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Does Paid Sick Leave Affect Job Turnover?

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Does Paid Sick Leave Affect Job Turnover?

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Abstract: This paper examines the impact of Seattle’s 2012 paid sick leave law on job turnover using unique administrative employer-employee matched data. Proponents of paid sick leave laws argue that the policy can improve workers’ economic security, yet there is relatively little work that examines the impact of local laws on job turnover. Using difference-in-differences and generalized synthetic control designs, this study finds that the Seattle’s paid sick leave policy did not have an impact on hires, separations, or turnover. These findings likely reflect the accrual-based policy design and exemptions for key employment groups, patterns that are supported by survey and qualitative evidence.

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Introduction

More than 30 cities and 14 states across the U.S. have adopted paid sick leave laws, which allow workers to take paid time off work when they, or a family member, are sick (National Partnership for Women & Families 2020). These laws are intended to improve public health and increase economic security. Workers without access to paid sick leave are more likely to work while sick, due to fear of employer retaliation or losing their jobs for taking time off work (Smith and Kim 2010; J. Romich et al. 2014). Yet, workers who come to work sick are more likely to spread diseases in the workplace, leading to declines in productivity, poor health outcomes, and ultimately more time out of the workforce (Davis et al., 2005; DeRigne et al., 2016; Smith & Kim, 2010; Drago & Miller, 2010). Evidence on state paid sick leave policy evaluations found that paid sick leave laws can work to reduce the number of days a worker is out of the work place when sick, suggesting that the policy can stabilize employment by allowing workers to take off a short period of time to care for themselves or a loved one (Asfaw, Rosa, and Pana-Cryan 2017). In reducing contagion and improving worker's health and productivity, paid sick leave policies have the potential to reduce job flows and stabilize employment in the labor market.¹

While there has been considerable research on how paid sick leave policies have shaped employment and earnings levels (Ahn and Yelowitz 2015; Pichler and Ziebarth 2018; J. Romich et al. 2014), little research to date has examined the impact of paid sick leave policies on job turnover. Job turnover is costly to firms and economically destabilizing for workers (Johnson, Kalil, and Dunifon 2012; Kim and von dem Knesebeck 2015; Morduch and Schneider 2017; Kuhn

¹ Evidence shows that paid sick leave policies reduce the number of workers who show up to work sick, and reduces the number of influenza-like illnesses (Pichler and Ziebarth 2017; Stearns and White 2018).

and Yu 2021; Hill 2013). By allowing workers to take off time when they become ill without fear of employer retaliation, paid sick leave policies have the potential to improve job quality and compensation. As a result, paid sick leave policies may induce workers to extend their employment contract and firms to retain employees longer, leading to reductions in turnover. Job turnover, therefore, is a critical outcome to examine to understand the ways in which paid sick leave policies can produce a healthier, economically stable, and productive workforce.

This study examines the effect of Seattle’s 2012 Paid Sick and Safe Time (PSST) law on job turnover. The design of Seattle’s paid sick leave law is emblematic of the recent wave of state and local paid sick leave policies passed throughout the country. These policies are designed based on accrual schedules, in which workers generally accrue one hour of paid time off for every 30–40 hours worked for an employer. These accrual schedules have the potential to incentivize employers to shift employment to short-term work. Seattle’s law intended to cover workers in temporary or otherwise short-term jobs, groups that have very low levels of paid sick leave coverage. On an accrual-based system, workers in these positions may not be able to accrue enough time to use it in a way that would alter their employment behavior. As a result, employers may shift employment composition towards these low-accrual workers. Alternatively, workers in short-term positions may try to extend their employment contract to gain access to the new benefit. Owing to compromises with industry groups, Seattle’s law further exempted certain employment groups, including employers in the food and accommodation industry and employers of new firms, from complying with the law.

Using individual-level administrative data on employment flows from the Washington State Unemployment Insurance Program, I employ difference-in-differences and generalized synthetic control models to quantify the effects of the PSST policy on employment outcomes. The

administrative data is an advantage over prior studies of local policy for its ability to put firms in space based on their precise longitude and latitude (Pichler and Ziebarth 2018) and for its ability to precisely identify policy-relevant firm sizes using quarterly hours data. Seattle's PSST policy only affected firms with more than 4 full-time equivalents and assigned accrual rates based on firms' full-time-equivalent size. Using the administrative data, I am able to identify firm full-time-equivalent size for each job and in doing so, identify jobs that are exclusively affected by the policy. I compare employment outcomes for a variety of job types, including wage-rate and firm sizes, in Seattle to those in other regions across Washington state. These comparisons allow me to contribute an analysis of the effects of paid sick leave on many functions and segments of the labor market.

I find that Seattle's PSST policy had no effect on overall job turnover and employment flows. The results are consistent with the design of the policy, which only provided an accrued maximum of 5 to 7 days of sick leave for full-time workers, and less for part-time and temporary workers. The benefit, therefore, may not be enough to affect employer or worker behavior. The results are also consistent with a policy design which excluded worker groups that would have benefitted the most from access to paid sick leave. Workers in the food and accommodation industry had some of the lowest levels of paid sick leave coverage prior to the policy, and the omission of these groups may have rendered the policy ineffective. Evidence from service worker interviews in Seattle at the time the ordinance went into place corroborates this pattern. The interviews revealed that 50 percent of interviewed workers in service industries eligible for the policy knew nothing about the law, and two-thirds reported not having access to paid sick leave in the year following policy enactment (J. Romich et al. 2014).

Among low wage jobs in small firms outside the food and accommodation industry, I find evidence that the paid sick leave policy reduced turnover by 4.7- 5.1 percent and turnover in short-term jobs by 5.1-5.6 percent, suggesting that for a small share of workers, the policy may have affected their employment behavior. In addition to the accrual-based system, the Seattle policy did not cover new workers in their first six months at a job, but I find no evidence of substitution away from long-term jobs towards short-term jobs, suggesting that employers did not respond to the policy rules by shifting their labor supply.

The remainder of this paper is structured as follows. I first discuss the institutions and background for Seattle’s paid sick leave law in section II and the relevant existing literature in section III. I describe the Washington state UI program data in section IV, and the generalized synthetic control approach in section V. I present results and mechanisms in section VII. I conclude with a discussion of the results and policy implications in Section VIII.

Background and Institutional Context

Seattle’s paid sick leave law was the second local paid sick leave ordinance in the United States. It was enacted to reduce exposure to infectious disease, “resulting in a healthier and more productive workforce . . . and improv[ing] family economic security” (Seattle Office of Labor Standards 2011). Passed in September 2011 and taking effect in September 2012, the PSST ordinance required firms with more than four full-time equivalents (FTEs) to provide one hour of paid time off for every 30–40 hours worked by any employee.² The leave could be used for illness,

² To determine compliance, a firm must calculate their full-time equivalent (FTE) size, which is found by dividing the number of hours required for full-time full-year work (2,080 hours) by the total number of compensated hours worked at that firm in a year (Seattle Office of Labor Standards 2011). For the first calendar year of a firms’ existence, a firm’s FTE size is defined by their FTE size within the first 90 days of existence. Each subsequent calendar year, a firms’ FTE size is determined by the FTE size from their previous calendar year. For example, if a

to take care of a family member, or for safety appointments in instances of domestic violence. The policy applied to all workers, including part-time and temporary workers, a departure from the structure of private employer benefits which generally only provided paid leave for full-time, full-year workers. Hours were accrued at the job level: If a worker transitioned from one Seattle firm to another, they would have to restart the accrual process, regardless of hours accrued at their old firm.

When the Seattle Paid Sick and Safe Time (PSST) ordinance was enacted in 2012, nearly 40 percent of private workers across the country did not have access to paid sick leave (BLS, 2020). In a survey of 315 Seattle employers in the service industry at the time the law was passed, employers reported providing paid sick leave to full-time workers at higher rates than part-time workers (80.8 percent relative to 47.1 percent) and employers with 50 or more employees covered their workers at greater rates than employers with fewer than 50 employees (89.3 percent relative to 75.2 percent)(J. L. Romich 2017). Table 1 shows the proportion of workers across the country with access to paid sick leave, by wage and firm group, the share of Seattle firms that offered paid sick leave, and the proportion of Seattle jobs in each of those groups in Seattle in the third quarter of 2011, when the policy passed. The table shows that the share of workers with access to paid sick leave was larger for those in large firms, for workers that earned in the 25th or higher percentile of wages, and for those not in service industries, covering over 55 percent of workers in firms with more than 50 employees, 74.3 percent of high wage workers, 91 percent of non-service industry firms, respectively(J. L. Romich 2017; Bureau of Labor Statistics 2020). By contrast, workers in smaller firms, workers who earned low wages, and workers in service industries like hospitality had much lower coverage rates: At the national level, only 29 percent or more of workers earning

firm's FTE size is 5.5 in 2011, they must provide paid sick leave for calendar year 2012. If their FTE size falls to 3.4 in 2012, they would not be required to provide paid sick leave in 2013.

below the 25th wage percentile and 50 percent of workers in small firms had access to paid sick leave. In Seattle, 27.5 percent of employers in the hospitality industry did not provide access to paid sick leave prior to the policy. These groups are a nontrivial part of Seattle's labor market. 36.6 percent of jobs covered by the law are in small firms, and 11.5 percent of jobs are in the hospitality industry. In the analysis, I examine the impact of paid sick leave on jobs in all firm sizes and wage levels, as well as groups that have the least amount of access paid sick leave—low-wage jobs and low-wage jobs in small firms—to assess whether varying treatment intensity affects treatment effects.

The law specified several key exemptions. First, although workers began accruing hours to use for paid sick leave immediately, new jobs had a probationary period of 180 days before any accrued paid sick leave time could be used. Second, the policy exempted jobs in the restaurant industry from providing paid sick leave, allowing workers to swap shifts rather than call out sick and use PSST pay. Finally, new firms with less than 250 FTEs were exempted from law until 24 months after the hire date of the first employee. Conversations with staff members from the Seattle Office of Labor Standards revealed that industry groups were aware and utilizing these exemptions. The law is enforced through anonymous worker complaints to the Office of Labor Standards.³

For my empirical strategy, it is important that paid sick leave eligibility, the number of hours that workers were allowed to use for sick leave, and accrual rates were determined by the number of FTEs in each firm. The policy split firms into four groups: firms with fewer than 4 FTEs; firms with more than 4 FTEs but fewer than 50 FTEs, firms with more than 50 FTEs but

³ Employers are required to keep track of accrued employee time for two years, but they are not required to report hours or earnings used for the PSST policy to the state. To the extent that employers have informal modes of keeping track of workers' hours (a spreadsheet, for example), some of the quarterly hours reported by employers may be inclusive of paid sick leave, while some are exclusive, depending on employer preference.

fewer than 250 FTEs and firms with more than 250 FTEs. Firms with fewer than 4 FTEs were not required to provide paid sick leave. Employees were able to take a maximum of 40 hours of leave if they worked in firms with more than 4 but fewer than 50 FTEs, 56 hours of leave if they worked in firms with more than 50 but fewer than 250 FTEs, and 72 hours of leave (nearly 9 days) if they worked in firms with >250 FTEs. Workers in firms with more than 4 FTEs but fewer than 250 FTEs accrued one hour of leave for every 40 hours worked, while workers in firms with more than 250 FTEs accrued one hour of leave for every 30 hours worked.

