have firm identifiers, single-unit flags are available in the IAB Establishment Panel (hereafter IABBP), which is linked to the LIAB. I use this single-unit flag to determine that the establishment is a small firm.

firm's median wage quartile group, and the firm's current-year growth quartile group.[64] $I\{state * industry\ (two - digit) * year\}_{j,t}$ denotes the set of dummy variables defined by the interaction terms of state dummies, two-digit industry code dummies, and year dummies used to control for local product market shocks and local labor market shocks.[65] Standard errors are clustered by firms.

4. Estimation Results

4.1. The Baseline Estimation: Firing Intensity, Match Quality, and Firm Growth

I first examine whether firing intensity is indeed a managerial practice that varies by firms, by regressing the firing intensity variables on the firm dummies, and by regressing the firing intensity variables on the industry (two-digit) industry dummies. Table II.1 reports the estimation results. Each row uses different counts of the fired workers at the firm-year. The first row uses the aggregate number of high human capital workers fired; the second row uses the aggregate number of low human capital workers fired; the third row uses high human capital workers fired only during non-mass-layoff months; the fourth row uses low human capital workers fired only during non-mass-layoff months.

In column (1) and column (2), the adjusted R-squared values for the firm dummy regressions, and the adjusted R-squared values for the industry dummy regressions, are reported respectively. We observe that regardless of the firing intensity measure being used and for all human capital levels, the variation in firing intensity explained by the firm dummies are much larger that explained by the industry dummies. In column (3) and column (4), the standard

[64] That is, it denotes the dummy variables for the quartile group in which the firm's median wage (and current-year growth) falls into, in the distribution of median wages (and current-year growth) of firms.

[65] "State" refers to the German federal state (Bundesland).

deviations of the coefficient estimates of the firm dummies, and those of the industry dummies are reported, respectively. We observe that the standard deviations are larger for the firm dummy estimates compared to the industry dummy estimates. These estimation results suggest that firing intensities vary across firms (than across industries), and thus firing intensity is a managerial practice (rather than an industry characteristic).

Given that firing intensity is a managerial practice, I then examine how this managerial practice is associated with match quality improvement at the firm, by regressing the post-year match-improvement variables on the firing intensity variables. The regression estimates are reported in Table II.2. For columns (1)-(3), the dependent variable is post-year growth in the standard deviation of the match component. For columns (4)-(6), the dependent variable is post-year growth in the distance between the 1st percentile and the 25th percentile of the match component (hereafter referred to as Low Match Quality Dispersion). Columns (1), (4) include all types of fired workers in calculating the firing intensity, column (2), (5) include workers fired during non-mass-layoff months only, and column (3), (6) include workers fired during mass-layoff months only.

In Panel A, I examine this relation using fired workers of either high human capital or low human capital in calculating the firing intensity. We observe that firing intensity is negatively associated with match quality dispersion growth, regardless of the dispersion measure being used. We also observe that firing that occurred in non-mass layoff months drive this relation: columns (2) and (5) have large coefficients estimates that are statistically significant, while columns (3) and (6) have small coefficient estimates that are not statistically significant. This suggests that firing as a means of trying-out different workers is statistically significantly associated with match quality improvement of the firm, while firing as a response to negative product market shocks does not.

In Panel B, firing intensity of high human capital workers and that of low human capital workers are used separately, as in equation (2). We observe estimates that are similar in pattern to those in Panel A. That is, firing intensity is negatively associated with match quality dispersion growth, for both high human capital workers and low human capital workers, and this negative relation is driven by worker firing that occurred during non-mass-layoff months.

So far, we have examined the firm's match quality improvement using match dispersion measured across all workers at the firm. In Table II.3, I examine this relation at a further disaggregated level: I repeat the regressions in Table II.2, after measuring the firm's match dispersion for high human capital workers at the firm and low human capital workers at the firm separately. Columns (1), (3), (5), (7) report estimation results using the growth in dispersion in high human capital workers' match quality as the dependent variable, and columns (2), (4), (6), (8) report estimation results using the growth in dispersion in low human capital workers' match quality as the dependent variable. For columns (1)-(4), the dependent variable is post-year growth in the standard deviation of the match component at the firm. For columns (5)-(8), the dependent variable is post-year growth in Low Match Quality Dispersion at the firm. The independent variables are the firm's firing intensity of high human capital workers and firing intensity of low human capital workers. Columns (1), (2), (5), (6) includes all types of fired workers in calculating the firing intensity, and columns (3), (4), (7), (8) includes workers fired during non-mass-layoff months only.

The estimates suggest that match quality of high human capital workers at the firm is improved by firing high human capital workers, and that match quality of low human capital workers is improved by firing low human capital workers. This relation is most strongly observed with firing of workers that occurred in non-layoff months: the coefficient estimates of high human capital worker firing intensity in columns (3) and (7), and the coefficient estimates of low human capital worker firing intensity in columns (4) and (8) are statistically significant and negative.

The match quality improvement we have observed thus far are reflected in the firm's future employment growth. In Table II.4, I repeat Table II.2 after replacing the post-year match quality improvement variables with the firm's post-year employment growth as the dependent variable. The format of the table is identical to that of Table II.2, except that the dependent variable is the firm's employment growth. Column (1) reports estimation results using aggregate number of fired workers, column (2) reports estimation results using workers fired during non-mass-layoff months only, and column (3) reports estimation results using workers fired during mass-layoff months only, in calculating the firm's firing intensity. Panel A reports estimation results using fired workers of either high human capital or low human capital in calculating the firing intensity, while Panel B reports estimation results using firing intensity of high human capital workers and firing intensity of low human capital workers separately.

Consistent with the analysis thus far, we observe that firing that occur in non-mass layoff months (estimation results reported in column (2)) have a positive relation to the firm's future growth. While the coefficient estimate of high human capital worker firing intensity (in column (2) of Panel B) is not statistically significant (this may be attributable to the possibility that trying out new high human capital workers, compared to trying out new low human capital workers, can be costlier to the firm; I explore further on this aspect in section 5), it is similar to the coefficient estimate of low human capital worker firing intensity (reported in column (2) of Panel

B) in magnitude. Another point to note is that the coefficient estimates of firing intensity using firing that occur in non-mass-layoff months (reported in column (2) of Panel A and Panel B) are much larger in magnitude than the coefficient estimate of firing intensity using firing that occur in mass-layoff months (reported in column (3) of Panel A and Panel B). Overall, the relation between firing and future growth is much weaker for firing that occur in mass-layoff months than for firing that occur in non-mass-layoff months.

There are several interesting points that we can draw from these estimation results. First, the results in Table II.4 suggest that firing as a mean of trying-out different workers is statistically significantly associated with stronger future growth of the firm, accompanied by the match quality improvement suggested by the estimates in Table II.2 and Table II.3. Second, they suggest that the negative association between firing and future growth are not likely to be attributable to the regression-to -mean effect that is pervasive in firm growth (Davis, Haltiwanger, Schuh (1996)): regression-to-the-mean in growth would occur as a response to the change in the firm's labor demand, which is expected to be salient in the relation between mass-layoff and future growth than in the relation between non-mass-layoff firing and future growth.[66]

4.2. Trying-Out Early Career Workers vs Late Career Workers

The analysis in subsection 4A suggests that trying out different workers can improve the match quality between the firm and its workforce, and thereby enhance the firm's growth. In this subsection, I examine whether trying out workers who are early in their career is more effective than trying-out other workers. I conduct this analysis by decomposing the firing intensity

[66] Futhermore, if regression-to-the mean effect in growth is driving the negative association between firing intensity and future growth, then it is difficult to explain the decrease in match quality dispersion we observe in Table II.2 and Table II.3. Also, the growth measure used in this paper mitigates the concern about the regression-to-the-mean effect (Davis, Haltiwanger, Schuh (1996)).

measures, using the indicator variables for early jobs described in section 3. That is, firing intensity of high (low) human capital workers is decomposed into firing intensity of high (low) human capital workers whose employment at the firm was the first or second job in her employment history (hereafter referred to as early job), and firing intensity of high (low) human capital workers whose employment at the firm was the third or later job in her employment history (hereafter referred to as late job). Then I use the four resulting firing intensity measures instead of the two firing intensity measures used in Tables 2 - 4, and repeat the analysis.

The match quality improvement regression results are reported in Table II.5. The dependent variable is growth in Low Match Quality Dispersion in Panel A and in Panel B, and it is growth in the standard deviation of match quality in Panel C and in Panel D. Panel A and Panel C include all workers at the firm in counting the number of fired workers, while Panel B and Panel D include only workers who are not left-censored in counting the number of fired workers.

The match improvement measures (i.e., the dependent variables) are calculated using all workers employed at the firm in columns (1), (4); they are calculated using only high human capital workers in columns (2), (5); and they are calculated using only low human capital workers in columns (3), (6). Columns (1) - (3) include all fired workers in calculating firing intensity (i.e., the regressors), while columns (4) - (6) include workers fired during non-mass-layoff months only in calculating firing intensity.

The estimates are similar in pattern with the estimation results in Table II.3, while highlighting the stronger relation between firing intensity and match quality improvement for firing workers in early jobs. The estimation results in columns (2), (3), (5), (6) suggest that match quality of high (low) human capital workers at the firm is improved by firing high (low) human capital workers. The magnitudes of the coefficient estimates are mostly larger for firing of early job workers than for firing of late job workers, which suggests that firing early job workers is more effective in improving the match quality, as hypothesized.[67]

The growth regression results are reported in Table II.6. The dependent variable is the firm's future employment growth as in Table II.4, and the independent variables are the four firing intensity measures as in Table II.5. Columns (1), (2) include all types of fired workers in calculating the firing intensity, and columns (3), (4) include workers fired during non-mass-layoff months only in calculating the firing intensity. Columns (1), (3) include all workers at the firm in counting the number of fired workers, while columns (2), (4) include only workers who are not left-censored in counting the number of fired workers.

The estimation results indicate that firing early job high human capital workers and firing late job high human capital workers are similarly associated with the firm's future growth. On the other hand, the association between firing low human capital workers and firm growth is much stronger for early job worker firing than late job worker firing: the coefficient estimates of early job low human capital worker firing are larger in magnitude and are statistically significant.

As a whole, the estimation results in Table II.5 and Table II.6 suggest that while trying out early job workers are more effective in improving the firm's match quality (for both high human capital workers and low human capital workers), the link between the larger match quality improvement and stronger firm growth is ambiguous for trying out high human capital workers. This set of results are also consistent with the statistically insignificant association between firing intensity and firm growth for high human capital workers exhibited in Panel B of

[67] Some of the coefficient estimates of high human capital worker firing in columns (3) and (6) are positive and statistically significant. One possible explanation for this result can be that the fired high human capital workers are replaced by low human capital workers of random match quality (and while holding firing of low human capital workers, presumably of low match quality, constant), which increases the dispersion in the match quality of low human capital workers.

Table II.4. Together, they suggest that the ramification of trying-out different workers on firm growth are affected by other factors than match quality improvement. In the next section, I investigate the industry heterogeneity in costs and benefits of trying out different workers, as an exploration on other factors that could modulate the relation between firing and growth.

5. Extension: Industry Heterogeneity in Replacement Frictions and the Importance of Match Quality

The previous section examined how trying out different workers can improve the firm's match quality with its workforce, and thereby enhance its future growth. In this section, I explore how the relation of firing and growth can be different by industry characteristics that can reflect the cost and benefit of trying out different workers.

Although the small firms examined in this paper are not subject to legal protection against worker dismissal, these firms still bear the cost of hiring a new worker in the process of trying out different workers. That is, costs in searching for a new worker at the labor market and costs in training the newly hired worker to have her equipped with the skills that the firm needs can diminish the value of trying out different workers. To examine how the industry heterogeneity in search costs and training costs (hereafter collectively referred to as replacement costs) modulates the relation between worker firing and future growth, I subsample the firms at the industry-level using proxies of replacement costs.

The search costs proxy is constructed using an indicator variable for the firm having unfilled vacancies that it was willing to fill in.[68] Having unfilled vacant positions that firms are willing to, but unable to, fill in would be an immediate indicator of the firm having higher search

[68] This indicator variable is available in the IABBP dataset, which is an establishment-year-level survey dataset that can be linked to the LIAB.

costs. Thus, firms in industries that more often have unfilled vacancies are more likely to have difficulty in finding a new worker, and the cost of trying out different workers would be higher for these firms.

The training costs proxy is constructed using an indicator variable for the firm having released its employees to receive training, and having covered the training expense in part or in whole.[69] Standard human capital theory argues that employers would bear the cost of their employee's human capital investment in part or in whole, if the employee's human capital is firm-specific (Becker, 1962). Thus, whether the firm covers the training costs (in part or in whole) of their employees would well reflect the firm-specificity of the workers' skills at the firm. The more firm-specific the skills that are required at the firm, the more costly it would be to try-out different workers as it would require training at the firm's expense. The cost of trying out different workers would be higher for firms in industries where the firm more often covers their employees' training costs.

I construct the replacement costs proxies as the three-digit industry mean of the firm-level variables discussed above.[70] That is, the search costs proxy is a measure of industry-level propensity of having unfilled vacancies, and the training costs proxy is a measure of industry-level propensity of the firm covering their employees' training costs (in part or in whole). I subsample the estimation sample using these proxies of replacement costs, and comparing the three-digit industry means to their median. That is, firms are subsampled into high or low replacement costs groups, by the median of the three-digit industry means.[71]

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[69] This indicator variable is also available in the IABBP dataset.

[70] There are 101 industries at the three-digit level in the estimation sample.

[71] The subsampling by industry is done at a finer level (three-digit industry classification) than the fixed effects (two-digit industry classification) used in the regressions.

The benefit of trying out different workers can be different by industries, as well. That is, match quality can be more important in terms of firm performance in some industries compared to others. For instance, the labor market of some industries may be more homogenous compared to others so that wage dispersion due to match quality is small, while the labor market of other industries may be more heterogenous so that there is wider wage dispersion due to match quality. The benefit of trying out different workers would be larger for firms in industries where the match quality component has a relatively wider dispersion (compared to the dispersion of the human capital component).

I examine this industry heterogeneity by constructing an industry-level proxy that would reflect the importance of match quality for firm performance (hereafter referred to as match-importance proxy). I use the wage decomposition in equation (1), and collect the human capital component and the match quality component of wage at the firm-year. I then calculate the ratio of the standard deviation of the match quality component at the firm-year to the standard deviation of the human capital component at the firm-year. Similar to the replacement costs proxies, the match-importance proxy is defined as the three-digit industry median of these firm-year-level values. This ratio would capture the relative importance of match quality compared to human capital at the industry-level. I then subsample the estimation sample using this match-importance proxy, and comparing the three-digit industry medians to their median. That is, firms are subsampled into high or low match quality importance groups, by the median of the three-digit industry medians.[72]

[72] Analysis using three-digit industry means, as opposed to medians, yields materially similar results (reported in Appendix 2 Table A2.2). However, the distribution of the means is much more skewed than the distribution of the medians, and thus I use the three-digit industry medians as the main proxy.

Another proxy for the benefits of trying-out different workers is industry growth. I assume that the benefits of trying-out different workers are larger at firms in growing industries, because these firms can expect a longer time horizon in extracting the benefits (Lazear, 1995). This proxy is constructed as the three-digit industry mean of firm-level employment growth. I then subsample the estimation sample using this growth proxy, and comparing the three-digit industry mean to their median. That is, firms are subsampled into high or low growth groups, by the median of the three-digit industry means.

Estimation results using the replacement costs proxies are reported in Table II.7. I repeat equation (2), after subsampling the estimation sample into firms characterized to have high replacement costs (whose estimation results are reported in the odd-numbered columns) and firms characterized to have low replacement costs (whose estimation results are reported in the even-numbered columns). The search costs proxy is used in columns (1) – (4) and the training costs proxy is used in columns (5) – (8). The dependent variable is employment growth, and the independent variables are the firing intensity measures of high human capital workers and the firing intensity measures of low human capital workers. Columns (1), (2), (5), (6) use all workers fired from the firm in calculating the firing intensity, and columns (3), (4), (7), (8) use workers fired only during non-mass-layoff months in calculating the firing intensity.

Overall, we observe that the coefficient estimates are larger for firms with low replacement costs than for firms with high replacement costs, for both high human capital worker firing and low human capital worker firing. Also note that the coefficient estimates of high human capital worker non-layoff firing are larger in magnitude than those of low human capital worker non-layoff firing at low-replacement costs firms (column (4) and column (8)), while they are not at high-replacement costs firms ((column (3) and column (7)).

One interpretation of these set of results is that higher replacement costs for high human capital workers undermine the match quality improvement effect of firing on growth. That is, trying-out different workers is costlier for high human capital workers because searching for, and training, a new worker requires more time and larger expense when replacing a high human capital worker.[73] The higher cost in trying out high human capital workers often mutes the match quality improvement effect, which is more noticeable for firms in high replacement costs industries.

Table II.8 reports the estimation results using the benefits proxies. The estimation sample is subsampled into firms that are expected to enjoy larger benefits from trying-out different workers (whose estimation results are reported in the odd-numbered columns) and those that are expected to enjoy smaller benefits (whose estimation results are reported in the even-numbered columns), using the match-importance proxy (columns (1) – (4)) and the industry growth proxy (columns (5) – (8)). The format of the table is identical to that of Table II.7, except for the subsampling proxies being used.

We observe that the firing and growth relation is stronger for firms characterized to have larger benefits of trying-out workers, in particular for firing high human capital workers. This empirical pattern is notable, as the baseline results in Table II.4 displayed statistically insignificant relation between high human capital worker firing and growth, while the magnitude of the coefficient estimate was similar to that of low human capital worker firing (column 2 of Table II.2 Panel B). When the firm expects larger benefits from trying out different workers, perhaps because match quality is relatively more important (compared to the worker's

[73] One explanation for the higher replacement costs for high human capital workers can be the larger moving costs that rises from the worker's productivity being tied to the firm (Acemoglu and Pischke, 1999), which could lead to thinner labor markets for high human capital workers.

human capital) in the firm's industry or because the firm expects to enjoy the higher match quality for a longer time horizon, then the positive effect of trying out high human capital workers can be larger than that of trying out low human capital workers. This is consistent with the labor market of low human capital workers being more competitive, and therefore the firm enjoying larger rent in the employment relation with its high human capital workers than its low human capital workers.

The results reported in Table II.7 and Table II.8 collectively suggest that the heterogeneity in the costs and benefits of trying out different workers can be an important factor that managers should consider in assessing how effective trying out different workers can be on their firm's growth.

6. Conclusion

This paper studies how firing, worker-firm match quality, and the firm's growth are related. I find empirical evidence that support the hypothesis that firms that try out different workers by firing low match quality workers can improve its match quality with its workforce, and thereby achieve stronger future growth. As the uncertainty in the match quality would be larger for workers with few past job experiences, this relation is stronger for firing workers whose current job is the first or second job in her employment history. Further, I find that the growth effect of firing workers is larger for firms in industries where replacing works is less costly, and for firms in industries where the relative dispersion in the match quality component of wage (compared to the dispersion in the human capital component of wage) is larger.

The findings of this paper suggest that different human resource management can lead to different firm performance (Syverson, 2011), and that managers would need to be concerned about the fit between the workers and the firm, as firms that actively seek improvement in match quality can achieve stronger future growth. This study also suggests that trying out different workers would be more valuable for firms in industries where there is more variation in match quality, which is assumed to reflect the significance of match quality to the firm's performance.

Measuring match quality would be the crucial empirical challenge in studying this research question. Assuming that match quality is a component of the worker's productivity reflected in the worker's wage, this paper takes a novel approach of using an individual worker's wage, and comparing it with her wage at other firms and with the wage of her colleagues at the firm, in constructing a match quality measure. As the measure is a relative measure from construction that adds up to zero when aggregated to the firm-level, the researcher cannot use its firm-level aggregate to compare match quality across firms. This paper examines the change in firm-level match quality dispersion while acknowledging that decrease in match quality dispersion may be a consequence of worker flows that may not necessarily be associated with improvement in match quality. That is, worker flows that lead to homogenous match quality (with either only low match quality workers or only high match quality workers) would yield a decrease in match quality dispersion. However, the empirical relations that this paper consistently documents, that is, non-layoff firing being negatively associated with growth in match quality dispersion, and it being positively associated with firm growth, suggests that the variation that this paper is capturing is likely to be that due to improved match quality.[74]

While the analysis of this paper uses industry heterogeneity in the importance of match quality, there can also be heterogeneity in the importance of match quality by firm size. It is plausible that the importance of match quality would be larger for smaller firms than larger firms,

[74] In fact, the possibility of worsened match quality through worker firing would bias the coefficient estimates towards zero.

as larger firms would often enjoy a larger pool of job candidates generally equipped with higher level of human capital: larger firms can be more flexible in increasing the human capital component of productivity contribution in their workforce. This flexibility would be even larger if larger firms have market power and thereby enjoy larger rent in the labor market. In this regard, attention to match quality in its workforce can be more significant for small firm growth.

Because small and young firms are drivers of job creation (Haltiwanger, Jarmin, and Miranda, 2013), the findings of this paper also have policy implications. By showing that small firms can experience faster growth by trying out different workers, this paper is related to the literature of how labor regulation affect growth and employment (Lafontaine and Sivadasan, 2009; Caballero et al., 2013), and suggests that policy that encourages labor reallocation can stimulate job creation and economic growth (Davis and Haltiwanger, 2014). This policy implication resonates further in the current economic state that is characterized by declining economic dynamism (Decker, Haltiwanger, Jarmin, and Miranda, 2014).

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Table II.1. Variation in Firing Intensity: Firm vs Industry (two-digit)

This table shows the variation in the firing intensity variable of the estimation sample, across firms and across industries (at two-digit level). Columns (1), (2) report the adjusted R-squared values of the regressing the firing intensity variable on the firm dummies and the industry dummies, respectively. Columns (3) and (4) report the standard deviations of the estimated coefficients of the firm dummies and the industry dummies, respectively.

	(1)	(2)	(3)	(4)
	Adjusted R2		Std. Dev.	
	Firm	Industry	Firm	Industry
Firing Intensity: High Human Capital, All Fires	0.7146	0.0325	0.2980	0.0684
Firing Intensity: Low Human Capital, All Fires	0.2737	0.0224	0.1819	0.0486
Firing Intensity: High Human Capital, Non-Layoffs	0.1725	0.0281	0.0806	0.0257
Firing Intensity: Low Human Capital, Non-Layoffs	0.2345	0.0158	0.1329	0.0318

Table II.2. Firing Intensity and Match Improvement

This table reports the estimation results of estimating equation (2), when the dependent variables are measures of match improvement. For columns (1)-(3), the dependent variable is growth in the standard deviation of the match component at the firm at year *t+1*. For columns (4)-(6), the dependent variable is growth in the distance between the 1st percentile and the 25th percentile of the match component at the firm at year *t+1*. The independent variables are the firm's firing intensity at year *t*, across all human capital levels in Panel A, and firing intensity of high human capital workers and firing intensity of low human capital workers in Panel B. Columns (1), (4) include all types of fired workers in calculating the firing intensity, column (2), (5) include workers fired during non-mass-layoff months only, and column (3), (6) include workers fired during mass-layoff months only. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

Panel A. Firing Intensity across All Human Capital Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Match Improvement: Standard Deviation			Match Improvement: Low Match Quality Dispersion		
Firing Intensity	-0.0458*	-0.1461**	-0.0030	-0.3123*	-0.8164***	-0.1115
	(0.0269)	(0.0610)	(0.0169)	(0.1718)	(0.1431)	(0.0915)
Firing Intensity Type	All	Non-Layoffs	Layoffs	All	Non-Layoffs	Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	4.4302	4.4302	4.4302	5.5480	5.5480	5.5480
N	2271	2271	2271	1261	1261	1261
R2	0.2448	0.2478	0.2433	0.3273	0.3455	0.3093

Table II.2. Firing Intensity and Match Improvement (*continued*)

Panel B. Firing Intensity of High Human Capital Workers and Low Human Capital Workers

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Match Improvement: Standard Deviation			Match Improvement: Low Match Quality Dispersion		
Firing Intensity: High Human Capital	-0.0422	-0.1656	-0.0333	-0.1581	-1.0395***	-0.0465
	(0.0333)	(0.1213)	(0.0274)	(0.1379)	(0.2495)	(0.0713)
Firing Intensity: Low Human Capital	-0.0763	-0.1527*	0.0674	-0.7636***	-0.7104***	-0.5025**
	(0.0606)	(0.0813)	(0.0782)	(0.1461)	(0.1846)	(0.2172)
Firing Intensity:	All	Non-Layoffs	Layoffs	All	Non-Layoffs	Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	4.4302	4.4302	4.4302	5.5480	5.5480	5.5480
N	2271	2271	2271	1261	1261	1261
R2	0.2455	0.2485	0.2439	0.3386	0.3467	0.3114

Table II.3. Firing Intensity and Match Improvement: Match Improvement by Human Capital Level

This table reports the estimation results of estimating equation (2), measuring match improvement for high human capital workers and low human capital workers separately for each firm-year combination. For columns (1)-(4), the dependent variable is growth in the standard deviation of the match component at the firm at year *t+1*. For columns (5)-(8), the dependent variable is growth in the distance between the 1st percentile and the 25th percentile of the match component at the firm at year *t+1*. The independent variables are the firm's firing intensity of high human capital workers and firing intensity of low human capital workers at year *t*. Columns (1), (2), (5), (6) includes all types of fired workers in calculating the firing intensity, and columns (3), (4), (7), (8) includes workers fired during non-mass-layoff months only. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Match Improvement: Standard Deviation				Match Improvement: Low Match Quality Dispersion			
	High HC	Low HC	High HC	Low HC	High HC	Low HC	High HC	Low HC
Firing Intensity: High Human Capital	-0.0348	0.0029	-0.2742**	-0.0343	-0.1255	0.3666	-1.9209***	0.8841
	(0.0290)	(0.2212)	(0.1380)	(0.2217)	(0.1179)	(0.6441)	(0.4751)	(0.6456)
Firing Intensity: Low Human Capital	0.0362	-0.1620**	0.1358	-0.2118**	-0.1383	-1.2339***	0.0356	-1.1661***
	(0.1079)	(0.0700)	(0.1397)	(0.0926)	(0.2078)	(0.3019)	(0.2021)	(0.3050)
Firing Intensity:	All	All	Non-Layoffs	Non-Layoffs	All	All	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	5.4939	5.0877	5.4939	5.0877	6.7638	6.0933	6.7638	6.0933
N	1140	1209	1140	1209	453	450	453	450
R2	0.3288	0.2982	0.3337	0.2991	0.3848	0.4796	0.4319	0.4692

Table II.4. Firing Intensity and Employment Growth

This table reports the estimation results of estimating equation (2), when the dependent variable is the firm's employment growth at year *t+1*. The independent variables are the firm's firing intensity at year *t*, across all human capital levels in Panel A, and firing intensity of high human capital workers and firing intensity of low human capital workers in Panel B. Column (1) includes all types of fired workers in calculating the firing intensity, column (2) includes workers fired during non-mass-layoff months only, and column (3) includes workers fired during mass-layoff months only. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