Two years after the Seattle law was passed, Seattle's Ban the Box legislation was enacted in November of 2013. If this law affected employment flows, the Ban the Box legislation may contaminate treatment effects. To avoid contamination, I focus my analysis on the post-policy period beginning in the fourth quarter of 2011 to the fourth quarter of 2013. Moreover, evidence shows that the Ban the Box policy had no effect on ex-offender employment and earnings, suggesting that the policy is unlikely to be an important factor during the time period of my analysis (Rose 2021).

Contributing Theory and Literature

Several studies examine the impact of paid sick leave policies on behavior in the work place.⁴ Another strand of literature examines the impact of labor market policies on employment flows.⁵ To date, there is no study that examines the impact of paid sick leave policies on

⁴ See Ahn and Yellowitz (2015), Pichler and Ziebarth (2017, 2018), Schneider (2020), and Stearns and White (2018).

⁵ See Brochu and Green (2013), Dube, Lester, and Reich (2016), Gittings and Schmutte (2015), Meer and West (2015), and Portugal and Cardozo (2006).

employment flows and job turnover. I briefly summarize the existing literature and use their conclusions to formulate hypotheses on the impact of Seattle's PSST on job turnover.

In the general class of search and matching models, policies that improve the conditions of work improve the quality of matches between employers and employees and have the potential to extend the employment contract (Burdett and Mortensen 1998). By compensating workers for missing work due to a medical illness or to care for a family member, paid sick leave has the potential to improve the conditions of work due to reductions in contagion and through additional earnings when workers are sick and unable to show up to work.

Evidence from evaluations of state and local paid sick leave policies found that paid sick leave laws reduce influenza-like-illness, the share of workers coming to work sick, and aggregate leave taking (Pichler and Ziebarth 2017; Schneider 2020; Stearns and White 2018). Health improvements and reductions in presenteeism may improve a firm's productivity, which could increase employment duration and reduce job turnover rates for workers whom employers may value more than previously.⁶ Moreover, evidence from survey and administrative data found that city and state paid sick leave mandates (of which Seattle's PSST policy was included) had small negative or no effects on aggregate employment and earnings in the affected geographies, suggesting the policy was not so costly as to be passed down to the worker (Ahn and Yelowitz 2015; Pichler and Ziebarth 2018).

Although the investigation of paid sick leave on job flows is novel, there is a large literature that examines the effect of compensation policies, specifically minimum wages, on employment

⁶ Observational studies have also shown that access to paid sick leave in a workplace is linked with lower rates of absenteeism and fewer worker separations, and lower levels of psychological distress and occupational injury, however it is difficult to derive implications from these studies due to issues of selection (Asfaw, Pana-Cryan, and Rosa 2012; Grinyer and Singleton 2000; Hill 2013; Stoddard-Dare and DeRigne 2018).

flows (Brochu and Green 2013; Dube, Lester, and Reich 2016; Gittings and Schmutte 2016; Meer and West 2016; Portugal and Cardoso 2006). The evidence from this literature broadly shows that increased minimum wages reduces job flows for teenagers and restaurant workers, despite having small or insignificant impacts on employment and earnings levels (Dube, Lester, and Reich 2016; Dynarski et al. 1997; Gittings and Schmutte 2016; Portugal and Cardoso 2006). These reductions in job turnover were found predominantly in short-term and high-turnover jobs, suggesting that the increased compensation extends the contracts of workers with the highest levels of turnover (Brochu and Green 2013; Dube, Lester, and Reich 2016; Gittings and Schmutte 2016).⁷

Evidence from minimum wage evaluations is instructive for this study because workers earning low wages and workers in the food and accommodation industry are least likely to have access to paid sick leave. Table 1 showed that while 79.5 percent of Seattle employers surveyed in 2012 provided paid sick leave prior to the policy, only 27.5 percent of employers in the hospitality industry provided leave, and only 29 percent of workers with wages at or below the 25th wage percentile nationally had access to the paid sick leave.

A final consideration is the design of the paid sick leave policy. Unlike minimum wages, which increase shortly after policy enactment, paid sick leave time is accrued. This design choice means that workers that do not work full-time, full-year may not accrue enough paid sick leave to

⁷Dube, Lester, and Reich (2016) find that accessions and separations all fall among teenagers and fast food workers, especially those with short tenure, suggesting that both mechanisms occur in response to the policy. They indirectly measure quits and layoffs and find that both play a role in reducing job turnover. Brochu and Green (2013) study short-term employment exclusively and found a reduction in job separation rates due to reduced layoffs and a decline in hiring rates in response to Canada's minimum wage policies. Portugal and Cardozo (2006) and Meer and West (2015) incorporate the age or growth of firms into their analysis of minimum wages and find reductions in job turnover due to reductions in job opportunity in expanding firms (Meer and West) and reductions hires and separations (Portugal and Cardozo) but do not distinguish among transition types. Gittings and Schmutte (2015) examine the relationship between worker reallocation, nonemployment duration, and minimum wages to document the relationship between turnover and minimum wage increases, finding that minimum wage increases have a negative effect on employment in low-turnover markets, while in high-turnover markets, like that in low-wage work, they find employment increases in response to the minimum wage.

reap the benefit of the new law. It also may incentivize employers to shift employment to short-term work to avoid providing a substantial amount of paid sick leave. Workers in their first six months of employment were also exempt from the policy, furthering the possibility that employers may shift their employment composition towards short-term work. Alternatively, workers who want access to the benefit will be incentivized to extend their employment contract, which could lead to reductions in job turnover without changes in employment composition. Finally, the omission of worker groups like those in new firms and those in the food and accommodation industry, may hamstring the policy's effectiveness.

Data

The Washington State Employment Security Department collects quarterly payroll records from employers for all workers that receive earnings in Washington State and are eligible for the state Unemployment Insurance (UI) program. These records are uniquely identified by the employer and employee, through their Employer Identification Number (EIN) and Social Security Number, respectively. For every quarter the employee-employer match exists in Washington state, the employer must report the total number of earnings and hours worked by that employee. The employer must also report the physical address of their firm, their firm's industry, and whether

there are multiple locations under one EIN. Through an intra-state agreement, I have access to these data between the first quarter of 2005 through the second quarter of 2017.⁸⁹¹⁰

Washington state is one of only four states that collects quarterly hours worked for each employee-employer match and this unique feature lends itself well to evaluating the PSST policy. To determine which firms are eligible for analysis, I use the hours worked information to calculate the annual number of full-time equivalents (FTEs) and categorize firms into those not required to comply with the law (0 to < 4 FTEs), and those that are required to comply with the law (>4 FTEs). To group firms by size, I utilize the definitions from PSST policy: small firms are firms with more than 4 but fewer than 50 FTEs, medium firms are firms with more than 50 but fewer than 250 FTEs and large firms are firms with more than 250 FTEs. I omit firms that exhibit extreme volatility in their FTE size, which I define as firm-years in which the standard deviation in a firm's FTE size is greater than their average FTE size¹¹. To account for the law's exemptions, I identify jobs which are in the restaurant industry, (NAICS code 722: Food Services and Drinking Places industry), as employers were allowed to offer substitute hours/shifts to employees who request PSST rather than call out sick and utilize their PSST pay (Office of Labor Standards, 2012). I also

⁸ Washington is one of four states in the U.S. which collects hours worked during the quarter in the UI records. Data are cleaned to omit earnings records with zero hours worked information and hours records with zero earnings information. Earnings include wage and salary earnings and tips (if reported). I trim wages that were less than \$7 and greater than \$500 per hour to avoid measurement error (\$7 was the minimum wage in Washington State in 2000). In addition, I dropped observations of hours that were fewer than 10 per quarter or greater than 1,000 per quarter to exclude potentially faulty data.

⁹ The UI data does not provide wage information from tipped workers or employment information from workers who do not receive a W-2 tax form including workers in the informal sector, who are sole proprietors, or who are independent contractors. While Seattle's local ordinances do not cover self-employed workers or workers in the informal economy, treatment effects may overstate treatment effects if firms respond to the policy by shifting jobs under the table or outsourcing workers on payroll to contract positions, or if workers shift their employment out of formal work or move out of the state.

¹⁰ The ESD experienced a record collection issue with certain classes of domestic workers (NAICS code 814000) and home and health care aides (NAICS 624120). As a result, I exclude jobs in these industries.

¹¹ Firm years are chosen to match the policy design, which used annual firm FTE size in their determination of eligibility. Restricting the sample to omit firms with greater annual standard deviations in FTE size than their average annual FTE size omits 1.8 percent of firm-years.

identify the first eight quarters a firm exists in the data as firms were exempted from complying with the law until 24 months after the hire date of the first employee. I separately estimate the impact of the PSST on jobs in exempt firm groups, shown in the results section.

A. Geocoding firms for treatment and comparison selection

To determine whether a job is in the treated or comparison region, I geocoded each firm's mailing address and assigned each firm to a Public Use Microdata Area (PUMA) in Washington State.¹²¹³ As the PUMA boundaries of 5 contiguous PUMAs (PUMA IDs: 11601-11605) match the boundaries of the city of Seattle, the use of PUMAs offers an advantage over smaller geographies such as zip codes and Census tracts, which spillover beyond the Seattle boundary (see Appendix C for a complete list of PUMA IDs).

It is impossible to geocode firms with multiple establishments that opt to file UI claims under a single Employer Identification Number with any precision because it is unclear how many jobs in the firm are associated with the address provided. Appendix Table 1 compares the share of jobs unable to be geocoded due to their multisite designation across firm size. Among firm groups with less than 250 FTEs an average of 85.7 percent of firms are locatable. The share of locatable firms increases as FTE size decreases, so treatment effects among small firms are representative and generalizable. By contrast, only 51.7 percent of jobs in firms with > 250 FTEs

¹² A PUMA is a geographic unit with a population of approximately 100,000 people (US Census Bureau 2018). PUMAs are larger than census tracts, and as a result, less likely to be affected by random shocks, but smaller than counties, allowing for precise identification of the treatment geography.

¹³ I geocode mailing addresses to the exact latitude and longitude coordinates using the Business Analytics 2016 Street Map database from ARC GIS. If I did not have the street address information, I geocode to the centroid of the firms' zip code, depending on the level of detail of the mailing address. If the mailing address is misspelled, not inputted correctly, or unknown to the employer filing the records to the state, I am unable to geocode those addresses. Of the 403,597 unique addresses in the data, eight percent of firms statewide have invalid addresses or an address listed as "statewide" or "unknown" and, therefore, could not be geocoded to a specific location.

are locatable, lowering confidence that treatment effects for jobs in large firms will be generalizable. Because these large firm locational matches are poorer, I omit them from analysis. While the exclusion of these firm sizes may reduce the generalizability of the findings from this study, employees in large firms are also more likely to have access to paid sick leave.¹⁴ Accordingly, I do not expect large firms to be affected by the PSST policy.¹⁵

B. Outcomes

Employment outcomes are defined at the job-quarter level for the main analysis. The counts of each employment outcome are then aggregated into PUMAs for analysis. To account for disproportionate growth in the number of jobs with very high wages in Seattle in the study period relative to the rest of Washington state, I omit jobs in the top 2.5% of the wage distribution (jobs with wage rates greater than \$50 per hour) from the analysis.¹⁶ Throughout the analysis, I focus on low-wage jobs, which I define as jobs which have wages at or below 25th percentile of Seattle's wage distribution at the time the policy was enacted. In 2012, the 25th percentile wage rate was \$15.09 per hour so I define low-wage jobs as jobs which pay less than \$15 per hour.

¹⁴ Larger firms may also be better equipped to handle the added labor costs from their affected locations. If these firms retain pre-policy employment levels with higher wages or paid sick benefits, workers may try to gain employment with non-locatable firms, which could lead to an artificial increase in quits in locatable Seattle jobs. Jardim et al. (2020) assessed workers transition rates from locatable firms to non-locatable firms during the period following the MWO enactment. They did not find a significant change in workers' transition rates to non-locatable firms with a Seattle address, nor did they find a change in transition rates to non-locatable firms with addresses in nearby regions, so this pathway is unlikely.

¹⁵ According to the Bureau of Labor Statistics, 88 percent of workers in firms with 500 or more employees have access to paid sick leave, relative to 66 percent in firms with less than 50 employees (Bureau of Labor Statistics 2020).

¹⁶ The growth corresponds with the rapid growth of Seattle's information and technology sector. On average, 19.6 percent of Seattle jobs have wage rates >\$50 compared to 13 percent of jobs in PUMAs in the surrounding Washington state.

Employment Outcomes: The UI program data used in this study is an identifiable version of the Quarterly Workforce Indicator (QWI) dataset constructed by the U.S. Census.¹⁷ As such, I construct the employment outcomes in my analysis to match those generated in the U.S. Census Quarterly Workforce Indicator database (Abowd and Vilhuber 2011; Tibbets 2019). (1) *Hires:* I define hires as the total number of new jobs in quarter t among continuing firms (firms that existed prior to quarter t). (2) *Separations:* I define separations as the total number of jobs that existed in quarter t among continuing firms but have no record in the UI data in quarter $t + 1$. (3) *Job turnover:* I define job turnover as the sum of total separations and hires in quarter t , divided by the sum of *beginning-of-quarter* and *end-of-quarter* jobs in quarter t :

$$turnover_t = \frac{separations_t + hires_t}{employment_{beg,t} + employment_{end,t}} \quad (1)$$

The “beginning-of-quarter” and “end-of-quarter” measures of employment exclude jobs which may only last a few hours or a day and are similar to other point-in-time measures, such as the BLS’s Quarterly Census of Employment and Wages (Tibbets 2019). I define “beginning of quarter” employment as the total number of jobs in quarter t that existed in quarter $t - 1$ and quarter t . I define “end of quarter” employment as the total number of jobs in quarter t that existed in quarter t and quarter $t + 1$.

I also consider the impact of the PSST policy on jobs of varying duration, following the QWI definitions of “full quarter” employment and “stable” transitions. I define long-term outcomes as outcomes associated with employer-employee matches that existed in quarters $t - 1, t, \text{ and } t + 1$. I define short-term outcomes as outcomes associated with employer-employee

¹⁷ The Census collects quarterly employment records from all 50 states to create the Longitudinal Employer-Household Dynamics (LEHD) microdata, and, from this data, creates data products including the QWI for public use.

matches that existed in quarters $t - 1$ and t only. Appendix B provides the definitions for each long-term and short-term employment outcome.¹⁸

C. Descriptive Statistics

Figure 1 displays a time series of each of the five outcomes in Seattle and the comparison PUMAs in Washington state between 2006Q2 and 2014Q4 in levels, and their year-over-year change. The red lines denote the five Seattle PUMAs, which become bold after the third quarter of 2011 to indicate the period when the PSST policy was passed. In general, PUMAs in Seattle are at the very high end of the distribution for each of the five outcomes, suggesting that using levels to estimate treatment effects does not allow for careful comparison. The year-over-year changes in each outcome for the Seattle PUMAs, by contrast, are within the convex hull of the surrounding Washington state PUMAs during the pre-policy period, suggesting that year-over-year changes will lead to unbiased treatment effects. Given that the year-over-year changes in employment outcomes are comparable between Seattle PUMAs and those of the rest of Washington state, I utilize the year-over-year change of each employment outcome in the analysis.

Methods

I use a traditional difference-in-differences model and a generalized synthetic control estimator to capture estimated changes in employment flows in Seattle's labor market, relative to the rest of the state (Bai 2009; Gobillon and Magnac 2016; Xu 2017). The standard two-way fixed effects difference-in-differences model, which models an outcome, Y , in region, r , and time, t ,

¹⁸ When applicable, these definitions are consistent with those used in previous studies of employment flows using the QWI (Dube, Lester, and Reich 2016; Gittings and Schmutte 2016). The advantage of the UI program records is that individual workers can be followed over time.

based on a binary treatment indicator, D , time-invariant region shocks, α_r and region-invariant time shocks, γ_r , shown in the following equation:

$$Y_{rt} = \beta D_{rt} + \alpha_r + \gamma_r + \varepsilon_{rt} \quad (2)$$

The coefficient for the binary treatment indicator, β , represents the impact of paid sick leave policy on employment flows in Seattle in the years following policy enactment.

The generalized synthetic control estimator uses a linear interactive fixed effects approach to estimate a counterfactual for each of the five treated PUMAs. Synthetic control estimators have gained popularity in causal inference studies and have been used to study state and local employment policies (Dube and Zipperer 2015; Pichler and Ziebarth 2018; Powell 2017; Hansen and McNichols 2020). Compared to the matching synthetic control method popularized by Abadie et al. (2003; 2010), the generalized synthetic control approach is advantageous when there are multiple treated regions, which applies to the five treated PUMAs in Seattle (Hansen and McNichols 2020).

By incorporating a linear interactive fixed effects approach, the generalized synthetic approach can capture events such as the weather, migration patterns, or the Great Recession, reflecting the fact that regions may respond differently to shocks that occur at the same time. Seattle is the largest metropolitan area in Washington and has a very different industry and wage profile relative to the surrounding area. In addition to a disproportionate share of high wage jobs, Seattle has a strong concentration of firms in the technology and information industries. By contrast, Washington State is largely comprised of suburban and rural areas dominated by agriculture in the eastern part of the state. Through the incorporation of unobserved region-specific

linear factors with time-varying coefficients, the generalized synthetic control model can estimate these differential responses explicitly.

The generalized synthetic control model assumes that changes in employment flows in each region can be represented as a function of F unobserved linear factors, denoted by, f_t , plus the treatment effect (Bai 2009; Jardim et al. 2020; Xu 2017). Each region is allowed to be differentially exposed to these shocks, which are denoted by their factor loadings, λ_r . For an outcome, Y , in PUMA, r , and quarter, t , the model used to estimate treatment effects is:

$$Y_{rt} = \delta_{rt}D_{rt} + x_{rt}\beta + \lambda_r f_t + \varepsilon_{rt} \quad (3)$$

where D_{rt} is the treatment indicator, δ_{rt} is the treatment effect to be estimated, x_{rt} represents the control variable of population counts in each PUMA during the study period, β is a vector of unknown coefficients, λ_r are region-specific factor loadings, and f_t are unobserved factors.

The generalized synthetic control estimator is implemented in a three-step process (Xu 2017). The first step is to obtain estimates $\hat{\beta}$, $\hat{f}_t$ and $\hat{\lambda}_r$, for $r = 1 - 52$ (comparison PUMAs) using an interactive fixed effects model for the control group data. The second step estimates factor loadings, $\hat{\lambda}_r$, for each treated unit, $r = 53 - 57$, by minimizing the mean squared error of the predicted treated outcome in the pre-treatment periods. Finally, treated counterfactuals are calculated based on $\hat{\beta}$, $\hat{f}_t$ and $\hat{\lambda}_r$:

$$\hat{Y}_{it}(0) = x'_{it}\hat{\beta} + \hat{\lambda}'_r\hat{f}_t \quad (4)$$

The treatment effect on the treated is denoted as: $\widehat{\delta}_{rt} = Y_{it}(1) - \hat{Y}_{it}(0)$. The number of treated PUMAs in this analysis is small, so my preferred model relies on a parametric bootstrap procedure that, conditional on observed covariates and unobserved factors and factor loadings, resamples residuals to obtain the uncertainty estimates using 10,000 bootstraps. I test the

sensitivity of this choice by using generalized synthetic control estimator with a nonparametric procedure in the results section.