Panel A. Firing Intensity across All Human Capital Levels

	(1)	(2)	(3)
Dependent Variable	Employment Growth		
Firing Intensity	0.0334	0.0786***	0.0134
	(0.0233)	(0.0287)	(0.0221)
Firing Intensity Type	All	Non-Layoffs	Layoffs
Firm Quality Dummies	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes
Sample Mean Employment	3.6065	3.6065	3.6065
N	2818	2818	2818
R2	0.2160	0.2172	0.2144

Panel B. Firing Intensity of High Human Capital Workers and Low Human Capital Workers

	(1)	(2)	(3)
Dependent Variable	Employment Growth		
Firing Intensity: High Human Capital	0.0147	0.0760	0.0054
	(0.0283)	(0.0683)	(0.0276)
Firing Intensity: Low Human Capital	0.0633**	0.0812**	0.0342
	(0.0274)	(0.0363)	(0.0322)
Firing Intensity Type	All	Non-Layoffs	Layoffs
Firm Quality Dummies	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes
Sample Mean Employment	3.6065	3.6065	3.6065
N	2818	2818	2818
R2	0.2166	0.2171	0.2146

Table II.5. Firing Intensity and Match Improvement: Early Job vs Late Job

This table reports the estimation results of estimating equation (2), when the dependent variables are measures of match improvement. In Panel A and Panel B, the dependent variable is growth in the distance between the 1st percentile and the 25th percentile of the match component at the firm at year *t+1*. In Panel C and Panel D, the dependent variable is growth in the standard deviation of the match component at the firm at year *t+1*. Match dispersion is calculated using all workers employed at the firm in columns (1), (4), it is calculated using only high human capital workers in columns (2), (5), and it is calculated using only low human capital workers in columns (3), (6). The independent variables are the firm's firing intensity of high human capital workers whose job is an early job, firing intensity of high human capital workers whose job is a late job, firing intensity of low human capital workers whose job is an early job, and firing intensity of low human capital workers whose job is a late job, at year *t*. A worker's job is defined as an early job if it is the first or second job in her employment history, and it is defined as a late job otherwise. Columns (1) **-** (3) include all types of fired workers in calculating firing intensity, while columns (4) **-** (6) include workers fired during non-mass-layoff months only in calculating firing intensity. Panel A and Panel C include all workers at the firm in counting the number of fired workers, while Panel B and Panel D include only workers who are not left-censored in counting the number of fired workers. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

Table II.5. Firing Intensity and Match Improvement: Early Job vs Late Job (*continued*)

Panel A. Firing Intensity of All Workers and Growth in Low Match Quality Dispersion

	(1)	(2)	(3)	(4)	(5)	(6)
	Match Improvement: Low Match Quality Dispersion					
	All Workers	High HC	Low HC	All Workers	High HC	Low HC
Firing Intensity: Early-Job, High HC	-1.2728***	-2.2729***	-0.3079	-1.3074***	-2.9122***	0.2947
	(0.3970)	(0.6336)	(1.1735)	(0.4349)	(0.6593)	(1.2718)
Firing Intensity: Late-Job, High HC	-0.1035	-0.0833	0.6840	-0.9150***	-1.4801***	1.2582
	(0.1000)	(0.0859)	(0.8060)	(0.3157)	(0.4938)	(0.7983)
Firing Intensity: Early-Job, Low HC	-0.5847***	0.1392	-1.0762**	-0.5726**	0.2631	-1.1871**
	(0.2254)	(0.4440)	(0.5009)	(0.2619)	(0.4539)	(0.5297)
Firing Intensity: Late-Job, Low HC	-0.8340***	-0.2846	-1.3314***	-0.8793***	-0.2317	-1.0302**
	(0.1996)	(0.7178)	(0.4412)	(0.2498)	(0.7983)	(0.4621)
Firing Intensity Type	All	All	All	Non-Layoffs	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	5.5480	6.7638	6.0933	5.5480	6.7638	6.0933
N	1261	453	450	1261	453	450
R2	0.3476	0.4110	0.4809	0.3477	0.4385	0.4704

Table II.5. Firing Intensity and Match Improvement: Early Job vs Late Job (*continued*)

Panel B. Firing Intensity of Non-Left-Censored Workers and Growth in Low Match Quality Dispersion

	(1)	(2)	(3)	(4)	(5)	(6)
	Match Improvement: Low Match Quality Dispersion					
	All Workers	High HC	Low HC	All Workers	High HC	Low HC
Firing Intensity: Early-Job, High HC	-1.1014**	-2.1229**	-0.0999	-1.1509**	-2.9344***	0.6296
	(0.4612)	(0.8234)	(1.3639)	(0.5210)	(0.8432)	(1.3339)
Firing Intensity: Late-Job, High HC	-0.4412	-0.5171	1.7569	-0.7670	-1.7435	3.6851***
	(0.4229)	(0.3429)	(1.4970)	(0.5589)	(1.0911)	(1.1467)
Firing Intensity: Early-Job, Low HC	-0.4404**	0.0379	-1.0912**	-0.4326*	0.1723	-1.2599**
	(0.2165)	(0.4180)	(0.5270)	(0.2467)	(0.4146)	(0.5676)
Firing Intensity: Late-Job, Low HC	-0.7718***	0.2467	-1.0629**	-0.8907***	-0.5344	-0.6847
	(0.2071)	(0.8966)	(0.4891)	(0.2653)	(1.0626)	(0.5036)
Firing Intensity Type	All	All	All	Non-Layoffs	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	5.5480	6.7638	6.0933	5.5480	6.7638	6.0933
N	1261	453	450	1261	453	450
R2	0.3348	0.4008	0.4744	0.3285	0.4106	0.4739

Table II.5. Firing Intensity and Match Improvement: Early Job vs Late Job (*continued*)

Panel C. Firing Intensity of All Workers and Growth in Standard Deviation

	(1)	(2)	(3)	(4)	(5)	(6)
	Match Improvement: Standard Deviation					
	All Workers	High HC	Low HC	All Workers	High HC	Low HC
Firing Intensity: Early-Job, High HC	0.0721	-0.1367	0.2035	0.1357	-0.2897	0.2047
	(0.1398)	(0.2019)	(0.2716)	(0.1825)	(0.2604)	(0.3375)
Firing Intensity: Late-Job, High HC	-0.0526	-0.0279	-0.0899	-0.3182**	-0.2612*	-0.1716
	(0.0400)	(0.0254)	(0.2876)	(0.1550)	(0.1479)	(0.3016)
Firing Intensity: Early-Job, Low HC	-0.0060	-0.0738	-0.2671***	-0.0536	0.0093	-0.3254***
	(0.0868)	(0.1313)	(0.1032)	(0.1062)	(0.1522)	(0.1221)
Firing Intensity: Late-Job, Low HC	-0.1465	0.1997	-0.0521	-0.2947**	0.3358	-0.0662
	(0.0954)	(0.2273)	(0.1032)	(0.1467)	(0.2572)	(0.1815)
Firing Intensity Type	All	All	All	Non-Layoffs	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	4.4302	5.4939	5.0877	4.4302	5.4939	5.0877
N	2271	1140	1209	2271	1140	1209
R2	0.2469	0.3313	0.3004	0.2530	0.3359	0.3013

Table II.5. Firing Intensity and Match Improvement: Early Job vs Late Job (*continued*)

Panel D. Firing Intensity of Non-Left-Censored Workers and Growth in Standard Deviation

	(1)	(2)	(3)	(4)	(5)	(6)
	Match Improvement: Standard Deviation					
	All Workers	High HC	Low HC	All Workers	High HC	Low HC
Firing Intensity: Early-Job, High HC	0.2622	0.0601	0.5815*	0.3665*	-0.0830	0.6722*
	(0.1736)	(0.2416)	(0.3380)	(0.2180)	(0.2843)	(0.3754)
Firing Intensity: Late-Job, High HC	-0.0982	-0.0567	-0.1482	-0.0575	-0.0410	0.1328
	(0.0941)	(0.0881)	(0.3638)	(0.2267)	(0.2585)	(0.3631)
Firing Intensity: Early-Job, Low HC	-0.0454	-0.1532	-0.3267***	-0.1057	-0.0592	-0.4011***
	(0.0951)	(0.1191)	(0.1113)	(0.1104)	(0.1325)	(0.1232)
Firing Intensity: Late-Job, Low HC	-0.2481***	0.2573	-0.1777*	-0.4743***	0.3872	-0.3360*
	(0.0899)	(0.2687)	(0.0909)	(0.1419)	(0.2861)	(0.1808)
Firing Intensity Type	All	All	All	Non-Layoffs	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	4.4302	5.4939	5.0877	4.4302	5.4939	5.0877
N	2271	1140	1209	2271	1140	1209
R2	0.2505	0.3318	0.3057	0.2546	0.3326	0.3075

Table II.6. Firing Intensity and Employment Growth: Early Job vs Late Job

This table reports the estimation results of estimating equation (2), when the dependent variable is the firm's employment growth at year *t+1*. The independent variables are the firm's firing intensity of high human capital workers whose job is an early job, firing intensity of high human capital workers whose job is a late job, firing intensity of low human capital workers whose job is an early job, and firing intensity of low human capital workers whose job is a late job, at year *t*. A worker's job is defined as an early job if it is the first or second job in her employment history, and it is defined as a late job otherwise. Columns (1), (2) include all types of fired workers in calculating the firing intensity, and columns (3), (4) include workers fired during non-mass-layoff months only in calculating the firing intensity. Columns (1), (3) include all workers at the firm in counting the number of fired workers, while columns (2), (4) include only workers who are not left-censored in counting the number of fired workers. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

	(1)	(2)	(3)	(4)
Dependent Variable	Employment Growth			
Firing Intensity: Early-Job, High HC	0.0198	0.0617	0.0105	0.1003
	(0.0646)	(0.0802)	(0.0916)	(0.1073)
Firing Intensity: Late-Job, High HC	0.0137	0.0525	0.1155	0.1425
	(0.0302)	(0.0629)	(0.0946)	(0.1291)
Firing Intensity: Early-Job, Low HC	0.0979***	0.0791**	0.1270**	0.1124**
	(0.0364)	(0.0379)	(0.0496)	(0.0519)
Firing Intensity: Late-Job, Low HC	0.0211	0.0310	0.0152	0.0226
	(0.0476)	(0.0545)	(0.0804)	(0.0954)
Firing Intensity Type	All	All	Non-Layoffs	Non-Layoffs
Left-Censored Workers	Included	Excluded	Included	Excluded
Firm Quality Dummies	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes
Sample Mean Employment	3.6065	3.6065	3.6065	3.6065
N	2818	2818	2818	2818
R2	0.2173	0.2170	0.2180	0.2176

Table II.7. Firing Intensity and Employment Growth: Large Replacement Frictions vs Low Replacement Frictions

This table reports the estimation results of estimating equation (2), using the firm's employment growth at year *t+1* as the dependent variable, on subsamples of firms by the difficulty in replacing the fired worker with a new worker. The difficulty in replacing a worker (i.e., replacement frictions) is characterized by the three-digit industry level propensity of having unfilled vacancies in columns (1) - (4), and by the three-digit industry level propensity of the firm covering the training costs of its employees in columns (5) – (8). The independent variables are the firm's firing intensity of high human capital workers and firing intensity of low human capital workers at year *t*. Columns (1), (2), (5), (6) include all types of fired workers in calculating the firing intensity, and columns (3), (4), (7), (8) include workers fired during non-mass-layoff months only in calculating the firing intensity. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Employment Growth							
Firing Intensity: High Human Capital	-0.0278	0.0174	-0.0675	0.1847*	0.0006	0.0206	0.0216	0.1095
	(0.0586)	(0.0317)	(0.0880)	(0.0958)	(0.0802)	(0.0319)	(0.1111)	(0.0845)
Firing Intensity: Low Human Capital	0.0320	0.1015**	0.0198	0.1039**	0.0619	0.0726**	0.0369	0.0984**
	(0.0375)	(0.0413)	(0.0676)	(0.0471)	(0.0560)	(0.0310)	(0.0657)	(0.0460)
Replacement Costs Proxy	Propensity of Having Unfilled Vacancies				Propensity of the Firm Covering Training Costs			
Subsample	High	Low	High	Low	High	Low	High	Low
Firing Intensity:	All	All	Non-Layoffs	Non-Layoffs	All	All	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	3.4418	3.7833	3.4418	3.7833	3.3956	3.6902	3.3956	3.6902
N	1288	1417	1288	1417	915	1798	915	1798
R2	0.2696	0.2110	0.2695	0.2142	0.2508	0.2297	0.2499	0.2310

Table II.8. Firing Intensity and Employment Growth: Benefits of Trying-Out a New Worker

This table reports the estimation results of estimating equation (2), using the firm's employment growth at year *t+1* as the dependent variable, on subsamples of firms by the benefits of trying out a new worker. In columns (1) - (4), benefit of trying-out a new worker is characterized by the three-digit industry median of the match quality importance, where match quality importance is measured as the ratio of the standard deviation of the match quality component at the firm-year to the standard deviation of the human capital component at the firm-year. In columns (5) – (8), benefit of trying-out a new worker is characterized by the three-digit industry mean of employment growth at the firm-year. The independent variables are the firm's firing intensity of high human capital workers and firing intensity of low human capital workers at year *t*. Columns (1), (2), (5), (6) include all types of fired workers in calculating the firing intensity, and columns (3), (4), (7), (8) include workers fired during non-mass-layoff months only in calculating the firing intensity. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Employment Growth							
Firing Intensity: High Human Capital	0.0320	-0.1010	0.2423**	-0.1138	0.1097**	-0.0250	0.2433**	-0.1197
	(0.0381)	(0.0666)	(0.1085)	(0.0807)	(0.0477)	(0.0166)	(0.0984)	(0.1010)
Firing Intensity: Low Human Capital	0.0798**	0.0245	0.0747	0.0471	0.0360	0.0712	0.0225	0.1139
	(0.0400)	(0.0393)	(0.0508)	(0.0563)	(0.0323)	(0.0581)	(0.0413)	(0.0753)
Benefits of Trying-Out Proxy	Industry-Level Match-Importance				Industry-Level Employment Growth			
Subsample	High	Low	High	Low	High	Low	High	Low
Firing Intensity:	All	All	Non-Layoffs	Non-Layoffs	All	All	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample Mean Employment	3.8926	3.3548	3.8926	3.3548	3.4976	3.6717	3.4976	3.6717
N	1052	1601	1052	1601	1451	1191	1451	1191
R2	0.2363	0.2171	0.2413	0.2171	0.2530	0.3158	0.2545	0.3168

CHAPTER III

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

"New practices have emerged to facilitate employer collusion, such as CNC 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)[75]

1. Introduction

In a recent op-ed, Furman and Krueger (2016) proposed that monopsony power is holding back wage growth, economic dynamism, and innovation:

> *In addition to holding down workers' paychecks, monopsony power can depress overall hiring and output.... If monopsony power creates barriers to workers switching jobs, it can slow labor turnover, reducing dynamism and innovation.* [76]

[75] See http://www.milkenreview.org/articles/the-rigged-labor-market

[76] https://www.wsj.com/articles/why-arent-americans-getting-raises-blame-the-monopsony-1478215983

This provocative thesis builds on recent scholarship examining frictions that reduce labor market competition (Manning 2011, Ashenfelter et al. 2010, Boal and Ransom 1997). One friction cited prominently by Furman and Krueger (2016) as contributing to increased monopsony power is covenants not to compete (CNCs), which are employment provisions that prohibit a worker from leaving to join or start a competing firm.[77] While covenants not to compete appear to be *prima facie* anticompetitive, the fact that they are voluntarily agreed upon makes inference more complicated: 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 make labor markets more competitive?

Existing studies do not clearly answer this question. Recently, a number of studies have found that the enforceability of CNCs, which varies across states in the U.S., reduces the rate of employee mobility of technology workers (Fallick et al. 2006, Marx et al. 2009) and CEOs (Garmaise 2011), and increased out-of-state movements (Marx et al. 2015). These findings have prompted policymakers to take action, including a recent ban on CNCs for *only* technology workers in Hawaii (Zillman 2015). However, given the voluntary nature of these provisions, the key question is not how CNC enforceability affects worker mobility *per se*. Rather, the question for workers is whether they will be better off as a result, either in their current job or over their career, despite the potentially negative mobility ramifications. If their wages are higher, either in their current or subsequent jobs, then it would appear that CNC enforceability makes labor markets more competitive, not less. This could happen if, for example, CNC enforceability solves the hold-up problem and incentivizes the firm to provide additional training, or invest in

[77] 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 et al. 2006).

valuable technology or information that leads to increased worker productivity (Rubin and Shedd 1981, Meccheri 2009).[78] However, if we observe that worker wages are lower in states that enforce CNCs, not only in their current job but also in subsequent jobs, *and* we observe increased employment durations, then it suggests that enforceability of CNCs locks workers into their jobs, preventing them from earning their maximal wage and working where they prefer.

In this paper, we use both cross-sectional and longitudinal variation in CNC enforceability to examine how it relates to mobility and wages. Our cross-sectional analysis uses quarterly employer-employee matched data for the universe of employees in thirty U.S. states between 1991 and 2008, which are considerably richer and more granular than data used in prior studies. Using this data, we examine how CNC enforceability is related to the rate and direction of worker mobility, and, more importantly, the time path of wages both within-jobs and across a worker's career.[79] We compare how the within-state difference between technology workers ("tech workers") and non-technology workers ("non-tech workers") changes as the enforceability of CNCs increases. We focus on technology workers because they are sources of knowledge spillovers and agglomeration economies (Gilson 1999, Fallick, Fleischman and Rebitzer 2006), and because they have the highest incidence of CNCs (Starr, Bishara, and Prescott, 2016). We supplement this cross-sectional analysis by examining the effects of a recent natural experiment in which Hawaii banned CNCs in 2015 *only* for high-tech workers.[80]

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[78] In Section 2 and Appendix 6, we set out a theoretical framework, drawn from the on-the-job search model in Cahuc, Postel-Vinay and Robin (2006) to guide our empirical analysis. This framework also yields the prediction that if firm investments are important to generating the worker-firm match value, higher enforceability could result in greater firm investments, and eventually greater worker productivity and wages (if reductions in bargaining power are modest).

[79] "Enforcement" refers to the act of enforcing a CNC by a firm or a court; "enforceability" refers to whether a CNC can withstand scrutiny in court.

[80] We use publicly available data from the Current Population Study (CPS) and the Quarterly Workforce Indicators (QWI) for this analysis as the July 2015 ban in Hawaii is outside of the range of our matched employer-employee data; also, restrictions on disclosure in the Longitudinal Employer-Household Dynamics (LEHD) database preclude analysis of within-state changes such as in Hawaii.

Our results on mobility corroborate and generalize extant findings (Fallick, Fleischman, and Rebitzer 2006, Marx et al. 2009, Marx et al. 2015, Garmaise 2011). The cross-sectional evidence suggests that a one-standard-deviation increase in CNC enforceability is associated with an increase in the length of job-spells for workers in technology industries ("tech jobs") by at least 1.5%, compared with the length of job-spells for workers in non-technology industries ("non-tech jobs"). This translates to about a 6% difference in the average length of job spells between an average enforceability state and the non-enforcing states after controlling for gender, cohort, age, employer size and starting wage. For Hawaii, using both CPS and QWI data, we find that mobility for tech workers rises following the July 2015 noncompete ban relative to other industries within Hawaii, relative to other states within tech, and in a triple difference specification that controls for industry-quarter, state-industry and state-quarter effects.

Our cross-sectional approach using matched employee-employer analysis allows us to more deeply analyze how CNC enforceability is related to the *type* and *direction* of worker movement. In particular, we find that individuals whose first jobs are in the technology sector in higher enforceability states have fewer jobs within their first eight years but are more likely to move across states. Due to human capital investment decisions or location choice by firms, it could be expected that tech workers in high-enforceability states have more industry-specific skills. Consistent with this expectation, we find that individuals starting work in the tech sector in high-enforceability states are also less likely to switch industries, and more likely to move across states without switching industries. To our knowledge, this is the first study to document these interesting and policy-relevant empirical regularities.

Beyond generalizing prior results on mobility, however, the major contribution of this study is in the analysis of wages. Neither the cross-sectional nor the longitudinal analysis shows evidence that the reduced mobility of tech workers is offset by higher wage levels. In fact, consistent with reduced bargaining power in high-CNC regimes, our cross-sectional analysis shows that tech workers earn lower wages (between –0.5% and –0.7% for a one-standard-deviation increase in CNC enforceability) throughout their job spell in higher enforceability states. Moreover, we find that eight years after starting the job, those in an average-enforceability state have 4.6% lower cumulative earnings relative to observably equivalent workers in a non-enforcing state. Consistent with these findings, the longitudinal analysis using Hawaii's CNC ban suggests that overall wages rose about 0.7 % (Col 3, Table III.9, triple difference analysis) and wages at hiring rose about 4.2% (Col 7, Table III.9, triple difference analysis).

The cross-sectional and longitudinal results for both mobility and wages are robust to a variety of alternative specifications and subsample analyses, which are described in detail in the robustness checks section and corresponding appendices.[81] Taken together, our results strongly suggest that CNC enforceability is associated with "job lock," similar to that discussed in 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. Though a few studies have found negative cross-sectional wage effects (Starr 2018, Garmaise 2011), these studies do not examine the intertemporal relationship between CNC enforceability and wages. In contrast, our principal contribution to this discussion is that CNC enforceability is not only associated with reduced mobility, but that the average worker does not appear to be compensated more for what he or she gives up in any of the first eight years after starting the job. Thus, these findings are directly relevant to the policy debate

[81] Among others, for the cross-sectional results using the LEHD, we check and confirm robustness using the effects for workers with high 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 et al. 2010), and various subsample checks.

about CNC enforceability (Treasury 2016, White House 2016), and for states that are actively debating changes to CNC policy.[82]

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 (e.g., Krueger and Katz 1992; see review by Bhasker, Manning and To 2002). Similar to Naidu (2010), our results suggest that CNC enforceability slows down and redirects worker movement, reducing the returns to tenure and experience more broadly. One implication of these results is that CNC enforceability, by locking incumbent workers into the firm, reduces the elasticity of labor supply facing new and expanding firms, moving labor markets away from the competitive ideal. 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.

2. Mobility and Wage Effects of CNC Enforceability

We use a simple model of mobility and wage determination to analyze how CNC enforceability affects the length of job spells and the pattern of wages. Our framework simplifies several features of the on-the-job search model in Cahuc, Postel-Vinay and Robin (2006), but extends it by linking match-value to firm and employee investments. We briefly describe the model and the underlying intuition here, leaving the details to the Appendix 6. In our model, the economic value of a worker-firm relationship is given by θ, which reflects the worker's human capital relevant to the firm. The worker searches for opportunities outside the firm, and receives

[82] E.g., see https://faircompetitionlaw.com/the-changing-landscape-of-trade-secrets-laws-and-noncompeteCNC-laws/.

a single offer with wage W_0, from a uniform distribution $[0, 1 + \mu]$. If the outside wage is greater than θ, the worker leaves. Otherwise, the worker negotiates with the firm and obtains a wage equal to the outside wage offer plus a share of the surplus, i.e., $W_0 + \alpha(\theta - W_0)$ where α is a parameter that reflects the bargaining power of the worker. Thus, in this model, worker mobility is determined by the probability of getting an offer above θ, which is determined by where θ is relative to the upper bound of outside offers $1 + \mu$. Average wage is a linear combination of outside wage offers and match surplus within the firm, conditional on staying.

Enforceable CNCs drive a legal wedge between a departing employee and competing firms, and reduce the range of wage offers received by a worker.[83] Therefore, increasing CNCs will decrease μ. In addition, enforceability also reduces the bargaining power of a worker within the firm, which affects the share of economic surplus that goes to the worker. That is, increasing enforceability decreases α. We refer to this as the "lock-in" effect of enforceability.

When θ is exogenously determined (that is, individual or firm investments in human capital do not affect θ), increasing enforceability does not affect θ, but the maximum possible wage offer, $1 + \mu$, decreases. This decreases the probability of exit, thus decreasing worker mobility. Furthermore, because α decreases, average wages also decrease.

However, when θ is affected by the level of investments made by the firm or worker, the effects are not uniformly unambiguous. As we show in the appendix, increasing CNC enforceability increases the firm's investment and decreases the worker's investments in human capital. In the case where human capital responds only to firm investment, higher CNC enforceability increases the probability that the worker stays but the effect on wages is

[83] This can be considered a simplification of a longer process, where enforceability reduces the potential number of outside offers, which in turn reduces the maximum of those wage offers (for many common distributions of wage offers). To the extent that CNC enforceability act as a barrier to entry for new firms (which may need to hire from existing pool of skilled workers), enforceability reduces the number of potential outside offers by reducing the number of firms in the market. The reduction in range of outside offers in our model indirectly captures this potential impact of CNC enforceability on firm entry.

ambiguous. This is because higher CNC enforceability increases the firm's investment in θ, which increases the threshold wage for the worker to leave. Since the upper bound of outside offers (1 + μ) falls, the probability of leaving (and worker mobility) declines unambiguously. If higher enforceability does not affect the bargaining power significantly, then the increased human capital from higher firm investments implies higher wages for workers. However, if higher enforceability significantly reduces workers' bargaining power, their wages may decline. This would be consistent with workers being "locked in."

In the case where human capital responds only to the worker's investment, both the mobility and wage effects of increasing enforceability are ambiguous. θ decreases due to decreased individual investment, but so does the upper bound of outside offers. Wages within the firm, conditional on staying, unambiguously decrease due to decreased worker bargaining power and decreased worker investment, but average wage levels may not decrease if the probability of leaving and accepting an outside wage offer increases.

Our simple framework illustrates that, although job-lock (as manifested in lower mobility and wages) resulting from high CNC enforceability is a distinct possibility under plausible assumptions, wages may in fact not be lower, particularly if firm investments are important for match value, and even mobility may not be lower if worker investments are very important for match value. Thus, ultimately, whether or not CNC enforceability decreases worker mobility and wages is an empirical question.

3. Analysis Using Cross-sectional (Across States) Variation in CNC Enforceability

3.1. LEHD Sample Construction

We construct a job-level (i.e., worker-firm combination) and a worker-level repeated cross-sectional dataset using the Longitudinal Employer-Household Dynamics (LEHD) database at the U.S. Census Bureau. The LEHD is a composite linked employer-employee dataset comprising multiple state databases. There are two advantages to using the LEHD for this study. First, the LEHD provides employment history data for individual workers over a long horizon for a full spectrum of industries in the U.S. economy across a large number of states that vary in CNC enforceability levels. Second, 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.

Linked employer-employee records of employment history are available for thirty states at the worker-firm-year-quarter level in the Employment History File (EHF) within the LEHD.[84] From the employment history of each worker, we identify jobs at each of the firms where the worker worked (i.e., when there is a change in the firm identifier in the worker's employment history, we identify that as a job change).[85] Because the firm identifiers of the EHF are within-state identifiers, we use the national-level firm identifier (ALPHA) available in the Business Register Bridge (BRB) for defining the job.[86] This ensures we do not wrongly capture within-firm, inter-state, or intra-state transfers as worker movements out of a firm. Because the link to the BRB is available only from 1991, our analysis covers the years 1991–2008.