Results

A. Hires, Separations & Job Turnover

Time series figures of the traditional two-way fixed effects difference-in-differences estimator and the generalized synthetic control estimators are presented in Figure 2. The figures present estimates for all jobs in firms with more than 4 but less than 250 full-time-equivalents (FTEs). For each outcome, I present the time series of the average employment outcome in Seattle (black line), the counterfactual produced by the generalized synthetic control estimator (dashed blue line), and the underlying raw data for each PUMA (thin blue and grey lines) for both approaches.¹⁹ The counterfactual trend using the difference-in-differences estimator does not have the same trend as Seattle’s average employment flow outcomes in the pre-policy period. By contrast, the trends in the counterfactual produced by the generalized synthetic control are nearly identical to the trends in Seattle, suggesting the generalized synthetic control estimator can produce a better counterfactual to Seattle prior to the PSST policy. I present results using both difference-in-differences and the generalized synthetic control throughout the analysis, however my preferred estimates utilize the generalized synthetic control estimator.

Treatment effects of the impact of the PSST law on employment flows for various wage levels and firm tier size are documented in Table 2.²⁰ Each panel shows coefficient estimates,

¹⁹ Appendix Table 7 shows the corresponding weights applied to each comparison PUMA that are used to form the synthetic counterfactual to the Seattle PUMAs for each outcome estimated in Figure 2.

²⁰ These firm distinctions also align with the law’s “Tier” sizes: Tier One firms are defined as firms with more than 4 but fewer than 50 FTEs and Tier Two firms are firms with more than 50 but fewer than 250 FTEs. Jobs in both tiers accrue hours of paid leave at the same rate, but jobs in Tier Two firms can use more paid sick leave hours within a year (Seattle Office of Labor Standards 2011).

standard errors and the pre-policy average outcome (in levels) from individual regression runs using the difference-in-differences and the generalized synthetic control (GSC) estimation strategies on the year-over-year changes in employment outcomes. I also include standard errors using an alternative nonparametric specification, and the pre-policy mean squared prediction error for the GSC estimator. Throughout the main analysis, I exclude jobs in groups exempt from the policy (jobs in Food Services and Drinking Places and jobs in new firms), however I test the sensitivity of excluding exempt jobs in Section C.

Table 2 Panel A presents results for jobs in all wages and all eligible firm sizes: firms with more than 4 but less than 250 full-time-equivalents (FTEs). The results show that the impact of the PSST policy on employment flows is also not distinguishably different from zero. The difference-in-differences estimation shows a decline in year-over-year change in hires of 6.1 percent (statistically significant at the <0.05 level), but no measurable policy effects using the GSC estimator.

To account for differing treatment intensities, Table 2 Panels B and C present results for the impact of PSST policy on jobs for low- wage jobs and small firms. Low-wage jobs are jobs which have wages at or below 25th percentile of Seattle's wage distribution at the time the policy was enacted (jobs that pay \$15 or less). Small firms are firms with more than 4 but fewer than 50 FTEs. Table 2 Panel B, shows that there is no policy impact of the PSST policy on low-wage jobs, echoing results in Panel A. Table 2 Panel C presents average treatment effects for low-wage jobs in small firms. The results show that job turnover in small firms declined by a precise 4.7 percent

in response to the PSST policy using the GSC estimator (statistically significant at the <0.05 level), however it is not confirmed by the traditional two-way fixed effects estimator.²¹

To compare these analyses to results found in prior studies, I also assess the impact of the PSST policy on employment and earnings levels (Appendix Table A2). The impact of the PSST policy on these outcomes is also statistically indistinguishable from zero. Panel A of Appendix Table A2 are the most comparable to the results found in Pichler and Ziebarth (2018), who also found that Seattle's paid sick leave policy did not lead to statistically significant declines in employment or earnings.

B. Job Duration & Employment Composition

I also consider the impact of the PSST policy on jobs of varying duration, presented in Table 3. Table 3, Panels A - D present results for the average treatment effects of the PSST policy on short-term and long-term jobs for jobs of all wages and jobs with low-wage jobs in small firms. Panels A and B show that the estimated effects of the PSST policy on employment flows for long-term jobs, jobs which existed for at least three consecutive quarters, are not statistically significant at conventional levels. Panel C shows that the estimated effects of the PSST policy on employment flows for short-term jobs of all wages, jobs which existed for two or fewer consecutive quarters, are not statistically significant at conventional levels. Using the difference-in-differences method, hires declined by 13.6 percent (statistically significant at the <0.05 level), however but it is not confirmed using the GSC method.

Restricting the analysis to short-term low-wage jobs in small firms, Panel D, results show that the PSST reduced job turnover by 5.1-5.6 percent (estimates are statistically significant at the

²¹Across all panels, Table 2 also shows that generalized synthetic control model using the nonparametric bootstrap procedure produces very similar estimates to that of the parametric model, however the standard errors of the parametric model are slightly larger. I chose to go with the parametric method as a conservative approach moving forward.

<0.05 level). Point estimates reveal a decline in hires by 7.6-7.8 percent and a decline in separations by 4.6-7.9 percent, although these declines are not all distinguishable from zero. To the extent that effects were concentrated in this group of jobs because low-wage jobs and jobs in small firms were less likely to have access to paid sick leave prior to the PSST ordinance (as shown in Table 1).

A key aspect of the policy design in the paid sick leave is the omission of new short-term jobs from coverage. Seattle's PSST policy did not cover new workers who were employed for less than 6 months, so job turnover may increase if employers shift the composition of their labor force to avoid the added cost¹. Table 4 presents results that explore whether employers shifted labor to short-term jobs in the wake of the PSST policy. For each firm and wage group, treatment effects are not statistically distinguishable from zero, suggesting that there was no labor-labor substitution in response to the PSST policy.

C. Analysis on Exempt Jobs

I perform a series of falsification tests in which I assign treatment to groups exempt from the PSST policy: jobs in firms with fewer than 4 full-time-equivalents (FTEs), jobs in the food and accommodation industry, and jobs in new firms. The results of these tests are presented in Appendix Tables 3, 4 and 5. Appendix Table 3 shows results from regression analysis that compare employment outcomes for jobs in firms with fewer than 4 FTEs in the Seattle PUMAs to those in the rest of Washington using the pre- and post-policy time periods corresponding to the PSST ordinance. If the point estimates from these regression analyses were statistically different from zero, it suggests that there may be a shift in employment flows to exempt firms in response to the policy. Panel A presents results for all jobs in firms with < 4FTE, Panel B restricts the analysis to

nonexempt jobs, and Panel C further restricts analysis to jobs with low wages. Across all outcomes in all panels, the point estimates are not statistically distinguishable from zero.

Results for analysis of jobs in Food and Accommodation industry and jobs in new firms are presented in Appendix Tables 4 and 5. As discussed earlier, the PSST policy allowed exemptions for jobs in these firm groups. While it is possible that employers of these job types provided paid sick leave, conversations with the Seattle Office of Labor Standards shows that their exempt status from was widely known among industry groups. Appendix Table 4 shows treatment effects for all jobs in the food and accommodation industry and for low-wage jobs in small firms. Appendix Table 5 show treatment effects for all jobs in new firms and for low-wage jobs in small firms. Across all outcomes in all panels, the point estimates are not statistically distinguishable from zero, confirming the larger pattern that exempt jobs had no behavioral employment change in response to the policy.

D. Spillover Effects

I also examine whether the effect of the PSST policy on employment flows changes when comparison counties right around Seattle are excluded. Due to the small geography and relatively open economy of cities, it's possible that local policies are more prone to spillover into the surrounding county (Jardim et al. 2017; Baum-Snow and Ferreira 2015), which can lead to attenuated treatment effects. Alternatively, PUMAs that are contiguous to the city may serve as better comparison regions, due to their similar economies. I test the sensitivity of including surrounding areas by re-estimating treatment effects excluding the PUMAs outside of Seattle within the surrounding King County from the sample. This exclusion results in five treated PUMAs and 40 comparison PUMAs.²² The results from this analysis are shown in Appendix Table 6. The

²² This excludes 12 PUMAs, covering a quarterly average of 21.3 % of jobs in Washington State.

results are consistent with the main results, suggesting that the inclusion of King County PUMAs is not attenuating treatment effects.

Discussion & Conclusion

Labor and health policies are often passed with the promise of improving economic security. However, there has been little empirical evidence on how a worker health policy like paid sick leave can increase economic security or employment stability. As a form of compensation, paid sick leave can be viewed as a policy that improves the conditions of work, which may extend the employment contract due to worker preference and enhanced productivity. Yet this transaction can only occur if workers who were not receiving paid sick leave prior to the ordinance gain access to leave in a timely manner that allows them to use the new benefit. As the coronavirus pandemic soon marks its two-year anniversary in the United States, policies that promote public health while stabilizing employment are at an all-time high. Implications of this study can inform legislation for states looking to enact effective public health measures.

To address this need, this paper investigates the effect of Seattle's 2011 Paid Sick and Safe Time (PSST) policy, which provides one hour of paid sick leave for every 30 to 40 hours worked on job turnover. Using a difference-in-differences and generalized synthetic control approach, I examine the impact of the policy on employment flows for a range of firm size and wage groups. I find that Seattle's PSST policy had no effect on overall job turnover and employment flows. The results are consistent with the accrual-based leave policy design choices common across many paid sick leave policies. In only accruing one hour of leave every 40 hours worked, a temporary worker working 20 hours a week would only accrue 13 hours of leave in their first six months. This design choice may not have allowed for workers to accrue enough paid time off to make a meaningful difference in their employment behavior.

Several other factors may also be at play. Seattle's paid sick leave policy design excluded worker groups, like those in the food and accommodation industry, that disproportionately lacked paid sick leave prior to the ordinance, and thus would have benefited the most from the policy. The lack of policy effects found in the examination of jobs in the food in accommodation industry confirms that in allowing employers to offer substitute hours or shifts to employees who request PSST, these workers were effectively not covered by the ordinance.

The lack of policy effects could also suggest that employees may not have known they had access to PSST benefits at their firm, a fact that has been documented in a qualitative evaluation of the Seattle's PSST policy (J. Romich et al. 2014). Interviews of workers newly eligible for the paid sick leave benefit revealed that ten of the 16 newly eligible workers reported not having access to leave or did not know whether they had access to the leave in the year following enactment (Romich et al., 2014). Fear of retaliation from employers may also be an issue, as employers did not voluntarily provide this benefit, but were mandated to do so. In a survey conducted prior to Seattle's PSST policy going into effect, one of the reasons Seattle workers reported not taking time off when they were sick was fear of retaliation from employers, suggesting this additional dimension of compensation is important (Romich et al., 2014).

Among low-wage jobs in small firms—jobs that are less likely to have access to paid sick leave—I find a decline in turnover of 4.7 percent using the generalized synthetic control strategy. Short-term low-wage workers in small firms experienced a decline in job turnover by 5.1 -5.3 percent (<0.05 significance in both strategies). As only 50 percent of workers in firms with less than 50 employees and 29 percent of workers earning wages below the 25th wage percentile had access to paid sick leave prior to the policy (9.3 percent of all workers), I infer that to the extent that treatment effects were concentrated in this group of jobs, it is because they

were less likely to have access to paid sick leave prior to the PSST ordinance. The 95 percent confidence intervals around the turnover estimates for both specifications among all low-wage jobs in small firms (Table 2, Panel C) suggest that job turnover declined by no more than 8.4 percent or increased by no more than 1.3 percent for this group. The confidence intervals around turnover estimates for all short-term low-wage jobs in small firms (Table 3, Panel D) suggest that job turnover declined between 10.1 and 1 percent. A back of the envelope calculation shows that for a firm employing low-wage workers with 50 or fewer employees, the average reduction in turnover of 4.7 percent due to the PSST policy had the potential to save employers just over \$2,300 a year²³.

Both the accrual design and exemption of new workers from the paid sick leave law could have led employers to shift their labor away from long-term workers to short-term workers, since they are not obligated to provide paid sick leave to workers in their first six months of employment. However, I also don't observe any changes in the labor composition from long-term jobs to short-term jobs, negating the theory that there will be labor-labor substitution to accommodate the cost of the paid sick leave policy.

These findings have implications for public policy. For policy makers worried that employers might shift their workforce to short-term workers to handle the added costs, a worry made explicit in the design of Seattle's PSST law, the results from this analysis show that this transition did not occur. Taken together with supplemental analysis in this study on the lack of

²³ Studies show that the cost of replacing an employee earning less than \$30,000 is around 16 percent of that employee's salary (Boushey and Glynn 2012). The average earnings for jobs in small firms that paid less than \$15 in Seattle was \$17,740, yielding a cost of \$2,838 per replacement. Workers in these jobs worked an average of 383 hours per quarter, which means they would accrue 38.3 hours of paid sick leave, costing the employer \$497.90 (\$13/hour wage).

policy effects on employment and earnings levels (echoed in Pichler and Zeibarth, 2018), the cost of Seattle's PSST policy appears to not have affected employment decisions at all.

For policymakers hoping to improve the economic security of working families, the Seattle PSST policy is a lesson in policy design. Workers who earn high wages or who work in larger firms were more likely to have access to paid sick leave prior to the law, so it is not too surprising that the analysis revealed no policy impacts among job of all wages in all firm sizes. This study found only modest declines in job turnover for workers employed in low-wage jobs and small firms, specifically those in short-term low-wage jobs, yet the results are small and not supported by commensurate declines in hires and separations among these job groups. These findings illustrates the policy design choices made in the PSST law, including exempting employers from the food and accommodation from providing paid sick leave, may have hindered the potential of the policy to reach vulnerable workers. Coupled with survey and observational research on the lack of knowledge and continual concern of employer retaliation, the findings suggest that access to paid sick leave may still be a problem for low-wage workers and workers in more vulnerable employment arrangements. Policymakers wishing to strengthen paid sick leave laws should be mindful of these possibilities to craft policy that will effectively meet the needs of these workers in their jurisdiction.

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Table 1. Access to Paid Sick Leave among Private Sector Workers, March 2012

	Proportion of Workers (U.S.)	Proportion of firms (Seattle)	<i>Employment share (Seattle)</i>
All workers	61	79.5	
<i>Wage distribution</i>			
Lowest 25 percent	29		
Earning <\$15			20.3
Highest 75 percent	74.3		
Earning >\$15			79.7
<i>Firm size</i>			
1-49 Workers	50		46.9
4-49 Workers		75.2	36.6
4-49 workers			
<\$15			9.3
50-99 workers	55		11.3
100-499 workers	66		23.1
50-250 workers			62.5
250 workers or			
more			27.5
500 workers or			
more	82		18.7
<i>Industry</i>			
Hospitality		27.5	11.5
Retail Trade		78	8.1
Health		58	12.5
Other		91.6	67.9

Source: Employment shares in Seattle are from the Washington state Unemployment Insurance program used in analysis. Column 1, the share of workers with access to sick leave is adopted from the Bureau of Labor Statistics, *Employee Benefits in the United States--March 2012*, Table 6. Selected paid leave benefits: Access, National Compensation Survey, March 2012. Column 2, the share of employers that provided sick leave is adopted from Romich (2017) Table 2 : Employer provision of paid sick time, 2012 and 2013. All estimates are for the year of policy adoption, 2012.

Table 2. Treatment effects from generalized synthetic control model for jobs in affected firms, by policy tier size and wage rate

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. All jobs, all firm sizes</i>						
coef.	-0.06*	-0.034	-0.019	-0.037	-0.017	0.013
se.	0.029	0.027	0.019	0.035	0.027	0.027
se. (nonparametric)				0.031	0.023	0.026
MSPE				0.015	0.016	0.008
Pre-policy mean	3351	3337	33.8%	3351	3337	33.8%
<i>Panel B. Jobs that pay <\$15 per hour all firms</i>						
coef.	-0.061	-0.055	-0.028	-0.027	-0.005	0.025
se.	0.036	0.037	0.024	0.038	0.041	0.033
se. (nonparametric)				0.035	0.038	0.037
MSPE				0.024	0.026	0.016
Pre-policy mean	1800	1687	60.4%	1800	1687	60.4%
<i>Panel C. Jobs that pay <\$15 per hour, small and medium size firms</i>						
coef.	-0.025	-0.022	-0.03	-0.017	-0.044	-0.041*
se.	0.035	0.032	0.022	0.04	0.031	0.022
se. (nonparametric)				0.029	0.019	0.016
MSPE				0.013	0.02	0.011
Pre-policy mean	1157	1097	68.4%	1157	1097	68.4%

Notes: The table displays the results from the DiD corresponding to equation 1 and GSC method corresponding to equation 2. The spanner heads above each set of columns, denoted by Panel A, B, and C indicate the group of jobs included in the estimation. Estimates are derived from running separate regressions. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. Exempt jobs are excluded from analysis. There are 1,736 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Table 3. Treatment effects from Difference-in-differences and Generalized Synthetic Control Model for all jobs and jobs which pay <\$15 in small firms, by job duration

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. Long-term jobs all firms</i>						
coef.	-0.057	-0.013	-0.007	-0.125*	-0.013	-0.013
se.	0.03	0.025	0.015	0.059	0.026	0.016
MSPE				0.025	0.012	0.007
Pre-policy mean	1614	1557	9%	1614	1557	9%
<i>Panel B. Long-term jobs in small firms for jobs that pay <\$15</i>						
coef.	0.012	0.006	0.002	0.006	-0.01	0.002
se.	0.029	0.029	0.017	0.03	0.031	0.021
MSPE				0.021	0.023	0.006
Pre-policy mean	442	367	15.8%	442	367	15.8%
<i>Panel C. Short-term jobs all firms</i>						
coef.	-0.136**	-0.064	-0.026	-0.113	-0.035	-0.027
se.	0.045	0.039	0.021	0.045	0.041	0.022
MSPE				0.073	0.024	0.018
Pre-policy mean	1737	1781	157%	1737	1781	157%
<i>Panel D. Short-term jobs in small firms for jobs that pay <\$15</i>						
coef.	-0.078	-0.046	-0.056*	-0.076	-0.079	-0.051*
se.	0.054	0.044	0.023	0.056	0.045	0.024
MSPE				0.081	0.031	0.021
Pre-policy mean	715	730	210.7%	715	730	210.7%

Notes: The table displays the results from the DiD corresponding to equation 2 and GSC method corresponding to equation 3. The spanner heads above each set of columns, denoted by Panel A, B, C, and D indicate the group of jobs included in the estimation. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. Exempt jobs are excluded from analysis. There are 1,680 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis.

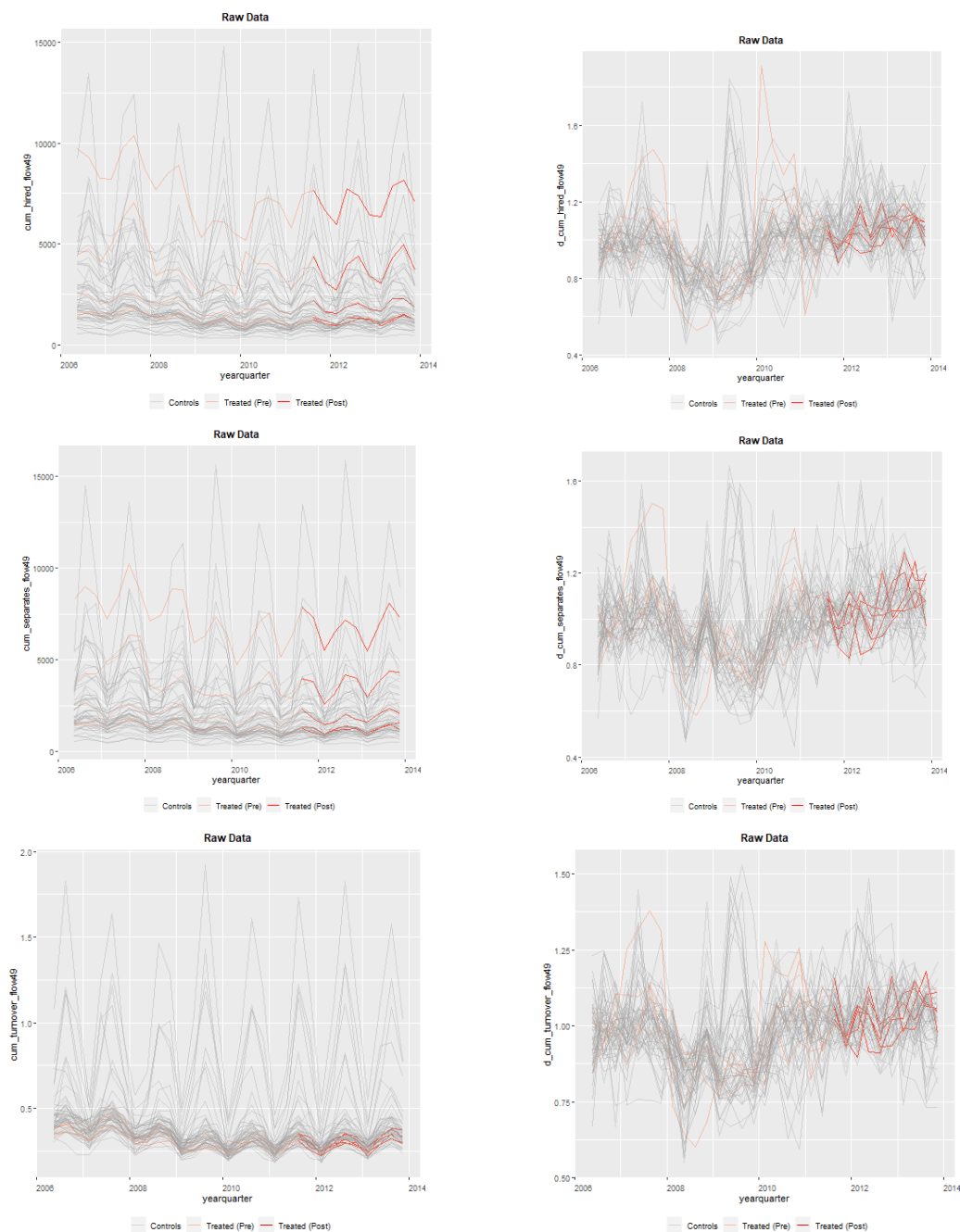
* p<.05, **p<.01

Table 4. Treatment effects from Difference-in-differences and Generalized Synthetic Control Model for all jobs and jobs which pay <\$15 in small firms