We keep left-censored workers, but drop any left-censored jobs from our dataset because not only we do not know the lengths of the latent spells for these jobs (and we can avoid the bias from stock sampling by dropping these jobs), but also we do not know the characteristics of these jobs at the beginning of the spell, which we use to construct our job-level fixed effects

[84] The thirty states are Arkansas, California, Colorado, Florida, Georgia, Hawaii, Iowa, Idaho, Illinois, Indiana, Louisiana, Maryland, Maine, Montana, North Carolina, New Jersey, New Mexico, Nevada, Oklahoma, Oregon, Rhode Island, South Carolina, Tennessee, Texas, Utah, Virginia, Vermont, Washington, Wisconsin, and West Virginia.

[85] We allow job spells of the previous job and the new job to overlap in a year-quarter in which the job transition occurs.

[86] "ALPHA" is a cleaned longitudinal firm identifier. Using ALPHA (instead of id's created for tax reporting) minimizes measurement concerns, including the concern about a single firm having multiple id's.

described below. We also drop workers whose first-year annual income in the LEHD is less than $35,000 in 2008 dollars, as these workers are not likely to have jobs that are knowledge intensive (and therefore are not suitable as a comparison group, either), and therefore are less likely to be affected by CNC enforceability (Starr, Bishara, and Prescott 2016). 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.

Finally, we obtain the NAICS industry classification information of the firms from the Employer Characteristics File of the LEHD, and biographic information such as sex, date of birth, and foreign-born status from the Individual Characteristics File of the LEHD.

3.2. Key Outcomes of Interest

<u>Job-level mobility and earnings:</u> For each job (i.e., worker-firm combination, with change in job identified when a worker changes his or her employer), we construct two dependent variables to measure a worker's mobility. The first is the length of the job spell defined as the log number of quarters the worker was employed at the firm. To mitigate concerns with right censoring, we restrict our sample to the jobs whose spell started in 2000 or earlier for this analysis.[87] The second is a set of dummy variables for the job spell surviving a given length of time: a dummy variable with value 1 if the job spell survives until the 4th quarter of its spell, a dummy variable with value 1 if the job spell survives until the 8th quarter of its spell, and so on. We examine the survival of job spells up to the 32nd quarter (or eight years) from the start of the job spell. Using these dependent variables not only circumvents the right censoring of spells (allowing us to use

[87] Duration model estimations are not computationally feasible alternatives for our analysis, because our identification strategy utilizes high dimensional fixed effects.

the full sample for this analysis, not just spells that started in 2000 or earlier), but also provides richer information on how CNC enforceability affects the distribution of job spells.

We examine the relation between CNC enforceability and wages, across job tenure by examining wages in various dimensions. Our primary measure is log *wage* at the 4th, 8th, …, 32nd quarters of the job spell, CPI-adjusted to 2008 dollars.[88] We also examine log *cumulative wage,* and wage growth relative to the starting wage at the 4th, 8th, …, 32nd quarters of the job spell.

Mobility and earnings over employment history: Beyond the potential effect of CNC enforceability on job-level outcomes, we examine its relation to workers' labor-market outcomes across their employment history. In parallel to the analysis of job-level mobility and wages, we examine how CNC enforceability is associated with the cumulative number of jobs taken (in logs) and workers' cumulative earnings (in logs) at the worker level. We also extend our analysis to the workers' choice of switching states or industries to circumvent CNC enforceability, by examining the cumulative number of switches in states or switches in industries at the worker level (in logs). We examine these outcomes across the 4th, 8th, …32nd quarters of workers' employment history.[89]

3.3. The CNC Enforceability Measure

A commonly used data source for the measure of CNC enforceability in each state is Malsberger's (1996) series *Covenants Not to Compete: A State by State Survey*, which tracks the case law for each state along numerous dimensions of enforceability. Bishara (2011) and

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[88] The LEHD has quarterly wage data. Thus if the job spell is censored at a certain quarter 4t (i.e., when the job spell ends at quarter 4t), then we take the wage at quarter 4t-1 of the worker's job spell.

[89] 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.

Garmaise (2011) each quantify these various dimensions of enforceability. We use the enforceability index developed in Starr (2018), which modifies the Bishara (2011) index by performing factor analysis to re-weight the seven dimensions of enforceability. The Starr-Bishara index has the advantage of removing the redundancy of the seven dimensions of enforceability and capturing a finer granularity of the way enforceability is construed along a spectrum of weak to strong enforceability. Figure III.1 presents the enforceability index scores by state for 2009. Note that the enforceability index scores are normalized to have mean 0 and standard deviation of 1 in a sample where each state is given equal weight.

3.4. Empirical Methodology

We analyze the relation between CNC enforceability and high-tech workers' mobility and wages across the worker's jobs and career by utilizing the significant inter-state variation in the 2009 enforceability index scores. Specifically, we estimate the differential relation to the jobs (and workers) that are in high-tech industries compared with the relation to other jobs (and workers). As discussed earlier, we choose high-tech workers as our focal group because these workers are relatively more likely to embody intellectual capital (as discussed in the literature, e.g., Marx 2011) and hence more likely to be affected by CNC enforceability. Indeed, while Starr, Prescott, and Bishara (2018) report a national signing rate of 18%, the incidence rate for Computer, Mathematical, Engineering, and Architecture jobs is much higher, at 36%. Further, studies of CNC litigations show that technology workers are frequently involved in such litigations (LaVan 2000) and a large literature in law and economics has focused on the effect of CNC enforceability on high technology workers (Hyde 2003).

We use the industry (NAICS) classification of the employer to create a dummy variable for the job being in "Technology Industries." We use the definition of "Technology Industries" by Paytas and Berglund (2004), which classifies the NAICS industries into technology industries by employment of occupations that are science-and-engineering-intensive based on the occupation-NAICS employment concordance provided by the Bureau of Labor Statistics. We define “Technology Industries” at the three-digit NAICS code level, and jobs in “Technology Industries” are hereafter referred to as “high-tech jobs.” Industries that are not “Technology Industries” are referred to as “Other Industries,” and jobs in “Other Industries” are referred to as “non-tech jobs” hereafter.

We use a large set of fixed effects, based on worker and job characteristics at the time the job spell starts. Each joint fixed effect defines a group of jobs that are common in terms of their three-digit NAICS codes, starting year, firm size group, starting wage group, starting age group of the worker, and gender of the worker. 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 eleven categories along the distribution of starting wages of jobs within three-digit NAICS codes. Starting age is the worker’s age in the job’s first year in quartiles of the distribution of starting ages for all jobs.

The LEHD does not contain detailed occupation or reliable education data for workers. To mitigate potential bias from unobserved heterogeneity on these characteristics, we use starting wages (defined as the second-quarter wage of each job, because wage data in the LEHD is quarterly and the worker’s first quarter at the job is likely to be left-censored) as a proxy for the initial level of the worker’s general human capital, using a categorical variable defined within jobs with the same three-digit NAICS codes. That is, we presume to the extent that workers with the same age and gender starting at the same time in similar-sized firms in the same industry have different educational backgrounds or occupations, this should be reflected in the starting wage.[90] Starting-year fixed effects are used to control for cohort-specific initial period shocks.

We then estimate the differential relation between CNC enforceability and the outcome variable for high-tech jobs using Equation (1):

$$Y_{ijs} = \alpha + \delta CNC_s * I\{Tech\}_{ij} + \Sigma_s + FE_{ij} + \gamma fb_i + \varepsilon_{ijs} \tag{1}$$

where the subscripts i, j and s are for individual, job and state, respectively. Job (*j*) is defined by the worker-firm pair. These semi-parametric regressions use fully saturated specifications, and the set of job/worker characteristics fixed effects (FE_{ij}) discussed above subsume the dummy variables that are absent in Equation (1). Y_{ijs} denotes the dependent variables discussed above. CNC_s is the 2009 CNC enforceability index measure of the state where the worker-firm pair is observed. $I\{Tech\}_{ij}$ is 1 if the firm of the worker-firm pair is in one of the "Technology Industries". FE_{ij} denotes the set of job/worker characteristics fixed effects discussed above.[91] fb_i is dummy that denotes whether the worker of the worker-firm pair was foreign-born. We control for foreign-born status, as foreign-born employees are subject to visa-related employment eligibility constraints that may affect their mobility. Σ_s denotes state fixed effects dummy variables.

Our coefficient of interest is δ, which estimates the differential association between CNC enforceability and the dependent variable for high-tech jobs relative to non-tech jobs. Thus, this implements a cross-sectional, pseudo difference-in-differences (DID) design similar to prior studies that exploit across state variation in enforceability by using a comparison group within

[90] This comports with the idea that workers may have more flexibility while deciding on their first job location, so that starting wage differences reflect underlying worker quality differences.

[91] For the worker-level analysis on 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. Likewise, the job-level variables and the job-level fixed effects are based on the initial characteristics of the first job.

each state to net out potential confounding state-level variables (Fallick, Fleischman, and Rebitzer 2006; Stuart and Sorenson 2003; Samila and Sorenson 2011; Garmaise 2011; Starr 2018; Starr, Balasubramanian, and Sakakibara 2018).[92]

4. Results from Analysis Using Cross-Sectional Variation in Enforceability

4.1. Mobility and Wage Across Job Tenure

Table III.1 presents the differential effect of CNC enforceability on mobility from estimating Equation (1). The column heads denote the dependent variables for each of the specifications. Table III.1 shows that a one-standard-deviation increase in the enforceability score is associated with a 1.5% increase in the mean job spell duration (Col 9). This is driven by rightward shifts in the job spell distribution in higher enforceability states beginning in year 2 (Col 2). A one-standard-deviation increase in enforceability is associated with a 0.5 percentage points increase in the probability that a job spell lasts at least eight years. Given that only 12.4% of all job spells last eight years, a one-standard-deviation increase in enforceability increases the likelihood that the job lasts at least eight years by 4% (0.5/12.4).[93]

To put these estimates in context, assuming a uniform causal effect over the distribution of enforceability scores, applying the average enforceability score to a non-enforcing state (a difference of four standard deviations) would imply a 6% increase in the mean job-spell length (similar to the 8% observed in Marx, Strumsky, and Fleming 2009) and a 16% increase in the likelihood that jobs last at least eight years. A graphic illustration of the coefficient estimates and

[92] Garmaise (2011) also analyzes three within-state changes (Texas 1994; Louisiana 2001; Florida 1996). Because data for Texas begins only in 1995, and because public disclosure of results requires at least three states, we are unable to utilize this time series variation to identify the effects of interest. In addition, the Louisiana change was brief and temporary (enacted in 2001 and reversed in 2003). Marx, Strumsky and Fleming (2009) have studied a change in Michigan non-compete in 1986, which occurred prior to our data period.

[93] Summary statistics for all dependent variables are presented in Appendix 7 Table A7.1; the population mean for the dummy indicator of job spell surviving more than 32 quarters is 0.124.

the 95% confidence intervals in Table III.1 is provided in Figure III.2. The increase in the coefficients on mobility over the tenure profile is consistent with employees gaining more intellectual capital and hence, being more strongly targeted by firms for CNC enforcement.

Table III.2 presents the differential coefficients (δ) using wages across job tenure as the outcome variable in Equation (1). We observe a persistent negative relation between enforceability and wages. The coefficient δ ranges from 0.5% to 0.7% for high-tech jobs compared with non-tech jobs, suggesting that moving from a ban to average enforceability would be associated with 2% to 2.8% lower wages for the average technical worker. The coefficient estimates and the 95% confidence intervals are plotted in Figure III.3. Unlike the tenure profile for mobility, the wage profile is relatively flat so the wage penalty of CNCs is similar in log difference terms over the job tenure. Overall, the results are consistent with a reduction in bargaining power starting early in the job tenure of tech workers. The different patterns can be reconciled by a stronger effect of enforceability on increases in relationship-specific value (as in Case 2A in Appendix 6, where firm investments matter) along the worker's tenure at the firm (which explains the decline in exit propensity over tenure), combined with reduced bargaining power offsetting potential gains to the employee (which explains the relatively flat the wage penalty).[94]

Next, in Table III.3, we examine the relation between CNC enforceability and workers' cumulative wage and wage growth across job tenure. We measure cumulative wage as the log of cumulative wage at 4th, 8th, …, 32nd quarter of the job spell since the job spell started. We measure wage growth as the difference between the log of quarterly wages at 4th, 8th, …, 32nd quarters of the job spell and the log of initial wage of the job. The results show that cumulative

[94] Alternatively, the wage patterns may reflect intertemporal arrangements; e.g., the flatter wage profile may reflect an initially lower wage penalty to offset future decline in outside options.

wages decline with CNC enforceability, 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 highest enforceability score to a non-enforcing state would be associated with 4% lower cumulative earnings eight years into a job for the average tech worker relative to a non-tech worker.

Together, the results in this section show consistent patterns that CNC enforceability is associated with longer job-spells, and with lower wages throughout these job spells.

4.2. Career Outcomes across Employment History

So far, we have examined how mobility and wage vary with CNC enforceability at the job-level. In this subsection, we examine the mobility and earnings outcomes at the worker-level, across the worker's employment history.[95]

In particular, we estimate Equation (1) across workers' employment history, using the cumulative number of jobs each worker has taken to examine mobility, and using the cumulative earnings of the worker to examine earnings. These dependent variables are examined at the 4th, 8th, …, 32nd quarters since the worker started his or her employment history. All right-hand-side variables, including the high-tech dummy, the CNC enforceability score, and fixed effects, are those of the worker's first job in the dataset. Thus, we estimate how outcomes over the career of a worker are different, depending on whether he or she started in a high-CNC-enforceability state relative to a similar age-gender worker with similar starting wage in a similar sized firm in the same industry starting his or her first job in the same year in a low-enforceability state.

[95] Because the LEHD covers only thirty states, examining worker-level outcomes (unlike job-level outcomes) potentially carries measurement error due to movement of workers into non-covered states. See Sections 4.4 and 4.5 for some evidence on the lack of correlation between the missing states and enforceability.

Table III.4 reports the estimation results. For both mobility (Panel A) and earnings (Panel B), we observe persistent differentials associated with CNC enforceability. The magnitude of the decline in mobility with CNC enforceability is gradually increasing across employment history, such that a one-standard-deviation increase in enforceability is associated with a 2.1% decrease in the number of jobs after eight years, which translates to an 8.2% differential when comparing between an average enforceability and a non-enforceability state. With regards to wages, we find a gradually increasing and then decreasing wage-suppression with CNC enforceability across employment history, such that 8 years after starting a job in an average enforceability state workers have 4.6% lower cumulative earnings relative to an equivalent worker who started a job at the same time in a non-enforcing state.

One notable distinction between the cumulative earnings regressions at the career level (Table III.4) versus at the job level (Table III.3) is that the latter are conditional on the employee staying in the *same job* at the tenure examined (e.g., the end of quarter 24 analysis in Table III.3 is conditional on workers staying in the job until quarter 24). The mobility results (Table III.1) suggest that high-tech workers have longer job spells than non-tech 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. Indeed, the larger magnitudes of the coefficients in Panel B of Table III.4 compared with those in Panel A of Table III.3 suggests this to be the case.

4.3 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 (e.g., 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 history. The analysis is conducted at the worker-level, similar to the analysis in Section 4.2, 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 by changes in state and the three-digit NAICS code of the worker's employer, respectively.

In Table III.5, we observe a greater frequency of state switches for high-tech workers with initial employment in a high-enforceability jurisdiction, compared with non-tech workers (Panel A). By contrast, greater enforceability is associated with a negative differential effect on the number of industry switches for workers in high-tech industries across their 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.[96]

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

[96] 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 (1) 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 Appendix 7 Table A7.3, 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 III.5.

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.

4.4. Unemployment or "Missing" Spells

While the above specifications consider movements of workers across states and industries, workers may also become unemployed as a result of CNC enforceability. For example, if an employee leaves for a competitor, the competitor can subsequently be sued by the employee's former employer, and the competitor could decide against hiring the employee (Viswanatha, 2016). We use Equation (1) with the dependent variable as the log number of quarters between observed jobs in the LEHD. Such "missing" spells in the data may be a result of either unemployment or movement into a non-LEHD state. The dummy variable for high-tech jobs is that of the job before the missing spell, and the CNC enforceability score is similarly applied to the job before the missing spell. The estimation sample consists of all missing spells between non-continuous job spells. Appendix 7 Table A7.4 reports the estimation results, where we observe small negative but insignificant effects. That is, we do not find any evidence for CNC enforceability being related to an increase in unemployment or "missing" spells.

4.5. Robustness Checks

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

Triple difference between high and low-wage jobs within Tech: If the technology sector has differences in characteristics across states in ways correlated with CNC enforceability, it could bias estimates from Equations (1). Appendix 3 provides a detailed discussion of our main analysis looking at how CNC enforceability differentially affects high earning tech workers versus low earning tech workers, relative to the same difference in non-high-tech industries. The analysis results reported in Appendix 3 Table A3.1 – A3.3 exhibit larger mobility constraining and wage suppressing coefficient estimates for high-initial-wage tech workers compared to low-initial-wage tech workers. This type of analysis also allows us to include state-by-industry fixed effects to account for state-by-industry unobservables that might be correlated with CNC enforceability. The results, presented in Appendix 3 Tables A4 and A5, confirm that the differential coefficients on CNC enforceability observed in the baseline results are robust to these concerns.

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). In the Appendix 7 Table A7.2, we repeat our main analysis of within-job mobility and wages controlling for local labor market thickness proxied using total employment in state/three-digit NAICS code/year (in logs). The results are remarkably similar to the baseline results.

Using CNC ranks instead of raw scores: Another potential concern is that California's large economy and near-complete CNC non-enforceability (an outlier among the CNC scores used in the baseline analysis) could be inordinately influencing the results. We therefore repeat our analyses in Section 4.1, 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 and are normalized to have mean 0 and standard deviation 1 across the fifty state values. Larger values correspond to stronger CNC enforceability and smaller values correspond to weaker CNC enforceability. In results presented in the Appendix 7 (Table A7.5 and Table A7.6), we find that estimates using CNC ranks are very similar to those using CNC scores in terms of statistical significance and signs, and that the magnitudes are similar as well (for a hypothetical change moving from California to Florida).

<u>Balance of enforceability measures across missing and available states</u>: Because the LEHD data we had access to does not cover all fifty US 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.

<u>Robustness of job spell analysis to using alternative samples</u>: 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 (columns (1) - (8) of Table III.1 and Table III.2) 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.

5. Longitudinal Variation in CNC Enforceability: Hawaii's Ban for Tech Workers

5.1 Hawaii's CNC Ban for Tech Workers

One limitation of the results described in the previous section is that, despite our use of a pseudo-DID and triple difference robustness checks, there may be unobserved state characteristics correlated with industry (or industry-wage group) behavior and with CNC enforceability in a way that potentially biases our results. An alternative approach is to exploit changes in state-CNC policies. Although states rarely adopt CNC bans (Bishara 2011), one recent policy change provides us a particularly suitable natural experiment to examine effects of CNC enforceability: Hawaii 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 the technology businesses, protect jobs, and encourage establishment of new technology businesses by tech employees.[97] 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 by analyzing a within-Hawaii, cross-industry DID analysis, a within-tech, cross-state DID analysis, and a triple difference model that controls for state and industry differences.

[97] 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.*"

5.2 Data and Empirical Design

Because the Hawaii ban is outside the range of our employer-employee matched data period, we use data from the Quarterly Workforce Indicators (QWI) and the Current Population Survey (CPS) to examine mobility and wage patterns before and after July 2015 in Hawaii. The QWI is a public use database provided by the US Census Bureau, which includes a set of quarterly "economic indicators" including employment, job creation, earnings, and other measures of employment flows at the state-industry-quarter level. In particular, for our purposes, the QWI includes variables that track both mobility, as well as the wages of workers.[98] The source data for the QWI is the LEHD linked employer-employee microdata which we described earlier.

The CPS is a monthly survey given to approximately 60,000 randomly sampled households who are surveyed for 2 sets of 4 consecutive months, with a break in-between of 8 months. In each of the consecutive months, household members are asked about employer switches, while at the end of each set of interviews household members are asked about their weekly wages. To focus on full-time, working-age workers, we limit the data to those employed with a single job and between the ages of 18 and 70, and to obtain a symmetric window of pre- and post-ban trends, we limit the sample to period July 2013-July 2017. While the CPS data is well-suited to study mobility of workers (by examining individual level decisions to leave their job), we caution that power is limited by the small sample size for our target population of interest (tech workers in Hawaii).[99]

Our principal empirical design is a DID and a triple-difference (DDD) analysis in which

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

[99] The small sample issue is more severe for wage data, because there are very few individuals in the CPS outgoing rotation groups which underlie the wage data. In fact, we have wage data for an average of only one worker per period for tech workers in Hawaii, so that using CPS data to study wage changes is infeasible.

firms/workers in the "technology business" per the Hawaii statute are the "treated" industries. For the DID analysis, we examine two alternatives for the control group: a "Within-Hawaii, Cross-Industry" (hereafter "Within-Hawaii") where the control group are other industries in Hawaii, and a "Cross-State, Within-Tech" DID analysis (hereafter "Cross-State") where the tech sectors in the other 50 states form the control group. In the DDD analysis, we exploit the fact that our shock is both state- and industry-specific, allowing us to examine the coefficient on the post-ban period dummy interacted with a dummy for Hawaii and a dummy for the technology sector, while including 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 et al. 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 4-digit subsectors (and corresponding Census classification codes used in the CPS) specifically related to software development, design and related services.[100]

To examine how mobility changes following the ban (in the QWI), 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 (variable named "EmpTotal" in

[100] The detailed list is provided in Appendix 7 Table A7.8. 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 4 above.

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. For examining changes in wages, 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 happen at year-end), larger effects should be expected for the latter variable (EarnHirAS), 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 to 2017Q2 (the latest available data).

Our CPS-based mobility analysis uses a dummy dependent variable of whether the individual reported leaving their employer in the following month. The basic specifications in the CPS (with a very similar specification for the QWI) are:

DID Cross-Industry, Within-Hawaii Analysis:

$$Y_{ijt} = \alpha + \beta Post_t * I\{Tech\}_j + \Sigma_t + \omega_j + \gamma X_{it} + \varepsilon_{ijt} \tag{2}$$

DID Cross-State, Within-Tech Analysis:

$$Y_{ikt} = \alpha + \beta Post_t * I\{Hawaii\}_k + \Sigma_t + \omega_k + \gamma X_{it} + \varepsilon_{ikt} \tag{3}$$

Triple Difference (DDD) Analysis:

$$Y_{ijkt} = \alpha + \beta Post_t * I\{Tech\}_j * I\{Hawaii\}_k + \Sigma_{jt} + \omega_{jk} + \Sigma_{kt} + \gamma X_{it} + \varepsilon_{ijkt} \tag{4}$$

where the subscripts *i*, *j*, *k*, and *t* are for individual, industry, state, and time (month or quarter), respectively, Σ_t are year-by-month fixed effects (or year-by-quarter in the QWI analysis), ω_j are a set of industry fixed effects (census 4-digit industry codes in the CPS and NAICS 4-digit codes in the QWI). For the Cross-State analysis, we also allow for industry by year-month (or year-quarter) and state by industry fixed effects. In the DDD analysis, ω_{jk} are a set of state-industry fixed effects, Σ_{jt} are industry X year-by-month (industry X year-by-quarter in QWI) fixed effects, and Σ_{kt} are state X year-by-month (state X year-by-quarter in QWI) fixed effects.[101] In the CPS analysis, X_{it} is set of time-varying individual controls including indicators for education level and whether the worker is unionized, and a linear and quadratic in age and hours worked. The QWI analysis, which is at the state-industry-quarter level, does not have any individual controls. Standard errors are clustered at the industry level in the Within-Hawaii analysis, and at the state level in the Cross-State and Triple Difference analysis.

Within each of the alternative DID approaches and for the DDD analysis, we check robustness to using alternative comparison groups. In particular, for the Within-Hawaii analysis, we use a smaller comparison group of subsets of 4-digit industries which belong to a 2-digit level definition of the Tech sector ("Tech 2d sector"), as well as a comparison group of all non-tech 4-digit sectors. Similarly, for the Cross-State and DDD analyses, we use a comparison group that excludes outlier CNC enforceability states by limiting sample to the 40 states with CNC enforceability scores closest (in absolute terms) to Hawaii in the pre-ban period, as well as a comparison to group of all other states.

5.3. Results from Analysis Exploiting Hawaii's Ban of CNC for Tech Workers

Figure III.4 displays a non-parametric binned scatter plot of 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 is a reversal and also more volatility in the post-ban trends. In contrast, the trends for other industries

[101] 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.

in Hawaii don't show a significant jump just after the ban, but there is a slow increase in mobility, so that towards the end of our window the mobility levels are similar for other industries in Hawaii. The bottom panel of Figure III.4 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 results for QWI mobility variables are confirmed by regression results in Table III.6. We find that there is an increase in 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% relative to other Tech sectors in column 1 translates to a 13.7% increase in mobility when compared to the mean separation rate of 9.1% for Tech sector in the pre-ban period. The point estimate is smaller in column 3 (0.272%, or 3% of pre-ban mean separation rate) though this overall effect is a combination of a surge in the short run (0.753% or 8.3% of pre-ban mean separation rate) and 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% of the pre-ban mean separation rate. Results for the alternative mobility variable in columns 5 to 8 are similar to the overall separation rate variable in columns 1-4, for both panels.

Results from the analysis of mobility using CPS data are similar. Figure III.5 reports the trends from the CPS for the dummy indicator variable for transition from one employer to another. Consistent with the trends from the QWI, we find a significant jump in mobility soon after the ban, and a greater volatility in the post ban period, both relative to trends for other industries within Hawaii, and also relative to Tech sector in other states. Again, the results suggest the ban facilitated mobility of Tech workers. The corresponding regression results in Table III.7 are very similar. There is evidence of systematically higher mobility in both the short and long run in the Within-Hawaii (Panel A) and Cross-State (Panel B) analysis, across almost all of the alternative specifications and samples. We have no mobility in CPS Tech sample in the 24 months prior to the ban, so the post-ban increases are striking; relative to the overall (across-all sectors) pre-ban mean mobility of 1.4%, the post ban increases of 4.4 percentage points (Panel A, column 2), 7.2 percentage points (Panel A, column 5), 6.5 percentage points (Panel B, column 2), and 6.6 percentage points (Panel B, column 6,) are materially large (in range of 314% to 514% increase relative to overall pre-ban mean mobility) and statistically significant as well.

Turning to the impact on wages, Figure III.6 presents the binned scatter plot of the time trends of the wage variables from QWI. In the top panel, the pre-ban trends in the wage variables are similar across 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). But 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. Again, the binned scatter patterns are largely confirmed by the regression results in Table III.8: while the overall post-ban change is small and not 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 significant than appears in the binned scatter plots. The magnitude of the cross-state effects is largest for the hiring wage, which shows increases of 0.078 log points in column 5 (or (0.071 in column 7), which is over 50% of the standard deviation in the pre-ban log wage rate for Tech workers in Hawaii (0.14 log points).[102] Overall, the results suggest modest effects on overall wages, but stronger increases in hiring wages (especially in the short run).

The DID analysis are confirmed by the DDD results in Table III.9 (QWI mobility and wage variables), and Table III.10 (CPS mobility variable), which find an average increase of 10.8% in overall separation rates and a 4.2% increase in new-hire wages after the ban. These results are reassuring, showing that the increase in mobility and 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.

In Appendix 4, 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 4-digit sectors (in the Within-Hawaii analysis) and across states (in the Cross-State analysis), allowing us to examine how our point estimates compare to the distribution of potential point estimates using similar sized alternative subsets of industries

[102] 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 are hence unlikely to be affected by the CNC ban, the lower magnitude of effects on overall average wage is not surprising.

and states. Table A4.1 shows that the there is an overall increase in both mobility measures (i.e., Overall Separation Rate and Beginning-of-Quarter Separation Rate) in the Cross-State analysis (column 3), as well as a significant (at 10%) short-run increase in the beginning of quarter separation rate even in the Within-Hawaii analysis; Table A4.3 confirms mobility increases overall (column 1) and in the short run (column 2) from the DDD analysis are statistically significant even with the permutation tests. The QWI overall average wage results presented in Table A4.1 are small and insignificant in the Within-Hawaii analysis, but larger and significant in the Cross-State analysis (column 3); Table A4.3 suggests stronger significance for short- run results but not above conventional cutoff levels. The hiring wage is also not significant in the Within-Hawaii analysis at conventional cutoff levels (in column 1 of Table A4.1), but the baseline regression estimate is systematically significant in comparison to the Tech sector for other states (columns 3 and 4 of Table A4.1), and for the triple difference analysis (Table A4.3) overall (column 1) and in the short-run (column 2). In Table A4.2, the randomization inference tests show that the mobility increases observed in the CPS from the baseline DID regressions are significant at the 10% level both in the within-Hawaii analysis (columns 1 and 2) as well as for the cross-state analysis (columns 3 and 4); Table A4.4 confirms significance of the DDD CPS mobility results.

For the Cross-State, Within-Tech analysis, we check the robustness of our results to using a synthetic control group approach proposed by Abadie, Diamond and Hainmueller (2010) to address challenges associated with the construction of a suitable control group. Under 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" state relative to placebo runs involving other states (as in Figure 8 in Abadie et al. 2010). In Appendix 5, we present synthetic matched control analysis comparing the Tech sector in Hawaii to the Tech sector in others states.[103] Figure A5.1 shows that for the QWI mobility variables there is a close match between Hawaii and the synthetic control state in the pre-ban period, and then a significant upward deviation for Hawaii. The permutation tests (bottom panel of Figure A5.1) shows that these effects are highly statistically significant, with the relative deviation for Hawaii being the largest among all the placebo runs for both mobility variables. Figure A5.2 presents results for wage variables, and the synthetic control again shows a good fit for the pre-ban trends for both wage variables. While the post-ban upward deviation is modest for the overall average (and has a p-value of only 0.19 in the permutation test), this is more striking for the average hiring wage, and statistically significant as well (p-value 0.07). Figure A5.3 confirms that the mobility variable in the CPS shows a significant upward deviation in the post-ban period. Thus, the synthetic control analysis confirms strong increases in mobility and hiring wages for Hawaii Tech workers relative to Tech workers in other states.[104]

[103] We use Abadie et al's (2010) Stata procedure "synth" (made available on Jens Hainmueller's website https://web.stanford.edu/~jhain/synthpage.html) to execute the analysis.

[104] We also checked robustness of the DDD QWI results to including industry X year-quarter log employment as a control. This addresses a potential concern that coincidental labor supply shocks could be driving people out of the tech sector in Hawaii; while such a supply shift (say facilitating exit for Hawaii workers) could be in fact induced by the ban on CNCs and so may not necessarily indicate the presence of some other shocks, results presented in Appendix 7 Table A7.9 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.

6. Discussion

This study uses detailed matched employer-employee data from the US Census Bureau and a recent natural experiment in Hawaii to examine how CNC enforceability affects the rate and direction of employee mobility and the time path of wages both within a job and across the employee's career. Cross-sectional variation in CNC enforceability is associated with longer job spells, a greater likelihood of leaving the state, and a reduced propensity for cross-industry movement. Most importantly, we also find that compared with their peers in low-enforceability states, workers in states with high enforceability receive reduced wages throughout a given job as well as over their career. The longitudinal analysis of the recent-ban of CNCs for tech workers in Hawaii suggests that the ban increased mobility as well as wages at hiring for tech workers.

Our finding of longer job spells, which is consistent with predictions generated by our model (see Appendix 6) under most scenarios, is also 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 with greater accuracy.

Further, our findings fill two important gaps related to (i) joint state-industry switching behavior and (ii) variations in the effect of CNC enforceability over job tenure. Regarding switching, we find that the increased propensity to move across states but the decreased propensity to switch across industries is explained primarily by individuals 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).

Over the employee job spell, we find that the potential impact of CNC enforceability is lowest at short tenures, but rises at longer tenures. Table III.1 shows that the enforceability-related increase in survival probability of a job spell at the 4th quarter is 0.02 percentage points, far less than at the 24th quarter (0.57 percentage points). 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.[105] The higher impact at mid-tenure is consistent with Lazear and Gibbs (2014, pp. 82–85), suggesting that the value of 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 (2014) 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 one year into the job and throughout the employee's tenure.[106] In this respect, our results are similar to those in Garmaise (2011) and Starr (2018). The cumulative effect of this wage reduction over an eight-year tenure is 3.9%

[105] 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.

[106] A potential reason for the difference is that we examine effects of enforceability which 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 a commitment to higher wages and wage raises in return for signing a CNC agreement (Black and Loewenstein, 1991).

lower cumulative earnings, based on the coefficients in Table III.3.[107] 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 the employee suggests that starting a job in an average enforceability state — regardless of whether the individual eventually leaves that state — is associated with reduced earnings of 4.6% eight years after starting the job compared to a worker in a non-enforcing state. These findings highlight that the initial legal conditions of employment may have persistent effects on future earnings, similar to how entering the job market in a recession has long-run effects on workers (Oreopoulos, von Wachter, and Heisz 2012). Indeed, the mechanism highlighted in Oreopoulos, von Wachter, and Heisz (2012) is the rate at which workers move to better jobs, which is precisely the role that CNC enforceability plays as a within-industry mobility friction.

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 (2018). Our results are thus consistent with CNC enforceability creating monopsony power, leading to deviations from the law of one wage, and reducing the elasticity of labor supply (Bhasker, Manning and To, 2002, Manning 2011, Ashenfelter et al. 2010), and dampening labor market dynamism (Furman and Krueger 2016). Consistent with Krueger's (2017) concerns about CNCs facilitating collusion quoted at the beginning of the paper, our findings highlight a potential similarity between labor

[107] A related possibility is that wages at the beginning of a job (which get subsumed by the starting wage fixed effects in our specifications) are higher in high-enforceability states, so workers in such states do not suffer any net wage losses over the entire job spell. However, we do not find any supporting evidence for this line of argument. Rather, subject to the caveat that data on detailed occupational characteristics is unavailable, we find that CNC enforceability is not associated with higher initial wages (Appendix 7 Table A7.7).

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).[108] Mukherjee and Vasconcelos (2012) model these agreements as alternate mechanisms for extracting surplus from productive workers, and our findings suggest that the outcomes for workers from high CNC enforceability may be similar to those due to labor market collusion under oligopsony (Kruger and Posner 2018).

To the extent that lower wages over workers' careers reflect a decline in match quality between workers and firms, our results suggest that allocative efficiency is lower when there is higher CNC enforceability. Further, if mobility generates knowledge spillover effects, as argued in the literature (Gilson 1999), then potential gains from increased firm investments in human capital (if any) have to be significantly high to offset these negative welfare effects.

We conclude with a brief discussion of future avenues for research. To keep the study focused, we did not examine any inter-industry heterogeneity. There may be industries where high enforceability benefits workers (e.g., as Lavetti, Simon, and White 2014 find among physicians), and future work could shed light on industry characteristics that moderate the impact of CNC enforceability. Further, while our results suggest that (potentially undetected) labor market collusion in low-enforceability states does not fully replicate the effects of formal CNC enforceability, there could be variations across sectors in the sustainability of labor market collusion (e.g., based on industry or labor market concentration), which could be another interesting source of heterogeneity worth further study.

[108] 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."

Also, as for most other studies, we cannot identify in the data who is and who is not bound by a CNC. To the extent that some employees are able to negotiate exemptions from CNCs, our results could be viewed as attenuated towards zero. With additional data collection, future work could jointly examine drivers of the actual use of CNC clauses and effects of enforceability on wages and mobility, disentangling the effect on those bound by CNC clauses and potential external effects on those not bound by them.

Time-series data on the actual use of CNCs would allow one to assess the role of potential increases in CNC usage over time in (at least partially) explaining the decline in U.S. labor market dynamism and wage stagnation (Davis and Haltiwanger 2014, Hyatt and Spletzer 2013, Biden 2016, Council of Economic Advisers 2016). Irrespective of the role of CNCs in lower dynamism, our results suggest that reductions in CNC enforceability could be one effective lever available to policymakers for increasing labor market dynamism.

Finally, our findings suggest a potential impact of CNC enforceability on aggregate productivity, which could be explored in future work. As discussed above, our finding of lower wages in high-enforceability states may suggest poorer worker-firm matching, suggesting that CNC enforceability could reduce allocative efficiency in a manner similar to other frictions found to impede labor reallocation in e.g., the literature on employment protection (Autor, Kerr and Kugler 2007; Petrin and Sivadasan 2013).

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Figure III.1. Factor Analysis CNC Enforceability Index Scores for 2009

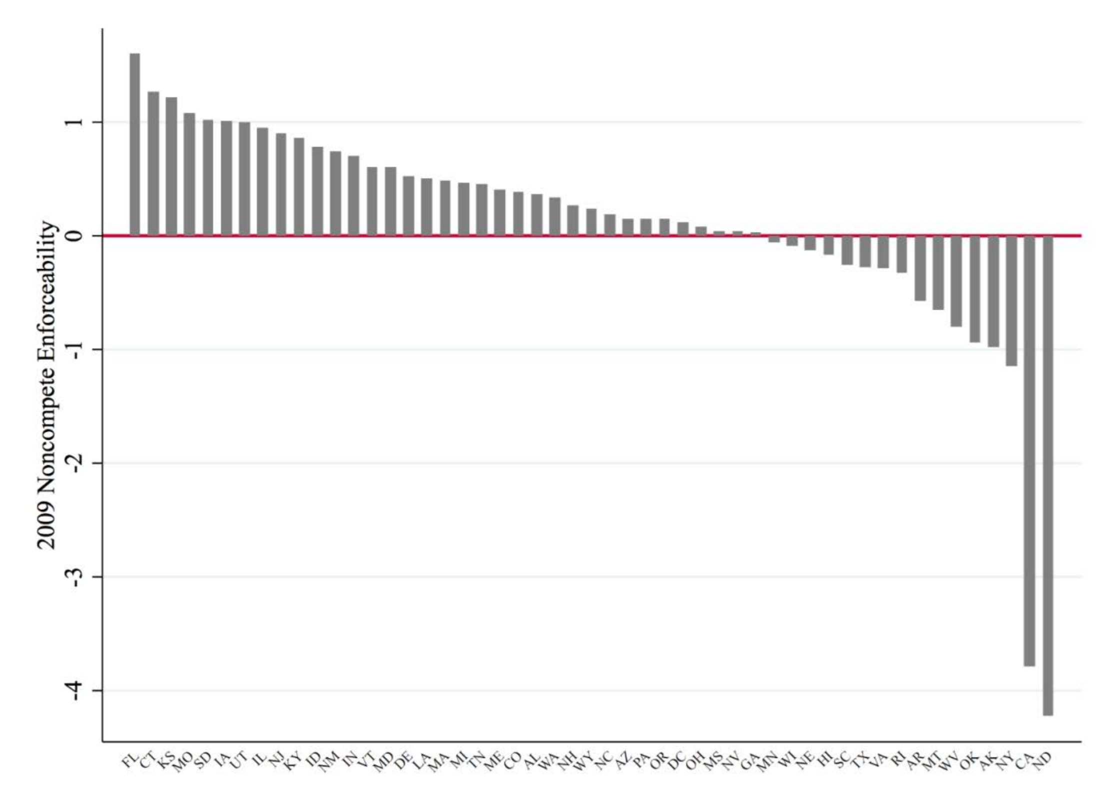


Figure III.2. CNCs and High-Tech Workers' Mobility across Job Tenure (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential relation of CNC enforceability with mobility, for high-tech jobs relative to non-tech jobs. Mobility is measured as the dummy variable for the spell surviving at 4th, …, 32nd quarter of the job spell.

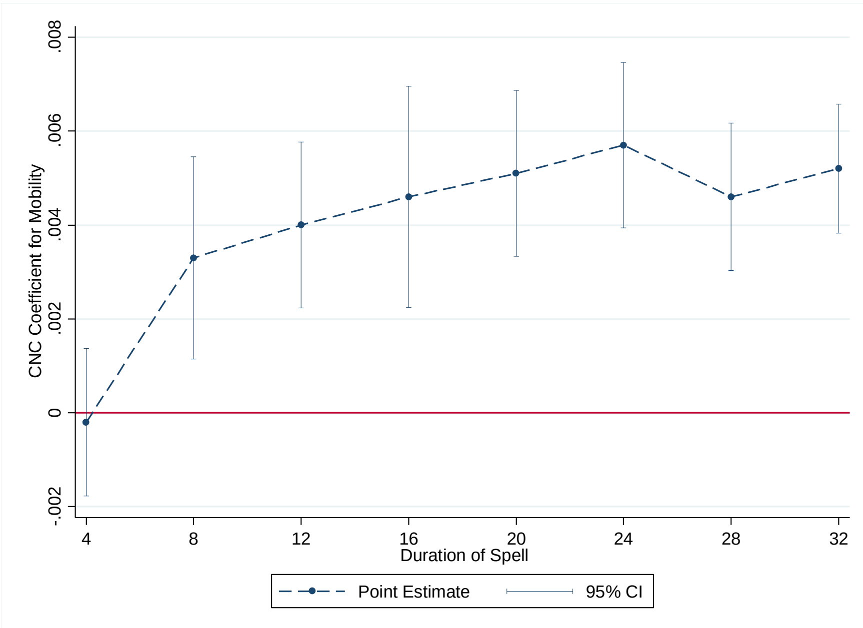


Figure III.3. CNCs and High-Tech Workers' Wage across Job Tenure (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential relation of CNC enforceability with wage, for high-tech jobs relative to non-tech jobs. Wage is the log of quarterly wage at 4th, …, 32nd quarter of the job spell.

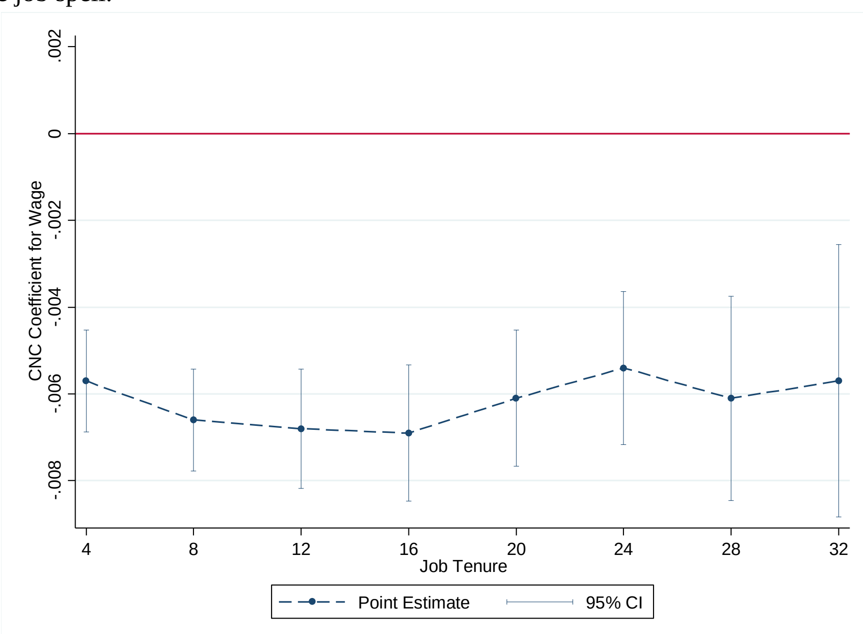


Figure III.4. Hawaii CNC Ban and Mobility Variables from QWI

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 is 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 4-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.

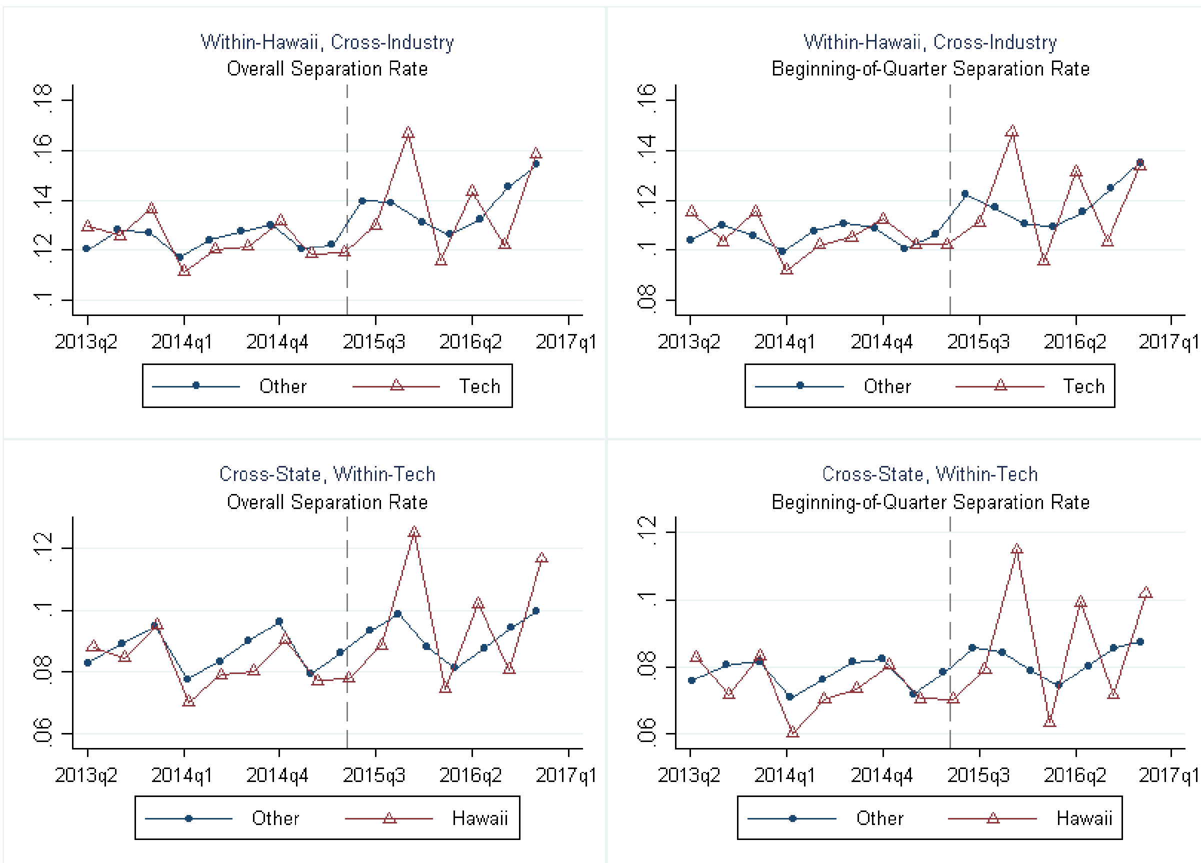


Figure III.5. Hawaii CNC Ban and Mobility in CPS

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 is 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 4-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 is a dummy indicator for leaving employer between month t and t+1. The group average means are weighted means, with CPS sample weights as (analytical) weights.

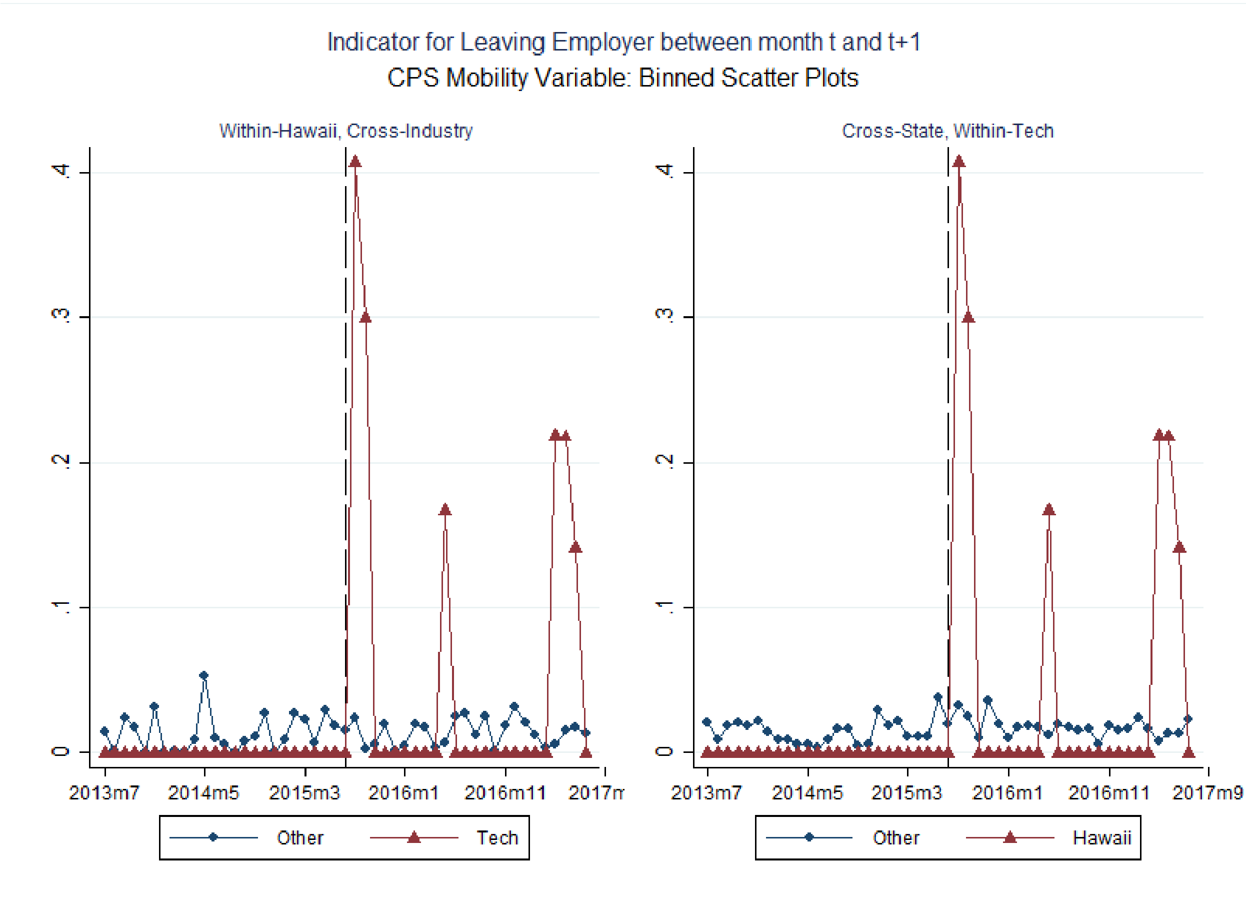


Figure III.6. Hawaii CNC Ban and Wage Variables from QWI

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 is 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 4-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) (i.e., log EarnS). Log Hires Average Monthly Earnings is the log of Average Monthly Earnings of All Hires into Full Quarter Employment (i.e., log EarnHirAS). The group average means are weighted means, with industry-period Beginning of Quarter Employment (Emp) as (analytical) weights.

Table III.1. CNCs and High-Tech Workers' Mobility (LEHD)

This table reports the differential effect of CNC enforceability on mobility by industry (high-tech jobs vs. non-tech jobs). The dependent variables are dummy variables for the job spell surviving at 4^{th}, …, 32^{nd} 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). All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable: Job spell survival at	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr	(9) Ln(job-spell)
Tech X 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)
# of observations	12984300	12425700	11971100	11602500	11334900	11127400	10861700	10661700	6492100
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

Table III.2. CNCs and High-Tech Workers' Wage across Job Tenure (LEHD)

This table reports the differential effect of CNC enforceability on wage across job tenure by industry (high-tech jobs vs. non-tech jobs). The dependent variables are the log of quarterly wages at 4^{th}, …, 32^{nd} quarter of the job spell. CNC Score is measured as the 2009 CNC enforceability index scores. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable: Log of wage at xth quarter	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X CNC Score	-0.0057***	-0.0066***	-0.0068***	-0.0069***	-0.0061***	-0.0054***	-0.0061***	-0.0057***
	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0012)	(0.0016)
# of observations	10904200	7397200	5399500	4048400	3145300	2478900	1858400	1412600
R-squared	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 Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Table III.3. CNCs and High-Tech Workers' Cumulative Wage and Wage Growth across Job Tenure (LEHD)

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. non-tech jobs). The dependent variables are the log of cumulative wage at 4^{th}, 8^{th}, …, 32^{nd} quarter of the job spell for Panel A, and the difference between the log of quarterly wages at 4^{th}, 8^{th}, …, 32^{nd} 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. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Cumulative Wage

Dependent Variable: Log of cumulative wage at	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X 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)
# of observations	10904000	7397000	5399000	4048000	3145000	2479000	1858000	1413000
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							

Panel B. Wage Growth

Dependent Variable: Log of wage at xth quarter - Log of initial wage	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X 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)
# of observations	10904000	7397000	5399000	4048000	3145000	2479000	1858000	1413000
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							

Table III.4. CNCs and High-Tech Workers' Career Outcomes across Employment History (LEHD)

This table reports the differential effect of CNC enforceability on cumulative number of jobs taken across workers' employment history in Panel A, and on cumulative earnings across workers' employment history in Panel B, by industry (high-tech jobs vs. non-tech jobs) of the worker's first job. The dependent variables are the log of cumulative number of jobs taken at 4^{th}, …, 32^{nd} quarter of the workers' employment history in Panel A, and the log of cumulative earnings at 4^{th}, …, 32^{nd} quarter of the workers' employment history in Panel B. 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 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. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Number of Jobs across Employment History

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log of cumulative number of jobs at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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)
# of observations	7517000	6389000	5594000	4973000	4485000	4057000	3671000	3229000
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							

Panel B. Cumulative Earnings across Employment History

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log of cumulative earnings at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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)
# of observations	7517000	6389000	5594000	4973000	4485000	4057000	3671000	3229000
R-squared	0.6245	0.6121	0.5951	0.5778	0.5603	0.5448	0.5291	0.5143
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All employed workers in the quarter							

Table III.5. CNCs and High-Tech Workers' Switching States or Industries (LEHD)

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. non-tech 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 4^{th}, …, 32^{nd} 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. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Switch States	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Ln(1+cumulative # of state switch) at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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
Panel B. Switch Industry	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Ln(1+cumulative # of industry switch) at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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
# of observations	7517000	6389000	5594000	4973000	4485000	4057000	3671000	3229000
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All employed workers in the quarter							

Panel C. Switch State but not Industry	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Ln(1+cumulative # of state switch without industry switch) at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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
Panel D. Switch Industry but not State	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable: Ln(1+cumulative # of industry switch without state switch) at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X 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
# of observations	7517000	6389000	5594000	4973000	4485000	4057000	3671000	3229000
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All employed workers in the quarter							

Table III.6. QWI Mobility Analysis from the Hawaii Natural Experiment – Difference-in-Differences Results

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the industry level in Panel A and state level in Panel B. Data is from the QWI, 2013Q2 to 2017Q2. Data is limited to the state of Hawaii in Panel A, and to "Tech" industries in Panel B. "Tech" is defined as QWI 4-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 Cols 1 to 4 is the Overall Separation Rate defined as All Separations (i.e., Sep) divided by Employment in the Reference Quarter (i.e., EmpTotal). The dependent variable in Cols 5 to 8 is the Beginning-of-Quarter separation rate (i.e., SepBegR). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3 to 2016Q2, and LR_Post is 2016Q3 to 2017Q2. In Panel A, Cols 1-2 and 5-6 are limited to 4-digit industries within the two-digit industries that contain the tech industries, while other columns include all industries. In Panel B, Cols 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, i.e., 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).