	Employment Share	Difference-in- difference	Generalized synthetic control
<i>Panel A Long-term workers all wages all jobs</i>			
coef.	88.6%	-0.001	-0.004
se.		0.003	0.003
MSPE			0
Pre-policy mean		17734	17734
<i>Panel B. Long-term worker in small firms earning <\$15</i>			
coef.	80.8%	-0.005	-0.005
se.		0.004	0.004
MSPE			0
Pre-policy mean		2528	2528
<i>Panel C. Short-term workers all wages all jobs</i>			
coef.	11.4%	0.001	0.004
se.		0.003	0.004
MSPE			0
Pre-policy mean		2240	2240
<i>Panel D. Short-term workers earning <\$15</i>			
coef.	19.2%	0.005	0.005
se.		0.004	0.004
MSPE			0
Pre-policy mean		632	632

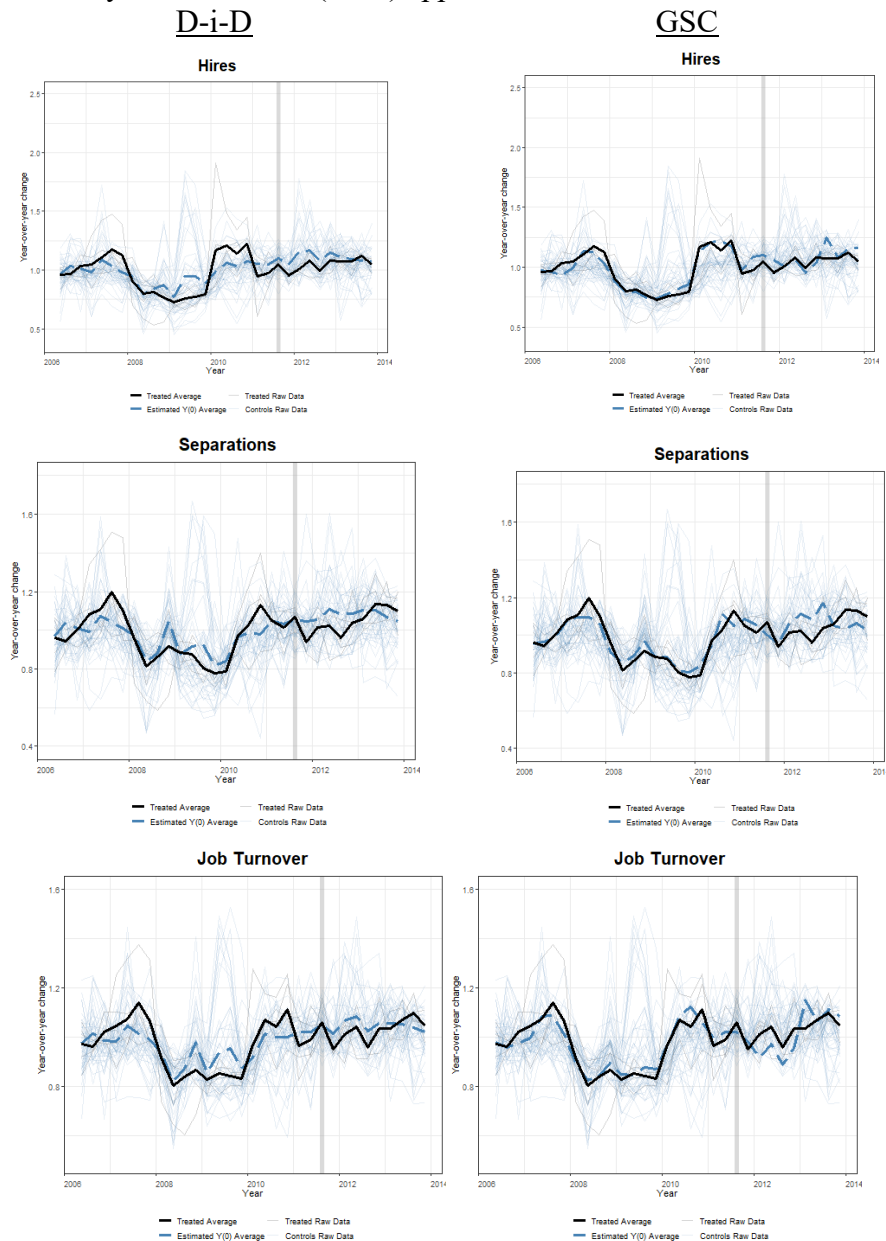
Notes: The table displays the results from the DiD corresponding to equation 1 and GSC method corresponding to equation 2. The spanner heads above each set of columns, denoted by Panel A, B, C, and D indicate the group of jobs included in the estimation. Short-term jobs are defined as jobs which existed for two or fewer consecutive quarters. Long-term jobs are defined as jobs which existed for at least three consecutive quarters. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. Exempt jobs are excluded from analysis. There are 1,680 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Figure 1. Time series of employment outcomes (levels) 2006q2- 2014q4
Levels
Year-over-year change



Note: The figures display the time series of each employment outcome for the five Seattle PUMAs (in red) and the 52 comparison PUMAs in Washington state outlying King County (in grey). Y.o.Y. = Year-over-year change in quarterly employment outcomes.

Figure 2. Treatment effects for low-wage jobs in small firms using difference-in-differences (D-i-D) and generalized synthetic control (GSC) approaches



Notes: The figure displays the time series of the Seattle treatment and counterfactual from a difference-in-difference (D-i-D) method in equation 1 and the GSC method corresponding to equation 2. Estimates are derived from running separate regressions for the year-over-year change in each employment outcome. The raw data for each PUMA is shown in the light blue and grey lines. Columns are labeled according to the method. The left column shows the time series using the D-i-D method. The right column shows the time series using the GSC method. All specifications include a control for population, $PUMA \times yearquarter$ fixed effects, and the GSC method controls for a number of unobserved factors chosen during the cross-validation procedure if applicable. Exempt jobs are excluded from analysis. There are 1,395 observations, 5 treated PUMAs and 52 untreated PUMAs.

Appendix A

Appendix Table 1. Average number and proportion of locatable jobs, by wage and firm group in Washington state

	All jobs	
	Number of jobs	Share locatable
Firms: 4 or fewer FTEs	226,505	99.3%
Firms: $4 < \text{FTE} \leq 50$	632,303	96.5%
Firms $50 < \text{FTE} \leq 250$	404,937	73.1%
Firms: more than 250 FTEs	617,804	51.7%

Note: The table contains quarterly averages for the number of locatable jobs during the study period, 2005Q1-2013Q4. The left column indicates the number of locatable jobs in each firm group and the right indicates the rate of coverage locatable firms have in Washington state.

Appendix Table 2. Treatment effects from generalized synthetic control model for jobs in affected firms, by policy tier size and wage rate

	Difference-in-difference		Generalized synthetic control	
	Employment	Earnings	Employment	Earnings
<i>Panel A. All jobs, all firm sizes</i>				
coef.	-0.023	0.004	0.017	0.027*
se.	0.016	0.009	0.03	0.013
se. (nonparametric)			0.026	0.009
MSPE			0.001	0
Pre-policy mean	19,975	\$10,127	19,975	\$10,127
<i>Panel B. Jobs that pay <\$15 per hour in all firms</i>				
coef.	-0.024	-0.005	-0.04	-0.002
se.	0.018	0.007	0.025	0.012
se. (nonparametric)			0.02	0.01
MSPE			0.002	0.001
Pre-policy mean	5,406	\$4,603	5,406	\$4,603
<i>Panel C. Jobs that pay <\$15 per hour in small firms only</i>				
coef.	0.008	-0.004	-0.027	-0.008
se.	0.016	0.006	0.026	0.008
se. (nonparametric)			0.023	0.008
MSPE			0.002	0.001
Pre-policy mean	3,160	\$4,435	3,160	\$4,435

Notes: The table displays the results from the GSC method corresponding to equation 2. The spanner heads above each set of columns, denoted by Panel A, B, C, and D indicate the group of jobs included in the estimation. Estimates are derived from running separate regressions. Columns are labeled according to the outcome. The pre-policy mean square prediction error of treated and control PUMAs and the pre-policy mean of the five treated PUMAs are included for each estimate. All specifications include a control for population, PUMAXyearquarter fixed effects, and a number of unobserved factors chosen during the cross validation process. Exempt jobs are excluded from analysis. There are 1,395 observations, 5 treated PUMAs and 40 untreated PUMAs. PUMA outside of Seattle in the surrounding King County are excluded. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<0.05, **p<0.01

Appendix Table 3. Falsification: Treatment effects from generalized synthetic control model for all jobs in firms with less than 4 FTEs

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. All jobs</i>						
coef.	-0.012	0.002	0.005	0.001	0	0.008
se.	0.029	0.025	0.017	0.033	0.025	0.018
MSPE				0.013	0.009	0.007
Pre-policy mean	1,025	949	42.3%	1,025	949	42.3%
<i>Panel B. All jobs excluding exempt jobs</i>						
coef.	0.007	-0.002	0.01	0.013	-0.004	0.009
se.	0.031	0.025	0.021	0.04	0.026	0.022
MSPE				0.01	0.012	0.007
Pre-policy mean	508	534	34.9%	508	534	34.9%
<i>Panel C. Jobs that pay less than \$15 excluding exempt jobs</i>						
coef.	0.031	0.01	0.021	0.02	-0.009	0.007
se.	0.035	0.03	0.022	0.038	0.032	0.025
MSPE				0.029	0.023	0.016
Pre-policy mean	259	261	53.8%	259	261	53.8%