	Overall Separation Rate				Quarter Beginning Separation Rate			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Cross-Industry, Within-Hawaii								
Post X Tech	0.0125*		0.00272		0.0109		0.00373	
	(0.00701)		(0.00219)		(0.00737)		(0.00282)	
SR_Post X Tech		0.0187***		0.00753***		0.0219***		0.00974***
		(0.00487)		(0.00206)		(0.00653)		(0.00286)
LR_Post X Tech		-0.000164		-0.00728**		-0.0118		-0.00876**
		(0.0185)		(0.00369)		(0.0211)		(0.00417)
# of 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
Panel B: Cross-State, Within-Tech								
Post X HI	0.0114***		0.0115***		0.0113***		0.0114***	
	(0.00119)		(0.000875)		(0.000966)		(0.000716)	
SR_Post X HI		0.0135***		0.0136***		0.0145***		0.0144***
		(0.00128)		(0.000930)		(0.000567)		(0.000504)
LR_Post X HI		0.00706***		0.00718***		0.00479*		0.00514***
		(0.00237)		(0.00170)		(0.00241)		(0.00173)
# of 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 X Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table III.7. CPS Mobility Analysis from the Hawaii Natural Experiment – Difference-in-Differences Results

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the industry level in Panel A and at the state level in Panel B. Data is from the CPS, July 2013 to July 2017. Data is limited to the state of Hawaii in Panel A, and to "Tech" industries in Panel B. "Tech" is defined as QWI 4-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 is a dummy indicator for leaving employer between month t and t+1. "Post" is defined as July 2015 and afterwards; SR_Post is 2015m7 to 2016m8, and LR_Post is 2016m9 to 2017m7. In Panel A, Cols 1-4 are limited to 4-digit industries within the two-digit industries that contain the tech industries, while other columns include all industries. In Panel B, Cols 1-4 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 CPS sample weights as (analytical) weights. Number of observations adjusts for weights and singleton cells, i.e., drops zero weights and singleton-cells (when fixed effects are added). The mean (sd) of the dummy dependent variable for Tech industries in the pre-July 2015 period is 0 (0) and for the full sample in the pre-July 2015 period is 0.014 (0.115).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Cross-Industry, Within-Hawaii								
Post X Tech	0.035**	0.044**			0.073***	0.072***		
	(0.015)	(0.017)			(0.023)	(0.023)		
SR_Post X Tech			0.042	0.047*			0.080***	0.078***
			(0.025)	(0.024)			(0.014)	(0.014)
LR_Post X Tech			0.029***	0.041***			0.067**	0.067**
			(0.008)	(0.012)			(0.033)	(0.033)
# of observations	537	537	537	537	17,226	17,226	17,226	17,226
R-squared	0.298	0.308	0.298	0.308	0.047	0.049	0.047	0.049
Sample	Tech2d	Tech2d	Tech2d	Tech2d	All	All	All	All
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year by Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Cross-State, Within-Tech								
Post X HI	0.055***	0.065***			0.056***	0.066***		
	(0.002)	(0.003)			(0.002)	(0.002)		
SR_Post X HI			0.056***	0.071***			0.056***	0.070***
			(0.003)	(0.004)			(0.002)	(0.003)
LR_Post X HI			0.054***	0.060***			0.055***	0.062***
			(0.003)	(0.003)			(0.002)	(0.003)
# of observations	26,181	26,178	26,181	26,178	37,104	37,100	37,104	37,100
R-squared	0.014	0.029	0.014	0.029	0.011	0.022	0.011	0.023
Sample	40 States	40 States	40 States	40 States	All	All	All	All
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	NA	Yes	NA	Yes	NA	Yes	NA
Year by Month FE	Yes	NA	Yes	NA	Yes	NA	Yes	NA
State FE	Yes	NA	Yes	NA	Yes	NA	Yes	NA
Ind X Year-Month	No	Yes	No	Yes	No	Yes	No	Yes
State X Ind	No	Yes	No	Yes	No	Yes	No	Yes

Table III.8. QWI Wage Variables Analysis from the Hawaii Natural Experiment – Difference-in-Differences Results

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the industry level in Panel A and state level in Panel B. Data is from the QWI, 2013Q2 to 2017Q2. Data is limited to the state of Hawaii in Panel A, and to "Tech" industries in Panel B. "Tech" is defined as QWI 4-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 Cols 1 to 4 is the log of overall Average Monthly Earnings (Full Quarter Employment) (i.e., log EarnS). The dependent variable in Cols 5 to 8 is the log of the Average Monthly Earnings of All Hires into Full Quarter Employment (i.e., log EarnHirAS). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3 to 2016Q2, and LR_Post is 2016Q3 to 2017Q2. In Panel A, Cols 1-2 and 5-6 are limited to 4-digit industries within the two-digit industries that contain the tech industries, while other columns include all industries. In Panel B, Cols 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, i.e., 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).

	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 X Tech	-0.000726		-0.00506		0.0389		0.0259***	
	(0.0245)		(0.0147)		(0.0348)		(0.00973)	
SR_Post X Tech		0.0190		0.00269		0.0943***		0.0440***
		(0.0187)		(0.0115)		(0.0278)		(0.0125)
LR_Post X Tech		-0.0414		-0.0212		-0.0751		-0.0115
		(0.0381)		(0.0216)		(0.0683)		(0.0139)
# of 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
Panel B: Cross-State, Within-Tech								
Post X HI	0.0223***		0.0178***		0.0778***		0.0711***	
	(0.00287)		(0.00430)		(0.00541)		(0.00617)	
SR_Post X HI		0.0217***		0.0180***		0.0825***		0.0776***
		(0.00313)		(0.00362)		(0.00602)		(0.00607)
LR_Post X HI		0.0237***		0.0175**		0.0681***		0.0575***
		(0.00548)		(0.00700)		(0.00584)		(0.00739)
# of 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 X Year-Qtr FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Ind FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table III.9. QWI Mobility and Wage Analysis from the Hawaii Natural Experiment – Triple Difference Results

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the state level. Data is from the QWI, 2013Q2 to 2017Q2. "Tech" is defined as QWI 4-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 Cols 1 to 4 is the Overall Separation Rate defined as All Separations (i.e., Sep) divided by Employment in the Reference Quarter (i.e., EmpTotal), and in Cols 5 to 8 is the Beginning-of-Quarter separation rate (i.e., SepBegR). In Panel B, the dependent variable in Cols 1 to 4 is the log of overall Average Monthly Earnings (Full Quarter Employment) (i.e., log EarnS), and in Cols 5 to 8 is the log of the Average Monthly Earnings of All Hires into Full Quarter Employment (i.e., log EarnHirAS). "Post" is defined as July 2015 and afterwards; SR_Post is 2015Q3 to 2016Q2, and LR_Post is 2016Q3 to 2017Q2. Cols 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, i.e., 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). 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).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: QWI Mobility Variables	Overall Separation Rate				Beginning-of-Quarter Separation Rate			
Post X HI X Tech	0.0104***		0.00979***		0.0104***		0.00960***	
	(0.00180)		(0.00130)		(0.00108)		(0.000912)	
SR_Post X HI X Tech		0.0114***		0.0112***		0.0129***		0.0126***
		(0.00177)		(0.00125)		(0.000840)		(0.000644)
LR_Post X HI X 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
Panel B: QWI Wage Variables	Log Overall Average Monthly Earnings				Log Hires Average Monthly Earnings			
Post X HI X Tech	0.00964***		0.00712**		0.0441***		0.0424***	
	(0.00282)		(0.00270)		(0.00457)		(0.00361)	
SR_Post X HI X Tech		0.0121***		0.0100***		0.0558***		0.0548***
		(0.00276)		(0.00238)		(0.00479)		(0.00352)
LR_Post X HI X 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
Sample	40 States	40 States	All	All	40 States	40 States	All	All
Ind X Year-Qtr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Ind	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Year-Qtr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table III.10. CPS Mobility Analysis from the Hawaii Natural Experiment – Triple Difference Results

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the state level. Data is from the CPS, July 2013 to July 2017. "Tech" is defined as QWI 4-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 is a dummy indicator for leaving employer between month t and t+1. "Post" is defined as July 2015 and afterwards; SR_Post is 2015m7 to 2016m8, and LR_Post is 2016m9 to 2017m7. Cols 1-4 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 CPS sample weights as (analytical) weights. Number of observations adjusts for weights and singleton cells, i.e., drops zero weights and singleton-cells (when fixed effects are added). The mean (sd) of the dummy dependent variable for Tech industries in the pre-July 2015 period is 0 (0) and for the full sample in the pre-July 2015 period is 0.014 (0.115).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post X HI X Tech	0.057***	0.068***			0.058***	0.069***		
	(0.002)	(0.002)			(0.002)	(0.002)		
SR_Post X HI X Tech			0.061***	0.075***			0.060***	0.074***
			(0.002)	(0.002)			(0.002)	(0.002)
LR_Post X HI X Tech			0.054***	0.062***			0.056***	0.063***
			(0.003)	(0.003)			(0.003)	(0.003)
HI X Tech	-0.012***		-0.012***		-0.013***			
	(0.001)		(0.001)		(0.001)			
Observations	899,350	899,165	899,350	899,165	1,219,093	1,218,873	1,219,093	1,218,873
R-squared	0.024	0.033	0.024	0.033	0.019	0.028	0.019	0.028
Sample	40 States	40 States	40 States	40 States	All	All	All	All
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Ind FE	No	Yes	No	Yes	No	Yes	No	Yes
State X Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind X Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

APPENDICES

Appendix 1

Supplementary Tables (Chapter I)

Table A1.1. Summary Statistics

Variable	N	Mean	Std. Dev.	Min	Max
Two Year Average Establishment Growth	5241	-0.034	0.156	-1.561	0.907
High Productivity Worker Voluntary Turnover Rate	5241	0.023	0.042	0	0.808
Low Productivity Worker Voluntary Turnover Rate	5241	0.032	0.055	0	0.964

Table A1.2. Effect of High Productivity Worker Turnover vs Low Productivity Worker Turnover on Future Growth: Voluntary Worker Turnover and Involuntary Worker Turnover

Dependent Variable	(1)	(2)
	Future Establishment Employment Growth	
Turnover Rate: High Productivity Worker	-0.292***	-0.007
	(0.099)	(0.041)
Turnover Rate: Low Productivity Worker	0.106	-0.022
	(0.109)	(0.062)
Turnover Type	Voluntary	Involuntary
Estab Quality Dummies	Yes	Yes
State-Ind-Year FE	Yes	Yes
N	5241	5241
R2	0.266	0.262
P-Value (H≠L)	0.0212	0.857

Table A1.3. Effect of Voluntary Worker Turnover on Future Growth: Quartile Analysis

	(1)	(2)	(3)
Dependent Variable	Future Establishment Employment Growth		
Turnover Rate: High Productivity Worker	-0.300*	-0.309**	-0.003
	(0.155)	(0.155)	(0.141)
Turnover Rate: Medium High Productivity Worker	-0.241	-0.233	-0.462**
	(0.188)	(0.191)	(0.186)
Turnover Rate: Medium Low Productivity Worker	-0.162	-0.150	0.011
	(0.172)	(0.173)	(0.141)
Turnover Rate: Low Productivity Worker	0.317**	0.309**	0.137
	(0.154)	(0.153)	(0.145)
Marginal Productivity Measure	Imputed Wage	Raw Wage	Worker Human Capital
Estab Quality Dummies	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes
N	5241	5241	5241
R2	0.268	0.268	0.266
P-Value (H≠L)	0.008	0.008	0.517

Table A1.4. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Labor Productivity Growth

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Future Sales/Employment Growth			Future Value Added/Employment Growth		
Turnover Rate: High Productivity Worker	-0.132	-0.125	-0.099	-0.301*	-0.290*	-0.103
	(0.126)	(0.128)	(0.110)	(0.162)	(0.163)	(0.148)
Turnover Rate: Low Productivity Worker	0.168	0.162	0.178*	0.292*	0.284*	0.152
	(0.113)	(0.113)	(0.106)	(0.160)	(0.162)	(0.180)
Marginal Productivity Measure	Imputed Wage	Raw Wage	Worker Human Capital	Imputed Wage	Raw Wage	Worker Human Capital
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
N	2840	2840	2840	2271	2271	2271
R2	0.316	0.316	0.316	0.318	0.318	0.316
P-Value (H≠L)	0.139	0.161	0.113	0.0265	0.035	0.337

Table A1.5. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Using Turnover Rate at *t-1* or *t-2* instead of the average of *t-1* and *t-2*

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable	Future Establishment Employment Growth					
Turnover Rate: High Productivity Worker	-0.251***	-0.318*	-0.243***	-0.156***	-0.216**	-0.155**
	(0.073)	(0.182)	(0.073)	(0.057)	(0.101)	(0.073)
Turnover Rate: Low Productivity Worker	0.032	0.042	0.034	0.083	0.221	0.064
	(0.061)	(0.146)	(0.063)	(0.077)	(0.231)	(0.071)
Turnover Rate at Time:	t-1			t-2		
Turnover Type	Voluntary Turnover	Mass Voluntary Turnover	Non-mass Voluntary Turnover	Voluntary Turnover	Mass Voluntary Turnover	Non-mass Voluntary Turnover
N	6432	6432	6432	5241	5241	5241
R2	0.257	0.255	0.256	0.264	0.263	0.264
P-Value (H≠L)	0.007	0.183	0.007	0.030	0.157	0.054
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table A1.6. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: The Extent of the Turnover Effect

	(1)	(2)	(3)	(4)	(5)
Dependent Variable	Growth at t+1	Growth at t+2	Growth at t+3	Growth at t+4	Growth at t+5
Turnover Rate: High Productivity Worker	-0.385***	-0.298***	-0.110	0.100	0.138
	(0.109)	(0.091)	(0.089)	(0.103)	(0.121)
Turnover Rate: Low Productivity Worker	-0.000	0.037	0.038	0.113	0.093
	(0.078)	(0.082)	(0.079)	(0.091)	(0.089)
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes
N	6866	6432	5361	4380	3428
R2	0.250	0.246	0.262	0.262	0.275

Table A1.7. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: The Tenure Effect with Tenure in Quartiles

	(1)	(2)	(3)
Dependent Variable	Future Establishment Employment Growth		
Turnover Rate: Tenure Q4 - High Productivity	-0.824**	-0.838**	-0.893**
	(0.398)	(0.405)	(0.382)
Turnover Rate: Tenure Q3 - High Productivity	-0.465	-0.455	-0.100
	(0.376)	(0.376)	(0.314)
Turnover Rate: Tenure Q2 - High Productivity	-0.590*	-0.586*	-0.480
	(0.329)	(0.329)	(0.327)
Turnover Rate: Tenure Q1 - High Productivity	0.047	0.042	0.012
	(0.147)	(0.145)	(0.167)
Turnover Rate: Tenure Q4 - Low Productivity	0.525	0.547	0.647
	(0.950)	(0.946)	(1.029)
Turnover Rate: Tenure Q3 - Low Productivity	0.209	0.211	-0.039
	(0.435)	(0.439)	(0.478)
Turnover Rate: Tenure Q2 - Low Productivity	-0.267	-0.277	-0.540*
	(0.299)	(0.301)	(0.311)
Turnover Rate: Tenure Q1 - Low Productivity	0.116	0.119	0.168
	(0.119)	(0.119)	(0.110)
Marginal Productivity Measure	Imputed Wage	Raw Wage	Worker Human Capital
Estab Quality Dummies	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes
N	5241	5241	5241
R2	0.270	0.270	0.269

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables

Panel A. Source Establishment Replacement Costs Proxy: Propensity of Having Unfilled Vacancies

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.223***	0.126***		0.141***	0.115***	
	(0.033)	(0.037)		(0.021)	(0.021)	
Inflow Rate: Low Productivity Worker	0.034**	0.106***		0.023**	0.048***	
	(0.013)	(0.015)		(0.009)	(0.010)	
Turnover Rate: High Productivity Worker			-0.834*			-1.255
			(0.467)			(1.584)
Turnover Rate: Low Productivity Worker			0.853*			2.094
			(0.471)			(1.507)
Replacement Costs Proxy	Propensity of Having Unfilled Vacancies					
Subsample	High			Low		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	23.48	36.41		34.54	27.07	
N	3474	3474	3474	1395	1395	1395
R2	0.521	0.577	0.247	0.466	0.510	0.202

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables (*continued*)

Panel B. Source Establishment Replacement Costs Proxy: Propensity of the Establishment Covering Training Costs

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.181***	0.118***		0.180***	0.082***	
	(0.027)	(0.032)		(0.023)	(0.019)	
Inflow Rate: Low Productivity Worker	0.026**	0.093***		0.020**	0.078***	
	(0.011)	(0.015)		(0.009)	(0.013)	
Turnover Rate: High Productivity Worker			-1.323*			-0.656
			(0.687)			(0.527)
Turnover Rate: Low Productivity Worker			0.955*			1.213**
			(0.573)			(0.589)
Replacement Costs Proxy	Propensity of the Establishment Covering Training Costs					
Subsample	Yes			No		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	24.88	28.99		46.02	34.87	
N	3097	3097	3097	1707	1707	1707
R2	0.568	0.645	0.227	0.519	0.545	0.308

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables (*continued*)

Panel C. Source Establishment Replacement Costs Proxy: High Educational Attainment Share

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.154***	0.090***		0.225***	0.191***	
	(0.023)	(0.023)		(0.041)	(0.047)	
Inflow Rate: Low Productivity Worker	0.015*	0.088***		0.026**	0.072***	
	(0.008)	(0.014)		(0.011)	(0.014)	
Turnover Rate: High Productivity Worker			-1.432**			-1.189
			(0.703)			(0.871)
Turnover Rate: Low Productivity Worker			0.830			1.572**
			(0.570)			(0.796)
Replacement Costs Proxy	High Educational Attainment Share					
Subsample	High			Low		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	31.70	28.98		15.37	26.75	
N	2930	2930	2930	1825	1825	1825
R2	0.415	0.465	0.253	0.662	0.687	0.217

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables (*continued*)

Panel D. Source Establishment Replacement Costs Proxy: R&D Worker Share

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.137***	0.067***		0.230***	0.165***	
	(0.025)	(0.017)		(0.034)	(0.037)	
Inflow Rate: Low Productivity Worker	0.020**	0.067***		0.019*	0.108***	
	(0.008)	(0.010)		(0.010)	(0.018)	
Turnover Rate: High Productivity Worker			-1.694*			-0.539
			(0.949)			(0.421)
Turnover Rate: Low Productivity Worker			1.487			0.709*
			(0.965)			(0.363)
Replacement Costs Proxy	R&D Worker Share					
Subsample	High			Low		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	27.81	40.12		23.31	28.92	
N	2608	2608	2608	2157	2157	2157
R2	0.412	0.447	0.237	0.663	0.644	0.301

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables (*continued*)

Panel E. Source Establishment Replacement Costs Proxy: Complexity in Operation

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.221***	0.155***		0.173***	0.086***	
	(0.052)	(0.046)		(0.017)	(0.021)	
Inflow Rate: Low Productivity Worker	0.037**	0.102***		0.022***	0.071***	
	(0.015)	(0.016)		(0.008)	(0.011)	
Turnover Rate: High Productivity Worker			-1.754**			-0.813
			(0.872)			(0.575)
Turnover Rate: Low Productivity Worker			1.491*			1.279**
			(0.793)			(0.621)
Replacement Costs Proxy	Complexity in Operation					
Subsample	High			Low		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	10.90	28.76		62.88	33.60	
N	2325	2325	2325	2509	2509	2509
R2	0.588	0.662	0.191	0.492	0.481	0.245

Table A1.8. Effect of High Productivity Worker Voluntary Turnover vs Low Productivity Worker Voluntary Turnover on Future Growth: Source Establishment Characteristics using Instrumental Variables (*continued*)

Panel F. Source Establishment Replacement Costs Proxy: Existence of Works Council

	(1)	(2)	(3)	(4)	(5)	(6)
	First Stage		Second Stage	First Stage		Second Stage
Dependent Variable	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth	Turnover Rate: High Productivity	Turnover Rate: Low Productivity	Future Estab. Employment Growth
Inflow Rate: High Productivity Worker	0.162***	0.056***		0.202***	0.179***	
	(0.030)	(0.015)		(0.027)	(0.047)	
Inflow Rate: Low Productivity Worker	0.012	0.058***		0.030***	0.109***	
	(0.008)	(0.009)		(0.009)	(0.021)	
Turnover Rate: High Productivity Worker			-1.309*			-0.414
			(0.694)			(0.648)
Turnover Rate: Low Productivity Worker			1.804**			0.525
			(0.814)			(0.503)
Replacement Costs Proxy	Works Council Exists					
Subsample	Yes			No		
Estab Quality Dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-Ind-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-stat	20.31	41.90		41.91	26.80	
N	3020	3020	3020	1544	1544	1544
R2	0.525	0.494	0.246	0.617	0.667	0.355

Appendix 2

Supplementary Tables (Chapter II)

Table A2.1. Summary Statistics of the Dependent Variables and the Independent Variables

This table reports the summary statistics of the dependent variables and the independent variables. The sample for the rows other than the second and third row (i.e., match improvement measures), is the estimation sample of regressing employment growth on non-layoff firing intensity, as in Table 4. The sample for the second and third row is the estimation sample of regressing the match improvement measures on non-layoff firing intensity, as in Table 2.

	(1)	(2)	(3)	(4)	(5)
	N	Mean	Std. Dev.	Min	Max
Employment Growth	2,818	-.0048	.3256	-1.6364	1.6667
Match Improvement: Standard Deviation	2,271	.0034	.4884	-2	2
Match Improvement: Low Match Quality Dispersion	1,261	-.0383	1.0414	-2	2
Firing Intensity: High Human Capital, All Fires	2,818	.0759	.3307	0	8
Firing Intensity: Low Human Capital, All Fires	2,818	.0986	.2684	0	4
Firing Intensity: High Human Capital, Non-Layoffs	2,818	.0412	.1310	0	1.5
Firing Intensity: Low Human Capital, Non-Layoffs	2,818	.0638	.1963	0	4

Table A2.2. Firing Intensity and Employment Growth: Using Industry Means of Match Quality Importance

This table reports the estimation results of estimating equation (2), using the firm's employment growth at year *t+1* as the dependent variable, on subsamples of firms by the benefits of trying out a new worker. Benefit of trying-out a new worker is characterized by the three-digit industry mean of match quality importance, where match quality importance is measured as the ratio of the standard deviation of the match quality component at the firm-year to the standard deviation of the human capital component at the firm-year. The independent variables are the firm's firing intensity of high human capital workers and firing intensity of low human capital workers at year *t*. Columns (1), (2) include all types of fired workers in calculating the firing intensity, and columns (3), (4) include workers fired during non-mass-layoff months only in calculating the firing intensity. All specifications include firm quality dummy variables and state-industry (two-digit)-year fixed effects. Sample Mean Employment denotes the mean of firm employment in the estimation samples. Standard errors are clustered by firm.

	(1)	(2)	(3)	(4)
Dependent Variable	Employment Growth			
Firing Intensity: High Human Capital	0.2306**	0.0025	0.1442	0.0691
	(0.0934)	(0.0229)	(0.2408)	(0.0751)
Firing Intensity: Low Human Capital	0.0733	0.0323	0.0495	0.0376
	(0.0449)	(0.0333)	(0.0559)	(0.0488)
Benefits of Trying-Out Proxy	Industry-Level Match-Importance			
Subsample	High	Low	High	Low
Firing Intensity:	All	All	Non-Layoffs	Non-Layoffs
Firm Quality Dummies	Yes	Yes	Yes	Yes
State-Ind-Year Fixed Effects	Yes	Yes	Yes	Yes
Sample Mean Employment	4.0339	3.5004	4.0339	3.5004
N	383	2248	383	2248
R2	0.4037	0.2076	0.3860	0.2081

Appendix 3

Triple Differences Exploiting Cross-Sectional Variation in Enforceability (Chapter III)

We extend the baseline analysis to a pseudo triple-differences approach, by focusing on a subgroup of workers in job spells with higher starting wages, which are likely to be more knowledge intensive reflecting higher levels of human capital, likely to develop greater appropriable intellectual capital, and are therefore more likely to be affected by CNC enforceability (Starr, Bishara, and Prescott 2016). Specifically, we use a dummy variable $I\{High\_Wage\_Init\}_{ij}$ for "high-initial-wage jobs" for the starting wage of the job being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes, and examine the following specification:

$$Y_{ijs} = \alpha + \beta_1 CNC_s * I\{Tech\}_{ij} * I\{High\_Wage\_Init\}_{ij} + \beta_2 CNC_s * I\{Tech\}_{ij}$$
$$+\beta_3 CNC_s * I\left\{High_{Wage_{Init}}\right\}_{ij} + \Sigma_s + FE_{ij} + \gamma fb_i + \varepsilon_{ijs} \qquad (A3.1)$$

All other terms are as described for Equation (1) in Section 3.4. Our coefficients of interest capturing differential effects of CNC enforceability are: (i) $\beta_1 + \beta_3$, which is the differential effect for high-initial-wage jobs compared with low-initial-wage jobs, within high-tech jobs, (ii) β_1, which is a pseudo difference-in-difference-in-differences (DDD) effect for high-tech jobs relative to non-tech jobs, after differencing out common unobservables across high-initial-wage jobs and low-initial-wage jobs, and (iii) $\beta_1 + \beta_2$, which is the effect for high-tech jobs compared with non-tech jobs, within high-initial-wage jobs.

In Table A3.1 which examines mobility, we observe results that are consistent with those in Table III.1. Among the high-tech jobs, high-initial-wage jobs experience a higher likelihood of survival compared with low-initial-wage jobs throughout the job tenure by a magnitude ranging in 0.2 percentage points to 0.5 percentage points, and a longer expected job spell (by 1.4%) when enforceability scores increase by one standard deviation ($\beta_1 + \beta_3$). Within high-initial-wage jobs, enforceability has a similar effect for high-tech jobs relative to non-tech jobs, resulting in 3.5% longer job spells ($\beta_1 + \beta_2$). The large and significant pseudo DDD estimate (β_1) shows that the effect of CNC enforceability on mobility is greatest when workers are in both high-tech industry and high-initial-wage jobs. The patterns (plotted in Appendix 7 Figures A7.1 to A7.6) suggest that the differential effect for high-initial-wage tech workers relative to low-initial-wage tech workers (Figure A7.1) is relatively flat. Relative to high-initial-wage workers in non-tech sectors, however, high-initial-wage tech workers see a sharp increase in the effect on mobility over the initial few years (Figure A7.2), consistent with these workers gaining appropriable capital over this period. This relative increase in the effect on mobility over the first few years of job tenure is also seen in the triple-differences in Figure A7.3.

In Table A3.2, which examines wages, we observe a persistent wage suppressing effect in all of the relevant comparisons as in Table III.2. Among the high-tech jobs, the differential effect between high-initial-wage jobs and low-initial-wage jobs is estimated to be in the range of 2.9% to 5.0% throughout job tenure ($\beta_1 + \beta_3$). Among high-initial-wage jobs, enforceability is associated with a differential tech effect between 2.0% in year 4 and 2.4% in year 8 ($\beta_1 + \beta_2$). The comparison of the magnitude of the coefficients suggests that being in a high-initial-wage job is a driving factor of the wage-suppression effect. All the estimates plotted in Appendix 7 Figures A7.4, A7.5, and A7.6, show a negative effect on wages that increases over time.

In Table A3.3, columns 1 through 4 show that the estimates of the differential effect on cumulative wages increase gradually over job tenure. Estimates for wage growth, presented in columns 5 through 8, show a similar trend over the job tenure, consistent with the wage estimates reported in Table III.3. Table A3.4 and A3.5 include state-by-industry fixed effects and replicate the job-level mobility and wage analyses. These results, both for the within-high-tech initial wage difference term, and the triple difference term, are consistent with our baseline results.

Table A3.1. CNCs and Mobility: Sub-Samples by Industry and Initial Wage (LEHD)

This table reports the differential effect of CNC enforceability on mobility across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs). High-initial-wage jobs are jobs whose starting wage is above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. 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). All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Job spell survival at:	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr	Ln(job-spell)
Tech X High-initial-wage X CNC Score (β_1)	0.0048***	0.0099***	0.0113***	0.0092***	0.0094***	0.0084***	0.0074***	0.0060***	0.0210***
	(0.0010)	(0.0019)	(0.0024)	(0.0016)	(0.0017)	(0.0017)	(0.0017)	(0.0017)	(0.0038)
Tech X CNC Score (β_2)	-0.0003	0.0031**	0.0038***	0.0044***	0.0049***	0.0056***	0.0044***	0.0051***	0.0148***
	(0.0008)	(0.0011)	(0.0009)	(0.0012)	(0.0009)	(0.0009)	(0.0008)	(0.0007)	(0.0027)
High-initial-wage X CNC Score (β_3)	0.0002	-0.0047***	-0.0059**	-0.0044***	-0.0043***	-0.0041***	-0.0040***	-0.0036***	-0.0074**
	(0.0007)	(0.0015)	(0.0022)	(0.0013)	(0.0013)	(0.0010)	(0.0008)	(0.0007)	(0.0032)
# of observations	12984300	12425700	11971100	11602500	11334900	11127400	10861700	10661700	6492100
R-squared	0.2108	0.1741	0.1732	0.1768	0.1817	0.1836	0.1831	0.1885	0.2113
High vs Low Wage in Tech industry (β_1+β_3)	0.00506***	0.00519***	0.00535***	0.00479***	0.00515***	0.00432***	0.00343**	0.00245*	0.0136***
p value	3.13e-06	1.18e-06	3.73e-08	3.26e-06	0.000248	0.00177	0.0141	0.0797	1.02e-05
Tech vs Non-Tech in High-initial-wage jobs (β_1+β_2)	0.0045***	0.0129***	0.0151***	0.0136***	0.0143***	0.0140***	0.0119***	0.0111***	0.0358***
p value	3.76e-05	6.75e-10	6.14e-07	9.60e-10	5.07e-11	4.97e-10	7.64e-08	4.89e-07	1.36e-10
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

Table A3.2. CNCs and Wage across Job Tenure: Sub-Samples by Industry and Initial Wage (LEHD)

This table reports the differential effect of CNC enforceability on wage throughout job tenure, across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs). High-initial-wage jobs are jobs whose starting wage is above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. The dependent variables are the log of quarterly wages at 4th, …, 32nd quarter of the job spell. CNC Score is measured as the 2009 CNC enforceability index scores. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable: Log wage at xth quarter	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X High-initial-wage X CNC Score (β_1)	-0.0098***	-0.0085**	-0.0123***	-0.0130***	-0.0146***	-0.0145***	-0.0159***	-0.0185***
	(0.0030)	(0.0040)	(0.0040)	(0.0037)	(0.0031)	(0.0034)	(0.0028)	(0.0043)
Tech X CNC Score (β_2)	-0.0055***	-0.0064***	-0.0065***	-0.0066***	-0.0057***	-0.0051***	-0.0057***	-0.0052***
	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0008)	(0.0009)	(0.0012)	(0.0015)
High-initial-wage X CNC Score (β_3)	-0.0215***	-0.0201***	-0.0196***	-0.0213***	-0.0245***	-0.0205**	-0.0246***	-0.0308***
	(0.0039)	(0.0039)	(0.0062)	(0.0068)	(0.0084)	(0.0077)	(0.0084)	(0.0100)
# of observations	10904200	7397200	5399500	4048400	3145300	2478900	1858400	1412600
R-squared	0.6726	0.6090	0.5764	0.5571	0.5430	0.5324	0.5238	0.5115
High vs Low Wage in Tech industry ($\beta_1+\beta_3$)	-0.0313***	-0.0286***	-0.0320***	-0.0343***	-0.0390***	-0.0350***	-0.0405***	-0.0493***
p value	1.52e-05	0.000279	0.00124	0.00104	7.45e-05	0.000110	5.48e-06	5.37e-07
Tech vs Non-Tech in High-initial-wage jobs ($\beta_1+\beta_2$)	-0.0152***	-0.0149***	-0.0188***	-0.0196***	-0.0203***	-0.0196***	-0.0216***	-0.0237***
p value	1.63e-05	0.000592	2.25e-05	2.44e-05	6.00e-06	2.24e-06	7.29e-07	6.16e-05
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Table A3.3. CNCs and Cumulative Wages and Wage Growth over Job Tenure: Sub-Samples by Industry and Initial Wage (LEHD)

This table reports the differential effect of CNC enforceability on cumulative wage and on wage growth from initial wage throughout job tenure, across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs). High-initial-wage jobs are jobs whose starting wage is above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. The dependent variables are the log of cumulative wage at 4^{th}, 12^{th}, 20^{th}, 28^{th} quarter of the job spell for columns (1) ~ (4), and the difference between the log of quarterly wages at 4^{th}, 12^{th}, 20^{th}, 28^{th} quarter of the job spell and the log of initial wage for columns (5) ~ (8). CNC Score is measured as the 2009 CNC enforceability index scores. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable	Log of cumulative wage at				Log of wage at xth quarter - Log of initial wage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	4th quarter	12th quarter	20th quarter	28th quarter	4th quarter	12th quarter	20th quarter	28th quarter
Tech X High-initial-wage X CNC Score (β_1)	-0.0112***	-0.0095***	-0.0192***	-0.0182***	-0.0027	-0.0084***	-0.0087**	-0.0130**
	(0.0029)	(0.0034)	(0.0031)	(0.0040)	(0.0026)	(0.0030)	(0.0038)	(0.0052)
Tech X CNC Score (β_2)	-0.0057***	-0.0074***	-0.0076***	-0.0077***	-0.0054***	-0.0063***	-0.0055***	-0.0054***
	(0.0008)	(0.0006)	(0.0007)	(0.0012)	(0.0005)	(0.0008)	(0.0008)	(0.0012)
High-initial-wage X CNC Score (β_3)	-0.0186***	-0.0224***	-0.0240***	-0.0257***	-0.0136***	-0.0094***	-0.0125***	-0.0122**
	(0.0043)	(0.0063)	(0.0078)	(0.0074)	(0.0015)	(0.0023)	(0.0044)	(0.0047)
# of observations	10904000	5399000	3145000	1858000	10904000	5399000	3145000	1858000
R-squared	0.5902	0.6709	0.6892	0.6889	0.1455	0.2047	0.2504	0.2947
High vs Low Wage in Tech industry (β_1+β_3)	-0.0298***	-0.0319***	-0.0432***	-0.0439***	-0.0163***	-0.0178***	-0.0212***	-0.0252***
p value	2.13e-05	0.000727	9.91e-05	0.000357	7.73e-06	1.25e-05	2.21e-10	0
Tech vs Non-Tech in High-initial-wage jobs (β_1+β_2)	-0.0169***	-0.0169***	-0.0268***	-0.0259***	-0.00813***	-0.0146***	-0.0142***	-0.0184***
p value	3.52e-07	1.21e-05	3.23e-09	4.75e-07	0.00369	1.80e-05	0.00142	0.00197
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Table A3.4. CNCs and Mobility: Sub-Samples by Industry and Initial Wage: State X Industry Fixed Effects (LEHD)

This table reports the differential effect of CNC enforceability on mobility across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs) (dummy variable for the starting wage of the job being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes), with state-industry (2 digit NAICS code) fixed effects. 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). All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable Job spell survival at:	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr	(9) Ln(job-spell)
Tech X High-initial-wage X CNC Score (β_1)	0.0050***	0.0085***	0.0096***	0.0078***	0.0081***	0.0072***	0.0064***	0.0051***	0.0185***
	(0.0010)	(0.0013)	(0.0015)	(0.0015)	(0.0017)	(0.0018)	(0.0018)	(0.0017)	(0.0037)
Tech X CNC Score (β_2)	-0.0018	-0.0009	-0.0008	0.0008	0.0008	0.0026**	0.0014	0.0021**	0.0072**
	(0.0013)	(0.0018)	(0.0016)	(0.0017)	(0.0013)	(0.0012)	(0.0013)	(0.0009)	(0.0032)
High-initial-wage X CNC Score (β_3)	-0.0001	-0.0038***	-0.0045***	-0.0034***	-0.0033***	-0.0032***	-0.0033***	-0.0028***	-0.0055*
	(0.0008)	(0.0009)	(0.0013)	(0.0010)	(0.0010)	(0.0009)	(0.0007)	(0.0007)	(0.0030)
# of observations	12984300	12425700	11971100	11602500	11334900	11127400	10861700	10661700	6492100
R-squared	0.2124	0.1772	0.1769	0.1802	0.1851	0.1867	0.1865	0.1916	0.2162
High vs Low Wage in Tech industry ($\beta1+\beta3$)	0.00488***	0.00464***	0.00513***	0.00440***	0.00481***	0.00404***	0.00312**	0.00233	0.0130***
p value	6.77e-06	1.93e-06	4.94e-08	8.67e-06	0.000411	0.00368	0.0261	0.101	5.34e-06
Tech vs Non-Tech in High-initial-wage jobs ($\beta1+\beta2$)	0.00315**	0.00757***	0.00884***	0.00859***	0.00893***	0.00986***	0.00785***	0.00717***	0.0257***
p value	0.0242	1.27e-05	8.71e-05	3.43e-05	2.59e-06	2.07e-06	0.000601	0.000477	5.67e-08
Fixed Effects	[State-Industry] + [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

Table A3.5. CNCs and Wage across Job Tenure: Sub-Samples by Industry and Initial Wage: State X Industry Fixed Effects (LEHD)

This table reports the differential effect of CNC enforceability on wage throughout job tenure, across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs) (dummy variable for the starting wage of the job being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes), with state-industry (2 digit NAICS code) fixed effects. The dependent variables are the log of quarterly wages at 4th, …, 32nd quarter of the job spell. CNC Score is measured as the 2009 CNC enforceability index scores. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable Log wage at xth quarter	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X High-initial-wage X CNC Score (β_1)	-0.0089***	-0.0077*	-0.0121***	-0.0124***	-0.0140***	-0.0138***	-0.0151***	-0.0185***
	(0.0030)	(0.0040)	(0.0040)	(0.0036)	(0.0031)	(0.0036)	(0.0029)	(0.0044)
Tech X CNC Score (β_2)	-0.0041***	-0.0052***	-0.0044***	-0.0047***	-0.0034***	-0.0038***	-0.0044***	-0.0079***
	(0.0006)	(0.0006)	(0.0008)	(0.0007)	(0.0005)	(0.0006)	(0.0007)	(0.0012)
High-initial-wage X CNC Score (β_3)	-0.0225***	-0.0209***	-0.0202***	-0.0224***	-0.0255***	-0.0215***	-0.0259***	-0.0314***
	(0.0038)	(0.0038)	(0.0061)	(0.0068)	(0.0085)	(0.0078)	(0.0085)	(0.0100)
# of observations	10904200	7397200	5399500	4048400	3145300	2478900	1858400	1412600
R-squared	0.6731	0.6096	0.5772	0.5580	0.5442	0.5339	0.5256	0.5135
High vs Low Wage in Tech industry ($\beta_1+\beta_3$)	-0.0315***	-0.0287***	-0.0323***	-0.0348***	-0.0395***	-0.0353***	-0.0410***	-0.0499***
p value	1.42e-05	0.000255	0.00109	0.000833	6.23e-05	0.000107	4.41e-06	4.90e-07
Tech vs Non-Tech in High-initial-wage jobs ($\beta_1+\beta_2$)	-0.0130***	-0.0130***	-0.0165***	-0.0171***	-0.0174***	-0.0176***	-0.0194***	-0.0264***
p value	4.76e-05	0.00193	0.000137	6.84e-05	5.05e-06	2.76e-05	1.02e-06	2.26e-06
Fixed Effects	[State-Industry] + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Appendix 4

Randomization Inference for the Hawaii Natural Experiment (Chapter III)

In this section, we use permutation tests (Hess 2017) to assess the robustness of our Hawaii results. In particular, to assess the significance of the within-state, cross-industry results, we randomly assign the Tech dummy to the same fraction of sectors as in the baseline analysis, clustering by 4-digit NAICS so that all observations in the industry are assigned the same value for the dummy, and then run our main difference-in-differences analysis within the full sample of the QWI or CPS for 500 replications. Similarly, we assign the ban "treatment" to a random state across 500 replications. We then compare the point estimates for Post*Tech (for within-state) and Post*HI (for the cross-state) in the baseline estimate relative to the estimates from the 500 replications, and examine (one-sided) p values (reported in brackets). Table A4.1 reports results for the specifications using QWI variables in Columns 3, 4 and 7, 8 of Tables 6 and 8; Table A4.2 reports results using the CPS mobility variable for specifications in column 6 and 8 of Table III.7. For the Triple Difference Analysis, alternative randomization inferences could be possible (e.g., across states, across industries or across state-industry combinations). Because tech-specific year shocks seemed to us the most significant concern, we undertook a randomization test by randomizing the "ban" treatment across states (like in the Cross-state analysis), with 500 replications. Table A4.3 reports specifications using QWI variables in Columns 3, 4 and 7, 8 of Table III.9, and Table A4.4 reports results using the CPS mobility variable for specifications in column 6 and 8 of Table III.10.

Table A4.1. DID Analysis of QWI Mobility and Wage Variables -- Baseline Estimates and P-values (one-sided) from Randomization Inference (Fisher Permutation) Tests

	Within-Hawaii, Cross-Industry			Cross-State, Within-Tech		
	(1)	(2)		(3)	(4)	
	Post	SR_Post	LR_Post	Post	SR_Post	LR_Post
Mobility variables						
Overall Separation Rate	0.003	0.008	-0.007	0.011	0.014	0.007
	[0.354]	[0.154]	[0.608]	[0.026]	[0.022]	[0.116]
Beginning-of-Quarter Separation Rate	0.004	0.010	-0.009	0.011	0.014	0.005
	[0.308]	[0.088]	[0.622]	[0.016]	[0.022]	[0.222]
Earnings variables						
Log Overall Average Monthly Earnings	-0.005	0.003	-0.021	0.018	0.018	0.017
	[0.482]	[0.348]	[0.646]	[0.086]	[0.162]	[0.224]
Log Hires Average Monthly Earnings	0.026	0.044	-0.012	0.071	0.078	0.058
	[0.230]	[0.160]	[0.458]	[0.014]	[0.062]	[0.078]

Table A4.2. DID Analysis of CPS Mobility Variable -- Baseline Estimates and P-values (one-sided) from Randomization Inference (Fisher Permutation) Tests

	Within-Hawaii, Cross-Industry			Cross-State, Within-Tech		
	(1)	(2)		(3)	(4)	
	Post	SR_Post	LR_Post	Post	SR_Post	LR_Post
Indicator for leaving employer between month t and t+1	0.061	0.072	0.053	0.028	0.033	0.024
	[0.004]	[0.016]	[0.010]	[0.014]	[0.028]	[0.088]

Table A4.3. Triple Difference Analysis of QWI Mobility and Wage Variables -- Baseline Estimates and P-values (one-sided) from Randomization Inference (Fisher Permutation) Tests

	(1)	(2)	
	Post	SR_Post	LR_Post
Panel A: Mobility variables			
Overall Separation Rate	0.00979	0.01124	0.00676
	[0.012]	[0.002]	[0.178]
Beginning-of-Quarter Separation Rate	0.0096	0.0126	0.0034
	[0.014]	[0.002]	[0.251]
Panel B: Earnings variables			
Log Overall Average Monthly Earnings	0.0071	0.0100	0.0010
	[0.263]	[0.21]	[0.343]
Log Hires Average Monthly Earnings	0.0424	0.0548	0.0166
	[0.04]	[0.02]	[0.232]

Table A4.4. Triple Difference Analysis of CPS Mobility Variable -- Baseline Estimates and P-values (one-sided) from Randomization Inference (Fisher Permutation) Tests

	(1)	(2)	
	Post	SR_Post	LR_Post
Indicator for leaving employer between month t and t+1	0.0686	0.0741	0.0634
	[0.002]	[0.002]	[0.002]

Appendix 5

Cross-State Synthetic Control Analysis for the Hawaii Natural Experiment (Chapter III)

In this section, we use the synthetic control approach proposed in Abadie, Diamond and Hainmueller (2010). Under this approach, the Tech sector of Hawaii is compared to a "synthetic" control composed of a weighted average combination of other states' Tech sector. To examine the statistical significance of the estimated effects, we construct the ratio of the post-treatment mean-square prediction error to the pre-treatment mean square prediction error for the Hawaii Tech sector and compare it to the same ratio for the placebo runs using other states (as in Figure 8 of Abadie Diamond and Hainmueller 2010). In Figure A5.1, we find close match between the separation rate patterns for Hawaii relative to the synthetic control in the pre-ban period, but a notable upward divergence for Hawaii in the post-ban period. This divergence is notably different from placebo runs, and in fact Hawaii has the largest ratio of pre- to post-ban mean square prediction errors, leading to a p-value of 0.02 for both mobility measures. Similarly, the synthetic control analysis for the Wage measures in the QWI in Figure A5.2 show a close match in pre-ban trends between Hawaii and the synthetic control for both the wage variables. The log overall average wage shows a short run upward deviation relative to the synthetic control which reverses in the longer run, while the log average wage of hires shows a more persistent post-ban upward deviation for Hawaii. The p-value for the log overall average wage is 0.19, while for the log average wage of hires is 0.07. The weaker effect on overall average wage is may be because wages remain sticky for workers that do not change jobs, and are impacted most for workers that are newly entering jobs. The results for the CPS mobility variable in Figure A5.3 are similar to those in Figure A5.1, with significant increase in Hawaii in the post-ban period, and a p-value of 0.03.

Figure A5.1. Synthetic Control Analysis for QWI Mobility Variables

The factor model uses 8, 5, and 1 period lags (prior to quarter of the ban) of the variable itself, and same lags for Log Hires Average Monthly Earnings as observed covariates. The bottom panel reports the distribution of the ratio of pre- to post-ban mean square prediction errors, with the red vertical line indicating the estimate for Hawaii.

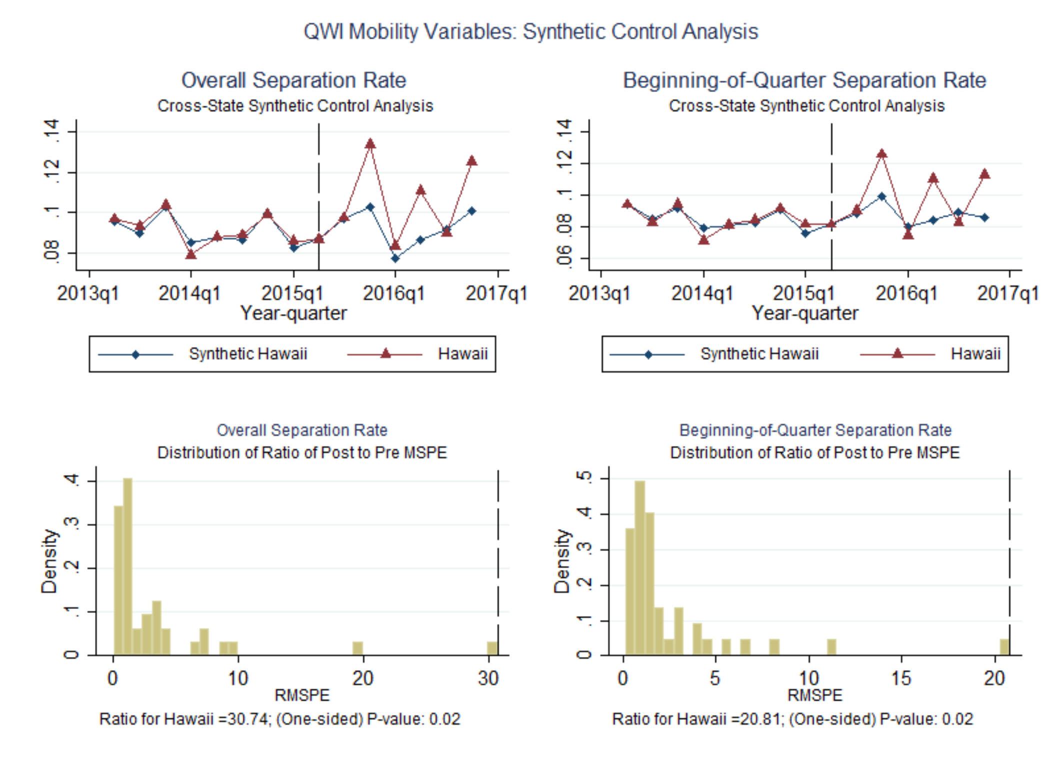


Figure A5.2. Synthetic Control Analysis for QWI Wage Variables

The factor model uses 8, 5 and 1 period lags (prior to the quarter of the ban) of the variable itself, and same lags for overall separation rate as observed covariates. The bottom panel reports the distribution of the ratio of pre- to post-ban mean square prediction errors, with the red vertical line indicating the estimate for Hawaii.

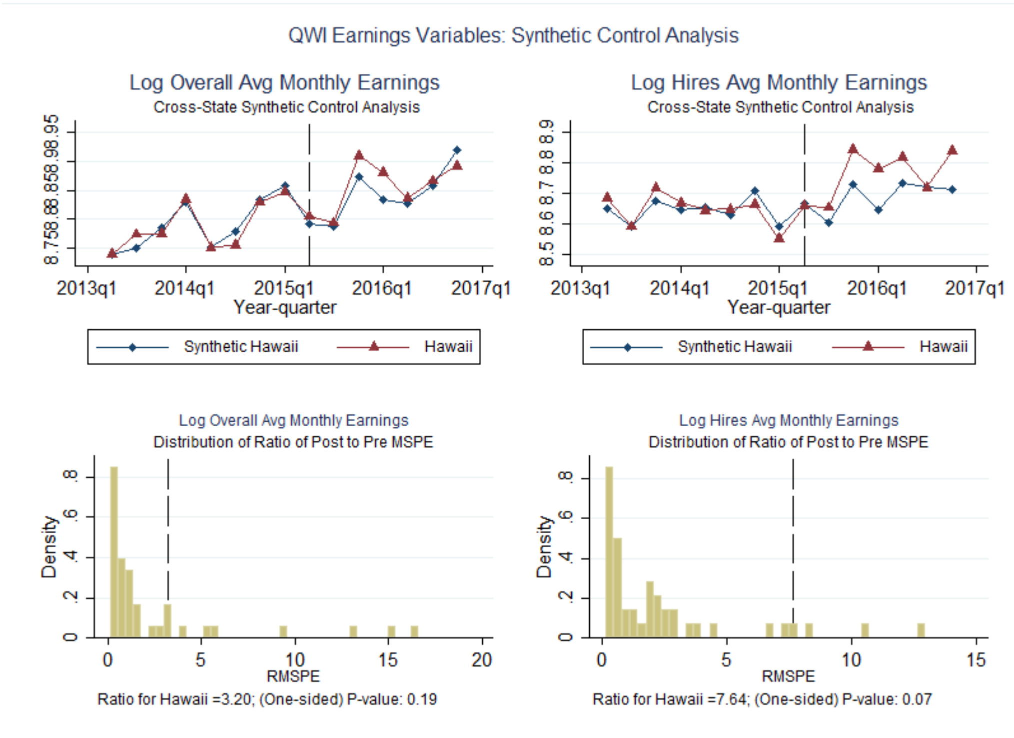
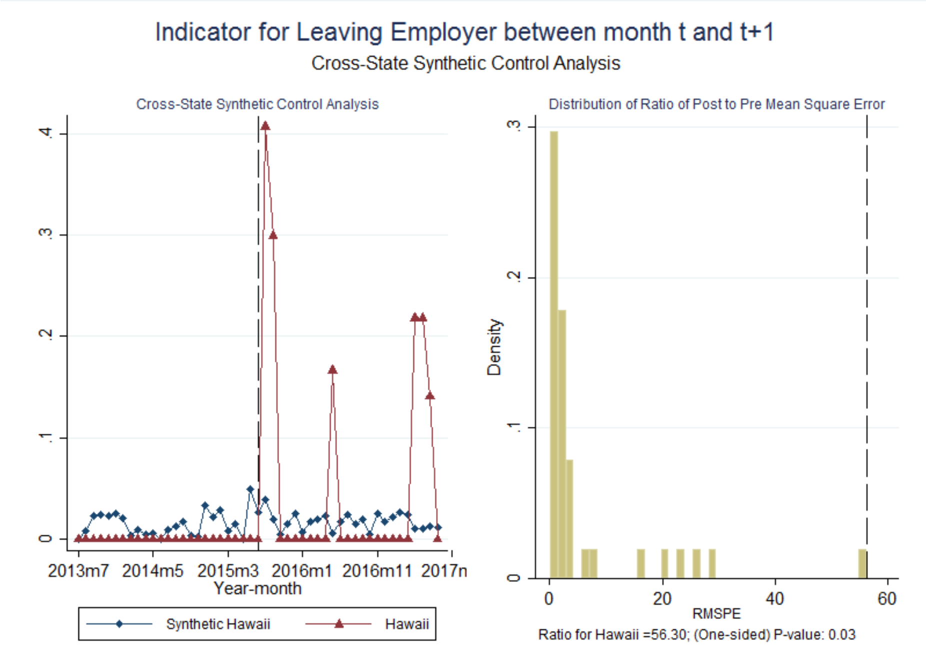


Figure A5.3. Synthetic Control Analysis for CPS Mobility Variable

The factor model uses 21, 12 and 1 period lags (prior to the month of ban) of the period mean of the dummy indicator for leaving employer between month t and t+1 for the Tech sector. The bottom panel reports the distribution of the ratio of pre- to post-ban mean square prediction errors, with the red vertical line indicating the estimate for Hawaii.