Notes: The table displays the results from the DiD corresponding to equation 2 and GSC method corresponding to equation 3. The spanner heads above each set of columns, denoted by Panel A, B, and C indicate the group of jobs included in the estimation. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. There are 1,680 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Appendix Table 4. Falsification: Treatment effects from generalized synthetic control model for all jobs in exempt firms

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. All jobs in food and accommodation industry</i>						
coef.	0.05 5	0.009	-0.003	-0.071	-0.087	-0.008
se.	0.05 7	0.052	0.033	0.065	0.063	0.035
MSPE				0.061	0.044	0.02
Pre-policy mean	1060	1030	51.3%	1060	1030	51.3%
<i>Panel B. Jobs that pay <\$15 in food and accommodation industry</i>						
coef.	0.09 5	0.04	-0.013	-0.003	-0.012	-0.013
se.	0.05 5	0.049	0.041	0.06	0.053	0.041
MSPE				0.037	0.031	0.014
Pre-policy mean	599	551	63.2%	599	551	63.2%

Notes: The table displays the results from the DiD corresponding to equation 2 and GSC method corresponding to equation 3. The spanner heads above each set of columns, denoted by Panel A, B, C, and D indicate the group of jobs included in the estimation. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. There are 1,680 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Appendix Table 5. Falsification: Treatment effects from generalized synthetic control model for all jobs in exempt firms

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. All jobs in new firms</i>						
coef.	0.183	0.129	-0.015	0.183	0.196	-0.015
se.	0.125	0.129	0.091	0.125	0.196	0.092
MSPE				0.289	0.209	0.067
Pre-policy mean	599	507	83.7%	599	507	83.7%
<i>Panel B. Jobs that pay <\$15 in in new firms</i>						
				-		
coef.	-0.076	-0.147	-0.142	0.076	-0.147	-0.142
se.	0.13	0.138	0.094	0.127	0.14	0.094
MSPE				0.356	0.284	0.101
Pre-policy mean	314	245	123.4%	314	245	123.4%

Notes: The table displays the results from the DiD corresponding to equation 2 and GSC method corresponding to equation 3. The spanner heads above each set of columns, denoted by Panels A and B indicate the group of jobs included in the estimation. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. There are 1,680 observations 5, treated PUMAs and 52 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Appendix Table 6. Robustness: Treatment effects from generalized synthetic control model for jobs in affected firms excluding firms in King County, by policy tier size and wage rate

	Difference-in-difference			Generalized synthetic control		
	Hires	Separations	Job Turnover	Hires	Separations	Job Turnover
<i>Panel A. All jobs, all firm sizes</i>						
coef.	-0.059	-0.03	-0.018	-0.048	-0.015	-0.004
se.	0.03	0.03	0.02	0.037	0.03	0.03
MSPE				0.018	0.018	0.008
Pre-policy mean	3351	3337	33.8%	3351	3337	33.8%
<i>Panel B. Jobs that pay <\$15 per hour all firms</i>						
coef.	-0.062	-0.053	-0.027	-0.05	-0.002	0.024
se.	0.037	0.038	0.025	0.034	0.048	0.032
MSPE				0.027	0.02	0.016
Pre-policy mean	1800	1687	60.4%	1800	1687	60.4%
<i>Panel C. Jobs that pay <\$15 per hour, small and medium size firms</i>						
coef.	-0.019	-0.013	-0.029	-0.038	-0.047	-0.047*
se.	0.036	0.035	0.023	0.035	0.032	0.021
MSPE				0.016	0.021	0.011
Pre-policy mean	1157	1097	68.4%	1157	1097	68.4%

Notes: The table displays the results from the DiD corresponding to equation 2 and GSC method corresponding to equation 3, excluding the 12 PUMAs in King County from the comparison group. The spanner heads above each set of columns, denoted by Panel A, B, and C indicate the group of jobs included in the estimation. Estimates are derived from running separate regressions. The pre-policy mean square prediction error of year-over-year change in employment outcomes for treated and control PUMAs and the pre-policy mean are included for each estimate. All specifications include a control for population, PUMA x yearquarter fixed effects. The GSC method also controls for a number of unobserved factors chosen during the cross validation process, listed under each estimate. Exempt jobs are excluded from analysis. There are 1,395 observations 5, treated PUMAs and 40 untreated PUMAs. Standard errors are based on parametric bootstraps of 10,000 times and are in parenthesis. * p<.05, **p<.01

Appendix Table 7. Weights associated with the GSC estimator for each Seattle PUMA

PUMA ID#	<i>Panel A. Hires</i>				
	11601	11602	11603	11604	11605
10100	0.045	0.004	0.004	-0.039	0.082
10200	-0.214	0.125	0.208	-0.458	0.072
10300	-2.084	-0.113	-0.091	-0.791	0.429
10400	0.105	-1.858	-2.845	5.297	0.083
10501	-0.221	0.179	0.449	-0.067	0.001
10502	0.128	0.134	0.365	0.228	-0.041
10503	-0.566	0.303	0.586	-0.824	0.081
10504	0.198	-0.053	-0.170	-0.014	-0.018
10600	-2.044	0.278	0.547	-1.777	0.474
10701	-0.928	0.202	-0.410	-3.518	0.195
10702	-1.154	0.085	-0.132	-1.932	0.395
10703	-2.953	0.222	0.226	-2.905	0.514
10800	-2.478	0.257	0.134	-3.350	0.783
10901	-1.688	-0.252	-0.217	0.267	0.106
10902	-3.350	0.494	0.571	-4.123	0.495
11000	-0.757	0.396	0.687	-1.316	0.053
11101	0.355	0.110	0.296	0.338	-0.070
11102	-0.090	0.451	0.824	-0.779	-0.248
11103	0.314	0.358	0.271	-1.801	0.120
11104	0.159	-0.038	-0.021	0.328	-0.019
11200	1.218	-0.801	-1.717	1.526	-0.305
11300	0.398	-0.287	-0.668	0.204	0.183
11401	1.226	-0.187	-0.639	0.171	-0.187
11402	1.615	-0.618	-1.691	0.360	-0.227
11501	0.364	-0.115	-0.130	0.719	-0.075
11502	0.014	0.118	0.160	-0.429	0.064
11503	0.321	0.004	0.013	0.199	-0.016
11504	0.982	-0.277	-0.295	1.858	-0.206
11505	1.561	-0.523	-0.818	2.345	-0.088
11506	0.808	-0.067	0.161	1.645	-0.295
11507	2.020	-0.658	-1.223	2.329	-0.061
11606	0.158	0.101	0.299	0.284	-0.037
11607	-0.867	0.908	1.437	-3.202	0.575
11608	-0.503	0.796	1.293	-2.403	0.200
11609	0.260	0.130	0.445	0.645	-0.153
11610	2.441	-0.854	-1.219	4.338	-0.520
11611	0.277	0.475	0.674	-1.364	-0.013
11612	0.571	-0.312	-0.205	2.234	-0.295

11613	0.569	0.452	0.821	-0.487	-0.126
11614	0.617	0.195	0.145	-0.782	0.143
11615	-0.446	0.007	0.257	0.650	-0.232
11616	1.012	-0.258	-0.073	2.489	-0.301
11701	0.635	-0.043	0.054	0.921	-0.112
11702	0.366	0.266	0.251	-0.982	-0.146
11703	-1.062	1.012	1.652	-3.123	-0.035
11704	1.586	-0.364	-0.372	2.829	-0.618
11705	1.120	-0.525	-0.660	2.808	-0.448
11706	0.869	-0.199	-0.023	2.109	-0.292
11801	-0.268	0.220	0.385	-0.659	0.082
11802	-0.015	-0.028	0.037	0.375	-0.092
11900	-0.628	0.146	0.367	-0.370	0.147

Panel B. Separations

PUMA ID#	11601	11602	11603	11604	11605
10100	3.655	-0.812	-3.191	1.618	0.108
10200	2.274	-0.507	-1.784	1.607	0.266
10300	14.041	-2.205	-15.788	10.073	0.133
10400	-16.587	2.694	12.177	-7.057	-0.426
10501	-2.085	-0.664	2.071	-1.787	0.087
10502	-10.308	1.293	10.143	-7.456	0.171
10503	2.969	-0.493	-2.340	1.883	-0.070
10504	6.123	-0.305	-5.084	4.781	-0.141
10600	14.595	-3.103	-14.529	8.906	0.286
10701	6.865	-0.869	-8.517	4.434	0.335
10702	16.252	-2.369	-17.148	10.940	0.390
10703	30.298	-4.390	-31.304	20.860	0.361
10800	20.041	-3.725	-21.556	12.611	0.859
10901	-1.645	-0.368	-1.109	0.217	-0.073
10902	35.508	-5.052	-36.132	23.683	0.087
11000	10.267	-1.711	-9.413	6.229	0.130
11101	0.742	-0.030	0.531	0.133	-0.083
11102	4.284	-1.018	-2.937	2.172	-0.131
11103	-25.573	3.154	23.090	-18.940	-0.319
11104	-6.403	0.509	6.138	-4.558	0.231
11200	13.638	0.331	-14.075	10.829	-0.402
11300	-13.703	1.569	12.500	-8.325	0.001
11401	-4.522	0.911	4.740	-2.962	0.138
11402	-6.047	1.194	4.500	-3.347	0.147
11501	-0.655	0.020	0.744	-0.181	-0.053