Appendix 6

Theory Appendix (Chapter III)

Our framework draws on key features of Cahuc, Postel-Vinay and Robin (2006) and Burdett and Mortensen (1998). We model wage setting and employment choice (stay or move to another firm) for an incumbent worker. We abstract from cross-worker and cross-firm heterogeneity, and extend the model to allow for endogenous determination of worker-firm "match surplus" (or relationship value). The match surplus generated by the worker is θ. At the beginning of the period the worker gets a single wage draw (W_0) from a uniform $[0,\ 1+\mu]$ distribution. The worker derives utility only from wages, so worker's decision rule is as follows:[109]

- If $W_0 > \theta$: Exit the firm and take outside offer
- If $W_0 \leq \theta$: Negotiate with the firm

The negotiated wage if the worker stays in the firm is given by (see Cahuc et al, equation 2):[110]

$$\mathrm{W\ (if\ stay)} = \text{Outside Option} \ + \ \alpha\ (\text{Match Surplus}\ ) = \mathrm{W}_0 + \alpha(\theta - \mathrm{W}_0)$$

where α reflects bargaining power of the workers, so when $\alpha = 1$, the workers get paid the full value of the relationship. (We discuss the effect of CNCs on α below.) Then probability of exit is:

$$\text{P[Exit]=P}[W_0 > \theta] = 1 - \frac{\theta}{1+\mu} \tag{1}$$

Expected wages (which correspond to our regressions estimates, assuming independent distributions and wage draws over time and across workers) are given by:[111]

$$\begin{aligned} \mathrm{E[W|Stay]} &= \mathrm{E[Outside\ Option]} + \alpha\mathrm{E[Match\ Surplus]} = \mathrm{E}[\mathrm{W}_0|\mathrm{W}_0 \leq \theta] + \alpha(\theta - \mathrm{E}[\mathrm{W}_0|\mathrm{W}_0 \leq \theta]) \\ &= \frac{\theta}{2} + \alpha\left(\theta - \frac{\theta}{2}\right) = \frac{(1+\alpha)\theta}{2} \end{aligned} \tag{2}$$

$$\mathrm{E[W|Exit]} = (\mathrm{E}[\mathrm{W}_0|\mathrm{W}_0 > \theta) = \frac{(1+\mu)+\theta}{2}$$

$$\begin{aligned} \mathrm{E[W]} &= \mathrm{P[Stay]E[W|Stay]} + \mathrm{P[Exit]E[W|Exit]} \\ &= P[W_0 \leq \theta]\ (\ \alpha\,\theta\ +\ (1-\alpha)E[W_0\ |W_0 \leq \theta]) + P[W_0 > \theta](E[W_0\ |W_0 > \theta]) \\ &= \frac{\theta}{1+\mu}\left(\frac{(1+\alpha)\theta}{2}\right) + \ \left(1 - \frac{\theta}{1+\mu}\right)\left(\frac{(1+\mu)+\theta}{2}\right) = \left[\frac{1+\mu}{2} + \frac{\alpha\theta^2}{2(1+\mu)}\right] \end{aligned} \tag{3}$$

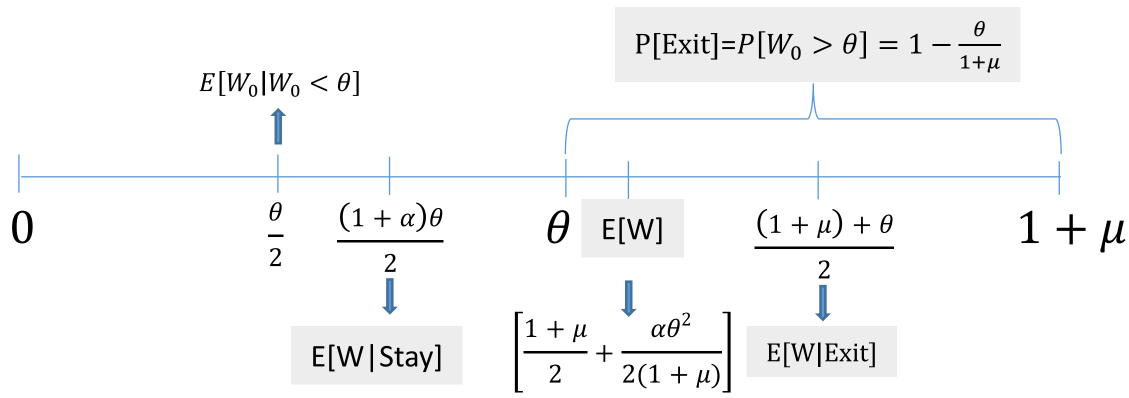
The expected wages and the probability of exiting the firm are illustrated in Figure A6.1 below.

[109] To focus on CNC enforceability, our framework abstracts from other drivers of worker turnover including e.g., health shocks, spousal career shocks, or learning about match quality. This is innocuous so long as these are uncorrelated with degree of CNC enforceability (or adequately controlled for in our empirical analysis).

[110] Cahuc et al (2006) show a dynamic version of the negotiated wage to be the outcome of a strategic bargaining game based on Rubinstein's (1982) alternating offers game.

[111] In particular, within job spell wage regressions correspond to E[Wage|stay] and worker career regressions correspond to the unconditional expected wage E[Wage].

Figure A6.1. Expected Wages, and Probability of Exit



Assumptions about effects of CNC enforceability: A0: A basic maintained assumption we make is that frictions make it costly to avoid enforceability by moving across states, and that firms cannot pre-commit to wages.[112] We make two other fairly straightforward assumptions about the effects of non-compete enforceability (η): A1: Increase in enforceability leads to reduction in worker bargaining power, i.e., $\frac{d\alpha}{d\eta} < 0$, and A2: The upper bound of outside wage distribution is decreasing in enforceability, i.e., $\frac{d\mu}{d\eta} < 0$. Assumption A1 is motivated by a widely discussed potential effect of CNC covenants (e.g., Arnow-Richman 2006). Assumption A2 tractably captures two plausible effects of CNC enforceability. First, the firms that can derive the highest value from the worker are likely to be close competitors who can exploit all of the worker's knowledge; so increase in CNC enforceability may induce some of the highest value outside bidders to drop out of bidding for the worker. Second, while we assume for tractability that the worker obtains one outside offer, in a more general case the worker may get multiple (say N) bids so that the relevant outside option is the maximum of N bids. Increase in CNC enforceability would likely decrease the number of firms willing to bid for the worker, which would decrease the expected maximum of the bids.[113]

We consider three alternative cases for the determination of the match surplus (or relationship value) θ.

Case 1: Exogenous θ

In this case, by assumption the relationship-specific value does not vary with degree of CNC enforceability. However, by assumptionsA1 and A2 above, the worker bargaining power and outside wage offer range varies leading to the following results:

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[112] Black and Loewenstein (1991) show that in a model with moving costs, if firms can commit to wages for the entire length of the employee's tenure, then there is no deviation from frictionless competitive market outcomes, as the firm and workers can negotiate upfront and prevent ex-post hold-up problems (Boal and Ransom, 1997). Our next two assumptions implicitly capture the outcome in Black and Loewenstein that when firms cannot commit, firms will enjoy monopsony power (which would increase with CNC enforceability) over incumbent workers whenever wages come up for renegotiation. If workers anticipate this, then the ex-post hold-up could be offset with front-loaded wages, so that the wage-tenure profile shows a downward slope. While the lower slope in high enforceability regions is consistent with our empirical results, we find no evidence that the initial wage levels are positively correlated with higher enforceability (see Table A7.7).

[113] This can be seen analytically in the case where the underlying wage distribution is a Gumbel with location and scale parameters ϕ and σ; then expected maximum of N draws = $\phi + \sigma \log(N)$. In a continuous time model as in Cahuc et al (2006), the notion would be that CNC enforceability dampens inter-firm competition by reducing the arrival rate of outside offers.

Result 1: Probability of exit goes down with increase in CNC enforceability.

Result 2a: $E[W|Stay]$ (i.e., average wage conditional on staying in the initial job spell) goes down with increase in CNC enforceability.

Result 2b: $E[W]$ (i.e., unconditional average wage) goes down with increase in CNC enforceability.

Result 1 follows directly from assumption A2, as decrease in μ reduces the probability that the outside offer will exceed the relationship-specific value (see Equation 1). Similarly, reduction in worker bargaining power (assumption A1) leads immediately to Result 2a (see Equation 2). Result 2b, follows from the fact that in Equation (3), both E[W|Exit] and E[W|Stay] go down, and weight on the larger (E[W|Exit]) also goes down (as P[Exit] goes up).

Case 2: Endogenous θ, firm and individual investments matter for relationship value

Suppose θ is endogenous and determined by firm investments(k) and individual investments (m), such that $\theta^2 = \mathrm{a}k + \mathrm{b}m - \frac{\mathrm{c}k^2}{2} - \frac{\mathrm{d}m^2}{2}$

Firm and individual investments are made ex-ante, based on expectations. The firm's optimization problem is: $\max_k E[\Pi] = \max_k \{\mathrm{P[stay]}(\theta - \mathrm{E[W|stay]}) - \mathrm{k}\} = \max_k \left\{\left(\frac{\theta}{1+\mu}\right)\left(\theta - \left(\frac{1+\alpha}{2}\right)\theta\right) - k\right\}$

$$= \max_k \left(\left(\frac{1-\alpha}{2(1+\mu)}\right)\theta^2 - k\right)$$

The individual's optimization problem is: $\max_m E[\mathrm{Surplus}] = \max_m \{\mathrm{E[W]}) - \mathrm{m}\}$

$$= \max_m \left\{\left[\frac{1+\mu}{2} + \frac{\alpha\theta^2}{2(1+\mu)}\right] - m\right\}$$

This yields optimal investment choices:

$$k^* = \frac{1}{c}\left(\mathrm{a} - \frac{2(1+\mu)}{(1-\alpha)}\right); \qquad m^* = \frac{1}{d}\left(\mathrm{b} - \frac{2(1+\mu)}{\alpha}\right) \tag{3}$$

Lemma 1a: Optimal investment (k^*) is unambiguously increasing in degree of CNC enforceability (as μ and α both decrease with CNC enforceability).

Lemma 1b: Optimal investment (m^*) is decreasing in degree of CNC enforceability so long as $\frac{d\alpha}{d\eta} < \frac{\alpha}{1+\mu}\frac{d\mu}{d\eta}$ (or $\left|\frac{d\alpha}{d\eta}\right| > \frac{\alpha}{1+\mu}\left|\frac{d\mu}{d\eta}\right|$).

For firms, the negative effects on both the bargaining and outside options increase investment incentives in high enforceability regimes. For individuals, the negative bargaining effect lowers investment incentive in high enforceability regimes, but the decrease in outside option increases the incentive to invest to increase relationship-specific value, so the net effect of an increase in CNC enforceability on individual investment is negative only if the magnitude of enforceability's effect on bargaining power is strong enough. Hereafter, to focus on the interesting case of varying implications for individual and firm investment we will assume A3: $\frac{d\alpha}{d\eta} < \frac{\alpha}{1+\mu}\frac{d\mu}{d\eta}$ i.e., $\left|\frac{d\alpha}{d\eta}\right| > \frac{\alpha}{1+\mu}\left|\frac{d\mu}{d\eta}\right|$.

Solving out for optimal relationship capital yields:

$$\theta^* = \left[\frac{a^2}{2c} - \frac{2\,(1+\mu)^2}{c(1-\alpha)^2} + \frac{b^2}{2d} - \frac{2\,(1+\mu)^2}{d\,\alpha^2}\right]^{0.5} \tag{4}$$

We now consider two polar cases, to understand differences in outcomes depending on whether firm or individual investments matter for relationship-specific value.

Case 2A: Only firm investments matter (i.e., b=d=0)

In equation 4, the third and fourth terms drop out, and we get the following results:

Result 3: Probability of exit is unambiguously decreasing in CNC enforceability.

This follows from the facts that optimal investment (Lemma 1a), and hence relationship capital level θ increases with enforceability (in equation 4, μ and α decrease with increase in enforceability), and the upper bound μ drops (by assumption A1).

Result 4a: Effect of increased enforceability on $E[W|Stay]$ (i.e., average wage conditional on staying in the initial job spell) is ambiguous.

This is because in equation 2, while θ increases with CNC enforceability, bargaining power α declines, so the net impact on the wages is unclear. Intuitively, the relationship-specific value is enhanced but workers' bargaining power may be lowered so much that they may not get any net benefit.

Result 4b: Effect of increased enforceability on $E[W]$ (i.e., unconditional average wage) is ambiguous.

This is because in equation 3, effect on both E[W|Exit] and E[W|stay] is unclear, though weight on larger quantity (E[W|Exit]) (i.e., probability of exit) does go down (from Result 3 above).

<u>Case 2B: Only individual investments matter (i.e., a=c=0)</u>

In equation 4 the first and second terms drop out, and we get the following results:

Result 5: Effect of CNC enforceability on probability of exit is ambiguous; if $\frac{d\theta^*}{d\eta} > \frac{\theta^*}{1+\mu}\frac{d\mu}{d\eta}$ $(i.e., if\ \left|\frac{d\theta^*}{d\eta}\right| < \frac{\theta^*}{1+\mu}\left|\frac{d\mu}{d\eta}\right|$) then probability of exit declines with enforceability.

This follows from the facts that while optimal investment (Lemma 1a), and hence relationship capital level θ decreases with enforceability (this is guaranteed by assumption A3, which makes $\frac{d\theta^*}{d\eta} < 0$), the upper bound μ drops (by assumption A1). Thus the net effect depends on which shift is larger; only if the magnitude of the decline in optimal relationship value is small enough relative to magnitude of the decline in the upper bound will the probability of exit decline with enforceability.

Result 6a: $E[W|Stay]$ (i.e., average wage conditional on staying in the initial job spell) decreases with increase in enforceability.

This is because in equation 2, both θ and bargaining power α declines with enforceability. Intuitively, relationship value and bargaining power being lower means workers are worse off.

Result 6b: Effect of increased enforceability on $E[W]$ (i.e., unconditional average wage) is ambiguous in general, but if probability of exit is declining with enforceability, then $E[W]$ also declines with enforceability.

This is because in equation 3, both E[W|Exit] and E[W|Stay] decrease with enforceability, but weight on larger quantity (E[W|Exit]) may <u>increase</u> (if P[Exit] goes up). If P[Exit] goes down (i.e., if shift in upper bound μ is modest relative to the shift in θ^*), then then the ambiguity is resolved and E[W] declines with enforceability.

<u>Endogeneity of enforceability choice by the firm</u>

The above analysis presumes that increase in enforceability results in decline of bargaining power (A1) and decrease in upper bound of outside offers (A2). In principle however, firms could choose not to include CNC clauses even in high-enforceability regimes; this raises the question of whether it would be the case that excluding CNC clauses may be beneficial to the firm. The following lemmas address this.

Lemma 2a: In case 2A (where firm investments matter for relationship-specific value), it is in the firm's interest to fully exploit enforceability, i.e., firm surplus is greater with enforcing (and reducing bargaining power (A1) and outside offers (A2)) than without.

Lemma 2b: In case 2B (where individual investments matter for relationship-specific value), sufficient conditions for the firm to fully exploit enforceability are that (i) probability of exit declines in enforceability, and (ii) $\frac{d\theta^*}{d\eta} > \frac{\theta^*}{1-\alpha}\frac{d\alpha}{d\eta}$ $(i.e., \left|\frac{d\theta^*}{d\eta}\right| < \frac{\theta^*}{1-\alpha}\left|\frac{d\alpha}{d\eta}\right|)$

Lemma 2a follows directly from taking a simple derivative of firm's optimal profit levels with respect to η and verifying that higher enforceability (η) in case 2A leads to greater profits. Lemma 2b follows form the fact that if probability of exit is lower, and if decline in bargaining power of the worker is steep enough, then the firm's share of the smaller pie (due to reduced worker investment) is larger with enforceability than without.

Note that in an incomplete information environment, A1 and A2 do not depend on *formal* inclusion of CNC clauses in employment contracts. In particular, if there are some firm types for whom Lemma 2a and/or 2b holds, and if outside bidders are unsure of the target worker's employer firm type, A2 would bind as bids would be more discouraged as enforceability increases. Similarly, if employees have incomplete information on whether CNC clauses are included in the contract (they may often be unaware of clauses in the contract e.g. Arnow-Richman 2006) or if they fear these could be introduced, that may be sufficient to reduce bargaining power, and make A1 bind as well.

Appendix 7

Additional Figures and Tables (Chapter III)

Figure A7.1. CNCs and Mobility of High-Tech Jobs: High-initial-wage Jobs vs Low-initial-wage Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential effect of CNC enforceability on mobility, of high-initial-wage jobs relative to low-initial-wage jobs within high-tech jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Mobility is measured as the dummy variable for the spell surviving at 4th, …, 32nd quarter of the job spell.

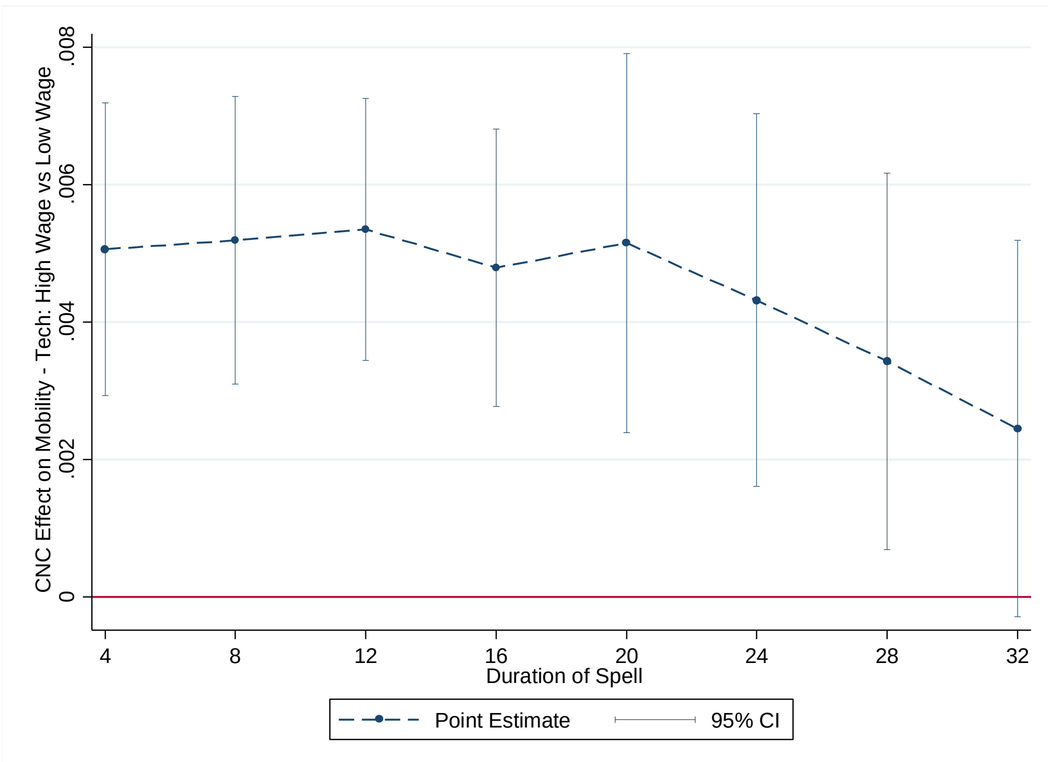

Figure A7.2. CNCs and Mobility of High-initial-wage Jobs: High-Tech Jobs vs Non-Tech Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential effect of CNC enforceability on mobility, of high-tech jobs relative to non-tech jobs within high-initial-wage jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Mobility is measured as the dummy variable for the spell surviving at 4th, …, 32nd quarter of the job spell.

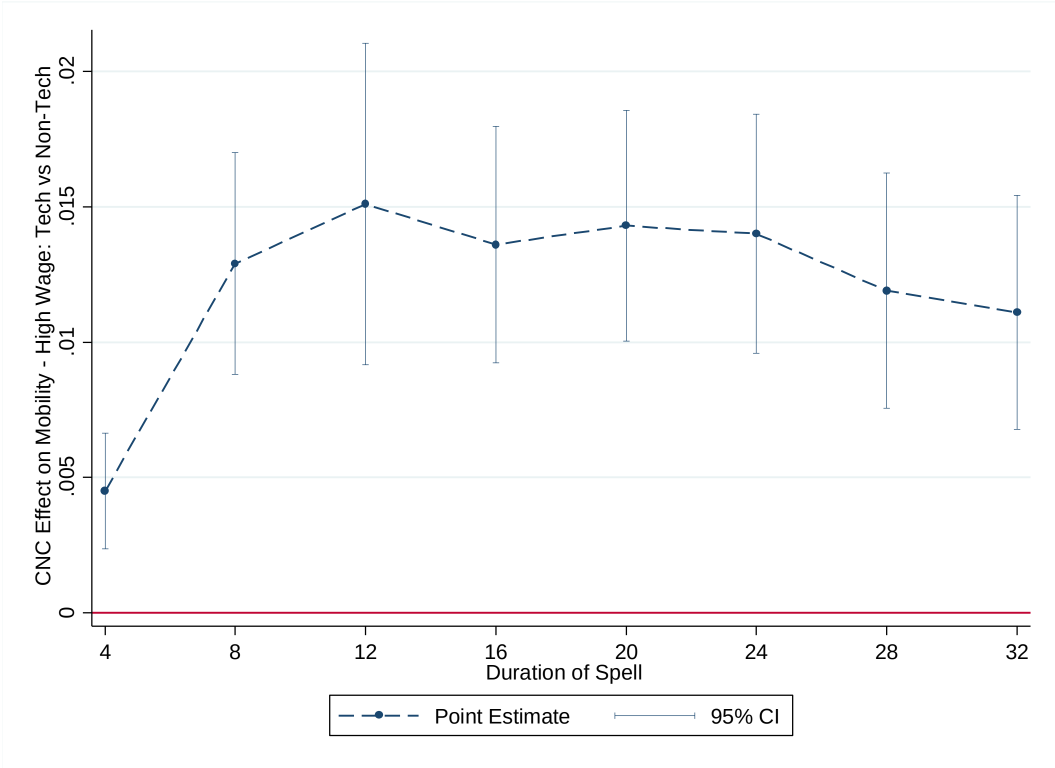

Figure A7.3. Pseudo Difference-in-Difference-in-Differences: CNCs and Mobility of High-Tech Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the pseudo difference-in-difference-in-differences effect of CNC enforceability on mobility, of high-tech jobs relative to non-tech jobs, after differencing out the common unobservables across high-initial-wage jobs and low-initial-wage jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Mobility is measured as the dummy variable for the spell surviving at 4th, …, 32nd quarter of the job spell.

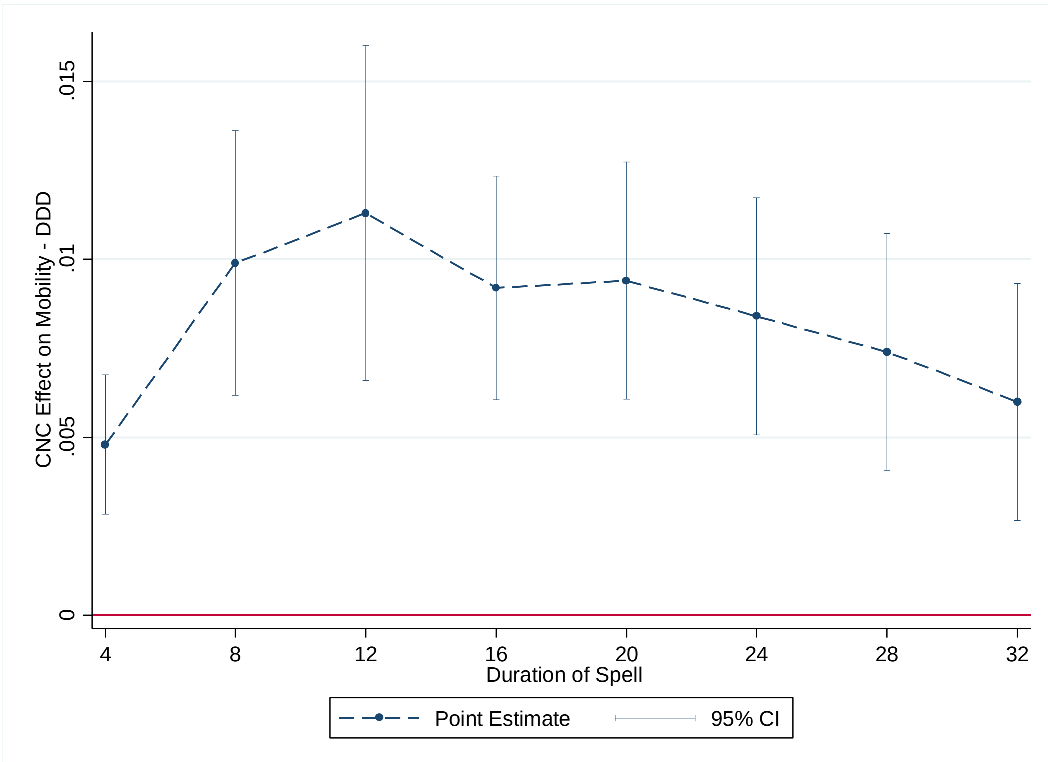

Figure A7.4. CNCs and Wage of High-Tech Jobs: High-initial-wage Jobs vs Low-initial-wage Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential effect of CNC enforceability on wage, of high-initial-wage jobs relative to low-initial-wage jobs within high-tech jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Wage is the log of quarterly wage at 4th, …, 32nd quarter of the job spell.

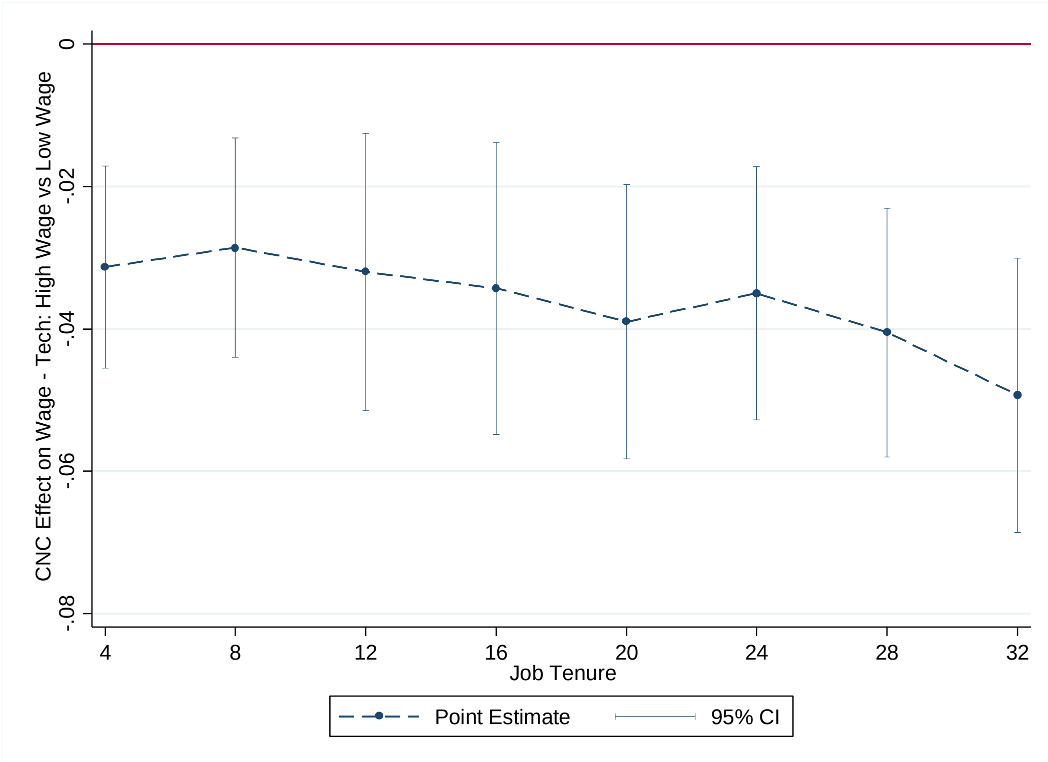

Figure A7.5. CNCs and Wage of High-initial-wage Jobs: High-Tech Jobs vs Non-Tech Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the differential effect of CNC enforceability on wage, of high-tech jobs relative to non-tech jobs within high-initial-wage jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Wage is the log of quarterly wage at 4th , …, 32nd quarter of the job spell.