11502	-6.176	0.232	6.504	-4.477	0.388
11503	-12.684	0.793	12.257	-9.153	0.146
11504	2.168	-0.076	-1.178	1.549	-0.103
11505	-0.147	0.471	1.019	-0.044	0.122
11506	-4.299	0.624	5.938	-2.987	-0.190
11507	-12.370	1.810	11.589	-9.839	-0.238
11606	-14.248	1.608	13.580	-10.024	0.361
11607	-0.836	-1.201	1.998	-2.704	0.571
11608	-10.951	0.995	11.246	-8.744	0.297
11609	-11.586	2.090	10.866	-7.105	-0.156
11610	1.980	0.809	-0.382	1.730	-0.318
11611	1.215	0.229	-0.452	0.372	0.176
11612	-10.478	1.676	9.592	-5.955	-0.365
11613	-3.650	0.687	5.499	-3.026	-0.183
11614	5.520	-0.540	-3.841	2.834	0.023
11615	-2.009	0.634	2.006	-0.321	-0.211
11616	0.092	0.630	0.899	0.726	-0.178
11701	-5.013	0.693	5.717	-3.557	-0.104
11702	9.396	-0.662	-8.104	5.887	-0.341
11703	-10.363	0.741	10.445	-8.907	-0.229
11704	7.393	0.007	-4.795	6.411	-0.835
11705	-1.427	1.027	1.884	0.999	-0.661
11706	1.128	1.225	0.371	1.579	-0.548
11801	-1.446	0.868	1.523	-0.469	-0.082
11802	-10.046	0.665	9.384	-6.866	0.292
11900	-5.185	-0.089	4.709	-4.273	0.331

Panel C. Job Turnover

PUMA ID#	11601	11602	11603	11604	11605
10100	2.060	-4.079	2.126	8.054	-1.485
10200	0.169	-0.529	0.361	0.678	-0.117
10300	1.401	-6.688	1.975	11.357	-2.019
10400	-11.588	19.216	-10.743	-40.838	7.911
10501	1.284	-3.061	1.727	5.221	-0.981
10502	-0.246	2.460	-0.807	-3.405	0.695
10503	1.325	-3.076	1.728	5.316	-1.074
10504	-2.962	5.403	-2.359	-12.035	2.016
10600	4.310	-12.563	5.589	21.856	-4.017
10701	6.436	-12.457	5.319	26.903	-5.098
10702	7.606	-16.189	6.407	34.238	-6.042
10703	9.662	-23.378	9.538	45.892	-8.345
10800	5.284	-14.430	5.629	27.228	-4.968

10901	-2.729	1.099	-1.327	-5.810	0.974
10902	10.855	-26.337	11.316	50.531	-9.631
11000	2.600	-6.051	3.169	10.833	-2.210
11101	-2.853	6.619	-2.697	-13.247	2.476
11102	0.083	-0.706	1.019	-0.415	-0.185
11103	-1.782	6.925	-3.551	-9.806	2.137
11104	1.457	-2.828	1.410	5.763	-1.043
11200	-3.085	6.415	-3.848	-11.507	1.892
11300	-0.199	1.831	-1.439	-1.025	0.619
11401	-1.068	3.548	-1.299	-6.313	1.172
11402	4.264	-6.333	2.518	16.287	-2.900
11501	0.406	-0.696	0.277	1.676	-0.188
11502	1.090	-1.704	1.005	3.706	-0.585
11503	-0.415	1.139	-0.033	-2.881	0.414
11504	-3.272	6.218	-2.428	-13.999	2.485
11505	-1.990	5.515	-2.561	-9.583	1.940
11506	-2.993	6.547	-2.319	-14.131	2.483
11507	0.950	1.738	-1.113	1.134	0.080
11606	-0.986	3.447	-1.382	-5.770	1.225
11607	4.775	-8.791	4.587	18.146	-3.186
11608	0.057	1.612	-0.287	-2.136	0.504
11609	-4.065	9.597	-4.463	-18.084	3.420
11610	-2.473	7.613	-3.461	-12.757	2.513
11611	0.293	2.062	-1.016	-1.075	0.497
11612	-4.600	9.910	-4.492	-19.804	3.666
11613	-0.158	2.094	-0.065	-3.892	0.628
11614	3.168	-4.646	2.536	10.823	-2.033
11615	-5.248	8.208	-3.640	-19.871	3.375
11616	-2.854	6.538	-2.764	-13.007	2.385
11701	-0.311	2.048	-0.707	-3.007	0.639
11702	-0.449	1.344	-0.219	-3.037	0.278
11703	3.548	-6.817	3.976	12.882	-2.688
11704	-3.978	7.652	-2.800	-17.515	2.829
11705	-6.768	13.316	-6.246	-27.600	5.009
11706	-3.552	7.703	-2.978	-16.275	2.816
11801	0.385	0.064	-0.032	0.829	-0.096
11802	-1.511	2.297	-0.613	-6.376	1.018
11900	-1.332	1.182	-0.520	-4.151	0.797

Notes: These weights are the weights of the comparison PUMAs for each of the five Seattle PUMAs used in the generalized synthetic control estimator in Table 2.

Appendix B

In this appendix, I provide the definitions for each of the short-term and long-term employment outcomes utilized in the main analysis. The long-term outcome definitions correspond to the “full-quarter” or “stable” definitions used in the QWI. The short-term outcome definitions encapsulate the jobs that do not fit the criteria of long-term jobs. Definitions are in Table B1. Below.

Table B1. Definitions of Employment Outcomes, by job duration status

Long-term Employment	Jobs for which the employer-employee match existed in quarters, $t - 1$, t , and $t + 1$ (referred to as “full quarter” employment in QWI)
Short-term Employment	Jobs for which the employer-employee match existed in quarters, $t - 1$ and t only.
Long-term Hires	New employer-employee matches among jobs that existed in quarters, $t - 1$, t , and $t + 1$ (referred to as “stable” hires).
Short-term Hires	New employer-employee matches among jobs that existed in quarters, $t - 1$, and t only.
Long-term Separations	Jobs that do not exist in quarter $t+1$ but did exist in quarter $t - 2$, $t - 1$, and t (referred to as “stable” separations).
Short-term Separations	Jobs that do not exist in quarter $t+1$ but did exist in quarter $t - 1$, and t only.
Long-term Job Turnover	$turnover_t = \frac{stable\ separations_t + stable\ hires_t}{2 * full-quarter\ employment}$
Short-term Job Turnover	$turnover_t = \frac{short\ term\ separations_t + short\ term\ hires_t}{2 * short-term\ employment_t}$

Appendix C

In this appendix, I provide the name of each comparison PUMA in Washington state and the corresponding PUMA identification number.

Table C1. Public Use Microdata Areas (PUMAs) in Washington State

10100	Whatcom County--Bellingham City PUMA
10200	Skagit, Island & San Juan Counties PUMA
10300	Chelan & Douglas Counties PUMA
10400	Stevens, Okanogan, Pend Oreille & Ferry Counties PUMA
10501	Spokane County (North Central)--Spokane City (North) PUMA
10502	Spokane County (South Central)--Spokane City (South) PUMA
10503	Spokane County (East Central)--Greater Spokane Valley City PUMA
10504	Spokane County (Outer)--Cheney City PUMA
10600	Whitman, Asotin, Adams, Lincoln, Columbia & Garfield Counties PUMA
10701	Benton & Franklin Counties--Pasco, Richland (North) & West Richland Cities PUMA
10702	Benton County (East Central)--Kennewick & Richland (South) Cities PUMA
10703	Walla Walla, Benton (Outer) & Franklin (Outer) Counties PUMA
10800	Grant & Kittitas Counties PUMA
10901	Yakima County (Central)--Greater Yakima City PUMA
10902	Yakima County (Outer)--Sunnyside & Grandview Cities PUMA
11000	Lewis, Klickitat & Skamania Counties PUMA
11101	Clark County (Southwest)--Vancouver City (West & Central) PUMA
11102	Clark County (West Central)--Salmon Creek & Hazel Dell PUMA
11103	Clark County (Southeast)--Vancouver (East), Camas & Washougal Cities PUMA
11104	Clark County (North)--Battle Ground City & Orchards PUMA
11200	Cowlitz, Pacific & Wahkiakum Counties PUMA
11300	Grays Harbor & Mason Counties PUMA
11401	Thurston County (Central)--Olympia, Lacey & Tumwater Cities PUMA
11402	Thurston County (Outer) PUMA
11501	Pierce County (Central)--Tacoma City (Central) PUMA
11502	Pierce County (Northwest)--Peninsula Region & Tacoma City (West) PUMA
11503	Pierce County (West Central)--Lakewood City & Joint Base Lewis-McChord PUMA
11504	Pierce County (South Central)--Tacoma City (South), Parkland & Spanaway PUMA
11505	Pierce County (North Central)--Tacoma (Port) & Bonney Lake (Northwest) Cities PUMA
11506	Pierce County (East Central)--Puyallup City & South Hill PUMA
11507	Pierce County (Southeast)--Graham, Elk Plain & Prairie Ridge PUMA
11601	Seattle City (Northwest) PUMA
11602	Seattle City (Northeast) PUMA
11603	Seattle City (Downtown)--Queen Anne & Magnolia PUMA

11604 Seattle City (Southeast)--Capitol Hill PUMA
 11605 Seattle City (West)--Duwamish & Beacon Hill PUMA
 11606 King County (Northwest)--Shoreline, Kenmore & Bothell (South) Cities PUMA
 11607 King County (Northwest)--Redmond, Kirkland Cities, Inglewood & Finn Hill PUMA
 11608 King County (Northwest Central)--Greater Bellevue City PUMA
 11609 King County (Central)--Sammamish, Issaquah, Mercer Island & Newcastle Cities PUMA
 11610 King County (Central)--Renton City, Fairwood, Bryn Mawr & Skyway PUMA
 11611 King County (West Central)--Burien, SeaTac, Tukwila Cities & White Center PUMA
 11612 King County (Far Southwest)--Federal Way, Des Moines Cities & Vashon Island PUMA
 11613 King County (Southwest Central)--Kent City PUMA
 11614 King County (Southwest)--Auburn City & Lakeland PUMA
 11615 King County (Southeast)--Maple Valley, Covington & Enumclaw Cities PUMA
 11616 King County (Northeast)--Snoqualmie City, Cottage Lake, Union Hill & Novelty Hill PUMA
 11701 Snohomish County (Southwest)--Edmonds, Lynnwood & Mountlake Terrace Cities PUMA
 11702 Snohomish County (West Central)--Mukilteo & Everett (Southwest) Cities PUMA
 11703 Snohomish County (Central)--Everett City (Central & East) & Eastmont PUMA
 11704 Snohomish County (South Central)--Bothell (North), Mill Creek Cities & Silver Firs PUMA
 11705 Snohomish County (Central & Southeast)--Lake Stevens & Monroe Cities PUMA
 11706 Snohomish County (North)--Marysville & Arlington Cities PUMA
 11801 Kitsap County (North)--Bainbridge Island City & Silverdale PUMA
 11802 Kitsap County (South)--Bremerton & Port Orchard Cities PUMA
 11900 Clallam & Jefferson Counties PUMA