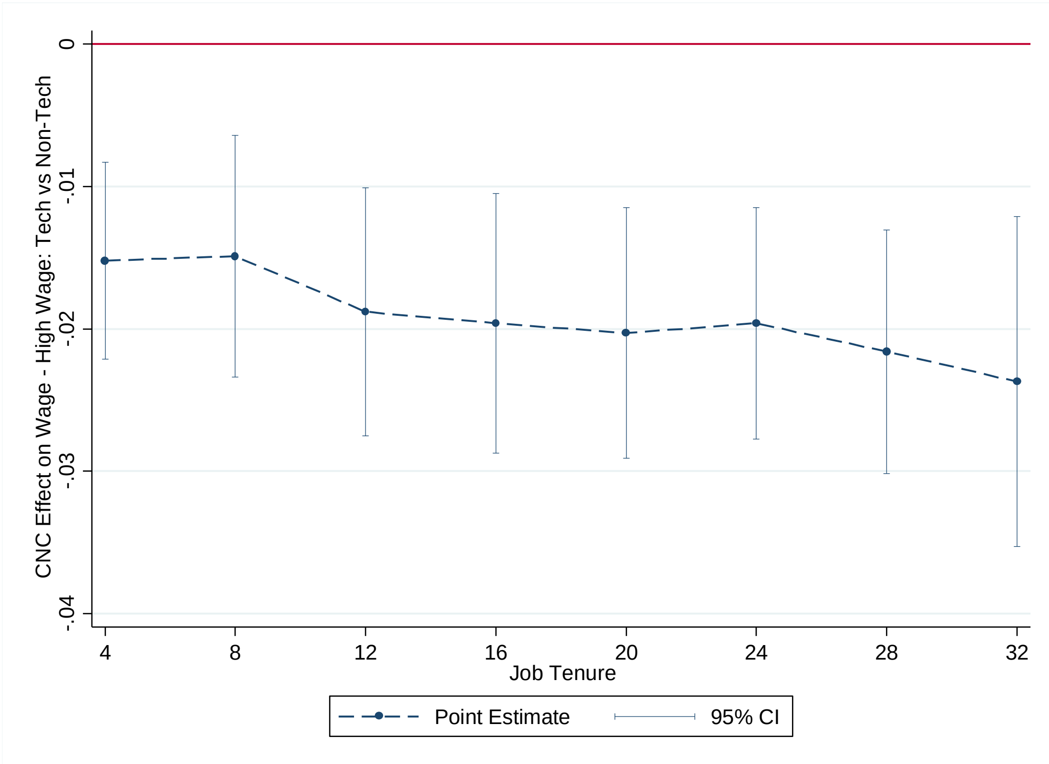

Figure A7.6. Pseudo Difference-in-Difference-in-Differences: CNCs and Wage of High-Tech Jobs (LEHD)

This figure plots the coefficient estimates and the 95% confidence intervals of the pseudo difference-in-difference-in-differences effect of CNC enforceability on wage, of high-tech jobs relative to non-tech jobs, after differencing out the common unobservables across high-initial-wage jobs and low-initial-wage jobs. High-initial-wage jobs are defined as jobs with starting wage being above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. Wage is the log of quarterly wage at 4th, …, 32nd quarter of the job spell.

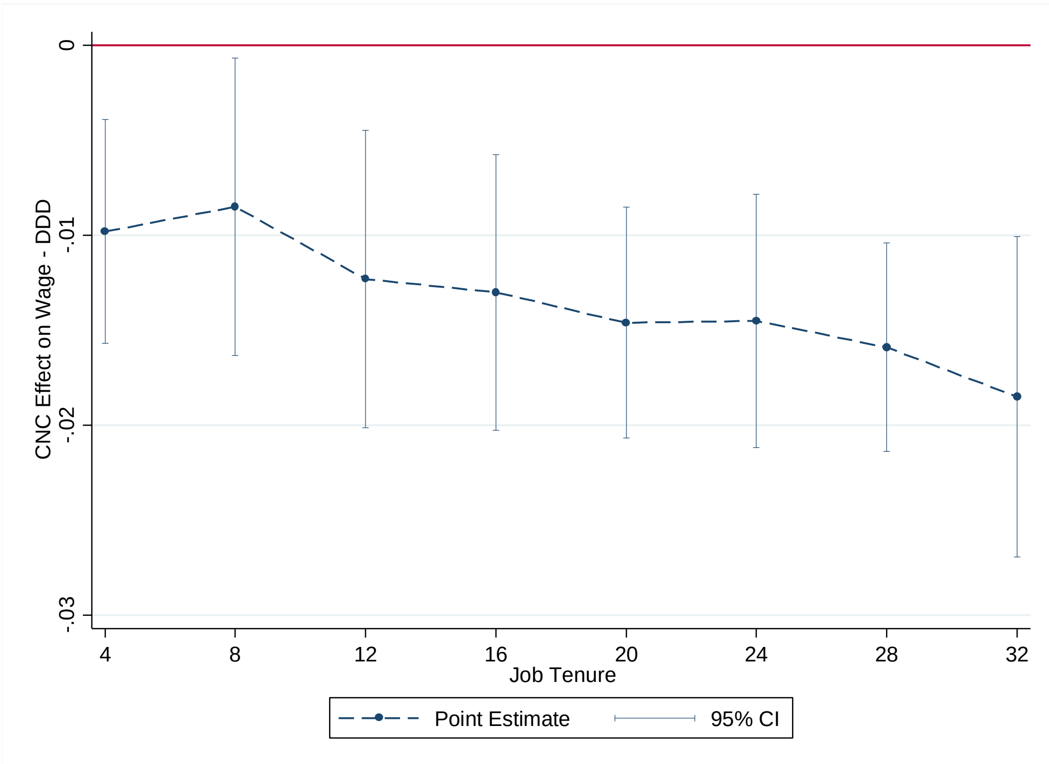

Table A7.1. Summary Statistics of the Dependent Variables (LEHD)

This table presents the summary statistics of the dependent variables reported. I{4}-I{32} denote indicator variables for the job spell surviving in the 4th-32nd quarter since the job spell started. Log(job-spell) denotes the number of quarters the job lasted in logs. Log(wage4)-Log(wage32) denote quarterly wages at the 4th-32nd quarter since the job spell started. Log(cwage4)-Log(cwage32) denote cumulative wage at the 4th-32nd quarter since the job spell started, in logs. dLog(wage4)-dLog(wage32) denote the logged differences in quarterly wage at the 4th-32nd quarter since the job spell started and the initial wage of the job. Log(cjobs4)-Log(cjob32) denote cumulative number of jobs taken (in logs) in the 4th-32nd quarter of the worker's employment history. Log(cwageE4)-Log(cwageE32) denote the worker's cumulative earnings (in logs) in the 4th-32nd quarter of the worker's employment history. Log(State4)-Log(State32) denote the cumulative number of switches in states, Log(Ind4)-Log(Ind32) denote the cumulative number of switches in industries, and Log(StNoInd4)-Log(StNoInd32) denote the cumulative number of switches in states but not in industries, in the 4th-32nd quarter of the worker's employment history.

Variable	Mean	St.Dev	Variable	Mean	St.Dev	Variable	Mean	St.Dev
I{4}	0.845	0.362	dLog(wage4)	0.049	0.465	Log(State8)	0.027	0.136
I{8}	0.583	0.493	dLog(wage8)	0.076	0.520	Log(State12)	0.033	0.154
I{12}	0.434	0.496	dLog(wage12)	0.101	0.546	Log(State16)	0.040	0.171
I{16}	0.331	0.471	dLog(wage16)	0.128	0.566	Log(State20)	0.048	0.187
I{20}	0.261	0.439	dLog(wage20)	0.151	0.582	Log(State24)	0.055	0.202
I{24}	0.208	0.406	dLog(wage24)	0.169	0.595	Log(State28)	0.062	0.216
I{28}	0.160	0.367	dLog(wage28)	0.191	0.614	Log(State32)	0.070	0.230
I{32}	0.124	0.329	dLog(wage32)	0.211	0.632	Log(Ind4)	0.051	0.186
Log(job-spell)	2.363	0.977	Log(cjobs4)	0.383	0.385	Log(Ind8)	0.106	0.270
Log(wage4)	9.578	0.777	Log(cjobs8)	0.498	0.435	Log(Ind12)	0.152	0.327
Log(wage8)	9.636	0.750	Log(cjobs12)	0.600	0.468	Log(Ind16)	0.195	0.371
Log(wage12)	9.675	0.739	Log(cjobs16)	0.686	0.492	Log(Ind20)	0.234	0.408
Log(wage16)	9.708	0.735	Log(cjobs20)	0.761	0.512	Log(Ind24)	0.270	0.438
Log(wage20)	9.740	0.731	Log(cjobs24)	0.825	0.528	Log(Ind28)	0.304	0.464
Log(wage24)	9.763	0.727	Log(cjobs28)	0.886	0.543	Log(Ind32)	0.340	0.489
Log(wage28)	9.785	0.731	Log(cjobs32)	0.939	0.557	Log(StNoInd4)	0.002	0.034
Log(wage32)	9.804	0.733	Log(cwageE4)	10.887	0.682	Log(StNoInd8)	0.005	0.058
Log(cwage4)	11.003	0.885	Log(cwageE8)	11.631	0.654	Log(StNoInd12)	0.008	0.075
Log(cwage8)	11.765	0.767	Log(cwageE12)	12.054	0.646	Log(StNoInd16)	0.011	0.089
Log(cwage12)	12.204	0.714	Log(cwageE16)	12.353	0.642	Log(StNoInd20)	0.014	0.100
Log(cwage16)	12.514	0.683	Log(cwageE20)	12.586	0.642	Log(StNoInd24)	0.016	0.110
Log(cwage20)	12.762	0.663	Log(cwageE24)	12.778	0.643	Log(StNoInd28)	0.019	0.119
Log(cwage24)	12.966	0.649	Log(cwageE28)	12.942	0.645	Log(StNoInd32)	0.021	0.127
Log(cwage28)	13.137	0.641	Log(cwageE32)	13.083	0.646			
Log(cwage32)	13.290	0.630	Log(State4)	0.017	0.108			

Table A7.2. CNCs and High-Tech Workers' Mobility and Wage: Controlling for Local Labor Market Thickness (LEHD)

This table reports the differential effect of CNC enforceability on mobility and wage across job tenure, by industry (high-tech jobs vs. non-tech jobs), after controlling for total employment in state-three-digit NAICS code-year (in logs). In Panel A, the dependent variables are dummy variables for the job spell surviving at 4^{th}, …, 32^{nd} quarter of the job spell for columns (1)-(8), and the log of length of job spells in number of quarters for column (9). In Panel B, the dependent variables are the log of quarterly wages at 4^{th}, …, 32^{nd} quarter of the job spell. 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) of Panel A, and all jobs that started its spell in year 2000 or earlier for column (9) of Panel A, and all continuing jobs in the quarter for Panel B. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Mobility

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Job spell survival at	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr	Ln(job-spell)
Tech X CNC Score	-0.0005	0.0029**	0.0037***	0.0044***	0.0049***	0.0056***	0.0045***	0.0051***	0.0146***
	(0.0008)	(0.0011)	(0.0009)	(0.0012)	(0.0010)	(0.0009)	(0.0008)	(0.0007)	(0.0028)
# of observations	12984300	12425700	11971100	11602500	11334900	11127400	10861700	10661700	6492100
R-squared	0.2108	0.1742	0.1732	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

Panel B. Wage

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log of wage at xth quarter	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X CNC Score	-0.0057***	-0.0065***	-0.0067***	-0.0068***	-0.0059***	-0.0052***	-0.0058***	-0.0056***
	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0010)	(0.0013)	(0.0017)
# of observations	10904200	7397200	5399500	4048400	3145300	2478900	1858400	1412600
R-squared	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 Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Table A7.3. CNCs and the Probability of High-Tech Workers' Switching States or Industries at Job Transition (LEHD)

This table reports the differential effect of CNC enforceability on the probability of state switches, industry switches, state switches but not industry switches, and industry switches but not state switches at job transition by industry (high-tech jobs vs. non-tech jobs). The dependent variables are dummy variables for switching states at job transitions in Panel A, dummy variables for three-digit NAICS code switches at job transitions in Panel B, dummy variables for changes in states, but no changes in three-digit NAICS codes at job transitions in Panel C, and dummy variables for changes in three-digit NAICS codes but no changes in states in Panel D, for job transitions occurring at any point in time in job tenure for column (1), and for job transitions occurring at 4^{th}, …, 32^{nd} quarter of job tenure in columns (2) ~ (9). The high-tech job dummy is that of the pre-transition job. CNC Score is measured as the 2009 CNC enforceability index scores of the state in which the pre-transition job is geographically located in. The job-level fixed effects controls for the job characteristics of the pre-transition job. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Switch States	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Dummy for switching state at	During job tenure	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X CNC Score	0.0106*	0.0087	0.0103*	0.0126**	0.0088	0.012	0.0122	0.0139*	0.005
	(0.0062)	(0.0076)	(0.0059)	(0.0059)	(0.0067)	(0.0072)	(0.0072)	(0.0069)	(0.0099)
R-squared	0.1194	0.1801	0.2047	0.2615	0.3083	0.3605	0.4086	0.4609	0.5054
Panel B. Switch Industry	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Dummy for switching industry at	During job tenure	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X CNC Score	0.0027	0.0018	0.0006	0.0046	0.0006	0.0062	0.0036	0.0089**	-0.0043
	(0.0029)	(0.0028)	(0.0028)	(0.0028)	(0.0037)	(0.0038)	(0.0046)	(0.0033)	(0.0061)
R-squared	0.1126	0.203	0.1901	0.242	0.2808	0.3423	0.3833	0.4379	0.4729
# of observations	12320000	4349000	2983000	1686000	1029000	679000	491000	345000	238000
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]								
Sample	All jobs in transition	All jobs in transitions in the quarter							

Panel C. Switch State but not Industry	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Dummy for switching state but not industry at	During job tenure	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X CNC Score	0.0016***	0.0014**	0.0021***	0.0021***	0.0014***	0.0014***	0.0013***	0.0007**	0.0008
	(0.0004)	(0.0006)	(0.0005)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0005)
R-squared	0.0486	0.096	0.1174	0.1603	0.2022	0.2524	0.2872	0.3324	0.3769
Panel D. Switch Industry but not State	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Dummy for switching industry but not state at	During job tenure	4th qr	8th qr	12th qr	16th qr	20th qr	24th qr	28th qr	32th qr
Tech X CNC Score	-0.0063*	-0.0055	-0.0076**	-0.0059	-0.0068*	-0.0044	-0.0073**	-0.0043	-0.0085**
	(0.0034)	(0.0045)	(0.0031)	(0.0035)	(0.0034)	(0.0039)	(0.0035)	(0.0047)	(0.0040)
R-squared	0.0992	0.1692	0.1745	0.2235	0.2732	0.3328	0.3713	0.4223	0.4590
# of observations	12320000	4349000	2983000	1686000	1029000	679000	491000	345000	238000
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]								
Sample	All jobs in transition	All jobs in transitions in the quarter							

Table A7.4. CNCs and High-Tech Workers' Unemployment Spell (LEHD)

This table reports the differential effect of CNC enforceability on the length of unemployment spell by industry (high-tech jobs vs. non-tech jobs). Unemployment is defined by the missing spell between two non-continuous job spells. The dependent variable is the log of length of unemployment spells in number of quarters. The high-tech job dummy is that of the pre-unemployment job. CNC Score is measured as the 2009 CNC enforceability index scores of the pre-unemployment job. The job-level fixed effects controls for the job characteristics of the pre-unemployment job. Estimation sample consists of all spells between non-continuous job spells. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

	(1)
Dependent Variable	Ln(unemployment-spell)
Tech X CNC Score	-0.0051
	(0.0033)
# of observations	4540000
R-squared	0.1241
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]
Sample	All spells between non-continuous job spells

Table A7.5. CNCs (in Ranks) and High-Tech Workers' Mobility and Wage across Job Tenure (LEHD)

This table reports the differential effect of CNC enforceability on mobility by industry (high-tech jobs vs. non-tech jobs) in Panel A, and on wage across job tenure by industry in Panel B. The dependent variables are dummy variables for the job spell surviving at 4th, 8th, …, 32nd quarter of the job spell for column (1) ~ (8) of Panel A, and the log of length of job spells in number of quarters for column (9) of Panel A, the log of quarterly wages at 4th, 8th, …, 32nd quarter of the job spell for Panel B. CNC Rank is measured as the ranks of the 2009 CNC enforceability index scores. Estimation samples are all jobs that are not right censored by the quarter for columns (1) ~ (8) of Panel A, all jobs that started its spell in year 2000 or earlier for column (9) of Panel A, and all continuing jobs in the quarter for Panel B. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Panel A. Mobility

Dependent Variable: Job spell survival at	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr	(9) Ln(job-spell)
Tech X CNC Rank	0.0004 (0.0012)	0.0052** (0.0021)	0.0065*** (0.0021)	0.0060** (0.0028)	0.0073*** (0.0024)	0.0085*** (0.0021)	0.0064*** (0.0023)	0.0072*** (0.0021)	0.0224*** (0.0063)
# of observations	12984300	12425700	11971100	11602500	11334900	11127400	10861700	10661700	6492100
R-squared	0.2108	0.1741	0.1731	0.1767	0.1817	0.1835	0.1831	0.1884	0.2112
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

Panel B. Wage

Dependent Variable Log of wage at xth quarter	(1) 4th qr	(2) 8th qr	(3) 12th qr	(4) 16th qr	(5) 20th qr	(6) 24th qr	(7) 28th qr	(8) 32th qr
Tech X CNC Rank	-0.0085*** (0.0015)	-0.0087*** (0.0027)	-0.0101*** (0.0024)	-0.0101*** (0.0021)	-0.0097*** (0.0020)	-0.0092*** (0.0018)	-0.0103*** (0.0026)	-0.0113*** (0.0032)
# of observations	10904200	7397200	5399500	4048400	3145300	2478900	1858400	1412600
R-squared	0.6726	0.6089	0.5764	0.5570	0.5429	0.5323	0.5237	0.5114
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Wage - Starting Age - Sex]							
Sample	All continuing jobs in the quarter							

Table A7.6. CNCs (in Ranks) and Mobility and Wage across Job Tenure: Sub-Samples by Industry and Initial Wage (LEHD)

This table reports the differential effect of CNC enforceability on mobility and wage throughout job tenure, across sub-samples by industry (high-tech jobs vs non-tech jobs) and initial wage (high-initial-wage jobs vs low-initial-wage jobs). High–initial-wage jobs are jobs whose starting wage is above the 98th percentile in the distribution of starting wages of jobs that have the same three-digit NAICS codes. The dependent variables are dummy variables for the job spell surviving at 4^{th}, 12^{th}, 20^{th}, 28^{th} quarter of the job spell for columns (1)-(4), and the log of length of job spells in number of quarters for column (5), the log of quarterly wages at 4^{th}, 12^{th}, 20^{th}, 28^{th} quarter of the job spell for columns (6) ~ (9). CNC Rank is measured as the ranks of the 2009 CNC enforceability index scores. Estimation samples are all jobs that are not right censored by the quarter for columns (1) ~ (4), all jobs that started its spell in year 2000 or earlier for column (5), and all continuing jobs in the quarter for columns (6) ~ (9). All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable	Job spell survival at					Log of wage at			
	(1) 4th qr	(2) 12th qr	(3) 20th qr	(4) 28th qr	(5) Ln(job-spell)	(6) 4th qr	(7) 12th qr	(8) 20th qr	(9) 28th qr
Tech X High-initial-wage X CNC Rank (β_1)	0.0087***	0.0210***	0.0183***	0.0139***	0.0425***	-0.0181**	-0.0227**	-0.0287***	-0.0350***
	(0.0014)	(0.0067)	(0.0043)	(0.0038)	(0.0078)	(0.0076)	(0.0086)	(0.0072)	(0.0074)
Tech X CNC Rank (β_2)	0.0002	0.0061***	0.0070***	0.0062**	0.0216***	-0.0081***	-0.0096***	-0.0091***	-0.0095***
	(0.0012)	(0.0021)	(0.0024)	(0.0023)	(0.0063)	(0.0014)	(0.0024)	(0.0019)	(0.0026)
High-initial-wage X CNC Rank (β_3)	0.0014	-0.0118*	-0.0086**	-0.0073***	-0.0150**	-0.0314***	-0.0322***	-0.0431***	-0.0426***
	(0.0016)	(0.0059)	(0.0035)	(0.0018)	(0.0057)	(0.0074)	(0.0096)	(0.0130)	(0.0140)
# of observations	12984300	11971100	11334900	10861700	6492100	10904200	5399500	3145300	1858400
R-squared	0.2108	0.1732	0.1817	0.1831	0.2112	0.6726	0.5764	0.5430	0.5238
High vs Low Wage in Tech industry (β_1+β_3)	0.0100***	0.0093***	0.0097***	0.00658**	0.0274***	-0.0495***	-0.0549***	-0.0717***	-0.0777***
p value	6.46e-09	0.000197	0.00157	0.0384	0.000151	0.000925	0.00289	3.76e-06	1.27e-07
Tech vs Non-Tech in High-initial-wage jobs (β_1+β_2)	0.0088***	0.0271***	0.0253***	0.0201***	0.0640***	-0.0262***	-0.0323***	-0.0377***	-0.0446***
p value	4.00e-06	0.000998	9.09e-06	5.00e-05	2.95e-07	0.00288	0.000592	0.000112	1.16e-05
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	All continuing jobs in the quarter			

Table A7.7. CNCs and Initial Wage (LEHD)

This table reports the differential effect of CNC enforceability on initial wage of job, corresponding to Table III.2 and Table A3.2. The dependent variables are the log of initial wage (i.e., second quarter wage) of each job. CNC Score is measured as the 2009 CNC enforceability index scores. The fixed effects dummy variables do not include the starting wage component for these results. All standard errors are clustered by state. ***, **, and * denote significance levels of 1%, 5%, and 10%, respectively.

Dependent Variable: Log of initial wage	(1) Corresponding Table: Table III.2	(2) Corresponding Table: Table A3.2
Tech X High-initial-wage X CNC Score (β_1)		-0.0775
		(0.1200)
Tech X CNC Score (δ or β_2)	-0.0259***	-0.0235***
	(0.0019)	(0.0033)
High-initial-wage X CNC Score (β_3)		-0.2399
		(0.2965)
# of observations	13205400	13205400
R-squared	0.1853	0.1919
High vs Low Wage in Tech industry (β_1+β_3)		-0.317
p value		0.0876
Tech vs Non-Tech in High-initial-wage jobs (β_1+β_2)		-0.101
p value		0.396
Fixed Effects	State + [Industry - Starting Year - Firm Size - Starting Age - Sex]	
Sample	All continuing jobs in the quarter	

Table A7.8. Industry Codes Corresponding to "Technology Business" Used in Analysis of the Hawaii Natural Experiment

US Census codes for the CPS analysis utilizes the bridge to the NAICS codes provided by the Census.

NAICS 4-digit codes for QWI analysis	Census Classification codes for CPS analysis
3341 Computer and Peripheral Equipment Manufacturing	3365: Computer and peripheral equipment manufacturing
3342 Communications Equipment Manufacturing	3370 Communications, audio, and video equipment manufacturing
3343 Audio and Video Equipment Manufacturing	3390 Electronic component and product manufacturing, n.e.c.
3344 Semiconductor and Other Electronic Component Manufacturing	6490 Software publishing
5112 Software Publishers	6695 Data processing, hosting, and related services
5182 Data Processing, Hosting, and Related Services	7380 Computer systems design and related services

Table A7.9. QWI Mobility and Wage Analysis for Hawaii – Triple Difference Results including Control for Employment

Notes: *** p<0.01, ** p<0.05, * p<0.1 Robust standard errors in parentheses are clustered at the state level. All specifications are the same as in Table III.9 except that an additional control variable -- Log Beginning-of-Quarter Employment (i.e., Log Emp) is included as a control. Data is from the QWI, 2013Q2 to 2017Q2. “Tech” is defined as QWI 4-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 Cols 1 to 4 is the Overall Separation Rate defined as All Separations (i.e., Sep) divided by Employment in the Reference Quarter (i.e., EmpTotal), and in Cols 5 to 8 is the Beginning-of-Quarter separation rate (i.e., SepBegR). In Panel B, the dependent variable in Cols 1 to 4 is the log of overall Average Monthly Earnings (Full Quarter Employment) (i.e., log EarnS), and in Cols 5 to 8 is the log of the Average Monthly Earnings of All Hires into Full Quarter Employment (i.e., log EarnHirAS). “Post” is defined as July 2015 and afterwards; SR_Post is 2015Q3 to 2016Q2, and LR_Post is 2016Q3 to 2017Q2. Cols 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, i.e., 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). 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).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: QWI Mobility Variables	Overall Separation Rate				Beginning-of-Quarter Separation Rate			
Post X HI X Tech	0.0167***		0.0176***		0.0188***		0.0202***	
	(0.00161)		(0.00121)		(0.00131)		(0.00117)	
SR_Post X HI X Tech		0.0158***		0.0170***		0.0189***		0.0203***
		(0.00154)		(0.00122)		(0.00105)		(0.00108)
LR_Post X HI X Tech		0.0184***		0.0190***		0.0188***		0.0200***
		(0.00210)		(0.00157)		(0.00228)		(0.00193)
Observations	163,965	163,965	205,608	205,608	166,450	166,450	208,632	208,632
R-squared	0.948	0.945	0.949	0.945	0.909	0.902	0.908	0.899
Panel B: QWI Wage Variables	Log Overall Average Monthly Earnings				Log Hires Average Monthly Earnings			
Post X HI X Tech	0.00964***		0.00712**		0.0441***		0.0424***	
	(0.00282)		(0.00270)		(0.00457)		(0.00361)	
SR_Post X HI X Tech		0.0121***		0.0100***		0.0558***		0.0548***
		(0.00276)		(0.00238)		(0.00479)		(0.00352)
LR_Post X HI X 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
Sample	40 States	40 States	All	All	40 States	40 States	All	All
Control for Log Beginning-of-Quarter Employment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Ind X Year-Qtr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Ind	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State X Year-Qtr	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes