Too Much Risk? Public Policy, Employment, and Earnings Volatility in Washington State

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Abstract

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In this dissertation, I conducted three studies of the causes and consequences of earnings volatility for workers in low-wage jobs using of administrative records from Washington State's Unemployment Insurance (UI) program between 2006 and 2016. In my first study, I asked: 1) What were the trends in intra-year earnings volatility for workers in low-wage jobs in Washington State between 2006 and 2016? 2) I used descriptive analysis and subsequently decomposed workers' intra-year earnings volatility to estimate the contribution of *between-work volatility*, the volatility that occurs from transitioning in and out of the UI-covered jobs, and *within-work volatility*, the volatility that occurs from fluctuations in hours and wages within continuous UI-covered employment. Among workers who were continuously employed during that period, I further estimated the contribution of workers' wages and hours volatility to their earnings volatility. I found that earnings swings (an earnings change > 25 percent of the previous

quarter's earnings) were frequent for workers in low-wage work. Workers were more likely to experience large declines during the economic downturn (2007-2009) and large increases during the period of strong labor market growth (2012-2016). However, the magnitude of large increases was smaller than those of large decreases. I further found that worker transitions in and out of UI-covered work—15.6 percent of all quarter-to-quarter transitions—accounted for nearly two-thirds of the total intra-year earnings volatility documented over the period. Among the subset of workers who experienced continuous employment, I found that volatility from workers' hours worked had a significant, positive association with workers' intra-year earnings volatility.

In the second chapter, I asked 1) What was the impact of a local paid sick leave policy on the employment levels, flows, and volatility of affected firms and workers? 2) Were these impacts larger for workers less likely to have access to paid sick leave before the policy? My analytic strategy was designed to evaluate changes in employment outcomes for firms and workers affected by the mandate (firms with at least four FTE employees) relative to those not covered by the policy (firms with four or fewer FTE employees) between 2010 and 2014. I focused on traditional measures of employment flows, such as hires, separations, and job duration, and on new measures of employment volatility, such as the quarterly arc percent change in workers' hours and earnings and their likelihood of experiencing a drop or gain in these outcomes. I found that firms right above the threshold of four full-time equivalent employees did not experience any statistically significant changes in their total headcount, payroll, hires, separations, or job turnover resulting from the policy. Workers in firms just above the FTE employment threshold for the PSST mandate experienced a 0.1 percent decrease in their hours worked in the year after the policy took effect. Subgroups analysis on part-time workers and workers with low earnings showed that these workers similarly experienced no change in their employment levels and small changes in their employment flows and volatility, echoing the impact estimates of workers overall.

In my third study, I asked 1) What is the impact of the Seattle Minimum Wage Ordinance (MWO) on employment flows (hires, separations, turnover) in the low-wage market? 2) What is the impact of the Ordinance on hours volatility among continuing jobs? I defined the low-wage labor market as jobs with wages greater than 150% of the post-policy minimum wage – jobs that pay <$19 – because research has shown that cascading effects from minimum wage policies are less likely to occur beyond this wage threshold. I used two quasi-experimental designs (synthetic control and interactive fixed-effect estimators) to estimate the impact of Seattle's MWO on hires, separations, job turnover, and two measures of hours volatility among jobs which remained post-policy. I found that total separations and hires in the low-wage labor market declined following the minimum wage enactment, with the most precise reductions occurring during the period in which the minimum wage was raised to $13 per hour. Hires fell by a range of 12.2 to 19.3 percent. This decline in hires is met with a less precise, but persistent decline in separations, yielding no statistically significant change in job turnover among low-wage jobs in Seattle, relative to the comparison groups. Jobs that continued to exist in the post-policy period exhibited an increase in quarterly hours volatility ranging from 1.1 to 2.8 percent in the post-policy period. The majority of the treatment effects for the number of large (>25 percent) hours declines within a job were negative and statistically significant when the minimum wage increased up to $13 per hour. The timing of the decline in large hours drops coincided with a statistically significant decline in separations, indicating that the increases in hours volatility may be due to hours increases from continuing jobs.

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DEDICATION

To the next generation of women scholars,

I am so excited to meet you.

INTRODUCTION

Frequent or large earnings and income shocks can have detrimental effects on workers' health and well-being and, for families with children, can have negative impacts on children's educational attainment and behavior (Gennetian et al., 2015; Schneider & Harknett, 2019). Earnings volatility has the potential to increase financial hardship and create anxiety and discontent from higher levels of household risk (Kalil & Ziol-Guest, 2008). An unexpected dip in income has been shown to be perceived as harder to cope with than an expense of the same financial amount (Larson et al., 2020). Families who experience large changes in earnings may have a hard time planning for the future, which can lead to indebtedness and inconsistent consumption (Dynarski et al., 1997; B. Meyer & Sullivan, 2009). For example, evidence on earnings volatility stemming from a job loss may lead to volatility in other parts of workers' lives and can have demonstrable effects on children's educational attainment, achievement, and behavior (Coelli, 2009; Stevens & Schaller, 2011).

Low-income families, who often depend on jobs with insufficient or unstable hours and lack substantial assets or access to good credit, are both more likely to experience volatility and are particularly vulnerable to its effects. Low-income individuals and workers who are more likely to be low-income or work for low wages, such as high school graduates or workers of color, are more likely sustain higher levels of volatility than their middle- and high-income peers (Bania & Leete, 2009; Gennetian et al., 2015; B. L. Hardy, 2017; H. D. Hill et al., 2013; Morris et al., 2015; Ziliak et al., 2011a). Growth in income volatility has been largest for those in the bottom quintile of the income distribution (Bania & Leete, 2009; Gosselin & Zimmerman, 2008; B. Hardy & Ziliak, 2014; Western et al., 2016). Low-income workers spend the largest share of their earnings on consumption goods, relative to middle- and high-income workers, implying that even a small fluctuation in a worker's monthly budget may have negative consequences for their economic security (Dynarski et al., 1997). Periods without earnings may affect these workers' ability to pay bills or purchase necessities.

One contributing factor to the high rates of employment and earnings volatility for low-income households is the rise in precarious employment practices (Western et al., 2012). Rather than set schedules, guaranteed hours, and healthcare benefits characteristic of full-time full-year work, low-wage jobs of the early 21st century take on more "precarious" forms such as the practice of scheduling workers for many hours one week and a few hours the next week or shifting employee classifications from full-time, full-year employment to part-time, temporary, and contractor work arrangements (Farber, 2008; A. Kalleberg, 2011; A. L. Kalleberg, 2009, 2012; A. L. Kalleberg & Marsden, 2013). These employment arrangements and scheduling practices, which are often out of workers' control, decrease the regularity and predictability of work time and can lead to instances of families having to manage earnings shocks. Evidence from nationally representative data found that a disproportionally large share of workers in low-wage jobs—66 percent of janitors and housekeepers, 90 percent of food-service workers, 87 percent of retail workers, and 71 percent of homecare workers—reported that their hours varied within the last month, illustrating the pervasiveness of such practices (Henly & Lambert, 2014; Lambert et al., 2014).

Accurately assessing the relationship between precarious work arrangements and earnings volatility for a broad group of workers requires detailed intra-year earnings and hours data for each job worked. Precarious work occurs week-to-week and season-to-season, in addition to year-to-year, and these time intervals cannot be captured in annual survey data (Morduch & Schneider, 2017). Survey data is also prone to mismeasurement due to survey nonresponse, imputation, and artificial changes in survey structure (Celik et al., 2012; Dahl et al., 2011). Moreover, without accurate data on hours worked, it is difficult to identify workers earning low wages or volatility within current work.

Documenting the scope of earnings volatility in the low-wage labor market is important for policy scholars because policymakers have the ability to create policy to protect workers from labor market inequities and risk. Scholars have attributed the rise of earnings volatility to the decline in unionization, the rise in short-term, temporary work, and self-employment, and the devaluing of the minimum wage over time—all processes that affect the low-wage labor market (R. A. Moffitt & Gottschalk, 2012). Additionally, many of today's current public administration programs target poor households based on annual earnings and income thresholds. These thresholds may not portray the true nature of households' welfare. In the absence of proper measurement, public programs may not ensure families against unpredictable earnings shortfalls as effectively as possible (B. Hardy et al., 2019). To date, the impact of public policy on workers' earnings volatility is an underexplored area of research.

Overview of the Dissertation

In this dissertation, I sought to understand the relationship between earnings volatility, employment instability, and public policy. My first aim was to understand how trends in intra-year volatility in low-wage work in Washington state evolved through the most recent economic cycle, 2006-2016. Identifying trends in employment and earnings volatility through the Great Recession is of pressing importance because Americans today find themselves on the brink of another, potentially deeper recession. My second aim was to understand how employment instability contributed to earnings volatility for workers in low-wage jobs. The rise in precarious work, and the impact precarious jobs can have on workers' economic security, merits a deeper understanding of the relationship between employment instability and earnings volatility. My third aim was to identify the degree to which public employment policy affects employment flows and earnings volatility. I examined two local policies: Seattle's Minimum Wage and Seattle's Paid Sick and Safe Time Ordinance. Both of these policies have the potential to affect the employment levels and flows of affected jobs, and as such, provide a theoretically interesting basis to conduct research.

Washington State provides a particularly unique environment to assess the degree and severity of precarious employment due to a large variety of industries and geography. In Seattle booming high-wage industries, such as the information and technology industry, sit side by side with low-wage sectors, such as the retail and food and restaurant industries, which allow for direct comparison. In suburban and rural areas, Washington boasts a strong share of workers in industries, such as agriculture, health care and education. Workers in different industries are likely to experience employment instability in different ways, and this volatility may lead to different assessments of workers' overall economic security.

A key innovation of this dissertation is the use of administrative data on wages and hours for a near census of workers engaged in formal work in the state. The data used throughout the dissertation comes from the Employment Security Department in Washington State, through an intra-state-agency agreement with the University of Washington. These data consist of quarterly employer filings of each employees' hours and earnings for every employee covered under Washington's Unemployment Insurance (UI) Program. While these data will not cover volatility that occurs week-to-week or month-to-month, they are an improvement from previous research, which as documented volatility year-to-year or in four-month increments. Moreover, these data allowed me to define treatment groups for the Paid Sick and Safe Time Ordinance and the Minimum Wage Ordinance with a novel degree of precision. The majority of the outstanding available survey employment data does not contain geography identifiers smaller than county level or employer-employee indicators at a quarterly level. Concerning Seattle's Paid Sick and Safe Time Ordinance, I determined the firms and workers in firms with more than 4 FTEs (the threshold for coverage) and assessed the impact of the policy for firms and workers right around the coverage threshold. Concerning the Minimum Wage Ordinance, I determined worker wages and assessed the impact of the minimum wage for low-wage jobs in Seattle, relative to low-wage jobs in Washington State. Below, I describe each chapter in more detail.

CHAPTER 1. TOO MUCH RISK? AN ASSESSMENT OF THE TRENDS AND DRIVERS OF EARNINGS VOLATILITY FOR WASHINGTON WORKERS IN LOW-WAGE JOBS

In the first chapter, I asked: 1) What were the trends in intra-year earnings volatility for workers in low-wage jobs between 2006 and 2016? 2) How did employment instability in low-wage work affect workers' intra-year earnings volatility? I investigated the trends and dispersion of intra-year earnings volatility using several measures of volatility, including the coefficient of variation and the share of workers experiencing large increases and decreases in their earnings, to account for the magnitude, frequency, and persistence of workers' earnings volatility (H. D. Hill et al., 2017; Western et al., 2016). I subsequently followed workers' hours worked, wages, and entry in and exit from the UI program data to assess whether overall trends in earnings volatility were masking underlying heterogeneity in key components of earnings volatility. These analyses will be the first to assess how contributions from hours, wages, entry to, and exit jobs contribute to workers' intra-year volatility.

Relative to workers in middle- and high-wage jobs, I found that intra-year earnings volatility was highest for workers earning low wages. Large earnings swings (an earnings change > 25 percent of previous quarter's earnings) were frequent for workers in low-wage work. An average of 84.0 percent of workers earning low wages experienced a large change in earnings each year. Workers were more likely to experience large declines during the economic downturn and large increases during the period of strong labor market growth. However, the magnitude of large increases was smaller than those of large decreases. Workers thus go hit with more declines during downturns and slower wage growth during upturns. I further found that worker transitions in and out of UI-covered work—15.6 percent of all quarter-to-quarter transitions—accounted for nearly two-thirds of the total intra-year earnings volatility over the period. A rising share of the volatility in transitions was from workers entering UI program work in the 2010-2016 recovery period. Among the subset of workers who were employed in at least two consecutive quarters, I found that workers' hours volatility had a large, significant, and positive effect on intra-year earnings volatility: an increase in hours volatility by one-percentage-point led to a 0.99-percentage point increase in earnings volatility. The magnitude of this coefficient was substantially larger than the effect of intra-year volatility from wages on earnings volatility, and it was smaller for workers outside of the arts, entertainment, food, and accommodation industry.

Chapter 2. Local Paid Sick Leave: An Examination of Firms and Workers

Workers in low-wage industries, or who work part-time hours, are least likely to have access to paid sick leave (Clemans-Cope et al., 2008; Lovell, 2003). Lack of access to sick leave means that workers are more likely to experience poor health outcomes or economic hardship if they, or a member of their family, become sick (Drago & Miller, 2010). To reduce inequality in workplace compensation, and promote public health, Seattle passed a Paid Sick and Safe Time Ordinance

(PSST) in 2011. The law required firms with more than four full-time equivalent (FTE) employees to provide one hour of paid time off for every 30-40 hours worked by employees within the city limits (Paid Sick and Safe TIme Ordinance, 2011). Evaluation of local paid sick leave policies is needed because cities and states across the country have enacted paid sick leave without clear evidence about how these policies will affect the low-wage labor market.

My analysis focused on Seattle workers and firms right around the policy's eligibility threshold for mandating coverage. I evaluated changes in employment outcomes for firms and workers affected by the mandate (firms with at least four FTE employees) relative to those not covered by the policy (firms with four or fewer FTE employees) between 2010 and 2014. Beyond identifying workers in Seattle, the administrative data allowed for precise identification of firm size to compare the labor market outcomes of these two groups over time. This level of precision is an improvement on previous analyses, which have used proxies for treatment to include non-treated geography (Morduch & Schneider, 2017) or non-treated firms (Ahn & Yelowitz, 2015).

I evaluated the PSST policy on firms' total headcount, hours, payroll, hires, separations, and job turnover using a regression discontinuity design to assess whether the increased cost of the policy had any effect on firms' employment levels or flows. I then used a difference-in-differences design on four longitudinal cohorts of workers to estimate the effects of the paid sick leave policy on workers' employment, hours, earnings, probability of being hired or separating, job duration, and employment volatility. This study is the first to examine the effect of a paid sick leave policy on traditional measures of employment flows, such as hires, separations, and job duration, as well as on new measures of employment volatility, such as the quarterly arc percent change in workers' hours and earnings and their likelihood of experiencing a drop or gain in these outcomes.

I found that firms right above the threshold of four full-time equivalent employees did not experience any statistically significant changes in their total headcount, payroll, hires, separations, or job turnover as a result of the policy. Workers in firms just above the FTE employment threshold for the PSST mandate experienced a modest decrease in their hours worked after the policy took effect, with estimates ranging from 0.014 to 0.021 percent. Subgroups of workers who were less likely to have access to paid sick leave before the PSST policy, part-time workers and workers with low-earnings, experienced minimal changes in their employment levels and their employment flows and a small increase in their volatility across the four cohorts, echoing the overall impact estimates of workers. These results do not support the hypothesis that the cost of policy implementation adversely affected the employment outcomes of firms. However, these results also do not support the hypothesis that the PSST policy demonstrably improved workers' employment stability, which suggests that the cost and consequences of the PSST were too small to cause employment changes. Another explanation for the null results could be that firms and workers around the threshold didn't know that they were mandated to provide, and eligible to receive, paid sick leave, respectively. Evidence for this hypothesis has been documented in qualitative research (Morduch & Schneider, 2017). This could affect compliance rates and suggests that alternative methods to regression discontinuity could strengthen the validity of the null results.

CHAPTER 3. THE EFFECTS OF MINIMUM WAGE ORDINANCES ON EMPLOYMENT FLOWS AND HOURS VOLATILITY IN LOW-WAGE JOBS

The Seattle Minimum Wage Ordinance (MWO), the first in a nation-wide policy push to raise the living wages of cities and states to $15 per hour, was passed with the intent to "advance workplace equity for all Seattle workers, including…historically disadvantaged communities who are disproportionately represented among low-income workers" (Wage Theft Prevention

Ordinance, 2014). My third chapter evaluated the impact of Seattle's 2015 Minimum Wage Ordinance, which mandated that employers raise their minimum wage to $15 over several years, on the employment flows of low-wage jobs in Seattle. Seattle was the first in a wave of cities, not just to raise its minimum wage, but to raise its minimum wage *by nearly 60 percent* of its pre-policy wage of $9.47 over a period ranging from three to seven years.

This chapter covered the first two phase-ins of the MWO (a 37.3 percent increase), in which the minimum wage increased to $13 per hour, and is intended to inform policymakers across the country how large increases in minimum wages affect employment flows in their jurisdiction. Workers earning low wages are disproportionately employed in jobs with short duration and high turnover rates, conditions which research has shown can cause negative health outcomes and economic instability (Morduch & Schneider, 2017; Schneider & Harknett, 2019; Western et al., 2016; Ziliak et al., 2011a). Understanding the implications of workplace regulation on the employment stability of the low-wage market is a key indicator of workplace equity. This paper asks 1) What was the impact of the Seattle Minimum Wage Ordinance on employment flows (hires, separations, turnover) in the low-wage market in the nine quarters following its passage? 2) What was the impact of the Ordinance on hours volatility among continuing jobs?

I used the administrative hours worked data from the Washington state UI program to estimated wages for each job-quarter, which represents an innovation in the identification of treatment and control group in the minimum wage literature. I used synthetic-control and interactive fixed-effects estimators and estimated the impact of Seattle's MWO on hires, separations, and job turnover, thereby contributing the first evaluation of local minimum wage ordinances on employment flows. I also estimated the impact of the MWO on two measures of hours volatility within jobs that remained post-policy enactment. This evidence contributes new dimensions of the impact of minimum wages on employment stability within low-wage jobs.

I found that total separations and hires in the low-wage labor market declined following the enactment of the MWO. The most precise reductions occurred after the minimum wage was raised to $13 per hour for large firms. Hires fell by a range of 12.2 to 19.3 percent during this period. This decline in hires was met with a less precise, but persistent decline in separations and commensurate decline in job turnover among low-wage jobs in Seattle, relative to the comparison groups. The elasticity estimates of hires and separations, which ranged from -0.23 to -0.37, were larger than those found in prior research, which used proxies of the low-wage labor market to analyze state and federal laws (Brochu & Green, 2013; Dube et al., 2016a; Portugal & Cardoso, 2006).This evidence furthers the notion that local labor markets may have more acute reactions to minimum wage laws, relative to state or federal minimum policy.

I found that jobs that continued to exist in the post-policy period exhibited an increase in hours volatility ranging from 1.1 to 2.8 percent during the implementation of the minimum wage increase to $11 and $13 per hour, respectively. However, the majority of the impact estimates on the number of large (>25 percent) hours declines within a job were negative and statistically significant in the quarter after the minimum wage increased to $13 per hour. The precise decline in large hours drops coincided with a statistically significant decline in separations, suggesting the decline in separations may affect the decline in large earnings drops among low-wage jobs.

Implications for Research and Policy

These three studies contribute to a nuanced understanding of the prevalence and patterns of intra-year volatility in low-wage work and how labor regulations mitigate or exacerbate this volatility. The causal analysis focused on local labor regulations, a domain of policy that is growing across the country but has received insufficient attention to date (National Partnership for Women & Families, 2020). Evaluation of these policies will inform policymakers across the country how minimum wage and paid sick leave legislation can affect the economic security of workers in their jurisdiction.

In contributing evidence that workers experienced substantially high levels of intra-year earnings volatility, I demonstrated the utility of administrative data in documenting the prevalence of earnings volatility in the low-wage labor market. Although quarterly data does not capture all the volatility experienced by workers in low-wage work, it nonetheless provided detailed information on all UI-covered jobs worked within a quarter and allowed for assessment season-to-season for workers within a year. Survey data, which is often annual, may not be able to pick up as season-to-season changes in workers' employment flows or may face other problems, such as seam bias or workers' tendency to underreport earnings (B. D. Meyer et al., 2015). As such, administrative data can shed new light on the dynamics of the formal low-wage labor market.

In response to the high levels of earnings volatility observed in formal work, policies intended to reduce volatility in hours worked, such as the recent secure scheduling laws and guaranteed minimum hours or paid sick leave, could be considered (B. Hardy et al., 2019). These policies have the potential to reduce the "risk of employment" for workers, provided the costs of these policies are not passed down to workers. If employers cannot create predictable schedules or support workers in unpredictable times, policymakers could consider regulations that ensure workers have a right to refuse certain scheduling practices, thereby providing them a degree of control over scheduling their time to work.

I also demonstrated the consequences of varying treatment intensities in employment policy. In my evaluation of Seattle's PSST policy, which had a marginal labor cost ranging from 2.8-3.3

percent, I found null employment effects for the majority of employment levels, flows, and volatility outcomes examined among both firms and workers. The evidence suggests that the policy had no effect on the employment or earnings of firms and workers. By contrast, in my evaluation of Seattle's Minimum Wage Ordinance, which had a marginal labor cost ranging from 10.8 to 37.3 percent, I found large and statistically significant declines in hires, separations, job turnover, and large hours declines within a job. Careful attention to the ways in which workers and firms differentially respond to employment policy is necessary to identify and address these gaps and improve economic stability and security for both workers and firms.

Finally, I demonstrated utility in using within-job volatility estimates as dependent variables. In Chapter 1, I found that within-job volatility accounted for one-third of intra-year earnings volatility for workers in low-wage jobs. Using variables to capture within job volatility can illustrate the ways in which employment conditions on the job change in response to public policy. Evidence from chapter 3 shows that the volatility in hours worked increased in response to the MWO. However, there was also a decline in large hours drops, a finding which is timed with the decline in separations. This means that while some jobs may last longer, they may also be more volatile as a result.

Chapter 1. TOO MUCH RISK? AN ASSESSMENT OF THE TRENDS AND DRIVERS OF EARNINGS VOLATILITY FOR WASHINGTON WORKERS IN LOW-WAGE JOBS

Earnings have become increasingly unstable (Gottschalk & Moffitt, 2009). While some earnings fluctuations can be planned for, others can be detrimental for families' economic security and well-being: In a survey of 235 low- and middle-income families, 78 percent of participants indicated they preferred financial stability relative to "moving up the income ladder" (Hannagan & Morduch, 2015). Larsen, Hall, and Wething (2020) find that an unexpected dip in income is perceived as harder to cope with than an expense of the same financial amount. For families with children, earnings and income shocks can have detrimental effects on child behavior and well-being (Barazzetta et al., 2019; Gennetian et al., 2015; H. D. Hill et al., 2013). Families who experience large changes in earnings may have a hard time planning for the future, which can lead to indebtedness and inconsistent consumption (Dynarski et al., 1997; B. Meyer & Sullivan, 2009). For example, evidence on earnings volatility stemming from a job loss may lead to volatility in other parts of workers' lives and can have demonstrable effects on children's educational attainment, achievement, and behavior (Coelli, 2009; Stevens & Schaller, 2011).

Workers in low-wage jobs are more likely to experience earnings volatility and are particularly vulnerable to earnings fluctuations because they lack the savings, assets, and access to credit to buffer against negative income fluctuations (Bania & Leete, 2009; Hannagan & Morduch, 2015). Studies of intra-year earnings volatility found that workers with low-income or economically disadvantaged in other ways have the highest levels of earnings volatility, relative to the rest of the workforce (Bania & Leete, 2009; Gennetian et al., 2015; H. D. Hill et al., 2013;

Morris et al., 2015). Evidence on the impact of small and large shocks shows that low-income families have limited ability to smooth consumption against shocks of any size (Blundell et al., 2008). The psychological, economic, and social implications of earnings volatility highlight the importance of understanding its pervasiveness and determinants for workers in low-wage jobs.

The Congressional Budget Office estimates that earnings from employment made up the majority of household income for workers' in the bottom income quintile in 2016 (Congressional Budget Office, 2019). Employment has historically been characterized by set schedules, guaranteed hours, and healthcare benefits (A. Kalleberg, 2011). However, the nature of work, particularly for workers in low-wage jobs, has changed. Low-wage jobs of the early 21st century have taken on more "precarious" forms, including jobs that are temporary or "on-demand," or jobs that have unstable and unpredictable scheduling practices (A. Kalleberg, 2011; A. L. Kalleberg, 2009, 2012; A. L. Kalleberg & Marsden, 2013). Evidence from nationally representative survey data found that as of 2011, 74 percent of all early-career workers experienced instability in their weekly work hours, and, in the food service industry, 90 percent of workers experienced hours fluctuations in the last month (Lambert et al., 2014). In the 21st century, this rise has coincided with a period of stagnating wage growth and the Great Recession, the largest economic downturn in US economic history since the Great Depression.

The rise in precarious work, and the impact precarious jobs can have on workers’ economic security, merits a deeper understanding of the relationship between employment instability and earnings volatility. In this study, I ask: 1) what are the trends in intra-year earnings volatility for workers in low-wage jobs between 2006 and 2016? 2) How does employment instability in low wage work affect workers’ intra-year earnings volatility? I estimate quarterly earnings changes within a year to document fluctuations that might not be otherwise observed from annual data, contributing new evidence on the frequency and pervasiveness of earnings volatility in low-wage work. Assessing these questions in a period spanning the Great Recession is of pressing importance because Americans today find themselves on the brink of another, potentially deeper recession. Industries most responsive to economic cycles, such as entertainment and food service, are largely comprised of low-wage jobs, and the Great Recession was one of the first tests of how workers in precarious employment circumstances, faired during the downturn and recovery.

Assessing the relationship of precarious work with earnings volatility requires accurate and detailed intra-year employment and earning data. Precarious work occurs week-to-week and season-to-season, in addition to year-to-year (Morduch & Schneider, 2017). A concern of prior studies assessing intra-year volatility are the limitations of survey data to estimate trends in earnings volatility, including nonresponse, seam bias, and workers' tendency to underreport earnings (B. D. Meyer et al., 2015). I use administrative data from Washington state to assess intra-year earnings volatility, data that provides a full census of jobs in Washington state covered by the Unemployment Insurance (UI) program between 2006 and 2016. These data, which include quarterly earnings and hours, allow me to calculate workers' wages and define workers in low-wage jobs by their position in each annual distribution of wages. I investigate the trends, dispersion, and frequency of intra-year earnings volatility over time. To account for the magnitude, persistence, and directionality of workers' earnings volatility, I use several measures of volatility, including the coefficient of variation, the arc percent change, and the share of workers experiencing large earnings increases and decreases within a year (H. D. Hill et al., 2017; Western et al., 2016).

I subsequently follow workers' hours worked, wages, and entry and exit from the UI program data to assess whether overall trends in earnings volatility are masking underlying heterogeneity in two components of earnings volatility: 1) *between-work volatility*, volatility that occurs from transitioning in and out of the UI data, and 2) *within-work volatility*, volatility that occurs from fluctuations in hours and wages within continuous UI-covered employment. Previous studies of inter-year volatility have shown that transitions in and out of employment are the largest contributing factor estimates of earnings volatility (Celik et al., 2012; Ziliak et al., 2011b), however, its' unclear whether these findings will hold up at the intra-year level.

I then assess the contribution of volatility from wages and hours worked to workers earnings volatility within continuous work, and I investigate the role that employers' industry plays in these contributions. Workers are "continuously employed" if they were employed in at least two consecutive quarters within a year. The relative impacts of hours worked and wage volatility on workers' earnings volatility within continuous work contributes an understanding of the ways that workers experience volatility on the job. Wage volatility within continuous work could be a sign of mobility in low-wage work. However, if the impact of hours volatility on earnings volatility within low-wage work is larger than that of wages, the results would imply that earnings volatility in low-wage work is due to irregularity and volatility in work schedules. Previous research has shown that precarious schedules in the service sector negatively impact worker well-being (Schneider & Harknett, 2019). My study contributes new information on the impact of wage and hours volatility on earnings volatility using intra-year variability, thereby contributing new information on employment dynamics for a large group of economically vulnerable workers at a granular level.

I first confirm previous survey evidence that shows that intra-year earnings volatility was highest for workers earning low wages over the 2006-2016 period.[1] These workers experienced multiple fluctuations within a year. Across the period studied, I find that an average of 84.0 percent of workers earning low wages experienced a quarterly change in earnings greater than 25 percent. While intra-year earnings volatility is relatively stable over the period, workers earning low wages are much more likely to experience declines during economic downturns and increases during a period of strong labor market growth. However, in the recovery period of 2011-2016, a period characterized by economic health, the magnitude of large increases were smaller than those of large decreases. Workers thus get hit with more declines during downturns and earnings growth during upturns. I further find that workers transitions in and out of UI-covered work—15.6 percent of all quarter-to-quarter transitions-- accounted for nearly two-thirds of the total intra-year earnings volatility over the period, and a rising share of the volatility in transitions was from workers entering UI program work in the 2011-2016 recovery period. Among the subset of workers who are continuously employed, I find that volatility from workers' hours worked has a large, significant, and positive effect on intra-year earnings volatility. An increase in hours volatility by one-percentage-point led to a 0.99-percentage point increase in earnings volatility. The magnitude of this coefficient is substantially larger than the effect of volatility from wages on earnings volatility: an increase in wage volatility by one-percentage-point lead to a 0.006 percentage point increase in earnings volatility. The magnitude of this relationship is smaller for workers outside of the arts, entertainment, food, and accommodation industry. These results contribute new

[1] Workers are in the low-wage employment group if their highest wage in a year falls in the bottom 33rd percentile of wages in a year.

information on how employment instability at the intensive and extensive margins impact workers' intra-earnings volatility.

Previous Literature

Studies of earnings volatility in the US originated in the early 1990s with an effort to understand whether the increases in earnings and income inequality between the 1970s and 1990s had been driven by "temporary, idiosyncratic" changes in workers' earnings volatility or by more "permanent," unobserved structural changes, such as de-unionization, globalization or technological change (Gottschalk et al., 1994; Gottschalk & Moffitt, 2009; Haider, 2001). Using longitudinal survey data from men in the Panel Studies of Income Dynamics (PSID), early studies of earnings volatility estimated workers' "permanent" and "transitory" earnings using an earnings component model, which estimated workers' permanent earnings through a parametric model controlling for worker age and defined workers' transitory earnings as the residual.[2] These studies found that both components increased in the 1970s and 1980s but leveled off beginning in the 1990s (R. A. Moffitt & Gottschalk, 2012).

More recent studies of year-to-year earnings volatility have utilized other survey data sources and non-parametric, descriptive measures to estimate annual earnings volatility (Dahl et al., 2011; Dynan et al., 2012; Shin & Solon, 2011; Ziliak et al., 2011b) .[3] These studies expanded the types of workers included beyond prime-age working men and broadened the definition of earnings to

[2] Prior studies using the PSID estimating earnings volatility used an earnings component model, which decomposes workers' earnings into a "permanent" component based on a parametric regression model of workers' age and experience and a "temporary" component based on the residual.

[3] Shin and Solon (2011) show that the earnings component model can be sensitive to model specification and cannot incorporate observations with zero earnings into the model. The authors go on to show that descriptive approaches, such as estimating percent change in log earnings, can approximate the earnings component model without a loss of generality (Shin and Solon, 2011).

reflect the range of employment experiences that can have a material effect on individuals and families.

Consensus has not been reached on the trends in earnings volatility from this more recent wave of literature, in part, because of the variety of data sources and samples used. Studies using descriptive measures in the PSID generally find an increase in annual earnings and income volatility through the 1990s and early 2010s (Dynan et al., 2012; Hacker, 2008; R. Moffitt & Zhang, 2018; Shin & Solon, 2011). However, these studies have been criticized due to issues of nonresponse, imputation, and artificial changes in survey structure that can make it difficult to harmonize trends over time (Celik et al., 2012; Dahl et al., 2011). Studies using the Current Population Survey (CPS) have also found increases in earnings volatility during the early 2000s and in the wake of the Great Recession (Koo, 2016; Ziliak et al., 2011b). Due to the large sample size of the CPS, researchers have used the survey to examine subgroups of workers and have found that high school dropouts, young workers, workers of color, low-income workers, and workers in low-wage jobs all have persistently high earnings volatility (Cameron & Tracy, 1998; B. L. Hardy, 2017; Ziliak et al., 2011b). By contrast, studies using administrative data, such as matched Social Security - Survey of Income and Program Participation data or the Longitudinal Employment and Household Dynamics data, do not observe increases in workers' annual earnings volatility over the last 40 years (Celik et al., 2012; Dahl et al., 2011; Ziliak et al., 2011b).

Simultaneously, researchers have also documented a rise in precarious forms of work, such as temporary and on-call work, jobs that have uncertain employment practices, such as the practice of scheduling workers for many hours one week and a few hours the next week to meet changing consumer demand, and the rise of the informal work sector (A. Kalleberg, 2011; A. L. Kalleberg, 2009, 2012; A. L. Kalleberg & Marsden, 2013). Precarious jobs can primarily affect workers' employment instability in the formal labor market through two vehicles. First, precarious employment increases the rate of *within-work (within-job) instability* through scheduling practices that increase the volatility in hours worked within a consistent job and the job's commensurate earnings (Harknett et al., 2017; Henly & Lambert, 2014; Lambert et al., 2014). Second, precarious jobs increase the rate of *between-work (between-job) instability* through shorter durations of employment contracts and higher rates of job churn and temporary work (Farber, 2008; Wenger & Kalleberg, 2006). In theory, some fluctuations in employment may benefit workers if earnings gains accompany the fluctuations, such as a promotion or year-end bonus. However, evidence shows that mobility out of low-wage has declined among workers entering low-wage work since the late 1990's (Schultz, 2019).

The rise in precarious work may have implications for the magnitude, frequency, and growth of earnings volatility in the formal labor market that may not be able to be detected through annual earnings. Survey research shows that workers experience fluctuations week-to-week and season-to-season, in addition to year-to-year fluctuations (Morduch & Schneider, 2017). Measures of intra-year volatility, therefore, may be more equipped to capture the volatility that arises from precarious employment (Bania & Leete, 2009; Gennetian et al., 2015; Hannagan & Morduch, 2015; H. D. Hill et al., 2013) Wolf et al., 2014. Those who have examined intra-year earnings volatility using survey or experimental data have found intra-year income volatility is generally higher than inter-year volatility, suggesting that omitting this volatility may be underestimating the degree to which workers experience instability in their lives. Intra-year volatility is also highest for low-income workers and workers who are more likely to be low-income or low-wage, such as high school graduates and workers of color (Morduch & Schneider, 2017; Wolf et al., 2014). This is concerning given that low-income workers are most likely to lack the savings, assets, and access to credit that would buffer against negative income fluctuations (Bania & Leete, 2009). Low-income workers have the highest consumption-earnings ratio, implying that periods without earnings may affect their ability to pay bills or purchase necessities (Dynarski et al., 1997).

To complement intra-year earnings trends using survey data, and to explicitly document trends for workers who are most vulnerable to changes in earnings, the first aim of this paper assesses the trends in intra-year earnings volatility for workers in low-wage jobs between 2006 and 2016. My use of quarterly administrative data to estimate intra-year changes provides the administrative complement to intra-year estimates from survey data (Gennetian et al., 2015; Morris et al., 2015; Wolf et al., 2014), without issues that have plagued surveys such as imputation bias and nonresponse. While earnings are not a strict measure of economic security, it is nonetheless a very important driver. Evidence shows that the share of the variance of earnings volatility experienced by workers historically contributed between 80 and 90 percent of a household's overall income volatility (B. Hardy & Ziliak, 2014). In estimating intra-year earnings volatility for workers in low-wage jobs through the Great Recession and recovery, this study uncovers how intra-year labor market dynamics affect the magnitude and direction of workers' earnings volatility during economic downturns and recoveries.

To bring together the precarious employment literature and earnings volatility literature, the second aim of this paper is to assess the contributions of intra-year earnings volatility due to continuous employment and volatility due transitions in and out of UI-covered work. This is the first paper to decompose intra-year earnings into volatility from intra-year job transitions and volatility from consistent employment. Given the rise in precarious work and the negative impact that earnings volatility can have on workers, understanding the relationship between employment instability and intra-year earnings volatility is critical. Previous research has shown found that that shifts in family structure and employment explained 43 percent of extreme income losses for children in low-income families between 1984 and 2010 (Western et al., 2016). In addition, evidence on job transitions has shown that job transitions, and the hours volatility that arises during these job transitions, account for the majority of annual earnings volatility for all workers, and for workers with low education levels (Celik et al., 2012; Koo, 2016; Ziliak et al., 2011b). I exploit the longitudinal structure of the UI program data so I can observe worker employment transitions within a year and document associated earnings changes in response to any observed employment changes.

The decomposition of the variance of earnings volatility shows the relative contributions of between-work and within-work volatility. However, it stops short of shedding light on the impact that employment characteristics may have on workers' earning volatility within work. I aim to advance the literature with the third aim of this paper, which assesses the contributions of volatility in hours worked and volatility in wages to earnings volatility for workers in low-wage continuous employment. Among workers who are continuously employed, the stagnation in wages for most working Americans means that workers' experience of earnings volatility may be driven by volatility in hours. In particular, workers in low-wage jobs, have been documented to have high rates of hours volatility due to irregular, unpredictable, and unstable scheduling practices (Schneider & Harknett, 2019). Volatility in hours worked makes planning for work difficult and can have spillover effects into other components of family life, such as family stress and work-life balance. In assessing whether employment characteristics moderate the effects on hours and wage volatility has on earnings volatility, I contribute new evidence on how volatility in hours worked and in wages, conditional on employment in low-wage jobs, affect workers' earnings volatility.

Survey data of hourly-wage workers in the retail and food and accommodation industries has shown that workers in these industries experience regular uncertainty in their work schedules, such as having little advanced notice of shifts, having a variable schedule, having a shifts canceled, in their income week-to-week (Henly & Lambert, 2014; Lambert et al., 2014; Schneider & Harknett, 2019). As prior research has shown demonstrable schedule instability in retail and other service industries, I am further interested in whether workers' earnings volatility is moderated by the industry of their main low-wage job. Assessing the interaction of employment in these industries and workers' hours and wage volatility could shed light on how various industry practices impact earnings volatility for workers with low earnings (Federal Reserve Board., 2014; Morduch & Schneider, 2017).

DATA

The Washington State Employment Security Department collects quarterly payroll records for all workers who receive earnings through formal work in Washington State and are eligible for the Washington State Unemployment Insurance (UI) program. These data, which are generated by employers' reports of quarterly payroll filings to the state, include quarterly employee hours and earnings information, and employers' industry (NAICS 6 digit).[4][5] In these data, a firm is defined by its unique Employer Identification Number. It can encompass a single establishment or several establishments if the establishments all fall under one umbrella firm. These data do not include

[4] Data are cleaned to omit workers that have observations with zero hours, zero earnings, or total hours worked greater than 1,820 (the equivalent of a 20-hour workday).

[5] Employers are not required to report earnings from local paid sick leave laws, allocated tips, jury-duty pay, or death benefits. Paid vacation, holidays, bonuses, non-cash payments, and tips reported by employees are reported. Hours include vacation pay (if it is not a cash payment), over time, and should include exact hours worked for all employees. If hours are not tracked, the employer may put 40 hours per week.

earnings from a variety of sources, including earnings from informal arrangements, contract employment, employment outside of Washington, and self-employment. This limited my ability to estimate earnings volatility because I was unable to capture the full breadth of workers' earnings. Each of these limitations is discussed below.

Workers employed in informal arrangements or who are paid in cash may have employers who underreport their earnings to the state. As such, these workers may have mismeasured earnings volatility estimates. Workers who engaged in self-employment or work outside of Washington for part of a year during the time period would also have inaccurate reports of their total quarterly earnings in a period, which may have affected estimates of earnings volatility. Research has shown that exclusion of self-employment earnings diminishes the levels and trends in earnings volatility and so the volatility estimates presented may understate the true volatility experienced by workers (Dynan et al., 2012; Jensen & Shore, 2015). By contrast, workers employed in multiple states will have unobserved earnings, which could lead to an overestimation of their quarterly earnings volatility. These workers were most likely to live near the Washington/Oregon border and the Washington/Idaho border. To assess the degree to which workers in low-wage jobs employed in multiple states affect earnings volatility estimates, I re-estimated my core measures of earnings volatility, excluding workers in regions lie on the perimeter of the state, defined by Public Use Microdata Areas (PUMAs).[6]

These data do not include the reason why a worker is unobserved in the data. Thus, workers who have earnings in one quarter, t, and do not the next quarter, $t+1$, may truly have no earnings for the quarter ($wages_{t+1} = 0$), may have earnings from other sources not captured by the

[6] Specifically, the PUMAs I exclude are PUMA IDs 10400, 10503, 10504, 10600, 10703, 11000, 11101, 11103, and 11200 (US Census, 2018).

Unemployment Insurance Records data, ($wages_{t+1} > 0$), or may not be in the labor force at all, ($wages_{t+1} = NA$).[7] For quarters prior to the first observation of a worker and after the last observation of a worker, I allowed the data to remain missing. For the quarters in between workers' first and last observations, and in years in which a worker had at least one-quarter of earnings, I imputed zeros for the remaining quarters during which a worker was not matched to an employer.[8] The interpretation of the earnings volatility measures is thus limited to workers' engagement in UI-covered work during the periods a worker was observed to be employed. While this interpretation falls short of estimating workers' total economic welfare from earnings, understanding workers' employment patterns in and out of UI-covered work, and the corresponding earnings volatility caused by these changes, provides a lens into workers' relationship with formal work.

Finally, the temporality of the data does not allow for an assessment of month-to-month or week-to-week volatility. Qualitative and survey literature documenting workers earnings and economic volatility has shown that workers' schedules fluctuate week to week and temporary jobs can occur on a monthly basis (Lambert et al., 2014; Morduch & Schneider, 2017). In the UI program data, I will capture all jobs worked, including jobs that are short term, however I will capture them in the duration of a quarter, rather than the time period during which they precisely

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[7] These administrative data further cannot answer questions of worker or employer intent. It is unclear, for example if a worker separation is a fire or quit or if quarters out denote unemployment or workers departure from the labor force.

[8] This imputation does not affect, or bias earnings volatility estimates, due to the construction of the coefficient of variation, discussed in the subsequent section. However, it does affect the estimation of employment transitions for workers who work at least one, but fewer than four quarters in a year. For these workers, missing observations are imputed as a zero to indicate nonemployment. By imputing zeros in these quarters, I allow for the possibility of workers to have nonemployment-to-nonemployment transitions. Nonemployment-to-nonemployment transitions make up 10 percent of total transitions and a negligible contribution to total volatility.

occurred. The temporality of my data will most likely understate the variability experienced by workers who are employed in these variable arrangements.

Workers were selected into the sample in year, t, based on their highest wage earned across all four quarters, q. A workers' wage within a quarter was the ratio of their total earnings across all jobs to their total hours across all jobs:

$$wages\ for\ a\ worker\ in\ quarter\ q = \frac{total\ earnings\ across\ all\ jobs\ in\ quarter\ q}{total\ hours\ across\ all\ jobs\ in\ quarter\ q} \quad (1.1)$$

If workers' maximum quarterly wage within a year falls was in the bottom 33rd percentile of the distribution of wages in a year, t, they were considered to be low wage in year, t. To exclude workers who may only be marginally attached to UI-covered work, I further reduced the sample to exclude workers who had no quarterly earnings in four consecutive quarters within a year.[9] In line with prior literature, I also excluded workers in low-wage jobs who had wages that fell in the top two percentiles of the distribution in any year. This exclusion reduces the possibility that trends in earnings volatility are driven by outliers (Dahl et al., 2011).[10] Workers who were employed in multiple jobs within a quarter may be employed in multiple industries. For workers employed in multiple jobs within a quarter, I assigned the worker's quarterly industry to match a worker's "main job." A worker's main job was defined as the job for which the worker earned more than 50 percent of their quarterly earnings.

[9] This restriction excludes 12,042,820 million (12.96 percent) percent of potential worker-quarter observations that occur between the first and last observation of a worker within the ESD data.
[10] I further restrict the data to workers who have a constructed wage greater than $7.00 per hour in all quarters observed to avoid measurement error driving earnings volatility.

Methods and Measures

I used several techniques to relate workers' employment dynamics to their intra-year earnings volatility. First, I assessed intra-year volatility for workers who earned low-, medium-, and high-wages using their highest wage within the year to determine their wage tertile. I then longitudinally captured the magnitude, frequency, and pervasiveness of intra-year earnings volatility for workers in low-wage jobs to create a comprehensive picture of their trends during the 2006 to 2016 period. Second, I assessed the contribution of between-work volatility and within-work volatility to total earnings volatility for workers who earned low wages during the 2006 to 2016 period using a decomposition of variance. This assessment indicates whether workers' economic volatility was due to entry and exit from jobs or volatility within employment. Third, I assessed the impact that particular employment characteristics had on workers' earnings volatility within-work using regression analysis. The analysis showed the association between volatility in hours worked and wages on workers' earnings volatility, and further assessed whether industry moderates the relationship between workers' hours volatility and wage volatility on intra-year earnings volatility.

Intra-year Measures

The primary measure of workers' intra-year earnings volatility I used was the coefficient of variation, which is defined as the ratio the standard deviation of workers' quarterly earnings to their mean quarterly earnings, within a year, t

$$CV_t = 100 * \frac{\sum_{q=1}^{4}\left((Y_q - \bar{Y}_t)^2\right)}{(\bar{Y}_t)}. \tag{1.2}$$

The coefficient of variation (CV) has been used widely in intra-year earnings estimates (Bania & Leete, 2009; Gennetian et al., 2015; Hannagan & Morduch, 2015; H. D. Hill et al., 2013; Wolf et al., 2014). It captures quarterly earnings volatility that arises within a single year, while still allowing for comparisons across years. The CV estimates the magnitude of workers' intra-year earnings volatility over time in a standardized way, which allows for comparison across years (Gennetian et al., 2015; Hannagan & Morduch, 2015)). The coefficient of variation is symmetric for earnings across the four quarters within a year, and it can be defined when there are zero earnings in a quarter. A CV value of 0 indicates no change in a workers' quarterly earnings across a year, whereas a value of 200 indicates that there was a change of double or more in workers' earnings.[11] I calculated a worker's CV for every year they had at least one-quarter of earnings between 2006 and 2016 and reported the mean coefficient of variation across workers in each year.

I used two subsequent measures to capture the directionally and frequency of volatility. I estimated the directionality of earnings volatility using the arc percent change (APC). Like the coefficient of variation, the arc percent change assesses the ratio of differences in quarterly earnings across a period, relative to the average earnings over that period. As work transitions occur in quarter-to-quarter changes, I estimated workers' quarter-to-quarter arc percentage change, which can be written as:

$$arc\ percentage\ change = 100 * \frac{(Y_q - Y_{q-1})}{(Y_q + Y_{q-1})/2}. \tag{1.3}$$

The arc percentage change (APC) is often used to measure inter-year volatility and provides a straightforward interpretation of changes in quarterly earnings (Dahl et al., 2011; Dynan et al., 2012; Gottschalk & Moffitt, 2009; Hannagan & Morduch, 2015; B. Hardy & Ziliak, 2014; Shin & Solon, 2011). Like the coefficient of variation, the arc percent change is symmetric, and its absolute value is bounded at 200. An APC value of 0 indicates there was no change in workers' earnings volatility. Unlike the CV, the APC indicates directionality: a value of -200 indicates a

[11] The sample exclusion excludes workers with no quarterly earnings in four consecutive quarters within a year, which, if included, would lead a coefficient of variation estimate that is undefined.

worker went from having positive earnings in Y_{q-1} to zero earning in Y_q, a value of 200 indicates a worker transitioned from having zero earnings in Y_{q-1} to positive earnings in Y_q.

To estimate the frequency of large earnings swings, I calculated the fraction of workers who experienced a change in their quarterly earnings that is greater than +/- 25 percent of their previous quarters' earnings. Because workers could experience multiple large changes within a year, I also estimated the average number of large changes a worker faced within a year. [12] This latter estimate provides a sense of the frequency of large changes as well as the pervasiveness of changes workers experienced within a year. To understand the directionality of these changes, I further decomposed large changes into declines and increases of 25 percent. Changes greater than 25 percent are considered large enough to affect the well-being of households (Pamela Winston et al., n.d.).

Decomposition: Between-work Volatility

To examine the impact of between-work instability on intra-year earnings volatility of workers earning low wages, I decomposed workers' intra-year employment patterns into four mutually exclusive categories and assessed the contribution of each category to workers' total intra-year volatility. Workers were considered employed if they were observed in any quarter, t, between 2006 and 2016. In each of the four quarter-to-quarter changes within a year, there were four possible states between two quarters: constant employment ($P = 1,1$), constant nonemployment ($P = 0,0$), nonemployment-to-employment ($P = 0,1$), and employment to nonemployment ($P = 1,0$). This decomposition was designed to illustrate how much of workers' intra-year earnings variance is due to changes in earnings, conditional on UI employment, and how much is due to changes in earnings from each of the four employment status transitions. Following the

[12] As the data is quarterly, workers can experience up to four large changes within a year.

methodology of Ziliak, Bollinger, and Hardy (2011), I exploited the additive properties of the variance estimator and utilize the variance of workers' arc percent change in quarterly earnings within a year, which can be written as the sum of the expected conditional variance of the percent change of earnings and the variance of the conditional mean of the percent change of earnings:

$$V(q) = E\{V(q|P)\} + V(E\{q|P\}). \tag{1.4}$$

Each of these terms can be rewritten in terms of the probability of each employment state occurring, the variance of the arc percent change in each state, and the expectation of the arc percent change in each state. With four possible states of employment, P, the first term in Equation 1.4, $E\{V(q|P)\}$, the expected conditional variance of the percent change can be rewritten as the variance of each possible state multiplied by the probability of a worker being in each state:

$$E\{V(q|P)\} = V(q|P = 0,0) * \Pr(P = 0,0) + V(q|P = 0,1) * \Pr(P = 0,1)$$
$$+ V(q|P = 1,0) * Pr(P = 1,0) + V(q|P = 1,1) * Pr(P = 1,1). \tag{1.5}$$

Earnings volatility associated with a worker's transition from nonemployment to nonemployment is always 0, and thus the first term of Equation 1.5 cancels out. Similarly, the variance of a worker transitioning from employment-to-nonemployment and nonemployment-to-employment is also zero because arc percent change for these transitions is constant at 200 and -200, respectively. The remaining term is the volatility of constant employment, weighted by the probability of being employed in two quarters. I can rewrite the first term of Equation 1.4 to be

$$E\{V(q|P)\} = V(q|P = 1,1) * Pr(P = 1,1). \tag{1.6}$$

The second term of Equation 1.4, $V(E\{q|P\})$, can be rewritten as the sum of the conditional mean of the arc percent change for each state weighted by the probability of being in each of the four employment states:

$$V(E\{q|P\}) = (E\{q|P = 0,0\} - E\{q\})^2 * \Pr(P = 0,0) + (E\{q|P = 0,1\}$$

$$-E\{q\})^2 * \Pr(P = 0,1) + (E\{q|P = 1,0\} - E\{q\})^2 * \Pr(P = 1,0))$$
$$+ (E\{q|P = 1,1\} - E\{q\})^2 * Pr(P = 1,1). \quad (1.7)$$

Note that the first term in Equation 1.7 can be re-written as the squared unconditional mean of two-period nonemployment since the conditional mean of the arc percent change of constant nonemployment is zero, $E\{q|P = 0,0\} = 0$, as shown in Equation 1.8:

$$V(E\{q|P\}) = (E\{q\})^2 * \Pr(P = 0,0) + (E\{q|P = 0,1\} - E\{q\})^2 * \Pr(P = 0,1))$$
$$+ (E\{q|P = 1,0\} - E\{q\})^2 * \Pr(P = 1,0))$$
$$+ (E\{q|P = 1,1\} - E\{q\})^2 * Pr(P = 1,1). \quad (1.8)$$

Putting these pieces together, the variance of the arc percent change is comprised of the sum of the volatility conditional on being employed in two consecutive quarters and sum the volatility driven by compositional changed in the workforce.

$$V(q) = V(q|P = 1,1) * \Pr(P = 1,1) + (-E\{q\})^2 * \Pr(P = 0,0) + (E\{q|P = 0,1\}$$
$$-E\{q\})^2 * \Pr(P = 0,1) + (E\{q|P = 1,0\} - E\{q\})^2 * \Pr(P = 1,0))$$
$$+ (E\{q|P = 1,1\} - E\{q\})^2 * Pr(P = 1,1). \quad (1.9)$$

Decomposition: Within-work Volatility

For workers in low-wage jobs who maintain continuous employment in UI work, I descriptively estimated the effects of their intra-year hours volatility and intra-year wage volatility on their intra-year earnings volatility. The basic model for the ordinary least squares regression for this can be written as:

$$y_{it} = \alpha + \beta_1 H_{it} + \beta_2 W_{it} + \gamma_t + \tau_i + \varepsilon_{it} \quad (1.10)$$

where $y_{it}$is the coefficient of variation of earnings for a worker, i, in time, t, H_{it} is the coefficient of variation of workers' hours in each year, and W_{it} is the coefficient of variation of workers' wage. I included year fixed effects, γ_t to control for changes associated with specific years, such as the Great Recession, and person fixed effects τ_i to control for any changes that are specific to each worker.

I was primarily interested in the degree to which hours and wage volatility cause earnings volatility for workers in low-wage jobs. Evidence from linked administrative data has shown that mobility rates have declined across all workers and remained stagnant for workers with less than a college degree.[13] Therefore, I expected that earnings volatility for workers in low-wage jobs is driven by volatility in their hours worked, rather than volatility in their wage. I further assessed whether the relationship between earnings volatility and workers' hours volatility or wage volatility was different for workers in different industry classifications by interacting the CV of workers' intra-year hours worked and the CV of workers' intra-year wage with industry identifiers, I_{it}. This interaction illustrates the degree to which a workers' industry moderates the relationship between the wage or hours volatility and earnings volatility. The industry classification analysis can be written as:

$$y_{it} = \alpha + \beta_1 H_{it} + \beta_2 W_{it} + \beta_3 H_{it} \times I_{it} + \beta_4 W_{it} \times I_{it} + \beta_5 h_{it} + \beta_6 w_{it} + \beta_7 I_{it} + \gamma_t + \tau_i + \varepsilon_{it}. \tag{1.11}$$

RESULTS

Summary Statistics

Figure 1.1 shows the average CV for workers earning low, middle, and high wages, defined by their highest wage earned within a year.[14] Workers earning low wages exhibit substantially higher

[13] Research has shown that the proportion of workers in the bottom 40 percent who move to the top 20 percent has declined from 6-5% and has not changed among workers with less than a college degree declined between1996 to 2008 (Carr & Wiemers, 2016).

[14] Workers in the low-wage group have a maximum wage in a year that falls in the lowest 33rd percentile of workers' wage within that year. Workers in the middle-wage group have a maximum wage in a year that falls between the 33rd and 66th percentile of workers' wage within that year.

levels of intra-year earnings volatility, relative to their counterparts earning middle- and high-wages. **Table 1.1** provides employment summary statistics for workers earning low, middle, and high wages. The years displayed, 2006, 2009, 2012, and 2016, include a year before the Great Recession, a year during the Great Recession, a year mid- recovery, and a year with strong labor market growth, respectively. Table 1.1 shows that employment outcomes for workers in low-wage jobs trend closely to the health of the economy than middle- and high-wage jobs. Before the Great Recession, the quarterly average employment rate for workers earning low wages was 82.8 percent. Their employment rate dipped two percentage points between 2006 and 2009. By contrast, the employment rate for workers earning middle and high wages dipped by 1.4 percentage points and one percentage point, respectively, during the same period. Between 2006 and 2009, workers in low-wage jobs experienced an earning decrease of $20 per quarter. However, workers in middle- and high-wage jobs experienced earnings *increases* during the same period. The decline experienced by workers in low-wage jobs persisted through 2012, before recovering back to pre-recession levels if 2016. The table provides descriptive evidence that workers earning low wages have employment patterns that are highly correlated to movements in the business cycle, relative to middle- and high-wage workers. Because workers earning low wages have the highest levels of intra-year earnings volatility and the lowest earnings levels to buffer against negative shocks, I focus on these workers for the rest of the paper.

Workers in the high-wage group have a maximum wage in a year that falls in the highest 33rd percentile of workers' wage within that year.

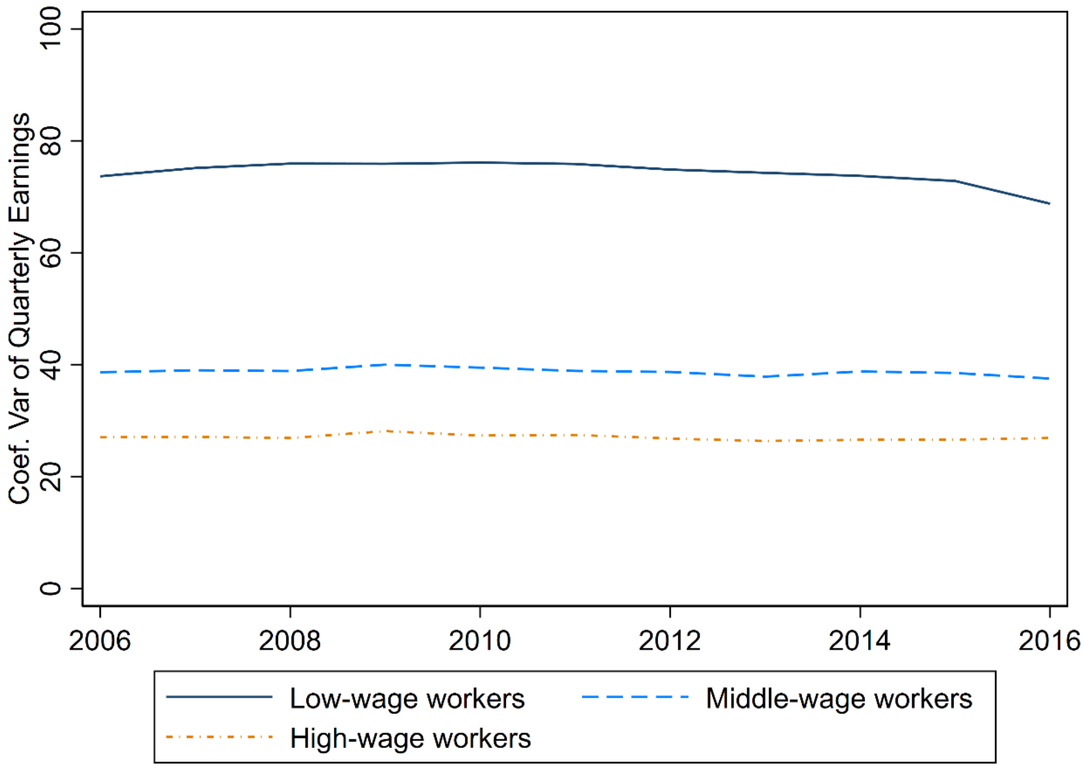

Figure 1.1. Coefficient of variation in workers in low-wage jobs' intra-year earnings for low-, middle-, and high-wage workers, 2006-2016.

Source: Authors' analysis of Washington state UI program records.
Note: Workers in the low-wage group have a maximum wage in a year that falls in the lowest 33rd percentile of workers' wage within that year. Workers in the middle-wage group have a maximum wage in a year that falls between the 33rd and 66th percentile of workers' wage within that year. Workers in the high-wage group have a maximum wage in a year that falls in the highest 33rd percentile of workers' wage within that year.

Table 1.1. Employment Characteristics of Workers, By Wage Group, 2006-2016

	2006	2009	2012	2016
Workers earning low wages				
Share employed	82.8%	80.7%	81.2%	84.8%
Quarterly Earnings	$2,855	$2,835	$2,822	$3,222
Quarterly Wage	$10.55	$10.82	$10.49	$11.40
Quarterly Hours	264	256	263	275
Workers earning middle wages				
Share employed	93.6%	92.2%	92.8%	93.8%
Quarterly Earnings	$7,128	$7,227	$7,155	$7,818
Quarterly Wage	$17.10	$17.86	$17.27	$18.57
Quarterly Hours	412	400	410	417
Workers earning high wages				
Share employed	96.6%	95.6%	96.3%	96.6%
Quarterly Earnings	$14,182	$14,747	$14,917	$16,801
Quarterly Wage	$31.71	$33.68	$33.29	$37.13
Quarterly Hours	449	438	447	452

Source: Author's analysis of Washington state UI program records.
Notes: Workers in the low-wage group have a maximum wage in a year that falls in the lowest 33rd percentile of workers' wage within that year. Workers in the middle-wage group have a maximum wage in a year that falls between the 33rd and 66th percentile of workers' wage within that year. Workers in the high-wage group have a maximum wage in a year that falls in the highest 33rd percentile of workers' wage within that year.

Trends in Earnings Volatility for Workers in Low-wage Jobs

Figure 1.2 shows the cross-sectional distribution of the coefficient of variation of workers' intra-year earnings for selected percentiles (the 10th, 25th, 50th, 75th, and 90th percentile) between 2006 and 2016. The figure shows that the median CV remained relatively constant throughout the period at 57.3, a higher level than found in previous survey research.[15] The CV for the 10th, 25th, 50th, and

[15] To put this result in context, researchers documenting earnings volatility for a group of 235 low- and middle-income families found an average intra-year coefficient of variation of 34 and researchers using the Survey of Income and Program participation found coefficients of variation for average earnings ranging from 44 to 48 between the 1984 and 2008 panels (Hannagan & Morduch, 2015; Morris et al., 2015).

75th percentiles workers' intra-year volatility within each year did not change throughout the period. The CV for the 90th percentile of earnings volatility, by contrast, trends in a pattern that coincides with the business cycle. The CV shifted upward in 2009 and remained at an elevated level until 2012, before shifting back down to a CV of 160.7 in 2016, an 11.9 percentage point drop relative to their 2006 pre-recession CV. Figure 1.2 shows that the change in the dispersion of low-wage earnings volatility within a year was driven by cyclicality in the tails. The dispersion grew during economic downturns and narrowed during economic booms.

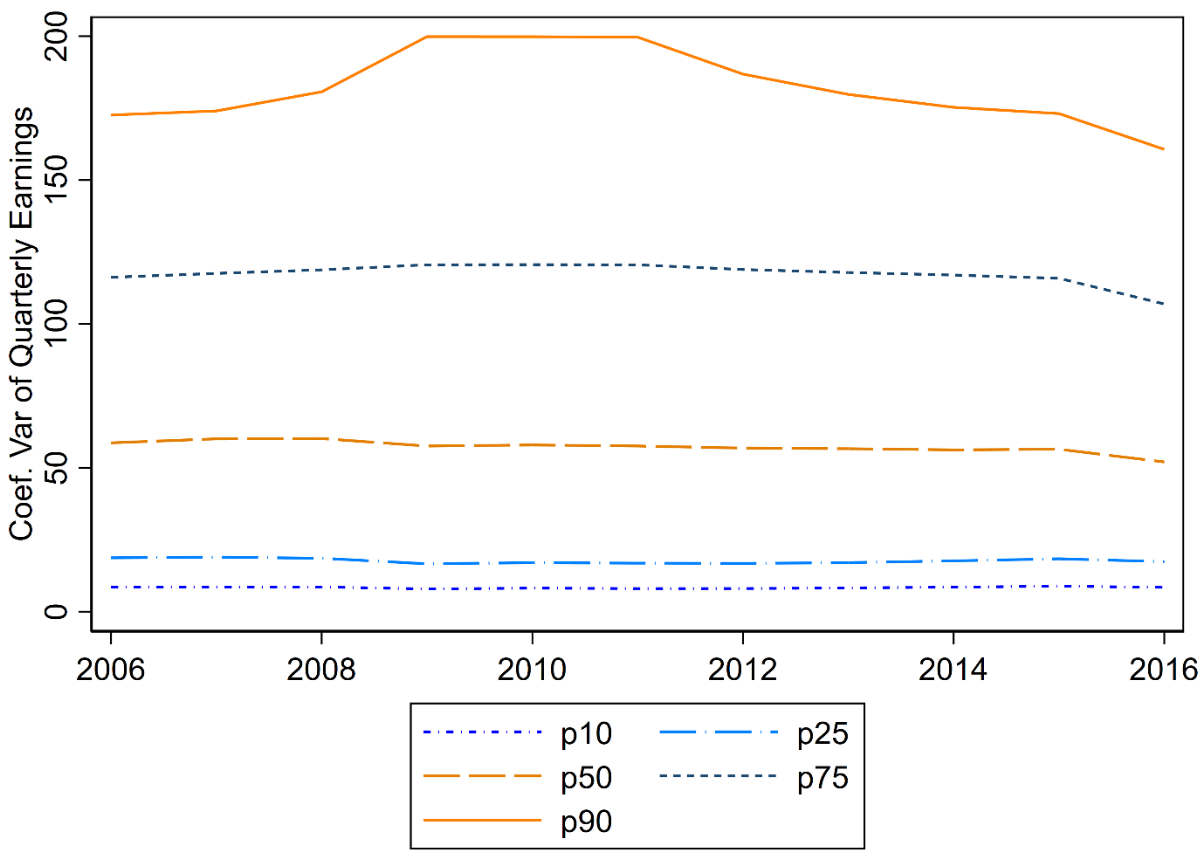

Figure 1.2. The distribution of the coefficient of variation in workers in low-wage jobs' intra-year earnings, 2006-2016.

Source: Authors' analysis of Washington state UI program records.

As an alternative measure to the coefficient of variation, **Figure 1.3** displays the share of workers in low-wage jobs who experienced a change in their quarterly earnings of 25 percent or more in each year over time, referred to as a "large change" in earnings. Across the period, an average of 84 percent of workers in low-wage jobs experienced a change in their earnings greater than 25 percent within a year.[16] With average quarterly earnings of $2,933 over the period, a 25 percent change translates into an average quarterly change in earnings greater than +/- $733. Figure 1.3 also shows the proportion of workers who only experienced large increases within a year, workers who only experienced decreases within a year, and workers who experienced both large increases and decreases in their quarterly earnings within a year. Across the period, an average of 43 percent of workers in low-wage jobs experienced a large increase(s) and a large decrease(s) in their earnings. A much smaller fraction of workers, 21 percent, experienced large unidirectional changes in their earnings. During the Great Recession, the proportion of workers that experienced unidirectional changes were near equal in size. However, during the recovery of the Great Recession, a period marked by labor market health, the share of workers that experienced increases outpaced the share of workers experiencing decreases, and the gap between the two proportions grew by 13.6 percentage points by 2016.

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[16] This share is substantially higher than comparable intra-year estimates using survey data. Evidence from the Survey of Income and Program Participation found that only 49.0 percent of the bottom income-quintile experienced a decrease greater than 25 percent across the 1980-2008 panels.

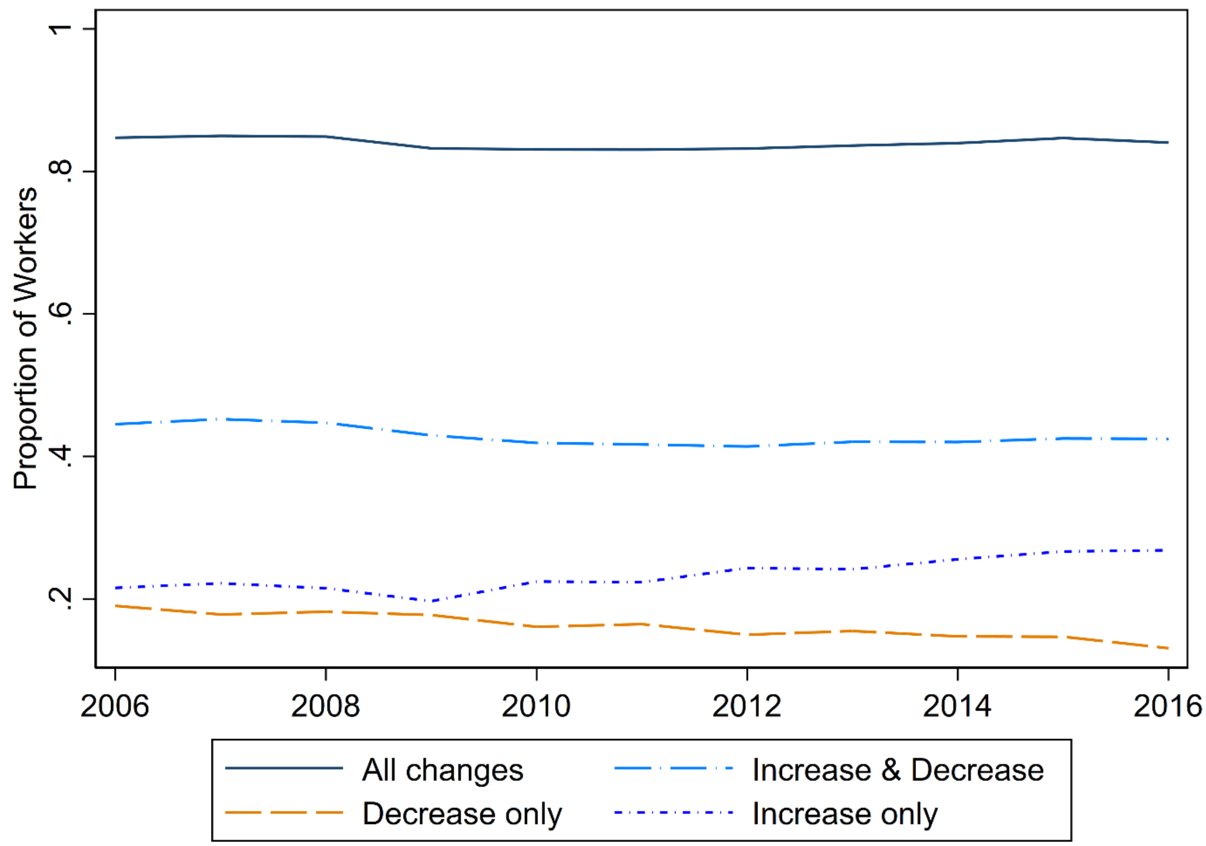

Figure 1.3. Proportion of workers that experience earnings changes greater than 25 percent within a year, 2006-2016.

Source: Author's analysis of Washington state UI program records.
Notes: Workers who experience multiple changes within a year are counted once in the proportion of all changes. The share of workers experiencing increases and decreases does not sum to the total share of changes because workers can experience both an increase and a decrease within a year.

To assess the magnitude of large earnings changes, **Table 1.2** shows the average annual within-person arc percent change (APC) associated with each type of large change type. Workers who experienced both increases and declines had fluctuations in earnings that roughly evened out within each year. The largest arc percent change for this group was 6.1 percent, which corresponds to a net change of $179 dollars within a year. The APC for this group was negative between 2006 and 2009, indicating that workers experienced net decreases during the Great Recession. However, the trend is reversed between 2010 and 2016. Among workers who experienced only unidirectional changes within a year, the average APC for workers who experienced increases was 116.9, 4 percentage-points smaller than for workers who experienced large decreases (-119.7). Using average quarterly earnings of $2,933, the APC estimates for workers who experienced only increases or decreases correspond to quarterly changes of $3,428 and $-3,511, respectively.

Table 1.2. Average Number of Intra-year Earnings Changes and Their Associated Arc Percent Change for Earnings Change Larger Than 25 Percent, 2006, 2009, 2011, 2016

	Frequency of large changes			Arc Percent Change		
	Total changes	Decreases	Increases	Total changes	Decreases	Increases
2006	1.77	0.87	0.90	111	-124	-1.98
2009	1.77	0.88	0.89	118	-121	-1.69
2012	1.73	0.78	0.96	118	-120	2.68
2016	1.73	0.73	1.00	119	-111	6.07

Source: Author's analysis of Washington state UI program records.
Note: The frequency of large changes and arc percent change are for intra-year quarterly changes in workers' earnings > 25% from the previous quarter.

While the share of workers experiencing large changes was unaffected by the business cycle, the decomposition of the types of changes experienced by workers reveal a cyclicality in the trend. An average of 43 percent of workers in low-wage jobs had both a large earnings dip and large earnings increases each year. This share of workers did not change during the downturn or recovery, and the APC each year for this group was small, suggesting the increases and decreases evened themselves out. With respect to unidirectional changes, Figure 1.3 shows that the proportion of workers in low-wage jobs who experienced a large positive or negative change *is* associated with the business cycle: as the economy recovers from the Great Recession, the proportion of workers experiencing large increases grew over time. However, the APC for workers who experienced large earnings increases were smaller in magnitude than the earnings lost for workers who experienced large declines.

Table 1.2 also shows the mean number of intra-year changes experienced between 2006 and 2016 to get a sense of the relative frequency of large changes experienced in a year. The average number of large changes for all workers earning low wages across the period was 1.75 changes. In decomposing these changes into positive and negative changes, workers experienced increases more frequently than decreases, and the difference between these frequencies grows from 2009 to 2016.

To summarize to this point: First, workers in low-wage jobs experienced intra-year volatility at very high rates during the 2006 to 2016 period. The majority of workers earning low wages, 84.0 percent, experienced a large change in their earnings within a year. Nearly half the workers who experienced large changes experienced both a large increase and a large decrease in the same year. Second, these large earnings changes were cyclical. A greater proportion of workers experienced large increases, and the average number of large increases grew during 2012- 2016, a period marked by economic growth. However, the magnitude of earnings volatility for workers who experienced increases was smaller than the magnitude of earnings volatility changes associated with earnings declines. While some of this volatility may be planned for, many workers appear to experience earnings fluctuations without associated gains. In the subsequent section, I show the decomposition of intra-year earnings volatility from volatility due to entering and exiting UI-covered work and volatility that occurred within consistent employment.

Contribution of Between-work Volatility to Intra-year Earnings Volatility

To assess the extent to which between-work instability affected workers' intra-year earnings volatility, I first document the proportion of workers in low-wage jobs in each employment status between 2006 and 2016, shown in **Figure 1.4**. Workers in low-wage jobs can be in one of four mutually exclusive categories of employment transition, P, in a quarter: consistently employed across two quarters ($P = 1{,}1$), consistently nonemployed across two quarters ($P = 0{,}0$), transitioning into employment ($P = 0{,}1$), or transitioning away from employment ($P = 1{,}0$). The figure shows that the majority of workers in low-wage jobs, 74.6 percent, were consistently employed across two quarters. The share of workers that experienced consistent nonemployment was 9.3 percent across the period, with a maximum proportion of 10.4 percent in 2009. The share of workers in low-wage jobs that experienced transitions into or out of employment remained constant throughout the period. On average, 7.9 percent of workers experienced transitions to employment, and 7.7 percent of workers experienced transitions away from employment.

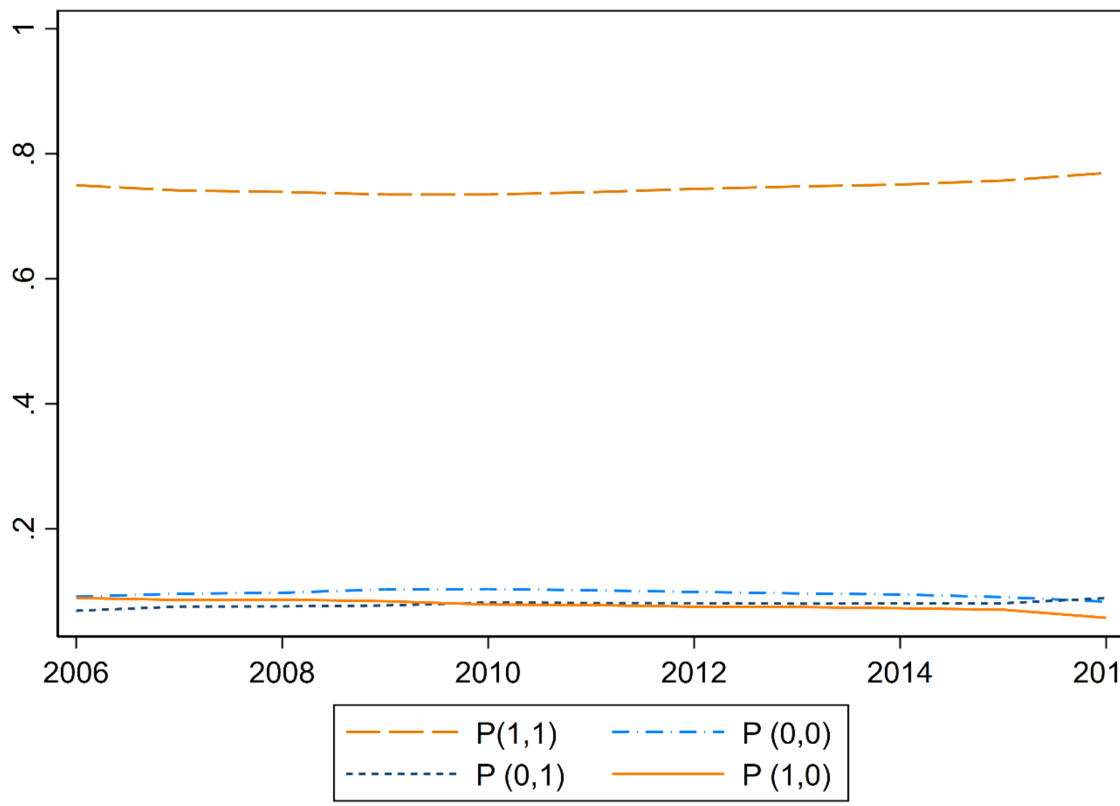

Figure 1.4. Proportion of workers in each employment transition category, 2006-2016.

Source: Authors' analysis of Washington state UI program records.
Notes: Workers in low-wage jobs can be in one of four mutually exclusive categories of employment transition, P, in a quarter: consistently employed across two quarters ($P = 1{,}1$), consistently nonemployed across two quarters ($P = 0{,}0$), transitioning into employment ($P = 0{,}1$), or transitioning away from employment ($P = 1{,}0$).

Results from the decomposition of the variance of earnings volatility for workers in low-wage jobs between 2006 and 2016 are shown in **Figures 1.5a** and **1.5b**. These figures display the trends in the five components of total earnings variance among workers earning low wages. Figure 1.5a shows the conditional variance of consistently employed workers and the variance of the conditional means for the workers transitioning towards and away from low-wage employment between 2006 and 2016. The figure reveals that the conditional variance of consistently employed workers $(P = 1{,}1)$ generates the same amount of earnings volatility as the volatility attributed to workers who transition into formal work $(P = 0{,}1)$ and the volatility attributed to workers who transition out of UI-covered work $(P = 1{,}0)$. The conditional variance of consistently employed workers $(P = 1{,}1)$, decreased during the period coinciding with the Great Recession and rebounded slightly in the recovery. Figure 1.5b shows the final two components of total earnings variance for workers in low-wage jobs: the variance of conditional means from continuous employment and continuous nonemployment. The total variance of these two components were small in magnitude, and the contribution of these transitions to total intra-year earnings volatility was negligible.

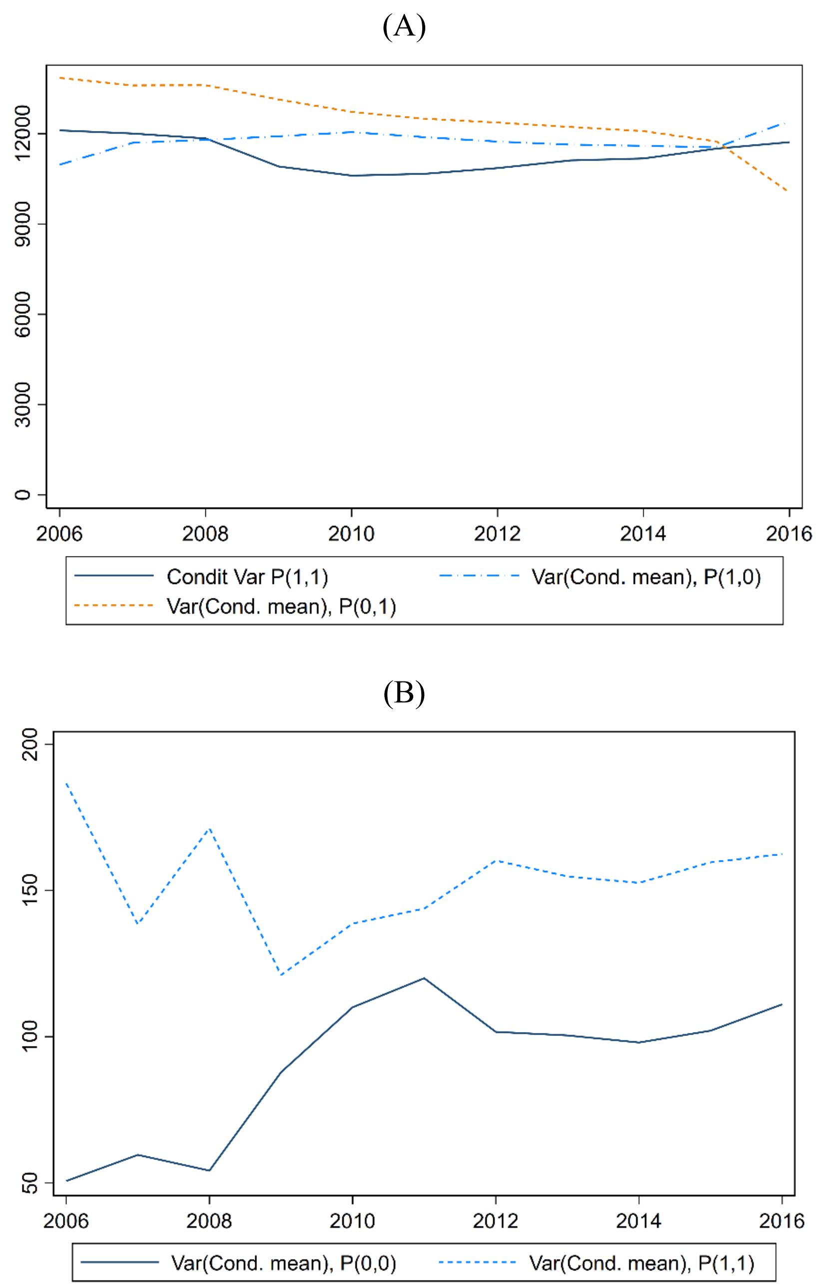

Figure 1.5. Variance decomposition of workers in low-wage jobs earnings, 2006-2016.

Notes: Figure 1.5A shows the variance attributed to the conditional variance of being consistently employed across two quarters ($P = 1{,}1$), the variance of the conditional mean of transitioning into employment ($P = 0{,}1$), and the variance of the conditional mean of transitioning away from employment ($P = 1{,}0$). Figure 1.5B shows the variance of the conditional mean of constant nonemployment across two quarters ($P = 0{,}0$), and the variance of the conditional mean of constant employment across two quarters ($P = 1{,}1$).

Table 1.3 presents the share of each of the five variance components that comprise the total variance of earnings volatility for workers in low-wage jobs between 2006 and 2016. The share of variance conditional on consistent low-wage employment across two quarters ranged from 29.9 to 34.2 percent and remained relatively consistent throughout the period. The share of variance attributed to entering and exiting from employment ranged from 65.8 to 70.1 percent. The shares of variance attributed to entering and exiting UI-covered employment remained similar during the Recession years but diverged during the recovery. The share of variance attributed to entering UI-covered employment increased over the period from 29.7 to 36.2 percent while the share exiting UI-covered employment *decreased* over the period from 37.5 to 29.3 percent. These findings corroborate Figure 1.3, which shows that a greater proportion of workers experienced large increases compared to large decreases within a year, and the gap between the two proportions has grown over the 2006-2016 period.

Table 1.3. Percent Contribution of Each Employment Transition Component to Total Quarterly Earnings Variance for All Workers and Workers in Various Earnings Groups

	Conditional variance, $(P = 1{,}1)$	Variance of conditional mean $(P = 0{,}1)$	Variance of conditional mean $(P = 1{,}0)$	Variance of conditional mean $(P = 0{,}0)$	Variance of conditional mean $(P = 1{,}1)$
2006	32.7	29.7	37.5	0.1	0.5
2007	32.1	31.3	36.4	0.2	0.4
2008	31.8	31.6	36.5	0.1	0.5
2009	30.3	33.1	36.4	0.2	0.3
2010	29.9	34.0	35.9	0.3	0.4
2011	30.3	33.8	35.5	0.3	0.4
2012	31.0	33.5	35.3	0.3	0.5
2013	31.7	33.2	34.9	0.3	0.4
2014	32.0	33.2	34.6	0.3	0.4
2015	33.0	33.1	33.7	0.3	0.5
2016	34.2	36.2	29.3	0.3	0.5

Source: Author's analysis of Washington state UI program records.
Notes: The table shows the variance attributed to the conditional variance $(P = 1{,}1)$, the variance of the conditional mean of transitioning into employment $(P = 0{,}1)$, the variance of the conditional mean of transitioning away from employment $(P = 1{,}0)$, the variance of the conditional mean of constant nonemployment across two quarters $(P = 0{,}0)$, and the variance of the conditional mean of constant employment across two quarters $(P = 1{,}1)$. Rows sum to 100 percent.

Although the probability of a transitioning into and out of UI-covered employment is small (15.6 percent), the contribution of these transitions to volatility is not. Nearly two-thirds of intra-year earnings volatility for workers in low-wage jobs are attributable to movements in and out of the labor market. The contribution of the conditional variance of consistently employed workers $(P = 1{,}1)$ can be interpreted as the contribution of within-work volatility to overall earnings volatility. The contribution of the variance attributed to employment transitions into formal work $(P = 0{,}1)$ and the variance attributed to employment transitions out of formal work $P(1{,}0)$ can be interpreted as the contribution of between-work volatility to workers' earnings volatility. In the next section, I will assess the extent to which employment characteristics, such as workers' hours or wage volatility, affected continuous workers' earnings volatility.

Contribution of Within-work Employer Characteristics to Intra-year Volatility

To explore the role that employment characteristics had on workers' within-work volatility during the 2006 to 2016 period, I restrict my analysis to workers who were consistently employed, defined as having earnings across two quarters or more within a year. **Figure 1.6** shows the average CV of intra-year hours worked and wages as well as the average CV of intra-year earnings for workers in low-wage jobs between 2006 and 2016. For workers with low earnings, volatility in hours worked was nearly as large as earnings volatility across the entire period. The average CV for hours worked volatility was 49.2, and the average CV for workers in low-wage jobs' earnings volatility was 50.1. By contrast, intra-year wage volatility for workers in low-wage jobs was much lower. The CV averaged 22.3 percent across the whole period. Figure 1.6 provides evidence that workers' hours may be the primary driver of within-work earnings volatility for workers in low-wage jobs.

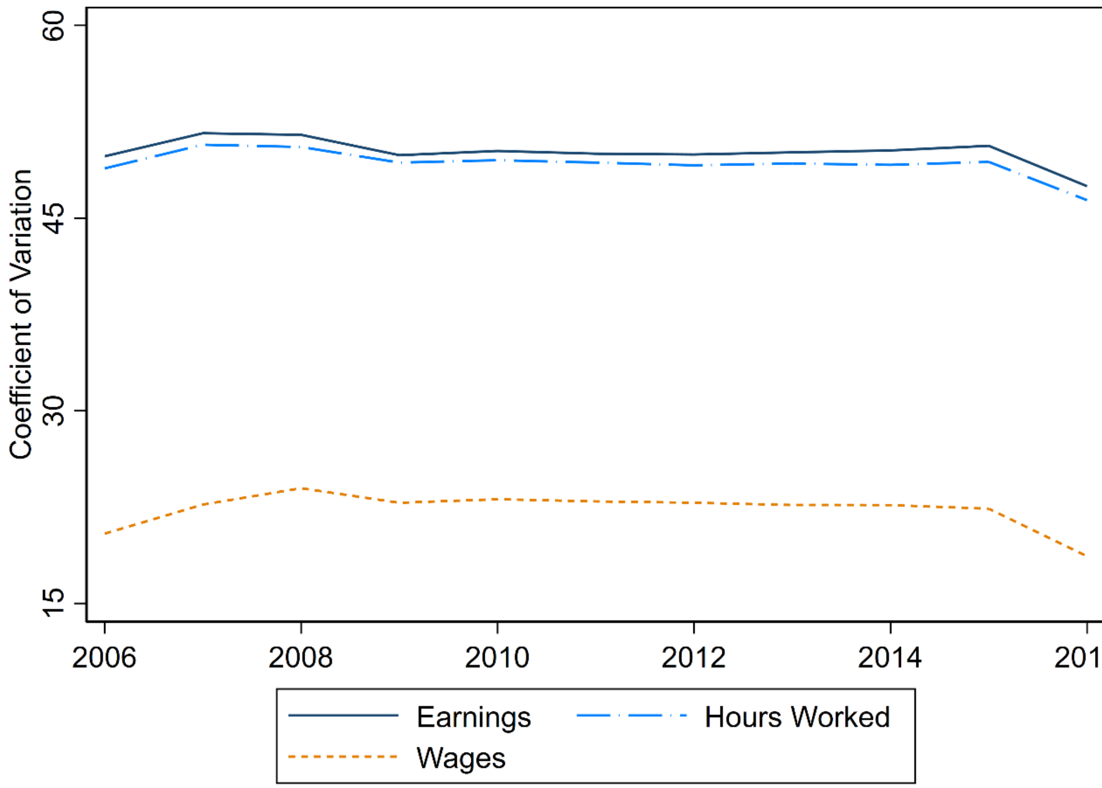

Figure 1.6. Coefficient of variation of intra-year earnings, hours and wages for workers who are consistently employed across two quarters, 2006-2016.

Source: Author's analysis of Washington state UI program records.

Previous research points to food, restaurant and retail industries as sectors with demonstrable irregularity and instability in work schedules (Henly & Lambert, 2014; Schneider & Harknett, 2019). **Figure 1.7** shows intra-year hours volatility for workers in low-wage jobs who were consistently employed, by industry. **Table 1.4** shows the proportion of workers who were consistently employed in each industry between 2006 and 2016. Figure 1.7 reveals that there is heterogeneity in intra-year hours volatility across industries. Half of the industries, including education and health, wholesale, transportation and warehousing, manufacturing, and retail trade, all have lower levels of volatility relative to the average hours-worked volatility (shown in Figure 1.6). By contrast, the mining, utilities, and construction industries, which make up only two percent of total low-wage jobs, have the highest rates of earnings volatility: the CV for this group averaged at 66.0. The average CV of intra-year earnings for the arts, entertainment, food, and accommodation industry, by contrast was 50.1, just slightly higher than the average for the entire group of workers in low-wage jobs. The differences in volatility in hours worked by industry suggests that the industry of a worker matters in assessing how earnings volatility occurs within a job.

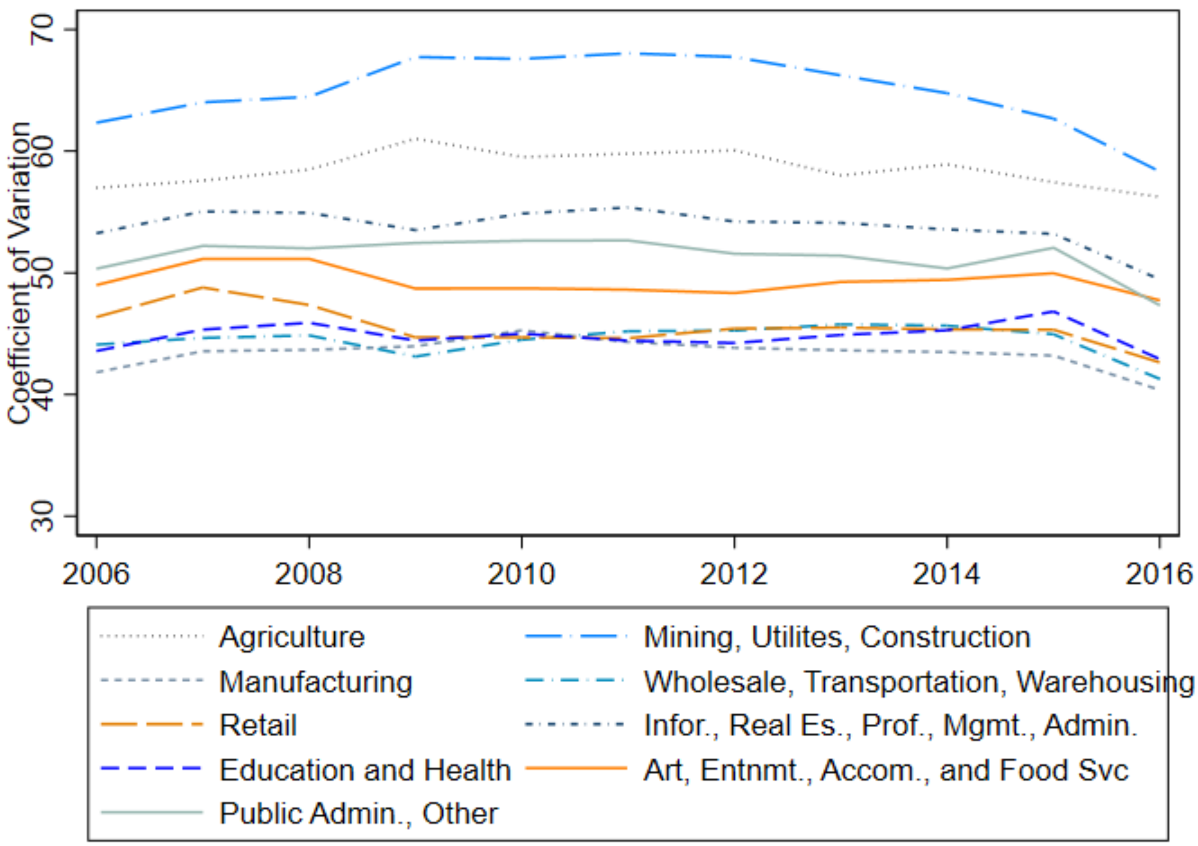

Figure 1.7. Coefficient of variation of intra-year hours for workers who are consistently employed across at least two quarters within a year, by industry, 2006-2016.

Source: Author's analysis of Washington state UI program records.

Table 1.4. Percentage of Workers in Low-wage Jobs in Major Industries, 2006-2016

	2006	2009	2012	2016
Agriculture, Forestry, Fishing and Hunting	8.9%	10.6%	8.7%	8.0%
Mining, Utilities, Construction	2.6%	1.7%	1.6%	2.1%
Manufacturing	6.9%	5.7%	6.0%	5.4%
Wholesale, Transportation, Warehousing	4.6%	4.4%	4.6%	4.6%
Retail Trade	18.4%	19.0%	19.9%	21.5%
Information, Finance, Real Estate, Prof., Mgmt. and Admin services	15.5%	13.5%	13.9%	13.5%
Education and Health	13.3%	14.1%	13.9%	13.5%
Arts, Entertainment, Recreation, Accommodation and Food Service	24.4%	25.4%	25.8%	25.6%
Public Admin and Other services	5.4%	5.6%	5.7%	5.7%

Source: Author's analysis of Washington state UI program records.
Note: Proportions are for workers in "continuous work", defined as having at least two consecutive observations within a year.

To assess the degree to which hours volatility and wage volatility contribute to overall earnings volatility, **Table 1.5** shows the results of regression analysis of earnings volatility on wage and hours volatility among workers in low-wage jobs who are consistently employed. Column 1 shows the results for a base regression of workers' earnings volatility on their intra-year hours volatility and wage volatility. Column 1 shows that a one-percentage-point increase in quarterly hours volatility is linked to a 0.99-percentage-point increase in quarterly earnings volatility. Column 1 also shows that a 1-percentage-point increase in quarterly wage volatility is linked to a 0.006-percentage-point-increase in earnings volatility. Holding all else constant, earnings volatility was largely driven by volatility in hours worked in low-wage jobs. Column 2 of Table 1.5 controls for workers' average quarterly wage and hours levels and the industry of the worker's main job.[17] The inclusion of levels and industry controls does not weaken the relationship between hours-volatility on earnings-volatility. Column 3 shows the results from the interaction

[17] Pairwise correlations for the quarterly levels and quarterly volatility of hours and wages were -0.47 and -0.47, respectively.

of industry classification with workers' hours and wage volatility. The reference group is the arts, entertainment, food, and accommodation industry (NAICS code: 7). The majority of interaction terms for hours volatility and industry are negative in directionality. Relative to the reference group, a one-percentage-point increase in intra-year hours volatility had a -0.007 to 0.001 percentage-point change in workers' intra-year earnings volatility, depending on the industry. The interaction coefficients show that relative to the arts, entertainment, food, and accommodation industry, nearly every other industry altered the magnitude and direction of the relationship between hours and earnings volatility towards lower earnings volatility. Intra-year wage volatility for workers of industries had mixed effects. An increase in the CV of wages led to a change in the CV of earnings ranging from -0.002- 0.004 percentage points, relative to workers in arts, entertainment, food, and accommodation industries. Overall, a workers' industry appears to moderate the relationship between hours volatility and earnings volatility and, for some industries, moderates the relationship between wage volatility and earnings volatility among workers in low-wage jobs.

Table 1.5. Regression Analysis of Intra-year Earnings Volatility for Workers Who Are Consistently Employed Across Two Quarters By Firm Group

	(1)	(2)	(3)
Quarterly hours volatility	0.990***	0.991***	0.992***
	(0.000)	(0.000)	(0.000)
Quarterly wage volatility	0.006***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)
Quarterly hours worked		0.004***	0.004***
		(0.000)	(0.000)
Quarterly wages		0.001	0.002
		(0.001)	(0.001)
Agriculture, Forestry, Fishing and Hunting		0.136***	0.509***
		(0.012)	(0.021)
Mining, Utilities, Construction		0.282***	0.547***
		(0.015)	(0.028)
Manufacturing		0.358***	0.549***
		(0.011)	(0.017)
Wholesale, Transportation, Warehousing		0.286***	0.435***
		(0.011)	(0.018)
Retail Trade		0.100***	0.231***
		(0.007)	(0.011)
Information, Finance, Real Estate, Prof., Mgmt. and Admin services		0.238***	0.326***
		(0.007)	(0.012)
Education and Health		0.041***	-0.009
		(0.008)	(0.013)
Public Admin and Other services		0.071***	0.082***
		(0.010)	(0.017)
Hours vol.×Agriculture, Forestry, Fishing and Hunting			-0.007***
			(0.000)
Hours vol.×Mining, Utilities, Construction			-0.003***
			(0.000)
Hours vol.×Manufacturing			-0.003***
			(0.000)
Hours vol.×Wholesale, Transportation, Warehousing			-0.002***
			(0.000)
Hours vol.×Retail Trade			-0.003***
			(0.000)
Hours vol.×Information, Finance, Real Estate, Prof., Mgmt. and Admin services			-0.001***
			(0.000)
Hours vol.×Education and Health			0.001***
			(0.000)
Hours vol.×Public Admin and Other services			0.001***
			(0.000)

(*continued on to next page)*

Table 1.5. *Continued*

	(1)	(2)	(3)
Wage vol.×Agriculture, Forestry, Fishing and Hunting			0.004***
			(0.000)
Wage vol.×Mining, Utilities, Construction			-0.001***
			(0.000)
Wage vol.×Manufacturing			-0.000
			(0.000)
Wage vol.×Wholesale, Transportation, Warehousing			0.000
			(0.000)
Wage vol.×Retail Trade			0.001***
			(0.000)
Wage vol.×Information, Finance, Real Estate, Prof., Mgmt. and Admin services			-0.002***
			(0.000)
Wage vol.×Education and Health			-0.001***
			(0.000)
Wage vol.×Public Admin and Other services			-0.002***
			(0.000)
Person and year FE	Y	Y	Y
Observations	20,356,895	20,356,895	20,356,895
Number of Persons	2,138,767	2,138,767	2,138,767
R-squared	0.993	0.993	0.993

Source: Author's analysis of Washington state UI program records.
Notes: Standard errors are clustered at the person level. ***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively

Sensitivity Analysis

To assess whether volatility patterns are artificially inflated due to workers near the border of Washington working in the surrounding states, **Figure 1.8** shows a comparison of the intra-year coefficient of variation of earnings for the main analytical sample and a sample in which workers employed in low-wage jobs in PUMAs on Washington’s state borders are excluded. While it's impossible to identify workers' place of residence from the data, workers who are employed near the state border may be more likely to work across the state border in Idaho or Oregon. Washington's UI program would not capture this out-of-state employment, and therefore may lead to mismeasurement of intra-year earnings volatility for these workers. Figure 1.8 shows that the exclusion does not change the intra-year coefficient of variation. The trends in intra-year earnings volatility are robust to workers who have the greatest potential of being employed in two states.

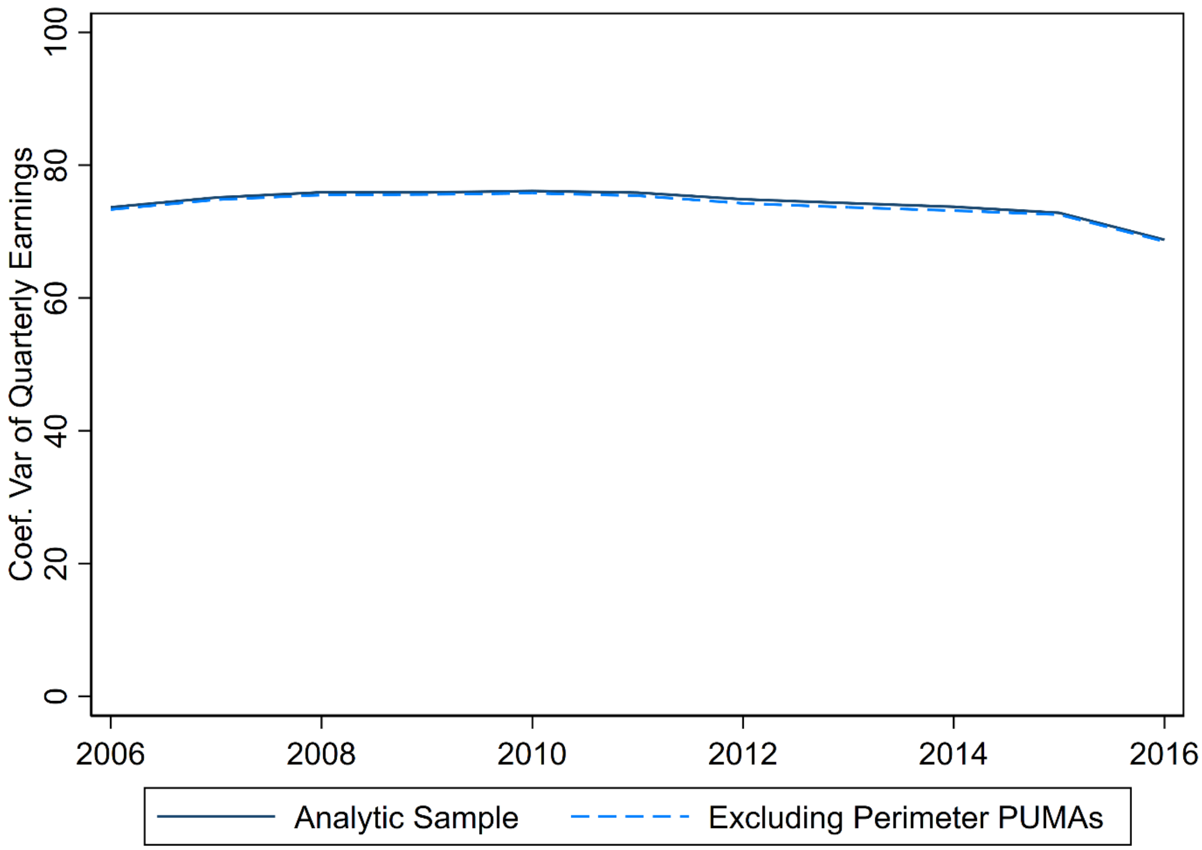

Figure 1.8. Coefficient of variation for workers in all of Washington State and workers in the interior PUMAs of Washington State, 2006-2016.

Source: Authors' analysis of Washington state UI program records.
Notes: The excluded PUMA IDs are 10400, 10503, 10504, 10600, 10703, 11000, 11101, 11103, and 11200 (U.S. Census, 2018). The navy line is the analytic sample and the dotted light blue line restricts workers observations in the excluded PUMAs.

Discussion and Policy Implications

In this study, I analyze intra-year measures of earnings volatility using quarterly administrative data for every job covered by Unemployment Insurance in Washington State. In doing so, I aim to build on prior work by Hannagan and Morduch (2015), Bania and Leete (2009), Morris et al. (2015), and Gennetian et al. (2014) and Wolfe et al. (2014) that documents intra-year changes in earnings and income. Using administrative data allows me to improve on estimates from survey data, which can be biased by nonresponse, seam bias, and underreporting (B. D. Meyer et al., 2015). While a comprehensive evaluation of workers' earnings would include informal work and 1099 contract work, UI-covered work still accounts for the majority of work performed in the US (Congressional Budget Office, 2019). From a policy perspective, estimating trends in between-work instability and in earnings volatility from UI-covered work is extremely important because policymakers have a say in the rules that create and support UI– covered work.

I find that the vast majority, 84 percent, of workers in low-wage jobs experienced earnings change greater than 25 percent of their previous quarters' earnings within a year. The share of workers in low-wage jobs that experienced large changes is higher than that found in the prior literature, and it is most likely due to both level of detail observed about workers' earnings and the limitations of the data, which do not capture earnings from non-UI-covered jobs. However, these results also omit volatility which occurs at the weekly and monthly level due to the quarterly nature of the UI-data, suggesting that these estimates may understate the true nature of workers' employment instability. As frequent or large changes are documented to be disruptive and stressful for children and their families (Sandstrom & Huerta, 2013), this finding should give policymakers pause when evaluating how well the formal labor market is working for workers in low-wage jobs. Nearly half the workers who experienced large changes experienced both a large increase and a large decrease in the same year, a finding that might be missed by inter-year calculations of volatility, in which earnings gains and losses can offset one another. Workers were more likely to experience large negative changes during the Great Recession, the magnitude of which outsized the magnitude of any earnings gains. In the recovery, a rising share of workers experienced large earnings gains. However, the size of these gains was smaller than the size of earnings losses that occurred at the same time. Workers thus got hit with more negative volatility during the downturn and smaller earnings growth during the upturn.

In addition, I find that worker transitions in and out of UI-covered work—16 percent of all quarter-to-quarter transitions—accounted for nearly two-thirds of the total intra-year earnings volatility over the period. Among the subset of workers who are continuously employed, I find that the relationship between workers' hours volatility and workers' earnings volatility is nearly one-to-one and substantially larger than the relationship between workers' wage volatility and earnings volatility. The magnitude of this relationship is smaller for workers outside of the arts, entertainment, food, and accommodation industry, indicating that the employment conditions in these firms are more likely to lead to higher earnings volatility than for other industries.

These results suggest that within-year variation at workplaces deserves more attention. Policies such as the recent secure scheduling laws and guaranteed minimum hours law have the potential to reduce the "risk of employment" for workers, provided the costs of these policies are not passed down to workers. "Secure scheduling" laws, in particular, could benefit workers by providing advance notice of work schedules, giving workers the necessary time to plan around their work schedules. Policymakers could also consider regulations that ensure workers have a right to refuse certain scheduling practices or the right to miss or reschedule a shift if they cannot make it, thereby providing workers a degree control in scheduling. If workers are cycling between formal and informal work or nonemployment, it could be because they cannot find a job which allows them to balance their employment with their family or personal needs. More broadly, the results of this paper show that the directionality of volatility and the probability of entry and exit from formal work are sensitive to the business cycle. Policymakers should consider policy to maintain employment during economic downturns, such as work sharing.

Chapter 2. LOCAL PAID SICK LEAVE: AN EXAMINATION OF FIRMS AND WORKERS

More than 45 percent of workers nationwide do not have access to paid sick leave (The Council of Economic Advisors, 2014). These employees are more likely to work in low-wage industries, to work part-time hours, to be single mothers, or to be people of color (Clemans-Cope et al., 2008; Lovell, 2003). Lack of access to sick leave means that workers are more likely to experience poor health outcomes or economic hardship if they, or a member of their family, become sick (Drago & Miller, 2010). Workers lacking paid sick time are also more likely to work while sick, which can increase the spread of diseases in the workplace and may lead to declines in productivity (Davis et al., 2005; DeRigne et al., 2016; Smith & Kim, 2010).

To reduce inequality in workplace compensation, and promote public health, cities across the United States have enacted paid sick leave laws.[18] These laws generally allow workers to accrue one hour of paid sick time for every 30-40 hours worked at a firm mandated to provide coverage (National Partnership for Women & Families, 2020; Paid Sick and Safe TIme Ordinance, 2011). If successful in their intent, paid sick leave policies have the potential to reduce employee absences due to infectious diseases and to increase worker productivity. Such effects would make workers more valuable to their employers, which could increase job tenure, decrease job turnover, and lead to a decline in the volatility of workers' earnings and hours. At present, there is no causal evidence that examines employment flows in response to paid sick leave policies, however observational

[18] Other cities that have passed paid sick leave include, San Francisco, CA, New York City, NY, San Diego, CA, Oakland CA, Tacoma, WA, Philadelphia, PA, Emeryville, CA, Pittsburgh, PA, Santa Monica, CA, Minneapolis, MN, Los Angeles, CA, Chicago, IL, Austin, TX, and Duluth, MN. Other jurisdictions that have passed paid sick leave include the District of Columbia, California, Massachusetts, Oregon, Vermont, Arizona, Washington, Rhode Island, Maryland, and New Jersey {Citation}.

studies of workers with employer-provided paid sick leave find that workers are more likely to stay home if they or a family member are sick and are less likely to separate from a job (Clemans-Cope et al., 2008; DeRigne et al., 2016; H. D. Hill, 2013).

Government-mandated employee benefits also have the potential to decrease employment and earnings for workers if employers compensate for the added cost of providing paid sick leave by cutting workers' hours or jobs or slowing their firm's growth. Cross-sectional evaluations of paid sick leave mandates using regional comparisons as proxies for treatment and control groups found that the introduction of paid sick leave policies led to a reduction in aggregate leave-taking and had mixed effects on workers' employment and earnings (Ahn & Yelowitz, 2015; Pichler & Ziebarth, 2018; Stearns & White, 2018).

At present, a multitude of localities and states are enacting paid sick leave laws without clear evidence about how these policies will impact the firms providing leave or the workers newly receiving the benefit. Policy adoption for paid sick leave can be compared with policy adoption of minimum wage, which is also getting passed at record rates among states and localities. However, unlike paid sick leave policies, there are decades of research documenting the impact of minimum wage policies on firms' and workers' labor market outcomes (Belman & Wolfson, 2014).

This paper uses quasi-experimental techniques to comprehensively evaluate the employment and earnings effects of a paid sick leave policy in one city, Seattle, on firms mandated to provide leave and on workers newly able to take advantage of paid time off. Passed in September 2011 and taking effect on September 2012, the Seattle Paid Sick and Safe Time (PSST) Ordinance requires firms with more than four full-time equivalent (FTE) employees to provide one hour of paid time off for every 30-40 hours worked by any employee within the city limits (Paid Sick and Safe TIme Ordinance, 2011). I focus analysis on Seattle workers and firms right around the policy's eligibility threshold for mandating coverage by drawing on a restricted-access administrative dataset from Washington State's Unemployment Insurance program, which provides quarterly hours and earnings data for all workers and firms engaged in formal work in Washington State. I evaluate changes in employment outcomes for firms and workers between 2010 and 2014. I evaluate whether any anticipatory effects occurred in the wake of the law's passage and enactment between 2010 and 2012. I subsequently evaluate treatment effects in the post-policy years of 2013 and 2014. I evaluate the PSST policy on firms' total headcount, hours, payroll, hires, separations, and job turnover using a regression discontinuity design to assess whether the increased cost of the policy had any effect on firms' employment levels or flows. I then use a difference-in-differences design on four longitudinal cohorts of workers to estimate the effects of the paid sick leave policy on workers' employment, hours, earnings, probability of being hired or separating, job duration, and employment volatility to assess whether the policy affected workers' employment stability in addition to their employment levels and flows. To see whether the PSST policy had heterogeneous effects for workers who are less likely to have access to paid sick leave prior to the law, I further estimate treatment effects using the difference-in-difference design for workers with low-earnings and part-time hours (Bureau of Labor Statistics, 2020).

I find that firms right above the threshold of four full-time equivalent employees did not experience any statistically significant changes in their total headcount, payroll, hires, separations, or job turnover as a result of the policy. Firms' total hours increased by 5.8 percent in 2012, the year the PSST policy was enacted. However, this effect did not persist in the later periods. Workers in firms just above the FTE employment threshold for the PSST mandate experienced a modest increase in their likelihood of being hired in the early post-policy 2013 cohort, which subsequently reversed in the later 2014 cohort. These estimates, however, are not robust to sample specification.

Workers experienced small changes in the magnitude of their earnings and hours volatility, however, these estimates are similarly sensitive to sample specification. Across all specifications, workers were more likely to experience a decrease in their hours worked after the policy took effect, with estimates ranging from 0.014 to 0.021 percent. Workers who were less likely to have access to paid sick leave before the PSST policy—low-earnings and part-time workers—similarly experienced no change in their employment levels and small, statistically significant changes in their employment flows and volatility across the four cohorts.

These results negate the hypothesis that the cost of the policy affected the employment outcomes of firms and rule out the possibility that the cost of implementation was passed down to workers in the form of employment reductions. Critically, these results are persistent for groups of workers who were disproportionately less likely to have paid sick leave before policy enactment. However, these results also negate the hypothesis that the PSST policy demonstrably improved workers' employment stability. The null treatment effects for both firms and workers suggest that the PSST policy's treatment dosage, 1 hour of PSST earned for every 40 hours worked, might be too small to cause employment changes. Additionally, 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 (Romich et al., 2014). These factors and others, such as employee health, reductions in contagion, and employee morale, while not readily evidenced in this study, should be considered as potential mechanisms in future paid sick leave legislation.

This study contributes to the growing literature on paid sick leave employment mandates in several ways. First, I demonstrate the utility of using administrative data in policy evaluation. The administrative data allow for precise identification of Seattle firms affected by the mandate (firms with at least four FTE employees) and those not covered by the policy (firms with four or fewer

FTE employees) to compare the labor market outcomes of these two groups over time. This level of precision is an improvement on previous analyses, which have used proxies for treatment such as county rather than city (Pichler & Ziebarth, 2018) or all firms in a state rather than the firms above a specific FTE employment threshold (Ahn & Yelowitz, 2015). This study also contributes impact estimates on the effect of a paid sick leave policy on traditional measures of employment flows, such as hires, separations, and job duration, as well as on new measures of employment volatility, such as the quarterly arc percent change in workers' hours and earnings and their likelihood of experiencing a drop or gain in these outcomes. This analysis of employment flows tests the hypothesis that a PSST policy affects employment stability and assess the changes in magnitude and directionality of workers' employment volatility. Finally, this study contributes evidence on the impact of a paid sick leave policy on policy-relevant subgroups: low-earnings and part-time workers. In doing so, this study contributes insight on how an accrual-type paid sick leave policy can affect economically disadvantaged workers.

Seattle's PSST Ordinance

Seattle's paid sick leave law was the second in a wave of state and local paid sick leave ordinances enacted to reduce exposure to infectious disease, "resulting in a healthier and more productive workforce . . . and improv[ing] family economic security" (Paid Sick and Safe TIme Ordinance, 2011). Passed in September 2011 and taking effect on September 2012, the Paid Sick and Safe Time (PSST) Ordinance requires firms with more than four full-time equivalent (FTE) employees to provide one hour of paid time off for every 30–40 hours worked by any employee within the city limits (Seattle Office of Labor Standards, 2016). The ordinance stipulates that the number of compensated hours that equate full-time, full-year work is equal to 2,080 hours per year (52 weeks * 40 hours = 2,080 hours). A firm's FTE employee size is determined 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 (Paid Sick and Safe TIme Ordinance, 2011). According to the Ordinance, firms must provide paid sick leave in the years when their annual average FTE employee size in the year prior was more than four FTE employees. New firms entering Seattle calculate their FTE size for their first year of operation based on the total number of hours worked in the first quarter of their existence and then update their firm size once every calendar year (Paid Sick and Safe TIme Ordinance, 2011). Businesses are required to keep track of accrued employee time for up to two years after accrual. The law is enforced through anonymous worker complaints to the Seattle Office of Labor Standards. Critically, the law stipulates a strict cutoff for treatment: employers must count fractions of FTE employees towards their FTE employee size. For example, a firm with 4.01 FTE employees is mandated to provide paid sick leave, while a firm with 4.00 FTE employees is not.

Workers in firms with an FTE employment size greater than four FTE employees but less than 250 FTE employees accrue one hour of paid time off for every 40 hours worked, while workers in firms with 250 or more FTE employees accrue one hour of paid time for every 30 hours worked. Employees accrue paid sick leave based on their hours worked and can roll over their accrued hours into the following year. The policy applies to all workers in firms with more than four FTE employees in Seattle, including part-time and temporary workers, which is a departure from the structure of private employer benefits that generally provide paid leave for full-time, full-year workers only. According to the Bureau of Labor Statistics (2020), only 40 percent of part-time workers have access to paid sick leave, relative to 85 percent of full-time workers.

CONTRIBUTING LITERATURE

Local, employer-based mandates, such as paid sick leave, are theorized to have a variety of effects on firms newly expected to provide paid sick leave and on workers newly able to receive the benefit. Previous research has centered evaluations of these policies on the cost of implementing and providing the new benefit. Firms mandated to provide paid sick leave may have face an increase in their fixed costs to implement new record-keeping systems to track accrued paid sick leave time. Employers may also face increased marginal costs to provide paid sick leave time, and this cost scales up with each additional hour an employee works at the firm. While it’s difficult to estimate a firm’s fixed cost, the law allows for a straightforward calculation of the marginal labor cost incurred, which ranges from 2.5 percent for firms with greater than four but fewer than 250 FTE employees (1 hour PSST accrued per 40 hours worked) to 3.3 percent for firms with greater than 250 FTE employees (1 hour PSST accrued per 30 hours worked).

Employers who did not provide paid sick leave before the law may respond to the new cost through changes in their employment structure. These firms may reduce employment, the number of hours worked, or their number of hires to maintain their current cost structure or to avoid providing leave altogether. Research using the American Community Survey to evaluate Connecticut’s Paid Sick Leave lends partial evidence to this line of inquiry. In comparing cross-sections of Connecticut workers to workers in surrounding states after the passage of the Connecticut paid sick leave law, the authors found a decline in the likelihood of working in the past week (Ahn & Yelowitz, 2015).

Employers may alternatively decide to incur the cost to comply with the law without any change in employment. This outcome may be particularly likely for paid sick leave policies, as employers may have the capacity to absorb the relatively low cost of the policy. Evidence gleaned from using administrative data to estimate local paid sick leave laws at the county level supports this hypothesis. Researchers found that local paid sick leave laws did not significantly change private employment levels or weekly earnings in the observed 12–36 post-policy months (Pichler & Ziebarth, 2018).

These studies provide insight into the average treatment impacts of a paid sick leave mandate across large groups of workers and geographies. However, in doing so, these studies estimate treatment effects inclusive of workers who are more likely to have paid sick leave benefits at their workplace. According to the Bureau of Labor Statistics (2020), nearly 90 percent of private industry workers in firms with 500 or more employees have access to paid sick leave, relative to only 61 percent of workers in firms with less than 50 employees. Grouping workers in large and small firms together to estimate treatment effects may, therefore, attenuate results. Attenuation may also occur when grouping specific employment groups together, including full- and part-time workers and workers of all earnings ranges. In addition to the 85 percent of U.S. full-time workers who have access to employer-provided paid sick leave on average, 77 percent of U.S. workers who earn above the 25th percentile of have access to employer-provided paid sick leave (Bureau of Labor Statistics, 2020). By contrast, only 40 percent of part-time workers and 41 percent of workers in the bottom 25th wage percentile have access to paid sick leave (Bureau of Labor Statistics, 2020). Because low-earnings and part-time workers are less likely to have access to paid sick leave, they may cost employers more than their higher-earning, full-time counterparts, and may experience larger employment changes as a result.

Although evaluations of paid sick leave policies are relatively new, the policy evaluation literature estimating employment and earnings responses to labor regulation is not. Researchers studying paid family leave, for example, have shown that the policy had a statistically significant increase in women's labor force participation and employment, and modest or insignificant effects on women's earnings (Baum & Ruhm, 2016; Rossin-Slater et al., 2013). Meta-analysis of employment responses to minimum wage policies find that minor increases in state and local minimum wage do not affect firms' or workers' employment or earnings in a substantial way (Belman & Wolfson, 2014). Using the same restricted-access employer-employee matched data as this study, a recent evaluation of Seattle's minimum wage ordinance found that the 16 percent increase in minimum wage did not affect the number of low-wage jobs or hours worked in those jobs, but increased the earnings for minimum-wage-eligible workers who were employed at the time the ordinance went into effect (Jardim et al., 2020).

Paid sick leave policies may also have an impact on firms' and workers' employment flows and volatility. Access to paid sick leave has the potential to limit the spread of diseases in workplaces and reduce absences for sickness. Evidence shows that workers who show up to work sick have higher rates of sick leave than if they had just taken a few hours or days of leave in the first place (Grinyer & Singleton, 2000). Paid sick leave policies may, therefore, be a remedy for missed work: an evaluation of state and local paid sick leave policies found that paid sick leave policies decreased aggregate leave-taking and work absences due to illness (Pichler & Ziebarth, 2017; Stearns & White, 2018). Less missed work improves a firm's productivity, which may increase a worker's value to their employer, resulting in higher earnings, employment duration, and lower separation rates. Workers with longer job duration may see lower rates of earnings and hours volatility due to fewer job switches in the same way that safety net and welfare programs have the potential to stabilize household income (Deshpande, 2016; B. L. Hardy, 2017).

Causal evidence supporting this hypothesis comes from research into state-level minimum wages, which demonstrates that jobs affected by higher minimum wages experienced a lower rate of separation due and job turnover to the policy (Dube et al., 2016a). Observational research on paid sick leave has shown that workers with paid sick leave have a lower probability of separating from a job (H. D. Hill, 2013). To date, the relationship between paid sick leave policies and traditional employment flows (hires, separations, job duration, and job turnover) and workers' employment volatility is an unexplored avenue of research.

METHODS

I exploit the variation in the FTE size and in the timing of Seattle's paid sick leave policy to assess the policy's impact on firms and workers employed in firms right around the FTE employment threshold for coverage. To assess firm-level impacts, I employ a regression discontinuity design using cross-sections of firms between 2010 and 2014. To assess worker-level impacts, I employ a difference-in-differences design using a series of longitudinal cohorts over the same period.

Firm-level Methods

Because the law stipulates a sharp cutoff of a firm's FTE employment size to mandate coverage, I use a regression discontinuity design to estimate firm-level impacts. In a regression discontinuity design, treatment is determined by a running variable (denoted as R_i), in this case, a firms' FTE employee size. I assign each firm with a running variable below the known threshold of four or fewer FTE employees to the comparison group and each firm with greater than four FTE employees to the treatment group.[19] Implicit in this design are the assumptions that there is perfect compliance among each firm and that firms do not have precise control over their FTE employee

[19] Given technological constraints in firms' measurement and precision and the potential for employers to round, I exclude firms with an FTE employee size that is within 0.004 FTE employees of the threshold.

size. The characteristics of firms and a firm's FTE employee size on either side of the cutoff should effectively be random.

To estimate the policy effects on firm-level outcomes, I use the following regression discontinuity (RD) model:

$$y_{it} = \alpha + \gamma R_{it} + \beta_1(r_{it} < c) + \beta_2(r_{it} > c) \times R_{it} + \epsilon_{it} \tag{2.1}$$

Where y_{it} is the firm outcome of interest in year, t, c, denotes the treatment cutoff of four FTE employees, and R_{it} denotes a firm's FTE employee size in year, t. I estimate Equation 2.1 in each year, t, for firms that have an FTE employee size, r_{it}, within a bandwidth, h, such that $(c - h < r_{it} < c + h)$. Each treatment effect for year, t, is estimated in a separate regression using a local linear estimator weighted by a triangular kernel, which places more weight on observations closer to the threshold. The year 2010 is a strictly pre-policy period, used as a falsification test. The years 2011 and 2012 can be thought of as anticipation and learning years, during which the PSST policy was passed by the city council (September of 2011), the rules regarding the policy were released (January of 2012), and the policy took effect (September of 2012). The years 2013 and 2014 are post-policy years, in which employers had full knowledge of the law and were required to act in compliance.

Several methodological choices need to be made to employ a regression discontinuity design. First, Equation 2.1 is written to employ a linear estimator to estimate treatment effects. If firms exhibit continuous nonlinear behavior around the cutoff, treatment effect estimates will be biased. I address this possibility with a descriptive analysis of employment outcomes by firms' FTE employee size in later sections. Second, the bandwidth chosen to estimate local average treatment effects play an outsized role in the precision and bias of treatment effect point estimates. If the bandwidth is too large, treatment effects will likely be biased. However, if the bandwidth is too small, the variance of each estimate derived from the regression discontinuity regression will increase. The rules determining the FTE employee size for mandating coverage provide an upper bound of the bandwidth selection, $h = 4\ FTE$. This bandwidth is the broadest bandwidth possible that would provide an equal number of FTE employment sizes on either side of the threshold. Beyond this broad restriction, however, the optimal bandwidth choice is unclear. Rather than make an ad hoc choice, my main specification estimates Equation 2.1 using bandwidths generated from a data-driven approach, following Imbens and Kalyanaraman (2012) such that the point estimate of each regression derived is mean square error optimal (Cattaneo et al., 2017; Imbens & Kalyanaraman, 2012). While the bandwidth chosen for each estimate will not be consistent across outcomes and time, it will generate point estimates that minimize bias from too large a bandwidth, and that minimize variance from too small bandwidth. To assess the sensitivity of this approach, I re-estimate treatment effects for each outcome and year using predetermined standardized bandwidth choices, ranging from firm size of $h = 0.25$ FTE employees to firm size of $h = 1.75$ FTE employees on either side of the FTE threshold for coverage.

Worker-level Methods

I use a difference-in-differences (DiD) estimation strategy on four longitudinal cohorts to estimate the effects of PSST on workers' employment outcomes. The paid sick leave law stipulates that workers are covered by paid sick leave law in year, t, if they are employed at a firm that had an annual average of more than four FTE employees in year, $t - 1$. As such, I assign treatment to workers in year, t, who worked 100 percent of their hours in Seattle firms with more than four FTE employees in their fourth quarter of employment in year, $t - 1$. A worker is assigned to the comparison group in year, t, if they worked 100 percent of their hours in Seattle firms with four or fewer FTE employees in their fourth quarter of employment in year, $t - 1$. To provide comparable results to the firm-level analysis, I restrict the analysis to workers in firms that are within 1 FTE employment size away from the 4 FTE employment cutoff.

Over the study period of 2010 through 2014, this treatment identification generates four distinct panel datasets by identifying workers who work 100 percent of their jobs in Seattle in firms with an FTE employment size of greater than three to five employees in the fourth quarter of the years 2010 through 2013 (the baseline quarter). I follow their employment outcomes backward and forward one year to define a pre-policy period and post-policy period for each cohort of workers. Each cohort is named for the post-policy year of the cohort. For example, a worker is considered to be treated in the *2011 cohort* if they worked 100 percent of their hours in a firm with more than 4 FTE employees in the *baseline quarter* of fourth quarter of 2010. Their employment history is tracked between the first quarter of 2010 through the fourth quarter of 2011 to create a longitudinal cohort. In this example, the worker's pre-policy period is the first quarter of 2010 through the fourth quarter of 2010, and their post-policy period is the first quarter of 2011 through the fourth quarter of 2011.

A DiD strategy requires two assumptions to be met: First, similar to the regression discontinuity design, a DiD design assumes that all firms in Seattle with more than four FTE employees are mandated to provide paid sick leave and that assignment to the treatment group in the post-policy period does not affect workers' outcomes in the pre-policy period. The second assumption in a DiD design is that the changes in outcomes between the pre- and post-policy period in the comparison group are equivalent to the counterfactual change in the outcomes between the pre- and post-policy period for the treatment group *absent* policy intervention. Commonly known as the "parallel trends" assumption, this assumption would be violated if trends in outcomes for uncovered workers and covered workers were different in the pre-policy period.

I derive causal inference for the difference-in-differences design through the interaction of workers who are covered by the policy (i.e., workers who worked 100 percent of their hours in a firm with more than 4 FTE employees in the baseline quarter of their cohort) and the post-policy period for each cohort. The resulting equation to estimate the average treatment effect on the treated is:

$$y_{ict} = \beta_0 + \beta_1(Covered_c \times Post_t) + \beta_2(Covered_c) + \gamma_i + \tau_t + \epsilon_{ict} \quad (2.2)$$

where y_{ict} is the outcome variable of interest for worker, i, that is covered by the paid sick leave law, c, and in the post-period, t. The interaction, $(Covered_c \times Post_t)$, denotes an indicator for being covered in the post-policy period. The parameter of interest, β_1, summarizes the average effect of the policy on covered workers. I additionally include individual fixed effects, γ_i, to control for any heterogeneity across workers, and quarter fixed effects, τ_t, to control for any unobserved trends common to both covered and uncovered workers across time. Treatment effects are estimated using an ordinary least squares linear regression with worker and quarter fixed effects. For outcomes with a 0/1 indicator, the ordinary least squares regression estimates a linear probability model using Equation 2.2. Standard errors clustered at the worker level.

This longitudinal design is powerful because it assesses workers' employment patterns using information from all of a workers' jobs, including secondary jobs, in response to the policy. In effect, I can assess the effect the PSST law on workers' overall employment behavior. However, using information on all jobs may bias treatment effects if workers transition to employment outside of Seattle or to employment in a firm with a different coverage status. To assess the sensitivity of this sample choice, I re-estimate Equation 2.2 for workers who remain employed in Seattle and for workers who remain in their initial treatment and comparison category, respectively. Additionally, I assess the sensitivity of the choice to examine workers in firms within

1 FTE employee on either side of the 4 FTE coverage threshold, by re-estimating Equation 2.2 for each outcome and cohort using predetermined standardized bandwidth choices, ranging from firm size of $h = 0.25$ FTE employees to firm size of $h = 1.75$ FTE employees on either side of the FTE threshold for coverage.

Worker Subgroup Analysis

As part-time workers and workers with low earnings are less likely to have access to paid sick leave in their current employment positions, these groups might experience larger employment changes as a result of the PSST policy (Bureau of Labor Statistics, 2020)(Clemans-Cope et al., 2008). To assess whether the paid sick leave policy differentially affected workers less likely to have paid sick leave before the ordinance, I estimate treatment effects using Equation 2.2 for workers with low earnings and workers who have part-time hours. I define workers with low-earnings as those whose earnings in the baseline quarter is in the bottom 25 percent of the earnings distribution. I define part-time workers as workers who work 31 or fewer hours to comport with Washington State Labor Standards law (Economic Service Administration, 2016).

Sample Sensitivity Analysis: Legal Exemptions

While the PSST law is intended to cover all workers in firms with four or more FTE employees, the law provides an exemption if workers would prefer to swap their work shift rather than take paid time off. In particular, employers of workers in the construction and food and restaurant industry (NAICS code 32 and 722) lobbied to include a provision to allow workers to swap shifts when they were sick rather than take paid leave in order to recover full earnings (Paid Sick and Safe TIme Ordinance, 2011). The ability to swap shifts is particularly crucial to restaurant workers, as the ordinance does not cover tip income lost from missed work. While the law states that workers in any industry can swap shifts, workers in the construction and food and restaurant industries may be less likely to exhibit any employment changes as a response to paid sick leave. To assess the sensitivity of firm and worker treatment effects to the inclusion of industries where shift swapping is dominant, I re-estimate Equation 2.1 and Equation 2.2 absent firms and workers in these industries.

DATA

The Washington State Employment Security Department collects quarterly payroll records for all workers who receive earnings through formal work in Washington State and are eligible for the Washington State Unemployment Insurance (UI) program. These data, which are generated by employers' reports of quarterly payroll filings to the state, include quarterly hours in addition to quarterly earnings information to determine UI eligibility. [20][21] In these data, a firm is defined by its unique Employer Identification Number and can encompass a single establishment or a combination of establishments. Firm sizes range from very small, such as self-employment firms, to very large, such as multinational corporations. These data do not include workers employed in contract employment, workers in informal arrangements, self-employed (without employees), or workers with jobs outside of Washington.[22] Critical to the methods outlined in the previous section,

[20] Data are cleaned to omit earnings records with zero hours information and hours records with zero earnings information.

[21] Employers 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.

[22] While Seattle's paid sick leave ordinance does not cover self-employed workers or workers in the informal economy, treatment effects may overstate any reductions in employment, hours and earnings 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.

these data allow for precise calculation of a firm's FTE employee size over the study period for all hours worked in Washington State. [23][24]

Beyond reporting the hours and earnings of employees, employers must provide the mailing address of their business, which I geocode to determine whether a firms' location is within Seattle city limits. If the mailing address is misspelled, not input correctly, or unknown to the employer filing the records to the state, it cannot be geocoded.[25] Of the 403,597 unique addresses in the data, eight percent of firms statewide have invalid addresses or an address listed as "state-wide" or "unknown" and, therefore, could not be geocoded to a specific location.[26] Additionally, firms with multiple establishments under a single Employer Identification Number are unable to be geocoded because it is unclear how many of the firms' establishments are associated with the address provided. **Table 2.1** shows the average number of Seattle firms between 2010 and 2014 and the number of firms that are non-locatable due to their decision to establish an umbrella Employer Identification Number, by the FTE employee size of the firm. Across FTE employee sizes, there is substantial variation in the share of firms that opt for an umbrella account across their

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[23] While many firms expand after their first operating quarter, some firms expand at an unprecedented rate upon opening, leading to a first-quarter FTE size that is not indicative of its actual size over the period. I drop these 0.1 percent of firms which have a quarterly growth rate of >100% if their size increased by at least 150 employees quarter-to-quarter to avoid these outliers affecting the analysis.

[24] The PSST policy pertains to firms worldwide; however, this data is for Washington State. To the extent that I misidentify some firms' FTE size because of this restriction, there will be measurement error.

[25] I geocode mailing addresses to exact latitude and longitude coordinates using the Business Analytics 2016 Street Map database from ARC GIS. If exact latitude and longitude coordinates cannot be defined, I geocode to the centroid of the firms' zip code, depending on the level of detail of the mailing address.

[26] In addition to these incorrect mailing records, there was also a record collection problem with certain classes of domestic workers (NAICS code 814000) and home and health care aides (NAICS 624120). As a result, I exclude jobs and workers in these industries from the firm-level analysis and cohort determination of workers.

establishments. Firms with small FTE employee sizes are nearly 100 percent locatable. Of the 12,520 firms with four or fewer FTE employees, only an average of 21 firms are unable to be geocoded. By contrast, only 64 percent of firms (208 firms/ 323 firms) with an average of 250 or more FTE employees are locatable due to having multiple establishments under one Employer Identification Number. Because of the UI data's inability to provide a representative locatable sample of these larger firms, I exclude them from the analysis.

Table 2.1. Characteristics of Seattle Firms, By Firm Size, 2010-2014

Firm size	Total	Non-locatable businesses	Share of businesses included in analysis
Uncovered firms: FTE < 4	12,521	21	99.8%
Covered firms: 4≤ FTE<250	6,953	259	96.4%
Covered firms: 250 ≤ FTE	208	115	64.3%

Source: Authors' calculations from Washington State UI program records.
Notes: The number of firms displayed reflect the annual average number of Seattle firms between 2010 and 2014. Firms are defined as entities with unique federal tax Employer Identification Numbers. Non-locatable firms are firms with multiple establishments under one Employer Identification Number. A firms' FTE employment size is determined by dividing annual full-time hours (52 weeks * 40 hours = 2,080 hours) by the total number of annual hours worked at the firm.

Firm-level Outcomes

I focus the firm-level analysis on locatable Seattle firms with an FTE employee size of less than 250 FTE employees between 2010 and 2014. I define a job in the data as an employer-employee match. From these data, I derive the total number of matches within a firm (jobs), the total number of hours worked, the total payroll generated, the total number of hires, and the total number of separations at each firm in a given year. These data also provide the ability to estimate a firms' job turnover. I define firms' job turnover to be the total number of separations in a year, t, (summed across quarters, $q = (1, 4)$), divided by the sum of quarterly employment in the first and last quarter of the year:

$$turnover_t = \frac{\sum_{q=1}^{4} separations - \sum_{q=1}^{4} hires}{(employment_{q=1} + employment_{q=4})} \tag{2.3}$$

Worker-level Outcomes

The worker-level outcomes include a worker's employment levels, employment flows, and employment volatility. I define a worker to be employed if they are in a cohort and are observed in the UI data in any quarter of the cohort period. Workers' quarterly hours, earnings are equal to the sum of each outcome across all workers' jobs.[27] I denote a workers' probability of being hired and probability of separating as a binary indicator equal to one if the event occurs at any of that worker's jobs, and equal to zero otherwise. I define a worker's employment duration to be the mean employment duration for all of a worker's jobs worked within that quarter, truncated at eight quarters to reflect the duration of each cohort period studied. To estimate workers' employment stability in response to the PSST policy, I use several measures of hours and earnings volatility to estimate the magnitude and directionality of fluctuations, as recommended by Hill et al. (2017). While qualitative and survey research on low-wage work has documented workers' earnings volatility at the weekly or monthly level, the data available is at the quarterly level, which may limit my ability to assess the policy impacts on volatility outcomes (Lambert et al., 2014; Morduch & Schneider, 2017).

To estimate the magnitude, I use the absolute value of the quarterly arc percent change (APC) in total earnings and hours, denoted as Y, for worker, i, in quarter, q, conditional on employment. The following equation defines the APC of workers' hours or earnings:

$$APC_{Yiq} = abs|100 * \frac{(Y_{iq} - Y_{iq-1})}{(Y_{iq} + Y_{iq-1})/2}| \tag{2.4}$$

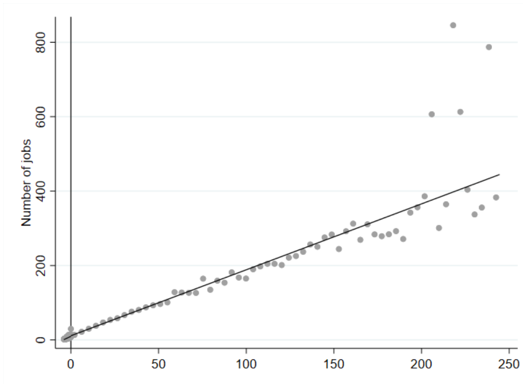
[27] Workers' hours and earnings are top coded at the 98th percentile to avoid outliers biasing treatment effects.

A value of 0 indicates no change in a worker's quarter-to-quarter earnings (hours), whereas a value of 200 indicates that there was a change of double or more in a worker's earnings (hours). To assess the directionality of a worker's earnings volatility, I further estimate workers' likelihood of experiencing an earnings (hours) increase or decrease relative to their previous quarter using binary indicators.

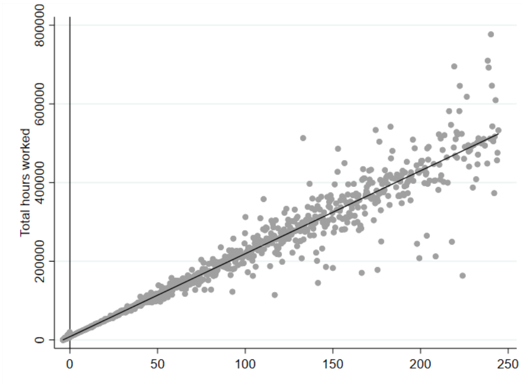
FIRM-LEVEL RESULTS

Descriptive Statistics

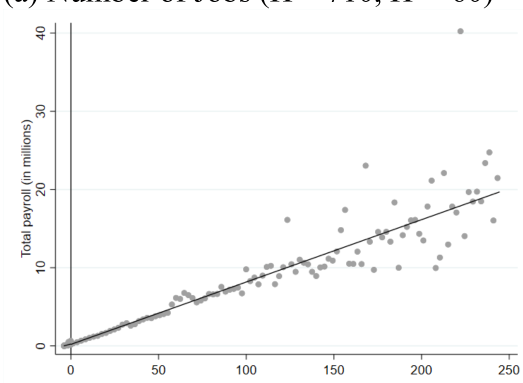
To assess whether employment characteristics of firms just above and just below the cutoff can be assumed to be random, **Figures 2.1a–f** show binned means of each firm-level outcome over the full support of the running variable (FTE employment size) during the pre-policy enactment period of 2010 through 2011**. Figures 2.2a–f** show the same graphic restricting the window of observation to two FTE employment sizes on either side of the 4 FTE threshold. In both sets of figures, the solid line in each graphic is a linear fitted estimate, estimated for each side of the cutoff separately, and the black vertical line indicates the FTE employee cutoff that employers must provide paid sick leave. Figures 2.1a–f show that firms' employment outcomes change as the FTE employee size of a firm increases. As a firm's FTE employee size increases, the number of jobs, hours, earnings, hires, and separations increase in a linear or slightly quadratic pattern. By contrast, firms' job turnover exhibits an erratic pattern among firms below the 4 FTE employee threshold, and a nonlinear, uniform pattern above the cutoff, indicating that turnover among firms with small FTE employment sizes may not be comparable to firms with larger FTE employment sizes. Figures 2.1a–f suggest that, in addition to larger firms being more likely to provide paid sick leave than smaller firms, firms across a wide range of FTE employee sizes may not be comparable for causal analysis.



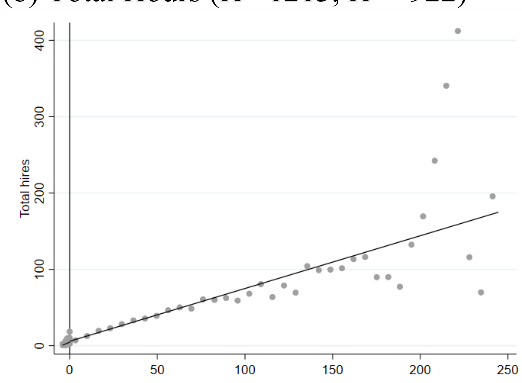
(a) Number of Jobs (H^-= 710, H^+ =60)



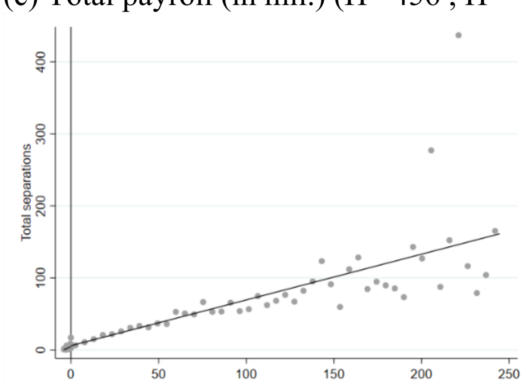
(b) Total Hours (H^-=1213, H^+ =922)



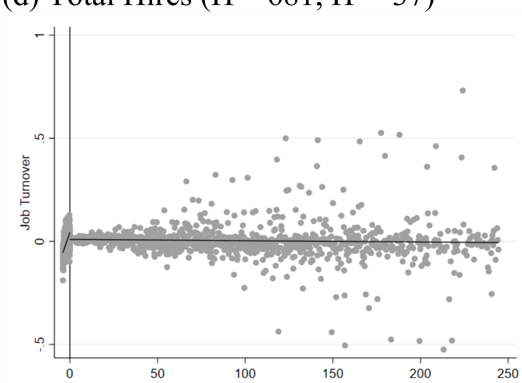
(c) Total payroll (in mil.) (H^-=456 , H^+ =104)



(d) Total Hires (H^-= 681, H^+ =37)



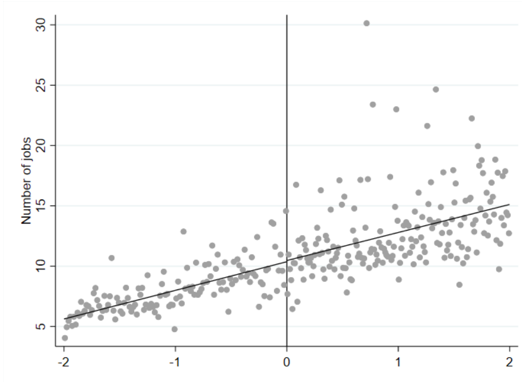
(e) Total separations (H^-= 661, H^+ =47)



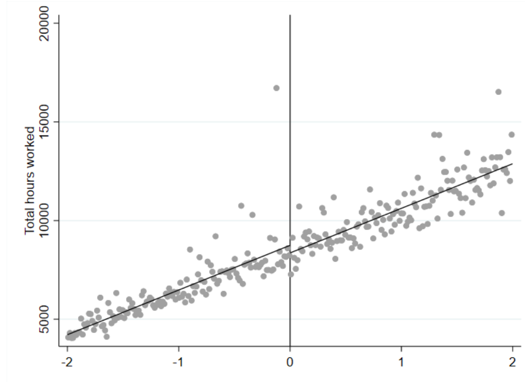
(f) Job turnover (H^-= 961, H^+ =1,070)

Figure 2.1. Regression discontinuity plots of firm-level outcome variables across the study period, 2010-2014.

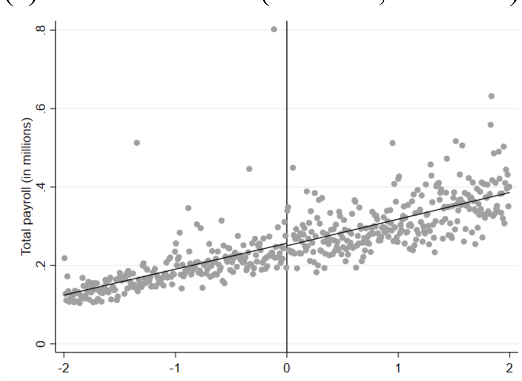
Source: Author's analysis of Washington State UI program records.
Notes: The figure shows binned means of firm level outcome variables with a cutoff threshold of 4 FTE employees. Bins are evenly spaced bins to reflect the true variability in each outcome variable. The number of bins on the left side of the cutoff is denoted by H^- and the number of bins on the right side of the cutoff is denoted by H^+. The number of observations on the left side of the cutoff for all variables is N^- = 52,685. The number of observations on the right side of the cutoff for all variables is N^+ = 19,952. The solid lines are linear model fits estimated separately for each side of the cutoff, and the vertical line indicates FTE cutoff for mandating PSST.



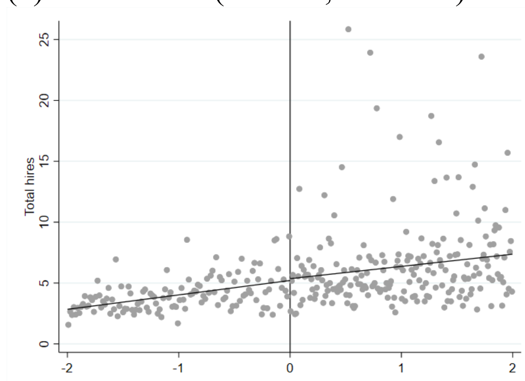
(a) Number of Jobs (H-= 124, H+ =180)



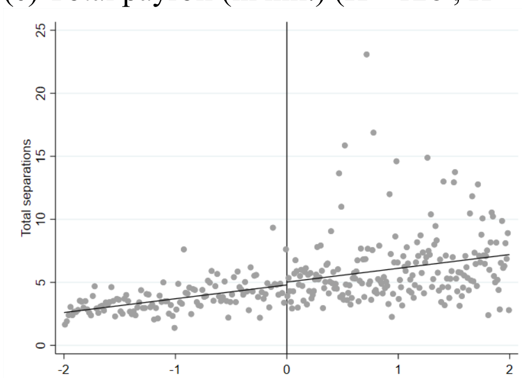
(b) Total Hours (H-=139, H+ =135)



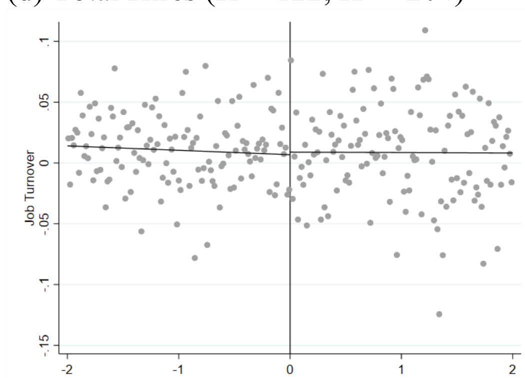
(c) Total payroll (in mil.) (H-=128 , H+ =115)



(d) Total Hires (H-= 122, H+ =204)



(e) Total separations (H-= 120, H+ =194)



(f) Job turnover (H-= 125, H+ =126)

Figure 2.2. Regression discontinuity plots of firm-level outcome variables across the study period, 2013-2014, FTE size 2-6, shifted 4 FTEs.

Notes: The figure shows binned means of firm level outcome variables with a cutoff threshold of 4 FTE employees. Bins are evenly spaced bins to reflect the true variability in each outcome variable. The number of bins on the left side of the cutoff is denoted by H^- and the number of bins on the right side of the cutoff is denoted by H^+. The number of observations on the right side of the cutoff for all variables is $N^+ = 6{,}911$. The number of observations on the left side of the cutoff for all variables is $N^- = 3{,}714$. The solid lines are linear model fits estimated separately for each side of the cutoff, and the vertical line indicates FTE cutoff for mandating PSST.

Figures 2.2a–f show firm-level outcomes within 2 FTE employee sizes, $h = 2$ of the threshold. In this window, employment outcomes for firms just over the FTE employee cutoff for paid sick leave coverage exhibit nearly identical trends to employment outcomes for firms just under the cutoff. Given the similarity in outcomes among firms just above and just below the 4 FTE employee threshold, Figures 2.2a–f confirm the choice to use a regression discontinuity to assesses local average treatment effects as treatment effects using a broad range of FTE employee sizes might lead to biased results. Figures 2.2a–f also show that there is no jump right at the threshold in these employment outcomes, indicating that the paid sick leave policy may not alter firms' employment behavior in the pre- or post-policy periods.

To formally assess whether a firm's FTE employee size on either side of the policy cutoff can be assumed to be random, **Table 2.2** presents the results from density tests performed for each year in the study period, following McCrary (2008). A density estimator statistically assesses whether discontinuities in the density of FTE employment sizes existed on either side of the running variable, R_i.[28] Following the methods of Catteneo et al. (2017), the density estimator is based on a local-linear third-order polynomial estimator with a triangular kernel and a bandwidth chosen using a data-driven procedure to minimize the mean-squared error. The first two columns of Table 2.2 show the bandwidth, $h-$ and $h+$, chosen from above and below the cutoff, respectively, to assess whether discontinuities in the density of the FTE employment size exists. The second two columns display the number of observations (firms) within each bandwidth window chosen. The final column shows the p-value derived from testing the equality of the

[28] The density estimator is calculated by first generating a finely gridded histogram and second by smoothing the histogram using local linear regression, separately on either side of the cutoff. Under the null hypothesis that the discontinuity is zero, continuity in the running variable, r, of the conditional density function, implies continuity of f(r), the density of the running variable {Citation}.

number of observations on either side of the cutoff. A p-value greater than 0.05 indicates there was no statistically significant difference in the density of a firm's FTE employee size on either side of the cutoff. The table shows the p-values range from 0.11 to 0.53, thereby ruling out the possibility that firms manipulated their FTE employee size to avoid treatment.

Table 2.2. Density Test Results of the Running Variable, 2010-2014

Year	Bandwidth		Number of observations		p-value testing the equality of the number of observations
	h-	h+	N-	N+	
2010	0.50	0.50	659	517	0.53
2011	0.49	0.50	597	554	0.51
2012	0.49	0.49	652	537	0.11
2013	0.47	0.47	642	543	0.46
2014	0.47	0.47	604	523	0.20

Source: Authors' analysis of Washington State UI program records
Notes: The test statistic (not shown) is constructed using a third-order polynomial with different bandwidths allowed to be chosen below, h-, and above, h+, the threshold, respectively. Each bandwidth h, is chosen by a data-driven procedure to minimize the mean-square error of the test statistics. P-values are computed using Gaussian distributional approximation to bias-corrected local-linear polynomial estimator with triangular kernel and robust standard errors, following Catteneo et al. (2017).

To assess whether discontinuities are present at places other than the actual cutoff, **Figures 2.3a-f** shows "pseudo-treatment" effects from regressions estimated from Equation 2.1 using cut points that range from 1 to 200 FTEs.[29] If impact estimates from cut points other than 4 FTEs were statistically significant, there would be concern about validity of the regression discontinuity design. In each figure, the margin of error increase as the FTE size gets larger. With the exception of a firms' job-turnover, nearly all of the estimates across the various pseudo cut points are null, indicating there are not discontinuities present at places other than the cutoff. Discontinuities exist

[29] **Appendix A Figure 1a-f** also shows "pseudo treatment" effects using cut points that range from 1 to 100 FTEs with a fixed bandwidth of 2 FTEs and the results are largely unchanged.

at the FTE cutoff of one and two FTEs for a firms' job turnover, so treatment effects on this outcome should be treated with some caution.

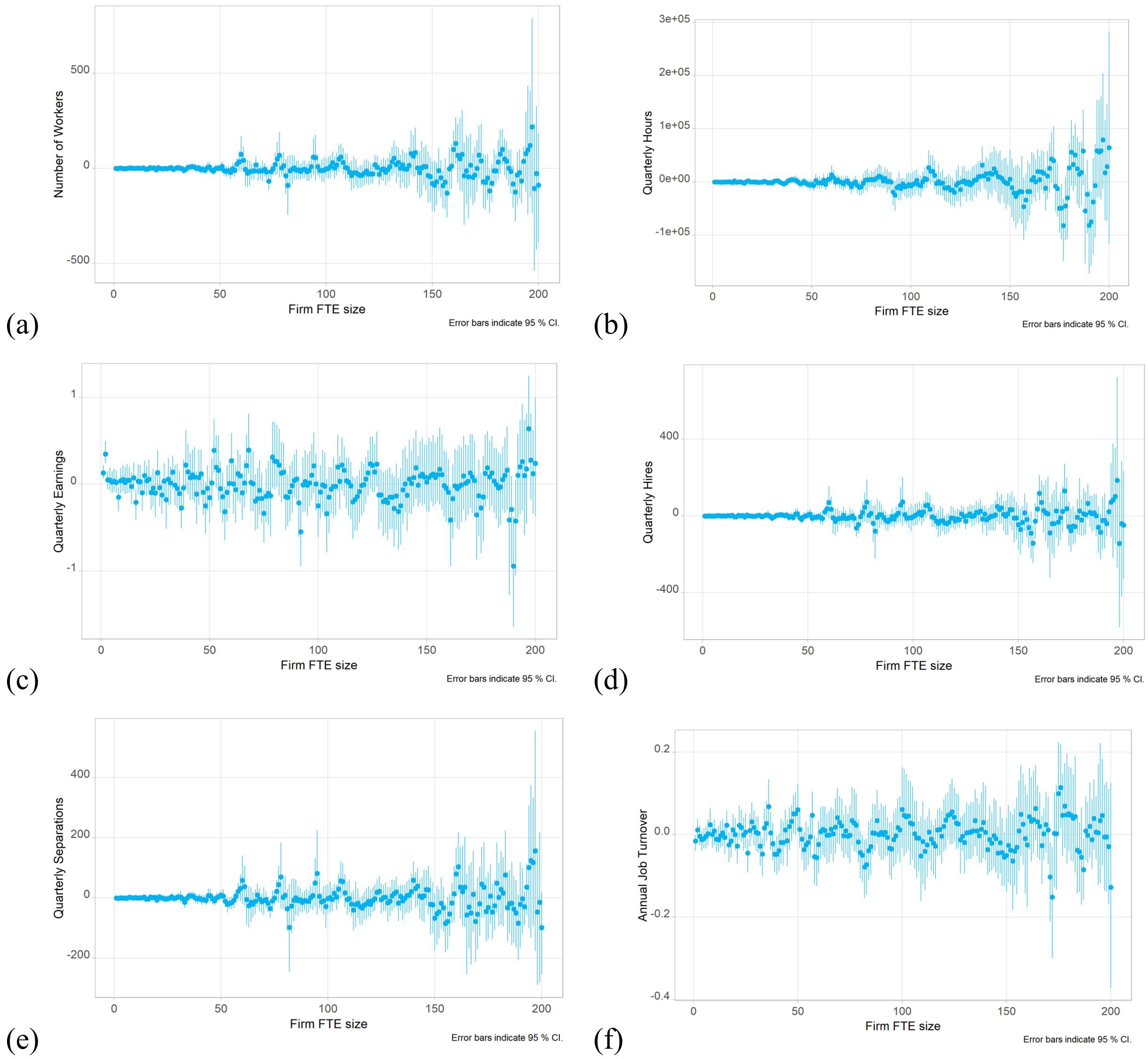


Figure 2.3. Local average treatment effects on firm-level outcomes using a regression discontinuity design for cutoff points ranging from 1 FTE to 200 FTE, 2013-2014.

Source: Author's analysis of Washington state UI program records.
Notes: The graphic shows the treatment effects and the 95 percent confidence interval for each impact estimate, for cutoff point ranging from 1 FTE to 200 FTEs. Treatment effects are estimated using a local linear estimator, weighted with a triangular kernel, and using bandwidths chosen by a data driven procedure such that that RD point estimate is MSE optimal.

Treatment Effects

Table 2.3 reports average treatment effects from regressions estimated from Equation 2.1**.** The table also displays the p-value of the point estimate (in brackets), the bandwidth (h) used to estimate treatment effects, and the pre-policy mean of each outcome for each year examined. Table 2.3 shows that there are no statistically significant effects in 2010, the year before the policy's passage, confirming that there was no relationship between the policy cutoff and any employment outcomes in the pre-policy period.

Table 2.3. Local Average Treatment Effects on Firm-level Outcomes Using a Regression Discontinuity Design, 2010-2014

	(1)	(2)	(3)	(4)	(5)
	Pre-policy	Policy passed	Policy enacted	Post-policy	
	2010	2011	2012	2013	2014
Number of jobs					
Treatment effect	0.052	0.347	0.040	-1.148	0.442
p value	[0.875]	[0.774]	[0.820]	[0.255]	[0.753]
h	1.25	1.13	1.13	1.08	1.06
Pre-policy mean	9.05	9.62	9.99	10.02	10.27
Total hours worked					
Treatment effect	0.89	77.4	479	-643	6.62
p value	[0.906]	[0.850]	[0.063]	[0.434]	[0.786]
h	1.4	1.3	1.3	1.5	1.7
Pre-policy mean	7710	8096	8244	8187	7823
Total payroll (log)					
Treatment effect	0.005	-0.033	0.102	-0.039	0.123
p value	[0.850]	[0.738]	[0.186]	[0.686]	[0.108]
h	0.82	0.90	0.80	0.85	0.78
Pre-policy mean	12.1	12.1	12.1	12.2	12.1
Total hires					
Treatment effect	0.05	0.375	-0.196	-0.727	0.472
p value	[0.947]	[0.785]	[0.642]	[0.432]	[0.600]
h	1.50	1.14	1.38	1.11	1.37
Pre-policy mean	4.18	4.61	4.74	4.96	5.16
Total separations					
Treatment effect	-0.288	0.285	-0.143	-0.998	0.285
p value	[0.526]	[0.763]	[0.580]	[0.169]	[0.882]
h	1.32	1.26	1.06	1.11	0.94
Pre-policy mean	3.96	4.30	4.53	4.66	5.08
Job turnover					
Treatment effect	-0.017	0.001	0.004	-0.028	-0.002
p value	[0.275]	[0.923]	[0.864]	[0.093]	[0.686]
h	1.70	1.59	1.40	1.15	1.03
Pre-policy mean	0.02	0.00	0.01	0.01	0.00

Source: Author's analysis of Washington State UI program records
Notes: Each column displays firm-level RD point estimates, the p-value associated with the estimate, the bandwidth used to estimate the RD point estimate, and the pre-policy mean between 2010 and 2014. Treatment effects are estimated using a local linear estimator, weighted with a triangular kernel, and using bandwidths chosen by a data driven procedure such that that RD point estimate is MSE optimal. Sample excludes firms within 0.004 FTE employees. P-values for each estimate is displayed in brackets.

Table 2.3 also shows that there are no effects on firm-level outcomes in the 2011 learning and anticipation period. In 2012, the year the policy was enacted, firms just above the FTE cutoff experienced an increase in total hours by 479 hours per year (p-value = 0.06), which amounts to a 5.8 percent increase in that year. This effect disappears by the post-policy period of 2013 and 2014, however, suggesting a small and short-lived effect of the PSST policy. The total number of jobs, earnings, hires, separations, and job turnover of firms just above the FTE threshold were not impacted in the post-policy period as well. Except for firms' total hours worked in 2012, the PSST mandate did not affect firms' employment levels or flows.

WORKER-LEVEL RESULTS

Descriptive Statistics

Assessing treatment effects using a DiD design requires that workers who are covered by the PSST policy do not have systematically different employment outcomes than the comparison group of workers. To assess whether workers employed by firms on either side of the FTE employee threshold are systematically different, **Table 2.4** displays average pre-policy summary statistics for four sets of workers: covered and uncovered workers in firms with less than 250 FTE employees, termed the *"unrestricted"* set of firms (Panel A), and covered and uncovered workers in firms with an FTE employment size ranging from greater than three to five, termed the *"restricted"* set of firms (Panel B). In each Panel, covered workers are workers who had baseline quarter employment in firms with more than four FTE employees, and uncovered workers are workers who had baseline quarter employment in firms with four or fewer FTE employees. Each panel provides the mean and standard deviation for worker employment outcomes averaged across all pre-policy cohorts. The normalized differences of each employment outcome between covered and uncovered workers are subsequently displayed for each cohort to assess whether differences in employment outcomes change across cohorts.

Table 2.4. Summary Statistics for Seattle Workers in the 2011-2014 Longitudinal Cohorts

Panel A. Workers in the "unrestricted" set of firms: (FTE employment <250)

	Covered workers		Uncovered workers		Normalized Difference			
	Mean	Std. Dev.	Mean	Std. Dev.	2011 cohort	2012 cohort	2013 cohort	2014 cohort
Employment levels								
Probability of being employed	0.92	0.06	0.88	0.09	0.14	0.14	0.13	0.13
Quarterly hours worked	404	29	320	29	0.42	0.43	0.42	0.42
Quarterly earnings (log)	7.86	1.01	6.84	1.41	0.21	0.22	0.20	0.20
Employment flows								
Probability of being hired	0.11	0.01	0.14	0.02	-0.10	-0.10	-0.08	-0.09
Probability of separating	0.09	0.03	0.11	0.05	-0.07	-0.06	-0.06	-0.06
Job duration (truncated at 8)	7.37	0.18	7.16	0.31	0.13	0.15	0.13	0.14
Employment volatility								
Arc Percent Change (abs.) in hours	34.4	1.0	44.8	1.6	-0.18	-0.19	-0.17	-0.18
Likelihood of an hours increase	0.41	0.09	0.40	0.08	0.04	0.02	0.04	0.03
Likelihood of an hours decrease	0.10	0.03	0.13	0.03	0.06	0.08	0.07	0.07
Arc Percent Change (abs.) in earnings	37.3	1.3	46.8	2.0	-0.17	-0.18	-0.16	-0.17
Likelihood of an earnings increase	0.49	0.10	0.45	0.10	0.09	0.07	0.08	0.07
Likelihood of an earnings decrease	0.35	0.08	0.32	0.06	0.08	0.08	0.07	0.08

(continued on next page)

Table 2.4. *Continued*

Panel B. Workers in the "restricted" set of firms: (3< FTE employment ≤5)	Covered workers		Uncovered workers		Normalized Difference			
	Mean	Std. Dev.	Mean	Std. Dev.	2011 cohort	2012 cohort	2013 cohort	2014 cohort
Employment levels								
Probability of being employed	0.90	0.08	0.90	0.08	0.00	0.01	0.03	-0.01
Quarterly hours worked	345	29	337	28	0.06	0.05	0.04	0.02
Quarterly earnings (log)	7.23	1.23	7.15	1.25	0.01	0.02	0.03	-0.01
Employment flows								
Probability of being hired	0.13	0.01	0.13	0.01	0.01	-0.02	-0.03	0.01
Probability of separating	0.11	0.04	0.11	0.04	0.01	0.00	-0.01	0.01
Job duration (truncated at 8)	7.24	0.26	7.21	0.26	-0.01	0.03	0.04	0.00
Employment volatility								
Arc Percent Change (abs.) in hours	40.5	1.4	42.0	1.0	0.00	-0.04	-0.03	-0.01
Likelihood of an hours increase	0.41	0.08	0.40	0.07	0.01	0.01	0.01	0.00
Likelihood of an hours decrease	0.12	0.03	0.13	0.03	0.01	0.01	0.02	0.00
Arc Percent Change (abs.) in earnings	42.5	2.0	44.3	1.5	-0.01	-0.06	-0.04	-0.01
Likelihood of an earnings increase	0.47	0.10	0.47	0.10	0.00	-0.01	0.00	0.00
Likelihood of an earnings decrease	0.33	0.07	0.33	0.06	0.02	0.01	0.01	0.00

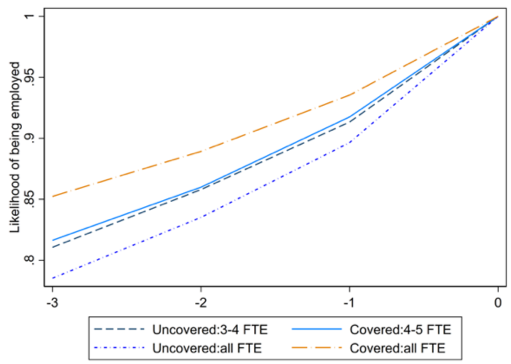
Notes: The table shows average mean and standard deviation for workers in their pre-policy period across all cohorts. Firms with an FTE employment size ranging from greater than one to less than 250 are termed the "unrestricted set" of firms, and workers in firms with an FTE employment size ranging from greater than three to five are termed the "restricted set" of firms. The average number of covered and uncovered workers in the unrestricted group is 598048 and 92964, respectively. The average number of covered and uncovered workers in the restricted group is 17,749 and 18460, respectively.

Like Seattle firms, employment outcomes are not comparable among covered and uncovered workers across the broad set of unrestricted FTE employment sizes. Covered workers in the unrestricted set of firms were 3.0 percentage points more likely to be employed, worked 84 more hours per quarter, and earned $1,658 more per quarter than workers in uncovered firms (normalized differences across cohorts range from 0.13–0.14, 0.42–0.43, and 0.20–0.22 standard deviations, respectively). By contrast, covered workers in the restricted set of firms were just 0.2 percentage points more likely to be employed, worked 7 hours more per quarter, and earned $106 more per quarter than uncovered workers in firms restricted to just one FTE employment size around the policy cutoff (normalized differences across cohorts ranges from -0.01–0.03, 0.02–0.06, and -0.01– 0.03 standard deviations, respectively).

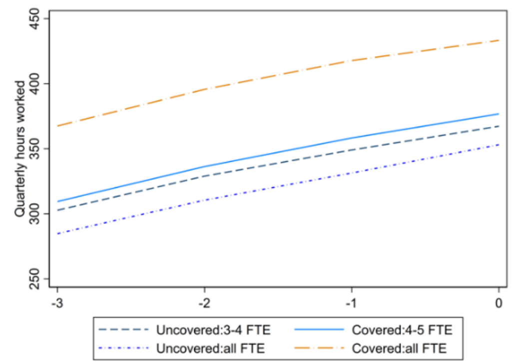
These differences in comparability between covered and uncovered workers persist across workers' employment flows and volatility outcomes. Covered workers in the unrestricted set of firms were 2.8 percentage points less likely to experience a hire, and 1.9 percentage points less likely to experience a separation. Consequently, these workers worked 0.2 quarters longer, have 5.2 percent lower levels of hours volatility, and 4.8 percent lower earnings volatility (normalized difference across cohorts ranges from 0.13–0.15; 0.17– 0.19; and -0.16–0.18 standard deviations, respectively). By contrast, covered workers in the restricted set of firms were 0.1 percentage points less likely to experience a hire, were equally likely to experience a separation, worked jobs of equal duration as uncovered workers, and consequently experienced only 1.3 percent less hours volatility, and 1.0 percent less earnings volatility (normalized differences across outcomes and cohorts range from 0.00–0.06 standard deviations). The differences in comparability between the unrestricted set of covered and uncovered workers in Panel A and the restricted set of covered and uncovered workers in Panel B highlight the importance of focusing on firms within a small range of the policy cutoff.

To assess whether differences in employment outcomes persist over time, **Figures 2.4a-l** show the pre-period quarterly trends in employment outcomes for uncovered and covered workers in the restricted and unrestricted set of firms averaged across all four cohorts of workers. Across the majority of figures, employment characteristics for uncovered and covered workers in the restricted set of firms had parallel pre-policy trends, relative to the trends in employment outcomes for uncovered and covered workers in the unrestricted sample (an exception is a worker's probability of being hired). Estimates focusing on workers in the restricted set of firms, for the majority of employment outcomes, will provide internally valid treatment effects.

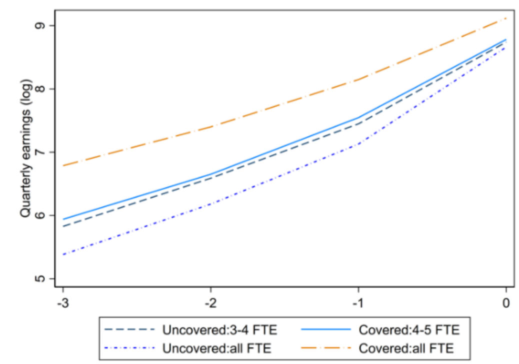
<u>Employment Levels</u>



(a) Likelihood of being employed

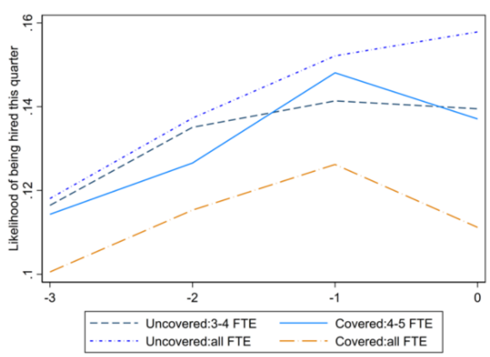


(b) Quarterly hours worked

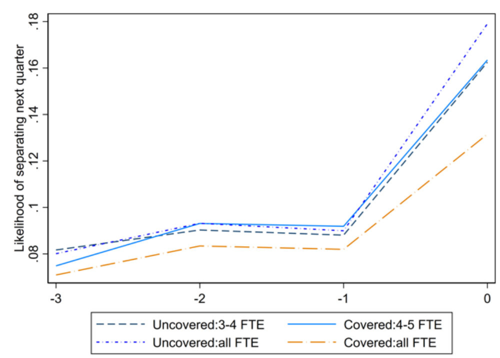


(c) Quarterly earnings

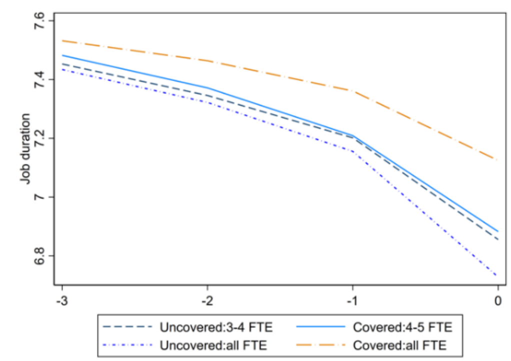
<u>Employment Flows</u>



(d) Likelihood of being hired this quarter



(e) Likelihood of separating next quarter

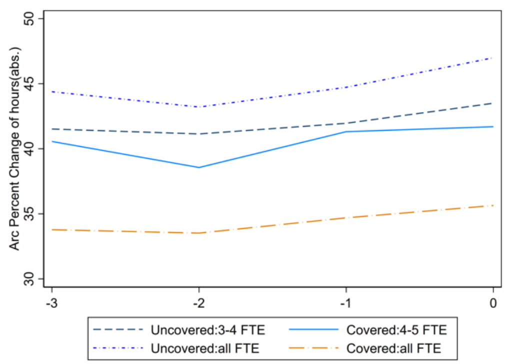


(f) Job duration

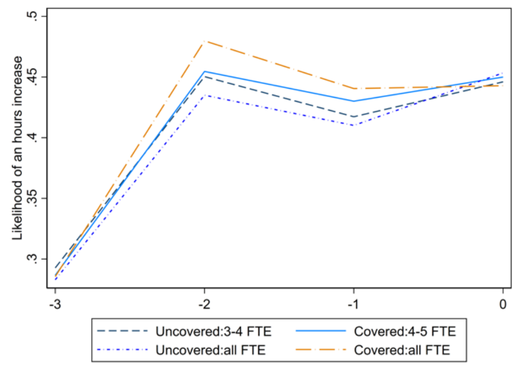
Figure 2.4. Quarterly trends in worker employment outcomes for the 2012 cohort of workers in the full FTE sample and for workers within a narrow bandwidth above and below the FTE threshold, 2011q1-2011q4.

(continued on next page)

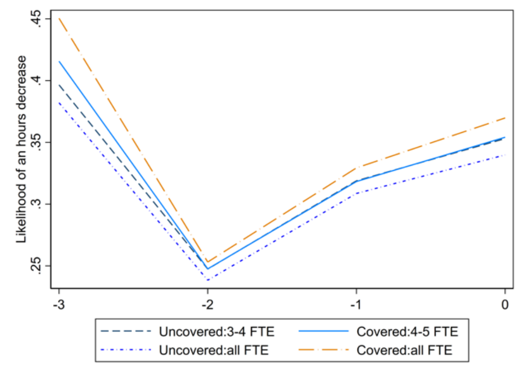
Employment Volatility



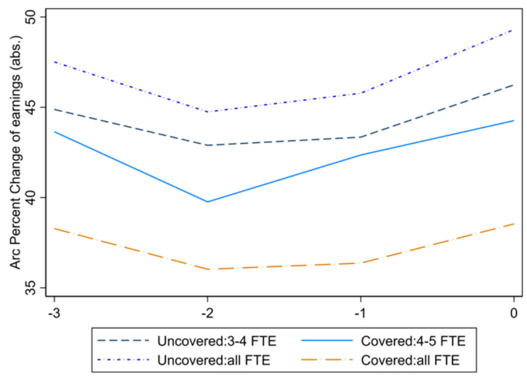
(g) Quarterly hours volatility



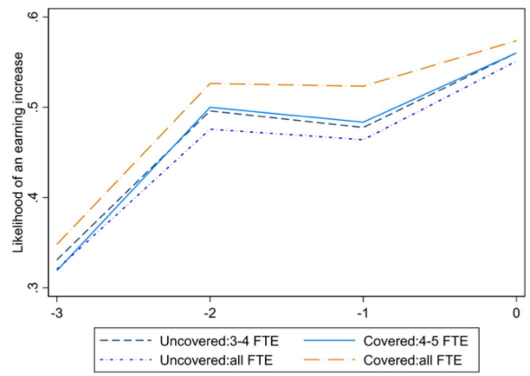
(h) Likelihood of hours increase



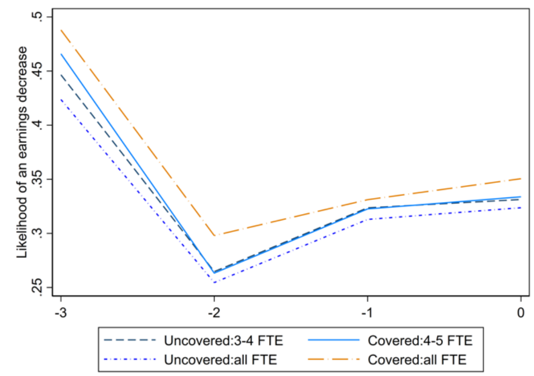
(i) Likelihood of hours decrease



(j) Quarterly earnings volatility



(k) Likelihood of earnings increase



(l) Likelihood of earnings decrease

Figure 2.4. *continued*

Source: Author's analysis of Washington State UI program records.
Notes: The figures show the average pre-policy trends in mean employment outcomes for workers in covered firms (yellow line) and uncovered firms (red line) that have an FTE employee size less than 250, and workers in covered firms (blue line) and uncovered (black line) firms that have an FTE employment sizes ranging from greater than three to five FTE employees. Quarterly averages are estimated across all four cohorts. Quarterly hours and earnings volatility is defined as the arc percent change, shown in Equation 2.4.

Treatment Effects

Table 2.5 displays the average treatment effects of the PSST policy on worker employment outcomes. Panel A shows results for employment level outcomes, Panel B shows results for employment flow outcomes, and Panel C displays results for employment volatility outcomes. A separate regression generates each point estimate. Each column displays the effects for each cohort during the years the PSST policy was passed (2011), subsequently enacted (2012), and the two post-policy years (2013 and 2014). All models include quarter and workers fixed effects.

Table 2.5. Treatment Effects for Worker-level Outcomes Using a Difference-in-differences Design, 2011-2014 Cohorts

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0.007	-0.004	-0.005	-0.003
	(0.006)	(0.006)	(0.005)	(0.006)
Quarterly hours worked	1.596	-3.862	-2.66	-2.537
	(2.737)	(2.647)	(2.657)	(2.894)
Quarterly earnings (log)	0.111	-0.07	-0.075	-0.037
	(0.09)	(0.087)	(0.086)	(0.095)
B. Employment Flows				
Likelihood of being hired this quarter	0.001	0.007*	0.006*	-0.006*
	(0.003)	(0.004)	(0.004)	(0.004)
Likelihood of separating next quarter	0	0.003	-0.001	-0.004
	(0.004)	(0.004)	(0.004)	(0.004)
Job duration (quarters)	-0.018	-0.016	-0.011	0.005
	(0.022)	(0.021)	(0.022)	(0.022)
C. Employment Volatility				
Arc percent change (abs.) in hours worked	-1.016	1.415	0.429	0.043
	(0.958)	(0.918)	(0.936)	(0.985)
Likelihood of an hours increase	0.001	-0.006	0.003	-0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Likelihood of an hours decrease	0.007	-0.003	-0.005	0.015***
	(0.005)	(0.005)	(0.005)	(0.005)
Arc percent change (abs.) in earnings	-0.356	1.912**	0.723	-0.012
	(0.941)	(0.905)	(0.92)	(0.967)
Likelihood of an earnings increase	0.007	0.011**	0.008	0.003
	(0.006)	(0.006)	(0.005)	(0.006)
Likelihood of an earnings decrease	-0.001	-0.002	-0.005	0.004
	(0.005)	(0.005)	(0.005)	(0.005)
Quarter FE	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	95,536	97,768	96,576	95,768
Persons	11,942	12,221	12,072	11,971

Source: Authors' analysis of Washington state UI program records.
Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

Panel A of Table 2.5 reveals that, like the impact estimates from the cross-sectional analysis of firms, workers in firms newly covered by the policy did not experience any statistically significant changes in their employment rate across all cohorts, respectively. Moreover, workers did not experience any statistically significant changes in their hours worked or in their log-earnings across all cohorts. These results suggest that the added labor cost of the PSST policy was not passed down to workers in the form of lower employment, hours worked, or earnings, and broadly confirm the cross-section firm-level results.

Panel B displays impact estimates on workers' employment flows. There was no change to workers' employment flows in the policy passage year, 2011, similar to firms. In the year the PSST policy went into effect, 2012, workers did experience a statistically significant change in their likelihood of being hired by 0.7 percent (statistically significant at the 0.1 percent level). In the post-policy cohorts, workers' likelihood of being hired continued to increase in 2013 by 0.6 percent but decreased by 0.6 percent in 2014 (both 2013 and 2014 estimates are statistically significant at the 0.1 percent level). Given the divergent parallel trends in worker's likelihood of being hired, caution should be applied in viewing these estimates as policy-relevant effects.

Panel C displays impact estimates on workers' employment volatility. Workers in the 2012 cohort experienced an increase in their quarterly earnings volatility by 1.92 units, which translates to a 0.96 percent change in workers' absolute value of their arc percentage change in earnings (statistically significant at the 0.05 percent level). Workers also experienced a commensurate increase in their likelihood of experiencing earnings increases of 0.01 percent (statistically significant at the 0.05 percent level). Taking these two results together suggests that workers' increased volatility is due to earnings mobility in the 2012 cohort. However, these results attenuate in later post-policy cohorts. In the 2014 cohort, workers were 1.5 percent more likely to experience an hours decrease (statistically significant at the 0.01 percent level), however, because this change in probability is not met with statistically significant impacts on the magnitude of workers' hours volatility, as estimated by the APC, the observed impacts in directionality appear to be too small to be policy significant.

The overall small or null results across the majority of employment outcomes for workers in Table 2.5 suggest that the PSST policy did not appear to affect workers' overall economic well-being as it relates to their employment behavior. Workers also did not experience a change in their duration or in their likelihood of separating, which curtails support to the notion that the PSST policy incentivized workers and employers to remain together for more extended periods. Workers experienced modest changes in their earnings volatility in the cohort during which the policy was enacted. However, these results do not persist into the post-policy cohorts. Taken together, these results lend credibility to hypotheses that the cost of PSST policy was not passed down to workers in the form of reduced employment. However, these results do not support the hypothesis that the Seattle PSST policy extended workers' employment duration or improved workers' employment stability. These results could be due to the small size of the policy benefit (1 hour of PSST accrued for every 40 hours worked), or due to potential attenuation in treatment effects from firms that provided paid sick leave to workers prior to the policy.

Subgroup Analysis

To assess the effect of the paid sick leave policy on employment groups that were less likely to have access to employer-provided paid sick leave before the PSST law, I re-estimate Equation 2.2 for two subgroups of workers: part-time workers and low-earning workers. These workers may experience more substantial effects than workers overall due to the potential increase in precision using this group as a treatment group. Alternatively, these workers may accrue less PSST relative to full-time workers, or be more susceptible to corrupt employment practices, such as wage theft, working portions of their job for cash, or less likely to know they have access to paid sick leave. As a result, they may not have experienced substantial employment changes as a result of the PSST law.

Part-time Workers

Table 2.6 displays difference-in-difference estimates for the subgroup of workers who work part-time hours. Panel A reveals that part-time workers did not experience any increase in precision or more substantial post-policy treatment effects in workers' employment, hours or earnings, relative to the overall sample. Panels B and C provide evidence that the PSST policy modestly affected part-time workers' employment flows or employment volatility. In the 2014 cohort, part-time workers' likelihood of being hired decreased nearly a whole percentage point more than for all workers (1.5 percent, statistically significant at the 0.01 percent level). Given the trends in Figure 2.4d, however, it is unclear if this estimate is internally valid. Panel C shows that the absolute value of APC of workers' hours and earnings decreased in 2011 cohorts, by 3.48 percent and 2.92 percent, respectively (estimates are statistically significant at the 0.05 percent and 0.1 percent level, respectively). However, these results attenuated in the post-policy cohorts. As with overall workers, part-time workers' likelihood of experiencing a decrease in their hours worked was statistically significant in the 2014 cohort and larger in magnitude, relative to the overall sample. Overall, the impacts of the PSST law for part-time workers appear to be very similar to the impacts for all workers.

Table 2.6. Treatment Effects for Worker-level Outcomes Using a Difference-in-differences Design, 2011-2014 Cohorts, Restricting Analysis to Part-time Workers

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0.014	-0.009	-0.003	-0.005
	(0.01)	(0.009)	(0.01)	(0.01)
Quarterly hours worked	4.606	-5.839	-3.28	-2.961
	(4.013)	(3.902)	(4.063)	(4.129)
Quarterly earnings (log)	0.212	-0.157	-0.043	-0.049
	(0.15)	(0.144)	(0.147)	(0.153)
B. Employment Flows				
Likelihood of being hired this quarter	-0.004	0.001	0.004	-0.015**
	(0.006)	(0.006)	(0.006)	(0.006)
Likelihood of separating next quarter	-0.003	-0.005	-0.003	0
	(0.006)	(0.006)	(0.006)	(0.007)
Job duration (quarters)	0.018	-0.012	0.004	-0.016
	(0.038)	(0.037)	(0.038)	(0.039)
C. Employment Volatility				
Arc percent change (abs.) in hours worked	-3.482**	1.918	-0.749	0.321
	(1.57)	(1.493)	(1.556)	(1.583)
Likelihood of an hours increase	-0.003	-0.003	-0.003	-0.014*
	(0.008)	(0.008)	(0.008)	(0.008)
Likelihood of an hours decrease	0.015*	-0.006	-0.004	0.020**
	(0.008)	(0.008)	(0.008)	(0.008)
Arc percent change (abs.) in earnings	-2.921*	2.509*	-0.339	-0.028
	(1.558)	(1.49)	(1.543)	(1.564)
Likelihood of an earnings increase	0.006	0.007	0.004	-0.004
	(0.008)	(0.008)	(0.008)	(0.009)
Likelihood of an earnings decrease	0.007	-0.01	-0.005	0.015*
	(0.008)	(0.008)	(0.008)	(0.008)
Quarter FE	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	46,800	48,520	46,688	46,224
Persons	5,850	6,065	5,836	5,778

Source: Authors' analysis of Washington state UI program records.
Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

The null results displayed in Table 6, in addition to providing evidence that the cost of the policy was not passed down to part-time workers, could be attributed to the design of the PSST policy. Part-time workers accrue fewer paid sick leave hours relative to their full-time counterparts. While part-time workers are less likely to have paid sick leave prior to the PSST policy, they also have a lower treatment intensity than their full-time counterparts. This lower treatment intensity could also be responsible for the null effects.

Workers with Low Earnings

Table 2.7 displays results for the workers who, in the baseline quarter of each cohort, earned in the bottom quartile of their cohorts' total quarterly earnings in the 2011 through 2014 cohorts. Table 2.7 shows that low-earnings workers experienced no change in their employment levels across all four cohorts. In the 2011 cohort, low-earnings workers experienced a decrease in their likelihood of being hired, however, this impact is not statistically significant in later cohorts. Low-earnings workers experienced similar changes to their volatility in hours worked as part-time workers did in the 2011 cohort (estimate for the APC in hours worked is -4.12 percent and statistically significant at the 0.1 percent level). This decrease in hours volatility was negative in directionality. Low-earnings workers experienced an increase in their likelihood of experiencing an hours decrease by 2.3 percent in 2011 (statistically significant at the 0.1 percent level). Beyond these anticipatory impacts, workers earning low wages experienced minimal employment changes in the post-policy cohorts as a result of the PSST policy, relative to the all-worker sample. Insignificant employment impacts for low-earnings workers are particularly surprising given that this subgroup should not suffer from a lower treatment intensity emblematic of part-time workers and were less likely to have access to paid sick leave (Clemans-Cope et al., 2008).

Table 2.7. Treatment Effects for Worker-level Outcomes Using a Difference-in-differences Design, 2011-2014 Cohorts, Restricting Analysis to Low-earnings Workers

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0.012	-0.005	0.003	-0.016
	(0.016)	(0.015)	(0.015)	(0.016)
Quarterly hours worked	4.609	-4.663	1.741	-2.971
	(5.516)	(5.417)	(5.422)	(5.508)
Quarterly earnings (log)	0.167	-0.073	0.041	-0.212
	(0.232)	(0.223)	(0.226)	(0.231)
B. Employment Flows				
Likelihood of being hired this quarter	-0.016**	-0.001	-0.004	-0.013
	(0.008)	(0.008)	(0.008)	(0.008)
Likelihood of separating next quarter	-0.007	-0.005	-0.007	0.003
	(0.01)	(0.009)	(0.01)	(0.01)
Job duration (quarters)	0.065	-0.02	0.021	0
	(0.061)	(0.061)	(0.06)	(0.06)
C. Employment Volatility				
Arc percent change (abs.) in hours worked	-4.126*	-0.455	-0.151	0.02
	(2.485)	(2.344)	(2.427)	(2.413)
Likelihood of an hours increase	-0.023*	-0.004	0.007	-0.01
	(0.012)	(0.012)	(0.012)	(0.012)
Likelihood of an hours decrease	0.023**	-0.002	0.005	0.021*
	(0.012)	(0.012)	(0.012)	(0.012)
Arc percent change (abs.) in earnings	-3.819	1.111	0.1	-0.105
	(2.461)	(2.336)	(2.397)	(2.379)
Likelihood of an earnings increase	-0.017	0.008	0	-0.012
	(0.012)	(0.012)	(0.012)	(0.012)
Likelihood of an earnings decrease	0.017	-0.005	0.004	0.023*
	(0.012)	(0.012)	(0.012)	(0.012)
Quarter FE	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	23,880	24,440	24,144	23,936
Persons	2,985	3,055	3,018	2,992

Source: Authors' analysis of Washington state UI program records. Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

There are several possible explanations for the insignificant treatment effects. Employers may have been able to handle the cost of the policy in such a way that it did not lead to employment changes. Another explanation comes from qualitative research on Seattle's PSST policy, which found that some workers in low-wage jobs did not know about the policy or may have had access withheld from their employer. In an evaluation for the City of Seattle, researchers interviewed workers newly eligible for the paid sick leave benefit and found 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 (Romich et al., 2014). Although this is not a generalizable finding, it nonetheless highlights how policy knowledge may affect treatment effects in workers' employment outcomes in response to public policy.

Sensitivity Analysis

Robustness: Legal Exemptions

Both worker- and firm-level results may be sensitive to the inclusion of industries that lobbied for an exemption to allow workers to swap their work shifts instead of using paid sick time. To assess the sensitivity of the inclusion of the industries that were most likely to swap work shifts, I re-estimate Equation 2.1 and Equation 2.2 for firms and workers excluding the food and restaurant industry and the construction industry. **Table 2.8** and **Table 2.9** display the results for firms and workers, respectively. Table 2.8 shows that treatment effects for firms absent those in the food and restaurant and construction industry mimic those for firms overall in Table 2.3. The main results in Table 2.3 are robust to this sample exclusion.

Table 2.8. Local Average Treatment Effects on Firm-level Outcomes Using a Regression Discontinuity Design, 2010-2014

	(1) Pre-policy	(2) Policy passed	(3) Policy enacted	(4) Post-policy	(5)
	2010	2011	2012	2013	2014
Number of jobs					
Treatment effect	0.316	0.193	-0.207	-1.289	-0.595
p value	[0.666]	[0.949]	[0.625]	[0.227]	[0.435]
h	1.204	0.989	0.989	1.029	0.86
Pre-policy mean	8.23	8.95	9.09	9.23	9.68
Total hours worked					
Treatment effect	314	387	453	-983	-143
p value	[0.249]	[0.309]	[0.086]	[0.342]	[0.86]
h	1.09	1.29	1.04	1.28	1.71
Pre-policy mean	7851	8158	8271	8256	7748
Total payroll (log)					
Treatment effect	0.064	0.052	0.123	-0.074	0.124
p value	[0.381]	[0.53]	[0.134]	[0.478]	[0.105]
h	0.875	0.897	0.742	0.847	0.838
Pre-policy mean	12.2	12.2	12.2	12.3	12.2
Total hires					
Treatment effect	0.315	0.289	-0.224	-0.913	-0.321
p value	[0.559]	[0.874]	[0.586]	[0.347]	[0.597]
h	1.392	1.02	1.084	1.056	0.981
Pre-policy mean	3.44	3.95	3.97	4.25	4.79
Total separations					
Treatment effect	-0.183	0.081	-0.541	-1.016	-0.36
p value	[0.536]	[0.995]	[0.287]	[0.209]	[0.516]
h	1.202	1.076	0.983	1.094	0.782
Pre-policy mean	3.32	3.54	3.77	4.00	4.57
Job turnover					
Treatment effect	-0.031	-0.004	-0.011	-0.03	0.005
p value	[0.12]	[0.884]	[0.475]	[0.079]	[0.999]
h	1.353	1.732	1.565	1.13	1.012
Pre-policy mean	0.01	0.00	0.01	0.01	0.00

Source: Author's analysis of Washington State UI program records
Notes: Each column displays firm-level RD point estimates, the p-value associated with the estimate, the bandwidth used to estimate the RD point estimate, and the pre-policy mean between 2010 and 2014. Treatment effects are estimated using a local linear estimator, weighted with a triangular kernel, and using bandwidths chosen by a data driven procedure such that that RD point estimate is MSE optimal. Sample excludes firms within 0.004 FTE employees. P-values for each estimate is displayed in brackets.

Table 2.9. Treatment Effects for Worker-level Outcomes Using a Difference-in-differences Design, 2011-2014 Cohorts, Excluding Food and Construction Industries

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0.012*	0.001	-0.005	0
	(0.006)	(0.006)	(0.006)	(0.007)
Quarterly hours worked	3.46	-2.94	-2.435	-1.787
	(3.077)	(2.996)	(2.97)	(3.287)
Quarterly earnings (log)	0.188*	-0.001	-0.076	0.007
	(0.1)	(0.096)	(0.095)	(0.106)
B. Employment Flows				
Likelihood of being hired this quarter	0.003	0.008**	0.001	-0.005
	(0.004)	(0.004)	(0.004)	(0.004)
Likelihood of separating next quarter	0.001	0.005	-0.007*	0.002
	(0.004)	(0.004)	(0.004)	(0.004)
Job duration (quarters)	-0.033	-0.027	0.018	-0.013
	(0.022)	(0.023)	(0.023)	(0.024)
C. Employment Volatility				
Arc percent change (abs.) in hours	-2.113**	1.333	0.423	-0.425
worked	(1.043)	(1.005)	(1.017)	(1.085)
Likelihood of an hours increase	0.002	-0.003	-0.002	0.002
	(0.006)	(0.006)	(0.006)	(0.006)
Likelihood of an hours decrease	0.010*	-0.003	-0.002	0.016***
	(0.006)	(0.006)	(0.006)	(0.006)
Arc percent change (abs.) in earnings	-1.392	1.901*	0.68	-0.365
	(1.022)	(0.99)	(0.998)	(1.061)
Likelihood of an earnings increase	0.011*	0.014**	0.008	0.008
	(0.006)	(0.006)	(0.006)	(0.006)
Likelihood of an earnings decrease	0	-0.002	-0.006	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Quarter FE	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	74,960	76,272	76,304	68,235
Persons	9,370	9,534	9,538	9,274

Source: Authors' analysis of Washington state UI program records.
Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort’s baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

Table 2.9 estimates the effect of the PSST policy for each cohort of Seattle workers, excluding workers in the food and beverage and construction industry. Workers experienced slightly different employment outcome changes in the 2011 and 2012 cohorts, relative to the impact estimates for workers in Table 2.5. Workers in 2011 experienced an increase in their likelihood of being employed by 0.12 percent and an increase in their log payroll by 18.8 percent. However, these results attenuate in the post-period cohorts (estimates are statistically significant at the 0.1 level). Workers also experienced a decrease in their hours volatility by 1.1 percent, which was coupled with a statistically significant increase in their likelihood of experiencing an hours decrease by 0.1 percent (estimates are statistically significant at the 0.05 percent level and the 0.1 percent level, respectively). In contrast to results in Table 2.5, workers in the 2013 cohort experienced a decline in their likelihood of separating by -0.7 percent (statistically significant at the 0.1 percent level). These estimates did not show up in the 2014 post-policy cohort of workers. Overall, Table 2.9 suggests that excluding workers in industries most likely to shift swap has modest, short-term effects on workers' employment levels, flows, and volatility.

Bandwidth Analysis

To test the sensitivity of the bandwidths used in analysis to estimate the treatment effects in Table 2.3, **Appendix A Figure 2a-f** shows treatment effects for each firm-level outcome in the post-policy years of 2013 and 2014 at various bandwidth sizes. As the bandwidths used to employ the density test range between $h = 0.47$ and $h = 0.50$, and bandwidths used to estimate treatment effects range from $h = 0.78$ to $h = 1.7$, the figure shows treatment effects for firms in bandwidths ranging from $h = 0.25$ to $h = 1.75$. In general, as the bandwidth increases from $h = 0.25$ to $h = 1.75$, the estimates become more precise and trend toward statistical insignificance. Appendix A

Figures 2a-f show that firm-level regression discontinuity results across a wide variety of bandwidth estimates are consistent with the main results in Table 2.3.

To test the sensitivity of the bandwidth used in analysis to estimate the treatment effects in Table 2.5, **Appendix A Figure 3a-l** shows treatment effects for each worker-level outcome in the post-policy years of 2013 and 2014 for bandwidth sizes ranging from $h = 0.25$ to $h = 1.75$. Similar to the firm-level bandwidth analysis, as the bandwidth increases from $h = 0.25$ to $h = 1.75$, the estimates become more precise and trend toward statistical insignificance. Estimates on workers' employment levels are robust to bandwidth choice. Estimates on workers' employment flows are more sensitive to bandwidth choice. Workers in firms with an FTE employee size that is within a bandwidth less of less than 1 FTE employee experienced statistically significant decreases in their likelihood of being hired and separating, which trend towards zero and are insignificant at larger bandwidths. These results suggest that workers in firms very close to the FTE threshold may have made employment adjustments firms as a result of the PSST law. The bandwidth choice for the majority of workers' hours and earnings volatility outcomes does not appear to affect the robustness of impact estimates in Table 2.5. Workers' likelihood of experiencing a decrease in their hours worked remains robust across bandwidth levels.

Worker Transitions

To assess the robustness of the worker-level results in Table 2.5 to the inclusion of worker transitions outside Seattle or into a different coverage category, **Appendix A Table 1 and Appendix A Table 2** display impact estimates of the PSST policy using the set of workers who remain employed in Seattle (Appendix A Table 1), and the set of worker who remain in their original treatment and comparison category (Appendix A Table 2). Appendix A Table 1 shows that restricting the set of workers to the 92 percent of workers who remain employed in Seattle did not change the statistical significance or the directionality of the trends in treatment effects. Exceptions in significance include the APC in hours for workers, which increased by 1.8 percent in the 2012 cohort. These changes are small and suggest using that using employment information for workers who transition outside of Seattle does not attenuate results in a policy meaningful way.

Appendix A Table 2 estimates the impact of the paid sick leave policy on the set of workers who remain in a firm with their pre-policy FTE employment size. This group is a smaller subset of workers. On average, only 30 percent of workers remain employed in a firm with their pre-policy FTE employment size. Workers who remain covered firms with an FTE employment size between greater than four and five experience a decrease in their likelihood of being employed by 0.9 percent and in their earnings by 0.87 dollars per quarter in the early post-policy cohort, 2013 (statistically significant at the 0.05 percent and 0.1 percent level, respectively). These effects are insignificant in the 2014 cohort. Panel B and Panel C show the effect of the paid sick leave policy on workers' employment flows and volatility. Panel B shows that the precision of workers' likelihood of becoming hired in 2011 through 2013 decreased, but workers experienced an increase in their employment duration by 0.31 quarters in 2013 (statistically significant at the 0.1 percent level). Worker's job duration, similar to the employment and earnings levels impacts, became insignificant in the later post-policy cohort, 2014. Workers experience no change in the magnitude or the directionality of their earnings or hours volatility. Appendix A Table 2 suggests that covered workers who remain within their pre-policy FTE employment size experienced minor employment changes in their employment levels and flows in the anticipation and early phase-in periods of the PSST policy, but these changes appear to be short-lived.

Discussion and Policy Implications

In 2012, the City of Seattle became a leader in local workplace policy by enacting the second local paid sick leave law in the United States. The law mandated that all workers could earn one hour of paid sick time for every 30-40 hours worked, extending coverage to employment groups less likely to have employer-sponsored benefits, such as part-time and temporary workers. As a public health policy, the PSST ordinance was intended to reduce exposure to infectious disease and, in turn, cultivate healthier, more productive workers. These public health benefits have the potential to increase worker's economic security: healthier, more productive workers are more likely to be employed, have longer job duration, lower job turnover, and lower earnings and hours volatility. As an employer mandate, however, there may be consequences on workers' employment outcomes if employers accommodate the added cost of the benefit by reducing employment, earnings, or employment growth. This study contributes cross-sectional and longitudinal evidence on firms and workers to assess whether the PSST policy affected employment outcomes using administrative data from Washington State's Employment Security Department.

To examine the response of firms, I use a regression discontinuity strategy and estimate local average treatment effects for firms right above the FTE threshold mandated to provide leave. The regression discontinuity design allows for careful examination of whether firms right above the FTE coverage threshold manipulated their FTE employee size to remain uncovered, which the evidence shows does not appear to have happened. Firms experience a modest increase in hours worked and no change in employment, earnings, hires, separations or job turnover, consistent with the research of Pichler and Zeibarth (2018) and supportive of theory in which the cost of a paid sick leave employment mandate is not so large that it hinders firms' employment or growth.

To examine the effects on workers, I use a difference-in-differences strategy, which longitudinally compares workers employed in firms right above the FTE threshold to workers right below the threshold to assess whether workers' employment levels, flows, or volatility were affected by the PSST law. Workers newly covered by the PSST mandate experienced no change in their likelihood of remaining employed, their hours worked, or their quarterly earnings in each of the four cohorts studied (2011-2014 cohorts), which contrasts with the results found by Ahn and Yelowitz (2015). Critically, these results persist even in the investigation of policy impacts for part-time and workers with low-earnings. Workers in firms just above the threshold of eligibility did experience a modest increase in their likelihood of being hired in the policy passage and early post-policy cohorts of 2012 and 2013. However, these results, which reverse in the later post-policy cohort of 2014, may not be internally valid. In the 2014 cohort, workers were 1.5 percent more likely to experience an hours decrease, however, because this change in probability is not met with statistically significant impacts on the magnitude of workers' hours volatility, as estimated by the APC, the observed impacts in directionality appear to be too small to be policy significant. These results on employment flows and volatility persist for part-time workers and workers with low earnings in the post-policy period, as well.

Taken together, the results suggest the cost of the policy may be too small to impact the employment outcomes of affected firms and workers right around the threshold for coverage. Alternatively, the null results from this analysis could occur for several reasons relating to compliance, implementation or the analytic method. Firms with many part-time roles may unwittingly confuse whether or not they provide paid sick leave if they miscount their hours. This could lead to issues of noncompliance (even if by mistake). Firms very close to the 4FTE threshold may be particularly likely to confuse their status, if for example, they round to the whole number

FTE size, rather than use their exact FTE size. Future research could test the sensitivity of this by assessing treatment effects for firms with full-time workers only. With respect to implementation, newly covered workers may not know about the PSST law or that they have access to paid sick leave time. Previous research workers' knowledge of policy suggests that many workers earning low wages do not know their rights at the time newly mandated wages and benefits are enacted (H. Hill & Wething, 2019). Moreover, City of Seattle researchers interviewed workers newly eligible for the paid sick leave benefit and found 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 (Romich et al., 2014). As this confusion may be particularly likely for workers near the threshold, the null results could also be driven by the choice to use a regression discontinuity or the sample of workers and firms right around the threshold.

Beyond the compliance and implementation of the policy, the null results could be driven by the design of the policy. The policy benefit is small and may be too small to be observed in the year or two following the ordinance (1 hour of PSST accrued for every 40 hours worked). One can also not rule out potential attenuation in treatment effects from firms that provided paid sick leave to workers prior to the policy. With respect the volatility measures, the UI data collection period at quarterly intervals may not capture month-to-month or week-to-week volatility.

As this evaluation is for a single local policy in a metropolitan area with a strong economy, these policy effects may not generalize to state or national paid sick leave policies. In presenting new evidence on firm and workers' employment outcomes as a response to paid sick leave laws, policymakers should consider the full range of pathways they can use to support workers' economic security as they determine their policy agendas. The evidence here is suggestive that paid sick leave policies may not be too costly to implement and could potentially be strengthened to provide more generous leave for workers in the future.

Chapter 3. THE EFFECTS OF MINIMUM WAGE ORDINANCES ON EMPLOYMENT FLOWS AND HOURS VOLATILITY IN LOW-WAGE JOBS

Seattle is at the frontier of local workplace regulations. In April of 2015, Seattle enacted a $15 Minimum Wage Ordinance. The Minimum Wage Ordinance, the first in a national policy push to raise the living wages of cities and states to $15 per hour, was passed with the intent to "advance workplace equity for all Seattle workers, including…historically disadvantaged communities who are disproportionately represented among low-income workers" (Wage Theft Prevention Ordinance, 2014).[30] In an effort to evaluate the efficacy of the Ordinance's intent, this paper estimates the effect of the raising Seattle's minimum wage on the employment flows of low-wage jobs in Seattle. Specifically, I ask 1) What is the impact of the Seattle Minimum Wage Ordinance on employment flows (hires, separations, and turnover) in the low-wage labor market? 2) What is the impact of the Ordinance on the volatility in hours worked among continuing jobs? Seattle was the first in a wave of cities, not just to raise its minimum wage, but to raise its minimum wage *to nearly 60 percent* of its pre-policy wage of $9.47 over a period ranging from three-seven years (this paper covers the first two phase-ins, a 37.3 percent increase). Evaluation of Seattle's Minimum Wage Ordinance (MWO) will inform policymakers across the country on how large increases in minimum wages may affect the employment flows in their jurisdiction.

While an extensive literature on minimum wage policy has estimated the impact of higher wages on the wages, hours, and employment levels of affected populations (Belman & Wolfson,

[30] Among other cities to legislate minimum wages are San Francisco and Oakland, CA, Chicago, IL, Santa Fe, NM, New York City, NY, Newark, NJ, Minneapolis, MN, Los Angeles, CA, and San Diego, CA (National Partnership for Women and Families, 2015).

2014; Neumark & Wascher, 2008), the effect of a minimum wage increase on employment flows, such as job turnover, hires, and separations is a relatively underexplored topic in the minimum wage literature. The few studies that have investigated the impact of a minimum wage policy on employment flows have used proxies for the low-wage labor market, such as teenagers, or restaurant workers to study state and federal wage policies (Brochu & Green, 2013; Dube et al., 2016a; Gittings & Schmutte, 2016; Meer & West, 2016). These studies found that minimum wage policies had a "chilling effect," reducing the rate of employment flows (reductions in hires and separations) to a larger degree than corresponding reductions in employment levels among affected populations.

Local minimum wages might have different effects than state or federal minimum wages. Because local labor markets are more open, relative to states and national economies, employers have more flexibility to avoid the regulation by moving their business across city lines. Workers also have more outside options in a local labor market: if raising the minimum wage leads to employers utilizing precarious scheduling practices, workers may seek employment elsewhere. The few surveys on minimum wage policies at the local level have included questions about one or more dimensions of a jurisdiction's employment flows. However, these surveys were often restricted to specific industries, and an evaluation of employment flows were not the main focus. Analysis of these surveys found evidence for an increase in job tenure and employee retention, and a decrease in job turnover in response to minimum wage or "living wage" laws (Dube et al., 2007; Fairris et al., 2015; Howes, 2005). To date, the question of how local minimum wages affect a wide variety of employment flows metrics, such as hires and separations, in addition to job tenure and turnover, for a broad swath of the low-wage labor market remains unanswered.

Minimum wage policy may also have implications for the variability of work hours on the job. Evidence from the retail and food and accommodation industries shows that low-wage jobs are disproportionately likely to be subjected to “just-in-time” scheduling practices, such as little advance schedule notice, fluctuating work hours, and little to no schedule control, that create volatility in hours worked (Harknett et al., 2017; Henly & Lambert, 2014; Lambert et al., 2014). These precarious employment practices have been shown to have negative impacts on the health and well-being of workers in low-wage jobs (Schneider & Harknett, 2019). In response to the MWO, employers may increase their use of "just-in-time" scheduling as a means of creatively cutting costs, leading to greater volatility in hours worked within jobs. Alternatively, if employers are able to handle the costs of a higher minimum wage, workers may respond with an increased attachment to Seattle employers through longer employment duration and fewer quits. These behaviors could have a stabilizing effect in low-wage jobs through reductions in hours volatility and in large drops in hours worked, if the variation occurs at the quarterly level. In this paper, I integrate this new wave of research on employment instability into the policy evaluation framework in order to provide a comprehensive evaluation of the MWO.

Using Unemployment Insurance (UI) records from Washington State, I investigate the effect of Seattle’s Minimum Wage Ordinance on job flows for the entire low-wage labor market. These data include a firm’s location and quarterly hours and earnings information of every employee-employer match eligible for UI. This study evaluates the first two phase-ins of the Minimum Wage Ordinance during which the minimum wage rose from $9.47 to $13 per hour for large employers (>500 employees) and to $11 per hour for small businesses (≤500 employees). The quarterly microdata allow me to precisely estimate wages for each job in firms, geocoded to their latitude-longitude coordinates, thereby constructing a non-proxied treatment and control group of low-wage jobs. I identify quarterly jobs, hires, and separations, and from these data, construct a measure of job turnover. I further use the quarterly hours data to estimate two measures of volatility in hours worked. I estimate the impact of the minimum wage on employment flows using two innovative methods: synthetic-control and interactive-fixed-effects estimation.

In doing so, I make three contributions to the literature: First, I demonstrate the utility of using administrative data in local policy evaluation. This represents a major improvement on prior studies of minimum wages and employment dynamics, which used age or industry as a proxy for workers in low-wage jobs and on prior studies of local policy, which have approximated localities by their respective county. Second, I contribute evidence of the impact of a local minimum wage ordinance on a broad range of employment flows. As cities and counties are enacting minimum wage ordinances throughout the country, an in-depth evaluation of the first city to raise its minimum wage up to $15 per hour will provide critical feedback for local policymakers around the country. Third, I contribute new knowledge on the effects of an employment policy on a job's hours volatility. I am able to estimate quarterly volatility in hours worked within a consistent job—a key form of volatility in low-wage employment. Chapter 1 of this dissertation shows that volatility within a job accounts for nearly one-third of the earnings volatility experienced by workers earning low wages. As such, understanding how public policy mitigates or exacerbates employment stability in low-wage work is crucial to understanding the pathways towards creating economic security for workers employed in low-wage jobs.

I find evidence that separations and hires in the low-wage labor market declined following the enactment of the minimum wage, with the most precise reductions occurring during the period in which the minimum wage was raised from $11 to $13 per hour. Year-over-year hires fell by a range of 12.2 to 19.3 percent in response to a maximum of a 37-percent increase in the statutory minimum wage. This translates into an elasticity in hires ranging from -0.32 to -0.52 when the denominator is the statutory increase in the minimum wage ($9.47-$13 per hour). The decline in hires was met with a less precise, but persistent decline in separations (statistically significant coefficients show an elasticity of -.31), and a decline job turnover (statistically significant coefficients display an 8.5 percent decrease) among low-wage jobs in Seattle, relative to the comparison groups.

The elasticity estimates of hires and separations found in this study are higher than elasticity estimates found in prior research, which found estimates for hires and separations elasticities ranging between -.23 and -.37 among teenagers and workers in the restaurant industry (Brochu & Green, 2013; Dube et al., 2016a; Portugal & Cardoso, 2006). The larger magnitude of my findings may be due to the local context, or because of the improvement in data quality from the use of administrative data. Previous research by Jardim et al. (2020) used the same UI program record data and found declines in employment levels in response to the Seattle Minimum Wage Ordinance that were larger than what had been previously observed in state and federal evaluations. My study furthers the notion that local labor markets may have more acute reactions to minimum wage laws, relative to state or federal minimum wage laws.

I further find that jobs that continued to exist in the post-policy period exhibited statistically significant increases in hours volatility. The significant estimates ranged from 1.1 to 2.8. The number of large (>25 percent) hours declines within a job were negative in directionality throughout the entire period studied, and these estimates became statistically significant in the quarter following the mandated increase to $13 per hour. The timing of this coincides with the decline in separations, indicating that jobs which continued to exist may be less volatile as a result of the MWO.

SEATTLE MINIMUM WAGE ORDINANCE

In June 2014, the City of Seattle passed the Seattle Minimum Wage Ordinance, which increased the minimum wage up to $15 per hour within Seattle's city limits for all workers who perform work in Seattle. The phase-in rate differed by employer size and employee benefits. Firms with more than 500 employees or firms that didn't provide benefits were mandated to raise wages at a faster rate than firms with 500 or fewer employees or firms that did provide benefits. **Table 3.1**, reproduced from Jardim et al. (2020), shows that the minimum wage rose from $9.47 per hour to as high as $11 per hour in April of 2015, and to as high as $13 per hour in January of 2016. This paper covers the first and second phase-in periods of the Seattle Minimum Wage Ordinance, during which the minimum wage rose from $9.47 to $13 for businesses with 500 or more employees worldwide – a 37.3 percent increase. In January of 2017, Washington state raised its minimum wage to $11 per hour, and as such, the analysis stops one quarter prior to Washington state's increase. **Figure 3.1 (Jardim et al., 2017)**, shows the distribution of quarterly hours worked across one-dollar wage bins in the second quarter of 2014, the quarter the Ordinance was passed, the distribution one year later, when $11 per hour minimum wage was implemented (orange line), and the distribution two years later, the second quarter of 2016, one quarter after the $13 minimum wage was implemented (blue line). The histogram shows that the distribution of low-wage hours shifts to higher wage levels as the minimum wage changes, corresponding to the Ordinance wage threshold changes.

Table 3.1. Minimum Wage Schedule in Seattle under the Seattle Minimum Wage Ordinance

	Large Employers[a]		Small Employers	
Effective Date	No benefits	With benefits[b]	No benefits or tips	Benefits or tips[c]
Before Ordinance				
1-Jan-15	\$9.47	\$9.47	\$9.47	\$9.47
After Ordinance				
1-Apr-15	\$11.00	\$11.00	\$11.00	\$10.00
1-Jan-16	\$13.00	\$12.50	\$12.00	\$10.50
1-Jan-17	\$15.00[d]	\$13.50	\$13.00	\$11.00
1-Jan-18		\$15.00[e]	\$14.00	\$11.50
1-Jan-19			\$15.00[f]	\$12.00
1-Jan-20				\$13.50
1-Jan-21				\$15.00[g]

Source: Author's analysis of Washington state UI program records.
Notes:
a A large employer employs 501 or more employees worldwide, including all franchises associated with a franchise or a network of franchises.
b Employers who pay towards medical benefits.
c Employers who pay toward medical benefits and/or employees who are paid tips. Total minimum hourly compensations (including tips and benefits) is the same as for small employers who do not pay towards medical benefits and/or tips.
d For large employers, in the years after the minimum wage reaches \$15.00 it is indexed to inflation using the CPI-W for Seattle-Tacoma-Bremerton Area.
e Starting January 1, 2019, payment by the employer of medical benefits for employees no longer affects the hourly minimum wage paid by a large employer.
f After the minimum hourly compensation for small employers reaches \$15 it goes up to \$15.75 until January 1, 2021 when it converges with the minimum wage schedule for large employers.
g The minimum wage for small employers with benefits or tips will converge with other employers by 2025.

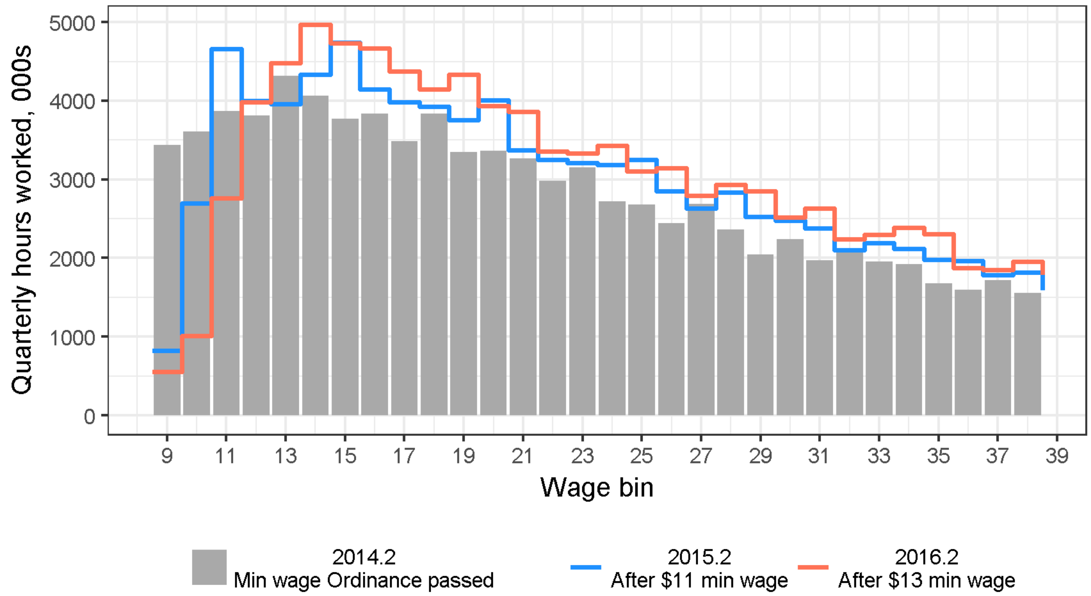


Figure 3.1. Distribution of hours worked in Seattle during the Seattle Minimum Wage Ordinance passage and phase-in periods.

Source: Reproduced from Jardim et al. (2017)

THEORY AND CONTRIBUTING LITERATURE

Search and matching models from economics theorize that minimum wages can have opposing effects on an economy's labor market. While higher minimum wages have the potential to increase labor costs, they also increase the gap between the expected utility of employment relative to unemployment for job seekers. These two forces can have offsetting effects on employment flows. On the one hand, higher minimum wages can increase the search effort among the unemployed and expand the number of unemployed looking for work. This expansion can increase the pool of potential job applicants and can improve the quality of matches between employers and employees. In this scenario, there would be no change in hires or potentially even an increase. On the other hand, the higher costs associated with an increase in the minimum wage may lead to a reduction in hires. This may be particularly true if there are additional costs of hiring and training a new employee beyond paying a higher wage (Oi, 1962). Empirical evidence shows that minimum wage workers are disproportionately likely to be recent hires (Even & Macpherson, 2003), and thus may be the group to bear the brunt of the cost in terms of reduced opportunity. Further, Jardim et al. (2020) descriptively evaluated changes in new entrants to the low-wage labor market in response to the Seattle Minimum Wage Ordinance. They found a 10-percent decline in new entrants relative to the comparison group at the same time as the phased-in increases occurred, which provides support for the notion that the proportion of hires made up of new entrants will decrease in response to the MWO.

Search and matching models also have ambiguous predictions on the impact of minimum wage policies on separations. In the job-ladder search model (Burdett and Mortenson, 1998), workers are continuously searching for a job. A higher minimum wage may lead to better job offers or to job transitions to more preferred types of low wage jobs, which can reduce quits as workers become more satisfied with their current job (Dube et al., 2016a). If search costs exist and minimum wage increases lead to improved quality of employer-employee matches, employers may find it more costly to recruit a worker with firm-specific knowledge. As a result, layoffs may decrease as well. Workers with firm-specific knowledge who now earn higher wages, in turn, may also be less likely to quit at their new higher wage (Hamermesh, 1987). Beyond the theory of search and matching models in economics, psychological factors, such as the negative feelings managers have when terminating employees like stress or remorse, may make managers are less likely to terminate an employee in the face of higher minimum wages (Gilbert, 2000).

The empirical research on employment flows is limited, although consistent in its evidence: Increases in state and national minimum wages reduced job flows (Dube et al., 2016a; Dynarski et al., 1997; Gittings & Schmutte, 2016; Portugal & Cardoso, 2006). Dube, Lester, and Reich

(2016) used data from the Quarterly Workforce Index to assess the impact of minimum wages on teenagers and restaurant workers in the U.S. and find that separations and accessions fall among affected workers, especially those with short tenure. Internationally, Portugal and Cardozo (2010) found that the share of teenagers separating from jobs decreased as companies decreased the share of teenagers among their newly hired workforce in response to Portugal's youth minimum wage increase. Brucho and Greene (2013) studied Canadian minimum wage laws and found a reduction in job separation rates due to reduced layoffs for workers in low-skill jobs. They also find a decline in hiring rates and conclude that jobs in higher minimum wage regimes may be more stable but harder to obtain. Gittings and Schmutte (2015) examine various labor market regimes and find that minimum wages can lead to employment declines when the labor market is characterized by short nonemployment durations and low-job turnover.

There is reason to think that a local minimum wage policy may behave differently than the state and federal policies. Local ordinances cover much smaller geographic areas than state and federal ordinances, and the economies of localities have a larger degree of embeddedness within their larger metropolitan region, relative to state and federal economies. If local employment policies increase labor costs to a detrimental level, employers can move the business out of the city's limits or shift business to locations outside the city for a lower cost than shifting businesses across state lines. If this occurs, employment flows may change significantly in response to higher minimum wages.

A byproduct of local ordinances governing a small region is that they also cover a smaller share of a worker's total labor market options. Workers who can access jobs in Seattle, as well as the surrounding area, may not adjust their reservation wage (the wage at which they'd agree to take a job) to the full increase of the minimum wage. If the reservation wage functions like a piecewise function, for example, a worker may have 50 percent of her reservation wage at Washington state's minimum wage of $9.47 and 50 percent of her wage at Seattle's new minimum of $13.00. If hires slow in Seattle and a job opens in a town over, the worker may very well take that job as it fully satisfies part of her reservation wages. These movements could lead to an observed net reduction in the labor force for Seattle. By contrast, workers who can access jobs in Seattle and the surrounding area may increase their reservation wage to Seattle's new minimum wage only, and as a result, be less likely to take on a job which pays less than the new minimum wage. These movements could lead to fewer separations in Seattle.

To the extent that local employment regulations may lead to changes in job tenure, it may also have implications for the conditions of employment on the job. The rise of precarious work, defined as "employment that is uncertain, unpredictable, and risky from the point of view of the worker," has transformed low wage work(A. Kalleberg, 2011). Low-wage jobs of the 21st century take on increasingly precarious forms with scheduling practices, termed "just-in-time" scheduling (Harknett et al., 2017; Henly & Lambert, 2014; Lambert et al., 2014). These "just-in -time" scheduling practices entail providing work schedules as little as a few days in advance, schedules for hours and days that change week-to-week, and the authority to move, add, or cancel shifts with no notice at all (Appelbaum et al., 2003; Lambert et al., 2014). These precarious employment practices can create volatility in hours worked, which has been shown to have negative impacts on the health and well-being of workers in low-wage jobs (Schneider & Harknett, 2019). While qualitative and survey research on low-wage work has documented workers' earnings volatility at the weekly or monthly level, the data available is at the quarterly level, which may limit my ability to assess the policy impacts on volatility outcomes (Lambert et al., 2014; Morduch & Schneider, 2017). If the MWO has a negative impact on job turnover for the reasons discussed in the previous paragraph, employers may find other ways to cut costs. For example, employers may increase their reliance on "just-in-time" scheduling, such as provide work schedules with little advance notice, schedule workers for hours and days that change week-to-week, and increase discretion in moving, adding, or canceling shifts with no notice at all. If these changes rise to the quarterly level, my measures of volatility would be able to pick up these changes.

Evidence on the impact of higher local minimum wages on job turnover has been based on sector- or industry-specific survey research, such as San Francisco restaurant workers and airport employers, Los Angeles government contractors, and homecare workers in selected California counties (Dube et al., 2007; Fairris et al., 2015; Howes, 2005; Reich et al., 2003). These studies found an increase in job tenure and employee retention, and a reduction in turnover in response to increased wages among low wage workers (Dube et al., 2007). To date, the impact of local minimum wages on employment flows, such as hires and separations, in addition to job tenure and turnover, for the entire low-wage labor market, has received less attention in economic and policy evaluation research.

A fundamental limitation of the vast majority of prior research on federal, state and local minimum wage policies is the reliance on proxies of low-wage workers, such as teenagers (Brochu & Green, 2013; Dube et al., 2016a; Gittings & Schmutte, 2016), or of low-wage industries, such as the restaurant or homecare and health industry (Dube et al., 2007, 2016a; Fairris et al., 2015; Howes, 2005; Reich et al., 2003). One of the contributions of this paper an assessment of employment flows using administrative data with information on actual hourly wage in local policy evaluation. This data will contribute evidence of the impact of a local minimum wage ordinance on a precisely defined low-wage labor market.

In assessing the impact on Seattle's Minimum Wage Ordinance using the UI-covered low-wage labor market as a treatment group, Jardim et al. (2020) found that the increase in the city's minimum wage to $13 per hour lead to a 3.3 to 3.4 percent increase in wages, indicating the minimum wage was binding. The increase in wages was accompanied by an employment decline ranging from 5.2 to 8.8 (Jardim et al., 2020). The prior empirical evidence documenting declines in hires and separations occurred when changes in employment levels (in response to a higher minimum wage) were small or insignificant. As this is not the case for Seattle, the effect of Seattle's minimum wage on job flows is uncertain. Seattle's higher minimum wage will both increase the desirability of jobs in Seattle, which could reduce the number of quits. However, the observed employment declines may instead lead to an increase in the number of layoffs.

Alternatively, workers may respond to the higher wage with an increased attachment to Seattle employers, which could have a stabilizing effect through reductions in the number of large declines in hours worked. Evidence from survey research has shown that workers would trade stability in earnings for mobility (Morduch & Schneider, 2017). If workers can work a few extra hours at their Seattle employer for a cost that nearly offsets the cost of taking on additional jobs, the result would appear as an improvement in earnings stability in UI-covered low-wage work. At present, the impacts of a minimum wage law on the volatility of hours worked within low-wage jobs is unknown. Both entry to and exit from employment and volatility in hours worked are key contributors to workers' experience of employment instability (B. Hardy et al., 2019). To the extent that minimum wages slow job flows, the Ordinance could stabilize employment in low-wage jobs, leading to reductions in job turnover and hours volatility. In this paper, I integrate this new wave of research on employment instability within a job into the policy evaluation framework in order to understand how minimum wage policy relates to working conditions on the intensive margin.

METHODS

To capture estimated changes in job flows in Seattle's low-wage labor market, relative to the rest of the state, I used two approaches that are recent advances in the policy evaluation literature: synthetic-control estimator and interactive fixed-effects estimator (Abadie et al., 2010; Abadie & Gardeazabal, 2003; Bai, 2009; Gobillon & Magnac, 2016). These designs are advantageous for studying local policy because they account for unobserved time-varying confounders that can bias treatment effects, such as migration patterns or the weather (Xu, 2017). The synthetic-control estimator minimizes unobserved time-varying heterogeneity by matching pre-treatment observables and outcomes between a treated unit and a set of control units using pre-treatment periods. The interactive fixed-effects estimator models unobserved time-varying confounders explicitly by incorporating region-specific intercepts interacted with time-varying coefficients. The regions used in this analysis are Public Use Microdata Areas (PUMAs). A PUMA is a geographic unit with a population of approximately 100,000 people (US Census Bureau, 2018).

To employ the synthetic-control estimator, I matched pre-policy observations and outcomes between Seattle PUMAs and the control PUMAs in the rest of Washington State to find an optimal set of weighted control PUMAs to serve as a counterfactual (Abadie et al., 2010; Abadie & Gardeazabal, 2003). Given a set of weights for each control PUMA $\widehat{w_r}$, the impact of the minimum wage is estimated as follows:

$$\hat{\beta}_t^{Synth} = Y_{r=1,t} - \sum_{r=2}^{R} \widehat{w_r}\, Y_{rt} \quad (3.1)$$

where Y_{rt} corresponds the observed outcome in each PUMA, r, and quarter, t, for regions $r = 1, \ldots, 40$ and time periods $t = 1, \ldots, 9$, where $t = 1$ in the second quarter of 2014, the quarter the minimum wage was passed. Treatment effects were estimated cumulatively from $t = 1 \ldots 9$, when $t = 9$ is the third quarter of 2016. The region, $r = 1$, corresponds to Seattle and w_r denotes the weight, w, associated with each region, r. The weights were found by minimizing forecasting error in the pre-policy time period, $t = -37, \dots, 0$:

$$\min_{w_r} \sum_{t=-37}^{0} \left(Y_{r=1,t} - \sum_{r=2}^{R} w_r Y_{tr}\right)^2, \tag{3.2}$$

subject to the constraints that the sum of the region-weights equaled to one and that all the region weights were positive ($\sum_r w_r = 1$ and $w_r \geq 0$).

Standard errors for the synthetic-control model were obtained using the "placebo-in-space" method, in which a placebo impact of the MWO was estimated on every group of five contiguous PUMAs in Washington state. The standard deviation of these estimated placebo impacts is the standard error for the parameter estimate. Placebo-in-space standard errors allow me to account for the uncertainty of whether the control group derived from the model can produce a valid counterfactual of how Seattle would have evolved absent the MWO.

To employ the interactive fixed-effect estimator, I modeled unobserved time-varying confounders semi-parametrically by incorporating unobserved region-specific linear factors, with time-varying coefficients. The interactive fixed-effects estimator assumes that changes in employment in each region can be represented as a function of K unobserved linear factors plus the treatment effect (Bai, 2009; Jardim et al., 2020; Xu, 2017). The unobserved factors, denoted by, μ_{tk}, can be thought of as common shocks which affected all PUMAs at the same time, such as migration patterns, or changes in weather. Each region is allowed to be differentially exposed to these shocks, called factor loadings, λ_{rk}. For an outcome, Y, in each PUMA, r, and time periods, t, the model used to estimate treatment effects is:

$$Y_{rt} = \sum_{t=1}^{9} \beta_t T_{rt} + \sum_{k=1}^{K} \lambda_{rk} \mu_{tk} + \varepsilon_{rt} \tag{3.3}$$

where λ_{rk} are region-specific factor loadings, constant across time, and μ_{tk} are unobserved factors, r, are regions, $r = 1, \dots, 40$ and t, are time periods $t = 1, \dots, 9$, where $t = 1$ in the second quarter of 2014. As in the synthetic-control estimator, treatment effects were estimated cumulatively from $t = 1 \ldots 9$, the third quarter of 2016.

To implement the interactive fixed-effects approach, I followed the procedure suggested by Bai (2009), which estimated treatment effects and factor loadings through two iteration processes and identified the set of estimates with the smallest sum of square residual. The first iteration process iterated between an ordinary least squares model for treatment without a common factor/factor loading structure and subsequently performed a factor analysis of the residuals with the given coefficient estimates from the OLS estimates. The second iteration process estimated the factor analysis first, assuming the coefficients for the treatment effect were zero and subsequently estimated an OLS model with the observed factor analysis.

While the number of unobserved factors is not known, β_t can only be identified if the number of factors is smaller than the number of periods in the data, less the number of coefficients to be estimated minus one (42 periods – 9 coefficients – 1=31). As such, I allowed for up to 30 factors and picked the model with the number of factors using the information criterion in Bai and Ng (2002)[31]. Standard errors for interactive fixed effects were assumed to be independent and identically distributed (iid). I assessed the sensitivity of the interactive fixed-effects estimator by estimating treatment effects for the full range of available factors.

Although both estimators allow for a comparison group to be identified using data-driven techniques, there are a few key distinctions. The synthetic-control estimator uses pre-policy observations to find an optimal set of weighted control PUMAs that match pre-policy trends in

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[31] The information criterion developed by Bai and Ng (2002) is similar to standard information criteria, AIC and BIC, with the main difference being that the information criterion utilizes both the number of regions, r and time, t in its function: $IC = log(SSR^2/(R*T)) + 30*((R+T)/(R*T))*log(min(R,T))$. The number of factors which minimizes the information criterion is chosen.

each outcome variable in Seattle. The interactive fixed-effects estimator, by contrast, allows for regions to be associated with time-varying factors, such as the economy or the weather, which, while all experienced at the same time, are felt differently within each region. The interactive fixed-effects estimator can be advantageous over synthetic control if there is spatial or cross-sectional correlation, a possible scenario for employment flows, which may be differentially sensitive to a variety of factors. Seattle is the largest metropolitan area in Washington, and it has a strong concentration of firms in the technology and information industries. Washington State is largely comprised of suburban and rural areas, with areas dominated by agriculture in the central and eastern parts of the state. These regions may respond differently to shocks which occur at the same time, such as the weather or the Great Recession. To ensure the synthetic control and interactive fixed effects identified control regions that best mimiced pre-trends in my employment outcomes, I conducted a falsification test by assessing the impact of Seattle's Minimum Wage Ordinance on jobs with wages higher than $19 per hour.

DATA

The Washington State Employment Security Department collects quarterly payroll records for all workers who receive earnings through formal work in Washington State and are eligible for the Washington State Unemployment Insurance (UI) program. These data, which are generated by employers' reports of quarterly payroll filings to the state, include quarterly hours in addition to quarterly earnings information to determine UI eligibility.[32] Washington state UI records provide a full census of UI-eligible employment at the job level, including employer address and industry,

[32] 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.

on a quarterly basis. This allows for precise estimation of wages and hours flows over the period, and for precise identification of jobs in which a worker earned a minimum wage at the onset of the Seattle Minimum Wage Ordinance.[33] One limitation of the UI data is that it does not capture earnings for jobs that are not covered by the UI program. In particular, these data miss wage information from tipped workers, and employment information from workers in the informal sector, workers who are sole proprietors, and workers who are independent contractors.

I geocode mailing address of each employer's establishment and assign each firm to their respective PUMA in Washington State. Critically, PUMA boundaries match the boundaries of the city of Seattle. PUMAs are larger than census tracts, and as a result, less likely to be affected by random shocks, but smaller than counties, allowing me to find areas with employment patterns similar to the city of Seattle. 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.[34] 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.[35] Additionally, firms with multiple establishments that opt to file UI claims under a single Employer Identification Number are unable to be geocoded because it is unclear how many jobs in the firm are associated with the address provided.

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[33] 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.

[34] I geocode mailing addresses to the exact latitude and longitude coordinates using the Business Analytics 2016 Street Map database from ARC GIS. If the exact latitude and longitude coordinates cannot be defined, I geocode to the centroid of the firms' zip code, depending on the level of detail of the mailing address.

[35] In addition to these incorrect mailing records, there was also a record collection problem with certain classes of domestic workers (NAICS code 814000) and home and health care aides (NAICS 624120). As a result, I exclude jobs and workers in these industries.

Table 3.2 shows the average number of Seattle firms and the number of firms that are "non-locatable" due to their decision to establish an umbrella Employer Identification Number by the wage of the job between 2005 and the year the minimum wage was enacted, 2014. Across wage thresholds, there is little variation in the share of employers that opt for an umbrella account across their establishments. Jobs in firms that have wage <$19 per hour are 62 percent locatable. Of the 2.6 million employees with wages <$19 per hour, 1 million are in firms are "non-locatable." Among jobs that are less than $13 dollars per hour, the highest jump in the minimum wage analyzed in this study, and across all jobs, this share remains constant at 62-63 percent of employees who are in firms that are locatable.

Table 3.2. Characteristics of Included and Excluded Jobs Washington State

	Included in Analysis	Excluded from Analysis	Share Included
Number of jobs	1,629,274	1,001,120	62%
Number of jobs which pay <$19/hour	700,401	413,288	63%
Number of jobs which pay <$13/hour	347,049	216,536	62%

Source: Author's analysis of Washington state UI program records.
Notes: Statistics are computed for the average quarter between 2005.1 and 2014.2. "Excluded from Analysis" includes firms whose location could not be determined.

The exclusion of non-locatable firms has the potential to bias results on job flows in several ways. As shown in Chapter 2, employers of firms that are non-locatable are more likely to be large and, as a result, may be more likely to face higher mandated minimum wages under the Seattle Ordinance.[36] Economic theory suggests that larger businesses should reduce employment faster than smaller businesses, and survey evidence shows that non-locatable firms were more likely to plan and implement staff reductions as a result of the Ordinance (Romich et al., 2017). For job flows, this could lead to an increase in separations, a decrease in hires, and an increase in large

[36] See Chapter 2, Table 2.1, for the distribution of non-locatable firms by firm size.

hours declines. Exclusion of these firms means that I may underestimate the change in separations, hires, and hours declines.

By contrast, non-locatable firms with locations in and out of Seattle may be better equipped to handle the added labor costs from their affected locations, leading to no change in employment levels for Seattle workers. If these firms retail pre-policy employment levels with higher wages, workers may try to gain employment with non-locatable firms, which could lead to an observed artificial increase in separations for the remaining jobs that are locatable in Seattle. Depending on how workers in locatable firms respond, an increase in separations to non-locatable firms may bias this analysis to underestimate a decline in separations or lead to outright increase. In Jardim et al. (2020), our team assessed workers transition rates from locatable firms to non-locatable firms during the period following the MWO enactment. We did not find a significant change in workers' transition rates to non-locatable firms with a Seattle address, nor did we find a change in transition rates to non-locatable firms with addresses in nearby regions, so this pathway is unlikely.

Sample

I follow previous research that has evaluated the Seattle Minimum Wage Ordinance and define low-wage jobs to be jobs that pay less than $19 per hour (Jardim et al., 2020). This threshold is more than twice the baseline minimum wage and covers 42.9 percent of total jobs in Seattle. Research has shown that cascading effects are less likely to occur beyond a threshold ranging from $2 dollars above post-policy minimum wage (amounting to 12.50- $15 per hour in the second phase-in of the MWO) to 150 percent of the post-policy minimum wage (amounting to $15.75- $19.50 per hour) (Brochu et al., 2018; Neumark, Schwizer, and Wascher, 2004). Figure 3.1 illustrates this point. The minimum wage was passed in the second quarter of 2014, and the gray bar shows the distribution in hours worked at that time. In the second quarter of 2015, when the first phase-in of minimum wage was enacted (orange line), there are spikes in the number of hours in the distribution around the $11 per hour marker. The first quarter of 2016 (blue line) similarly shows a peak of around $13 per hour and $15 per hour in response to the second phase-in. However, there is no evidence of cascading effects beyond $15 per hour. As such, the choice of $19 per hour as a wage threshold of low-wage work will, if anything, produce more conservative treatment effects. The falsification test will also test the sensitivity of this choice by evaluating the effects of the law for workers with a wage greater than $19 per hour. If there are statistically significant coefficient estimates for this group, the $19 per hour cut off may not be high enough.

Jobs are considered treated if their firm resides within the five geographic PUMAs of Seattle city limits. Because local economies do not have closed boundaries, and due to the small geographic boundary of the city, Seattle's a higher minimum wage may "spillover" to firms outside of Seattle boundaries that fall within the surrounding King County. If a spillover effect occurs and contaminates the comparison group, then treatment effects may be attenuated (Baum-Snow & Ferreira, 2015; Jardim et al., 2017). My primary analysis assumes that PUMAs in the county surrounding Seattle, King County, are contaminated by spillover effects from Seattle's Minimum Wage Ordinance. As such, these PUMAs in King County outside of Seattle are excluded from primary analysis. Due to their distance away from Seattle, jobs in PUMAS outside of King are much less likely to be contaminated by the MWO. However, their distance away from Seattle means that these jobs may not have similar employment flow patterns to those in Seattle, which may make jobs these PUMAs less viable controls. [37] As a result, PUMAs far away from Seattle

[37] There may also not be a presumed spillover effect. Dube, Lester, and Reich (2010) assess whether cross-border spillovers from minimum wage policies exist at the state level by comparing the effects of minimum wages on counties at state borders to the counties in the interior of the state and do not find any significant effects.

may not be equally valid controls. To assess the sensitivity of the exclusion of King County PUMAs to the donor pool, I re-estimate treatment effects, including PUMAs in King County, in the donor pool of control PUMAs.

Outcomes

I use five outcomes to assess the impact of the Seattle Minimum Wage on employment flows in low-wage jobs.[38] I define a job as an employee-employer match if the match exists in quarter, $t-1$, and quarter, $t,$ termed "beginning-of-quarter" jobs. Using "beginning-of-quarter" jobs eliminates jobs that only existed for a few hours or a day and produces the employment counts comparable to the U.S. Census Quarterly Workforce Indicators data set (Abowd & Vilhuber, 2011). I define hires in quarter, t, as the number of new employer-employee matches that occur during the quarter. This includes recalls, which I define as an employer-employee match existed prior to time, t, but not in time, $t-1$. I define separations in quarter, t, as the number of employer-employee matches that have no valid wage record in quarter, $t+1$.[39] I estimate the job turnover rate as the difference of total separations in a quarter and total hires in a quarter, divided by the two times the number of beginning of quarter jobs in the quarter, t, following Dube et al. (2016):

$$turnover_t = \frac{separations_t - hires_t}{2 \times employment_t} \tag{3.4}$$

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[38] A common outcome in duration literature is workers' duration of nonemployment (Dube et al., 2016a; Gittings & Schmutte, 2016). I exclude this outcome in my analysis because the rest of my outcomes are at the job level. However, future research could incorporate workers' duration of nonemployment and job-to-job transitions.

[39] A limitation to the UI program data is that I am unable to discern whether a separation occurs because of a layoff or because of a quit. This means I fall short of being able to assess whether low-wage jobs are becoming better employer-employee matches. I am also unable to see whether the pool of the "unemployed" expanded in response to the Ordinance.

These definitions are broadly defined to include the full range of information provided by the UI claims data. To assess the sensitivity of the choice to include short-term jobs, I re-estimate the impact of the MWO on employment flows, restricting my analysis to hires and separations that existed for at least one quarter. Using definitions from the U.S. Census, the restricted set of hires is defined as the number of jobs in quarter t that existed n quarter $t-1$ ("beginning-of-quarter" hires). The restricted set of separations is defined as the number of jobs that existed in $t-1$ and t but do not exist in quarter $t+1$ ("full-quarter" separations). To re-estimate job turnover, I use "full-quarter" jobs in the denominator, defined as that existed in $t-1$, t, and $t+1$.

I further estimate the impact of the minimum wage on hours volatility using two measures estimates: the absolute value arc percent change (APC) of quarterly hours within a job and the number of large declines in hours worked within a low-wage job. These measures, which are conditional on employment, investigate employment flows at the intensive margin, and measure whether or not the quarterly hours worked in a job became more or less volatile as a result of the MWO. The APC in quarterly hours creates a standardized value of the quarterly change in hours worked within a job. It is defined as:

$$abs.\, value\ of\ arc\ percentage\ change = |100 * \frac{(Y_t - Y_{t-1})}{\frac{(Y_t + Y_{t-1})}{2}}| \,. \qquad (3.5)$$

The APC has been used widely in the literature and provides a straightforward interpretation of changes in quarterly earnings (Dahl et al., 2011; Dynan et al., 2012; Hannagan & Morduch, 2015; B. Hardy & Ziliak, 2014; Shin & Solon, 2011). The APC is symmetric and bounded at $abs|200|$. A value of 0 indicates there was no change in hours volatility within a low-wage job. A value of 200 indicates the number of hours worked within a job changed by 200 percent *or more*. I define the number of large hours declines as the frequency of jobs in the quarter, t, that had an hours-worked value that was 25 percent lower than their hours worked in the quarter, $t-1$. The number of large hours declines estimates the frequency and direction of hours volatility within a job.

Seattle, which makes up five PUMAs, will have counts of hires, separations, and large hours declines that are systematically larger than any individual PUMA in Washington State. Because one of the assumptions of the synthetic-control estimator is that there is no intercept difference between the treatment and control group, I calculate the year-over-year change in the level of the outcomes and use the year-over-year change in hires, separations, job turnover, and large hours declines as the main outcome variables for these constructs (Doudchenko & Imbens, 2016).

Table 3.3 shows summary statistics of the five employment outcomes in the Seattle treatment group over the period in which the minimum wage was passed, the second quarter of 2014, through the nine post-policy quarters. The first three quarters reflect the time post-passage of the Ordinance and pre-enactment. The second quarter of 2015 marks the enactment of the first phase-in of the minimum wage up to $11 per hour, and the first quarter of 2016 marks the second phase-in, during which the minimum wage increased up to $13 per hour for large businesses. Summary statistics are shown for the treatment group of low-wage jobs, jobs with wage-rates <$19, and for all jobs in the city. Table 3.3 reveals a strong seasonal pattern in employment flows for low-wage jobs. Within each year, there is a peak in total hires and separations during the 3rd quarter of each year. The job turnover rate and volatility in hours worked for low-wage jobs are also highest during the 3rd quarter of each year, reflecting the changes in employment flows. By contrast, the number of large hours declines is largest during the first quarter of each year, when businesses generally contract. Table 3.3 also shows a growth in employment flows in Seattle following the passage of the Minimum Wage Ordinance. From peak-to-peak (3rd quarter to 3rd quarter) of 2014 to 2016, the number of hires and separations increased by 7.3 and 7.8 percent, and job turnover and hours volatility increased by 13.8 percent and 5.8 percent, respectively.

Table 3.3. Summary Statistics for All Jobs in Seattle and for Jobs Paying <$19

Quarter	Quarters After Passage/ Implementation	Number of Hires		Number of Separations		Job Turnover Rate		APC Quarterly Hours		Number of Large Declines in Hours	
		<$19	All	<$19	All	<$19	All	<$19	All	<$19	All
2014.2	0	29,247	57,838	22,311	49,265	0.28	0.18	0.38	0.29	15,139	37,466
2014.3	1	31,295	56,129	28,834	52,474	0.32	0.18	0.42	0.31	17,149	44,688
2014.4	2	25,986	48,577	24,285	46,237	0.29	0.16	0.44	0.32	19,415	43,501
2015.1	3	23,371	45,738	19,968	38,704	0.24	0.14	0.38	0.29	21,479	48,491
2015.2	4/1	31,495	58,056	24,324	48,394	0.31	0.17	0.40	0.30	15,491	39,411
2015.3	5/2	32,177	60,353	30,978	58,239	0.35	0.18	0.45	0.32	17,980	48,708
2015.4	6/3	27,226	54,864	25,756	51,954	0.31	0.17	0.46	0.32	19,265	44,599
2016.1	7/4	25,652	55,710	21,198	42,931	0.27	0.15	0.39	0.30	22,246	55,305
2016.2	8/5	32,810	63,040	27,346	57,409	0.34	0.18	0.40	0.29	14,913	38,813
2016.3	9/6	33,581	65,402	31,074	60,975	0.37	0.19	0.45	0.31	16,682	49,170

Source: Author's analysis of Washington state UI program records.
Note: APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Quarter numbers denote the time post-passage/ time post implementation.

By contrast, overall job flows increased by more than twice as fast as low-wage jobs in the two years following the Ordinance's passage. Hires and separations for all Seattle jobs increased by 16.5 and 16.2 percent, respectively, illustrative of Seattle's strong labor market between 2014 and 2016. Job turnover for all jobs, however, increased by 3.0 percent, indicating that the high wage jobs that were added during this period tended to be longer, relative to low-wage job creation. Quarterly hours volatility increased by 5.8 percent for low-wage jobs, while it decreased among all jobs by 0.1 percent, indicating greater stability within high wage jobs. The number of large hours declines dropped in Seattle over the period in low-wage jobs, by 2.7 percent, but increased across all jobs. Earnings in high-wage jobs in Seattle grew over this period, so increases in large hours drop may be from workers who actively cut their hours in response to wage growth.

To assess how changes in low-wage work in Seattle compare to the rest of Washington state, **Figures 3.2a-e** display a time series for each of the five employment flow outcomes for Seattle and the comparison PUMAs in Washington state between the second quarter of 2006 and the third quarter of 2016. The graph shows that each of the employment flow outcomes is within the convex hull of the surrounding Washington state PUMAs over the period, indicating that Seattle's labor market experience can be compared to that of the rest of the state.[40] Figures 3.2b and 3.2e also show descriptive evidence of a decline in the number of separations and number large hour drops in Seattle relative to other PUMAs in the onset of the second phase-in of the minimum wage, which began in the first quarter of 2016. These trends preview the results in the following section.

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[40] This is a requirement for the synthetic-control estimator, but not the interactive fixed-effects estimator.

Figure 3.2. Trends in employment flow outcomes in Seattle compared to PUMAs outside of King County for Jobs Paying <$19 Per hour.

Source: Author's analysis of Washington state UI program records.

RESULTS

Falsification

To assess whether the synthetic-control and interactive fixed-effects estimators construct valid counterfactual trends, I estimate the impact of the Seattle Minimum Wage Ordinance on a group that should not be affected by the Ordinance: jobs that pay more than $19 per hour. If the impact estimates from this high-wage job group are significant, then there would be concern that the identification strategies would produce biased estimates when the treatment group of low-wage jobs is used. **Table 3.4** shows the impact of the Seattle Minimum Wage Ordinance on high wage jobs using both the synthetic-control and the interactive fixed-effects estimators. Both estimators overwhelmingly pass the falsification test: Only 4 of the 90 estimated coefficients are significant. The impact estimates show that there are no statistically significant changes in the number of separations, job turnover, or volatility in hours worked among high wage workers in Seattle, relative to the comparison regions during the period of the minimum wage passage and enactment. Two of the estimated coefficients are significant for change in hires at the 90 and 95 percent confidence levels, and two of the estimated coefficients are significant for the change in large hours declines at the 99 percent confidence level.[41]

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[41] **Appendix B, Table 1** shows that the classic two-way fixed effects, difference-in-differences design, fails the falsification test. Under the difference-in-differences specification that uses high-wage jobs as the treatment group, the majority of the estimated coefficients for the five employment flow outcomes are significant at the 95-99 percent confidence level. These results indicate that this estimation strategy does not provide a valid counterfactual region for Seattle.

Table 3.4. Falsification Test: Impact of the Seattle Minimum Wage Ordinance on Jobs with Wages >$19 Per Hour

		Change in Hires		Change in Separations		Job Turnover Rate		APC Quarterly Hours	
Quarter	Quarters after Passage/ Implementation	S.C.	IFE	S.C.	IFE	S.C.	IFE	S.C.	IFE
2014.3	1	0.144	0.075	0.066	0.021	0.006	-0.019	0.01	-0.007
		[0.349]	[0.419]	[0.553]	[0.779]	[0.959]	[0.794]	[0.689]	[0.172]
2014.4	2	0.031	0.087	0.008	-0.03	-0.074	-0.054	0.008	-0.007
		[0.734]	[0.355]	[0.903]	[0.69]	[0.434]	[0.468]	[0.778]	[0.199]
2015.1	3	-0.276	-0.179**	-0.039	-0.057	-0.269**	-0.136*	-0.006	-0.007
		[0.123]	[0.049]	[0.711]	[0.44]	[0.045]	[0.067]	[0.839]	[0.167]
2015.2	4/1	-0.028	0.127	-0.188	0.021	-0.059	0.054	0.011	0.003
		[0.896]	[0.196]	[0.227]	[0.786]	[0.615]	[0.492]	[0.623]	[0.598]
2015.3	5/2	0.141	0.04	0.014	-0.007	-0.033	-0.027	0.006	-0.011
		[0.477]	[0.767]	[0.873]	[0.95]	[0.76]	[0.794]	[0.832]	[0.165]
2015.4	6/3	0.204*	0.135	0.131	0.02	0.047	0.036	0.002	-0.008
		[0.091]	[0.336]	[0.335]	[0.853]	[0.664]	[0.737]	[0.926]	[0.323]
2016.1	7/4	-0.298	-0.144	-0.03	-0.042	-0.299***	-0.084	0.03	-0.004
		[0.233]	[0.243]	[0.735]	[0.686]	[0.017]	[0.417]	[0.22]	[0.619]
2016.2	8/5	-0.005	0.07	-0.255	-0.069	-0.145	-0.093	0.011	0.003
		[0.979]	[0.618]	[0.44]	[0.596]	[0.344]	[0.371]	[0.794]	[0.674]
2016.3	9/6	0.279	0.081	0.067	0.017	0.054	-0.017	0.04	-0.01
		[0.156]	[0.638]	[0.582]	[0.897]	[0.577]	[0.898]	[0.305]	[0.364]
R2			0.50		0.65		0.49		0.93
RMSE		0.093		0.11		0.08		0.005	
Number of Observations		1845	1845	1845	1845	1845	1845	1845	1845

Source: Author's analysis of Washington state UI program records.

Notes: Quarter numbers denote the time post-passage/ Time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Standard errors are in brackets. Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying > $19, where the control region is defined as PUMAs in state of Washington outside of King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Quarter	Quarters after Passage/ Implementation	Change in large hours declines S.C.	IFE
2014.3	1	-0.038	-0.023
		[0.62]	[0.725]
2014.4	2	0.113*	-0.047
		[0.095]	[0.473]
2015.1	3	-0.066	-0.034
		[0.243]	[0.603]
2015.2	4/1	-0.045	-0.078
		[0.6]	[0.233]
2015.3	5/2	0.063	0.01
		[0.318]	[0.911]
2015.4	6/3	0.15***	0.012
		[0.015]	[0.901]
2016.1	7/4	-0.045	0.039
		[0.609]	[0.68]
2016.2	8/5	-0.159	-0.198***
		[0.27]	[0.018]
2016.3	9/6	0.034	0.105
		[0.658]	[0.398]
R2			0.39
RMSE		0.041	
Number of Observations		1845	1845

Source: Author's analysis of Washington state UI program records.

Notes: Quarter numbers denote the time post-passage/ Time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Standard errors are in brackets. Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying > $19, where the control region is defined as PUMAs in state of Washington outside of King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41). ***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Treatment Effects

Table 3.5 shows the cumulative impact estimates of the Seattle Minimum Wage Ordinance on low-wage job flows in the nine quarters following the passage of the law. Relative to previous evidence on the impact of the MWO on employment levels, which showed a decline ranging from 5.2 to 8.8 percent (found in Jardim et al., 2020, Table 6), the range of impact estimates for the year-over-year changes in hires and separations are substantially larger. Policy significant changes in hires and separations occur during the second phase-in of the minimum wage. The decline in hires was robust during this period, and the majority of estimated effects (4 of 6) are statistically significant, ranging from 5.1 to 19.3 percentage points in each quarter. This result corroborates analysis in Jardim et al. (2020), which found a decline in new entrants into the low-wage labor market relative to the constructed synthetic control group.[42] Year-over-year changes in separations declined in the quarters following the second phase-in as well. The estimates range from 3.3 to 12.1 percentage points, but these point estimates are imprecise, and only one is statistically significant ($p \leq 0.10$). Based on the decline in hires and separations, economic theory would suggest that job turnover would decrease as well. The treatment effects for job turnover are generally negative and statistically significant in the first quarter of 2016 (second phase-in). In this quarter job turnover declined by a range of 7.3 to 8.5 percent.

[42] In Jardim et al. (2020), a "new entrant" was defined as a worker who had not been employed in Washington State in the prior five years and earned a wage < $19 per hour.

Table 3.5. Impact of the Seattle Minimum Wage Ordinance on Job Flows for Jobs Paying <$19 Per Hour

Quarter	Quarters after Passage/ Implementation	Change in Hires		Change in Separations		Job Turnover Rate	
		S.C.	I.F.E	S.C.	I.F.E	S.C.	I.F.E
2014.3	1	-0.056	-0.059	-0.005	-0.004	-0.042	-0.043
		[0.39]	[0.191]	[0.937]	[0.921]	[0.431]	[0.251]
2014.4	2	-0.003	-0.052	0.005	-0.034	0.019	-0.05
		[0.961]	[0.258]	[0.921]	[0.449]	[0.689]	[0.19]
2015.1	3	-0.137***	-0.073	0.01	0.008	-0.056	-0.046
		[0.021]	[0.106]	[0.8]	[0.857]	[0.233]	[0.221]
2015.2	4/1	-0.016	-0.047	-0.048	0	-0.006	-0.018
		[0.749]	[0.293]	[0.417]	[0.998]	[0.898]	[0.619]
2015.3	5/2	-0.088	-0.134**	-0.031	-0.071	-0.038	-0.075
		[0.276]	[0.026]	[0.722]	[0.252]	[0.567]	[0.145]
2015.4	6/3	-0.019	-0.035	-0.034	-0.061	0.04	0.002
		[0.705]	[0.581]	[0.571]	[0.33]	[0.335]	[0.965]
2016.1	7/4	-0.193***	-0.166***	-0.036	-0.079	-0.073	-0.085*
		[0.02]	[0.005]	[0.651]	[0.2]	[0.414]	[0.094]
2016.2	8/5	-0.051	-0.122**	-0.048	-0.116*	0.061	-0.032
		[0.545]	[0.044]	[0.584]	[0.054]	[0.379]	[0.544]
2016.3	9/6	-0.082	-0.151**	-0.033	-0.121	0.012	-0.042
		[0.488]	[0.039]	[0.763]	[0.103]	[0.872]	[0.52]
R2			0.67		0.61		0.61
RMSE		0.048		0.038		0.03	
Number of Observations		1845	1845	1845	1845	1845	1845

Source: Author's analysis of Washington state UI program records.
Notes: Quarter numbers denote the time post-passage/ Time post implementation. Impact estimates in the SC column refer to estimates derived using synthetic-control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed-effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying < $19, where the control region is defined as PUMAs in state of Washington outside of King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).
***, **, and * denote statistically significance using a two-tailed test with p ≤ 0.01, 0.05, and 0.10, respectively.

Table 3.6 shows the impact of the Seattle Minimum Wage Ordinance on two measures of employment volatility conditional on the job existing in quarters, $t-1$, and quarter, t: the arc percent change (APC) in hours worked at a job and the year-over-year change in the number of large hours declines within a job. Across both models, and in both phase-ins, the APC increased significantly by 1.3 to 2.5 percent during the first phase-in (second through fourth quarter of 2015) and by 1.6 to 3.2 percent in the second phase-in (estimates are statistically significant at the 0.1 and 0.01 level). As the APC reflects changes in the magnitude of hours worked within a job, consistent, statistically significant increases in 5 out of the 6 post period quarters indicate that jobs that existed post-policy experienced fluctuations in their hours. Depending on the directionality of these fluctuations, these estimates may reflect hours declines due to employer cuts or because workers were choosing to work less, or hours increases if workers who remained employed took on more work. The year-over-year change in the number of large hours declines within a job was negative throughout the entire period studied and decreased in the second quarter of the second phase-in by 10.4 to 13.7 percent (statistically significant at the 0.1 and 0.01 level, respectively). The timing of this coincides with the decline in separations from Table 3.5, indicating that workers the decline in large hours drops may be due to the reduction in separations.

Table 3.6. Impact of the Seattle Minimum Wage Ordinance on Hours Flows for Jobs Paying <$19 Per Hour

Quarter	Quarters after Passage/ Implementation	APC Quarterly Hours S.C.	APC Quarterly Hours I.F.E.	Change in large hours declines S.C.	Change in large hours declines I.F.E.
2014.3	1	0.004	-0.001	-0.004	-0.022
		[0.818]	[0.908]	[0.913]	[0.556]
2014.4	2	-0.018	-0.001	-0.042	-0.003
		[0.339]	[0.867]	[0.153]	[0.932]
2015.1	3	0.005	0.013***	0.028	0.05
		[0.691]	[0.005]	[0.441]	[0.185]
2015.2	4/1	-0.006	0.011***	-0.055	-0.032
		[0.613]	[0.017]	[0.183]	[0.392]
2015.3	5/2	-0.005	0.003	0.009	-0.023
		[0.642]	[0.667]	[0.834]	[0.666]
2015.4	6/3	0.025*	0.013*	-0.068	-0.048
		[0.087]	[0.063]	[0.18]	[0.358]
2016.1	7/4	0.023	0.028***	0.005	-0.04
		[0.119]	[0.000]	[0.933]	[0.458]
2016.2	8/5	-0.006	0.016***	-0.104*	-0.137***
		[0.645]	[0.019]	[0.06]	[0.006]
2016.3	9/6	0.032*	0.008	-0.024	-0.087
		[0.094]	[0.377]	[0.701]	[0.17]
R2			0.96		0.50
RMSE		0.005		0.028	
Number of Observations		1845	1845	1845	1845

Source: Author's analysis of Washington state UI program records.
Notes: Quarter numbers denote the time post-passage/ Time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Standard errors are in brackets. Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying < $19, where the control region is defined as PUMAs in state of Washington outside of King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).
***, **, and * denote statistically significance using a two-tailed test with p ≤ 0.01, 0.05, and 0.10, respectively.

Taken together, the results show a sizable decline in year-over-year hires in response to the Seattle Minimum Wage Ordinance, with an elasticity ranging from -0.32 to -0.52 when the denominator is the statutory increase in the minimum wage ($9.47-$13 per hour). Separations declined by 3.3 to 12.1 percent in the second phase-in with precision in the second quarter of 2016 (elasticity of -.31), and job turnover declined by 7.3 to 8.5 in the second quarter of 2016. These results suggest a chilling effect in Seattle, one in which the flows of work slowed down for low-wage jobs. Jobs which continued to exist exhibited hours changes ranging from 1.1 to 2.8 percent during the enactment of the first and second phase-in of the Minimum Wage Ordinance. However, the percent of large hours drops declined at the same time, suggesting that while volatility increased, jobs experiencing severe negative hours fluctuations decreased.

Sensitivity Analysis

I first assess whether the treatment effect changed if the number of jobs was restricted to low-wage jobs that had more than one quarter of employment, relative to the main analytic sample. **Table 3.7** shows the results of impact estimates for low-wage jobs using beginning-of-quarter hires, full-quarter separations, and job turnover using full-quarter employment. The directionality and precision of the impact estimates match the impact estimates from the main results in Table 3.5. The magnitudes of the declines in beginning-of-quarter hires are larger, and the majority of estimated effects are significant in the second phase in (4 of 6), ranging from 13.4 percent to 21.9 percent percentage points in each quarter. Year-over-year changes in separations also increased in magnitude, and the third quarter of 2016 point estimate of 16.9 percent is statistically significant. Job turnover rates remained imprecise, although the magnitude of the job turnover rate grew, and the majority of the impact estimates are negative in directionality. Taken together, the impact estimates for employment flows with at least one quarter of employment do not differ from the main analytic sample. The results are robust to changes in employment definitions.

Table 3.7. Sensitivity: Impact of the Seattle Minimum Wage Ordiance on Jobs With More Than One Quarter Employment for Jobs Paying <$19 Per Hour

	Quarters after Passage/	Change in Hires		Change in Separations		Job Turnover Rate	
Quarter	Implementation	S.C.	I.F.E.	S.C.	I.F.E.	S.C.	I.F.E.
2014.3	1	-0.059	-0.056	0.002	-0.026	-0.016	-0.023
		[0.141]	[0.221]	[0.978]	[0.581]	[0.665]	[0.504]
2014.4	2	0.008	-0.03	-0.031	-0.045	-0.03	-0.042
		[0.86]	[0.53]	[0.369]	[0.334]	[0.35]	[0.219]
2015.1	3	-0.034	-0.048	0.041	0.061	-0.018	0.005
		[0.629]	[0.294]	[0.587]	[0.191]	[0.767]	[0.89]
2015.2	4/1	0.041	-0.048	-0.011	0.015	-0.009	-0.001
		[0.348]	[0.294]	[0.781]	[0.752]	[0.814]	[0.966]
2015.3	5/2	-0.073	-0.15***	-0.054	-0.096	-0.103	-0.131***
		[0.338]	[0.014]	[0.55]	[0.137]	[0.24]	[0.016]
2015.4	6/3	0.055	0.019	-0.05	-0.076	0.021	0.015
		[0.625]	[0.779]	[0.358]	[0.239]	[0.641]	[0.755]
2016.1	7/4	-0.166*	-0.219***	-0.053	-0.056	-0.121	-0.139***
		[0.069]	[0.000]	[0.483]	[0.386]	[0.146]	[0.011]
2016.2	8/5	0.034	-0.134**	-0.038	-0.102	0.041	-0.024
		[0.638]	[0.031]	[0.75]	[0.105]	[0.509]	[0.607]
2016.3	9/6	-0.031	-0.194***	-0.089	-0.169***	-0.053	-0.119**
		[0.702]	[0.007]	[0.292]	[0.022]	[0.454]	[0.046]
R2			0.64		0.53		0.71
RMSE		0.038		0.036		0.03	
Number of Observations		1845	1845	1845	1845	1845	1845

Source: Author's analysis of Washington state UI program records.
Notes: Quarter numbers denote the time post-passage/ Time post implementation. Hires which have more than one quarter of employment are defined if the job exists in quarter t and t+1. Separations which have more than one quarter of employment are defined if the job existed for a full-quarter prior to not being observed. Job turnover is the ratio of (hires + separations) to twice the number of "full-quarter jobs". Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying < $19, where the control region is defined as PUMAs in state of Washington outside of King County. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).
***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

To assess the sensitivity of restricting the available PUMAs in the control group to exclude PUMAs in King County, I re-estimate treatment effects including PUMAs in the King County in the donor pool and compare the PUMAs used in the synthetic-control estimator in the main analysis and the analysis with King County Pumas. **Table 3.8** shows the impact estimates when King County PUMAs are included in the synthetic-control and interactive fixed-effects estimation strategy. If there were spillover effects of the Seattle Minimum Wage Ordinance into King County, and if those PUMAs were selected as donors to the control group, then the impact estimates would be smaller in magnitude or insignificant, relative to the main results, displayed in Tables 5 and 6. Across all outcomes, impact estimates are smaller and less precise when the donor PUMAs from King County are included, suggesting that jobs in these PUMAs may have been contaminated by spillover from the MWO. **Figures 3.3a** and **3.3b** show the weight of each donor PUMA in the synthetic-control analysis, excluding King County (Figure 3.3a) and including King County (Figure 3.3b). When PUMAs from King County are excluded from the donor pool, the PUMAs comprising synthetic control come mostly from the Snohomish County, a suburban county close to Seattle. When the PUMAS from King County are included, the PUMAs that comprise the synthetic control group make up equal weights of King and Snohomish Counties, indicating that employment patterns in King are similar to Seattle. Figure 3.3b makes clear that while King County employment flows were similar to flows in Seattle, King County was also most likely contaminated by spillover effects from the MWO. As a result, the decision to exclude these PUMAs from King County is a stronger identification strategy.

Table 3.8. Sensitivity: Impact of the Seattle Minimum Wage Ordinance on Jobs Paying <$19 Per Hour Inclusive of Jobs in King County

Quarter	Quarters after Passage/ Implementation	Change in Hires		Change in Separations		Job turnover Rate		APC Quarterly Hours	
		S.C.	IFE	S.C.	IFE	S.C.	IFE	S.C.	IFE
2014.3	1	0.033	-0.018	0.006	0	-0.005	0	0.01	0.001
		[0.616]	[0.68]	[0.925]	[0.991]	[0.942]	[0.981]	[0.547]	[0.753]
2014.4	2	0.012	-0.023	0.002	-0.003	-0.01	-0.01	-0.003	0
		[0.85]	[0.607]	[0.97]	[0.939]	[0.838]	[0.264]	[0.87]	[0.949]
2015.1	3	-0.113*	-0.086*	-0.008	0.006	-0.033	-0.008	0.009	0.008*
		[0.057]	[0.051]	[0.87]	[0.896]	[0.256]	[0.381]	[0.518]	[0.067]
2015.2	4/1	-0.001	-0.043	-0.025	-0.002	-0.053	-0.004	0.011	0.011***
		[0.993]	[0.318]	[0.669]	[0.957]	[0.303]	[0.678]	[0.334]	[0.014]
2015.3	5/2	-0.009	-0.089	-0.003	-0.046	-0.037	-0.004	0.004	0.002
		[0.909]	[0.136]	[0.979]	[0.45]	[0.495]	[0.841]	[0.683]	[0.734]
2015.4	6/3	0.041	-0.006	-0.011	-0.025	-0.02	-0.002	0.025	0.011*
		[0.402]	[0.917]	[0.867]	[0.679]	[0.621]	[0.909]	[0.103]	[0.088]
2016.1	7/4	-0.098	-0.136***	0.039	-0.053	-0.042	-0.011	0.008	0.018***
		[0.261]	[0.02]	[0.644]	[0.379]	[0.476]	[0.432]	[0.52]	[0.006]
2016.2	8/5	-0.025	-0.081	-0.058	-0.089	0.01	-0.004	0.001	0.015***
		[0.78]	[0.181]	[0.533]	[0.133]	[0.81]	[0.781]	[0.952]	[0.02]
2016.3	9/6	0.032	-0.071	-0.022	-0.074	0.031	0.014	0.03	0.007
		[0.774]	[0.349]	[0.844]	[0.318]	[0.603]	[0.599]	[0.114]	[0.453]
R2			0.65		0.60		0.98		0.97
RMSE		0.041		0.03		0.005		0.003	
Number of Observations		2296	2296	2296	2296	2296	2296	2296	2296

Source: Author's analysis of Washington state UI program records.

Notes: Quarter numbers denote the time post-passage/ Time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Standard errors are in brackets Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying < $19, where the control region is defined as PUMAs in the state of Washington. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

Quarter	Quarters after Passage/ Implementation	Change in large hours declines S.C.	IFE
2014.3	1	-0.004	-0.029
		[0.927]	[0.419]
2014.4	2	-0.024	0.005
		[0.378]	[0.879]
2015.1	3	0.051	0.057
		[0.18]	[0.113]
2015.2	4/1	-0.053	-0.029
		[0.214]	[0.41]
2015.3	5/2	0.017	-0.025
		[0.704]	[0.617]
2015.4	6/3	-0.051	-0.033
		[0.289]	[0.508]
2016.1	7/4	0.024	-0.026
		[0.654]	[0.611]
2016.2	8/5	-0.115**	-0.12***
		[0.049]	[0.013]
2016.3	9/6	-0.037	-0.071
		[0.559]	[0.241]
R2			0.50
RMSE		0.025	
Number of Observations		2296	2296

Source: Author's analysis of Washington state UI program records.

Notes: Quarter numbers denote the time post-passage/ Time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job. Standard errors are in brackets Impact estimates in the SC column refer to estimates derived using synthetic control approach. Impact estimates in the IFE column refer to estimates derived using the interactive fixed effects approach. Permutation inference standard errors are reported for synthetic control, while iid standard errors are reported for interactive fixed effects. RMSE refers to the root-mean squared error. Estimates for all jobs paying < $19, where the control region is defined as PUMAs in the state of Washington. The number of observations equals the number of PUMAs (45) times the number of quarters included in this analysis (41).
***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

(a)

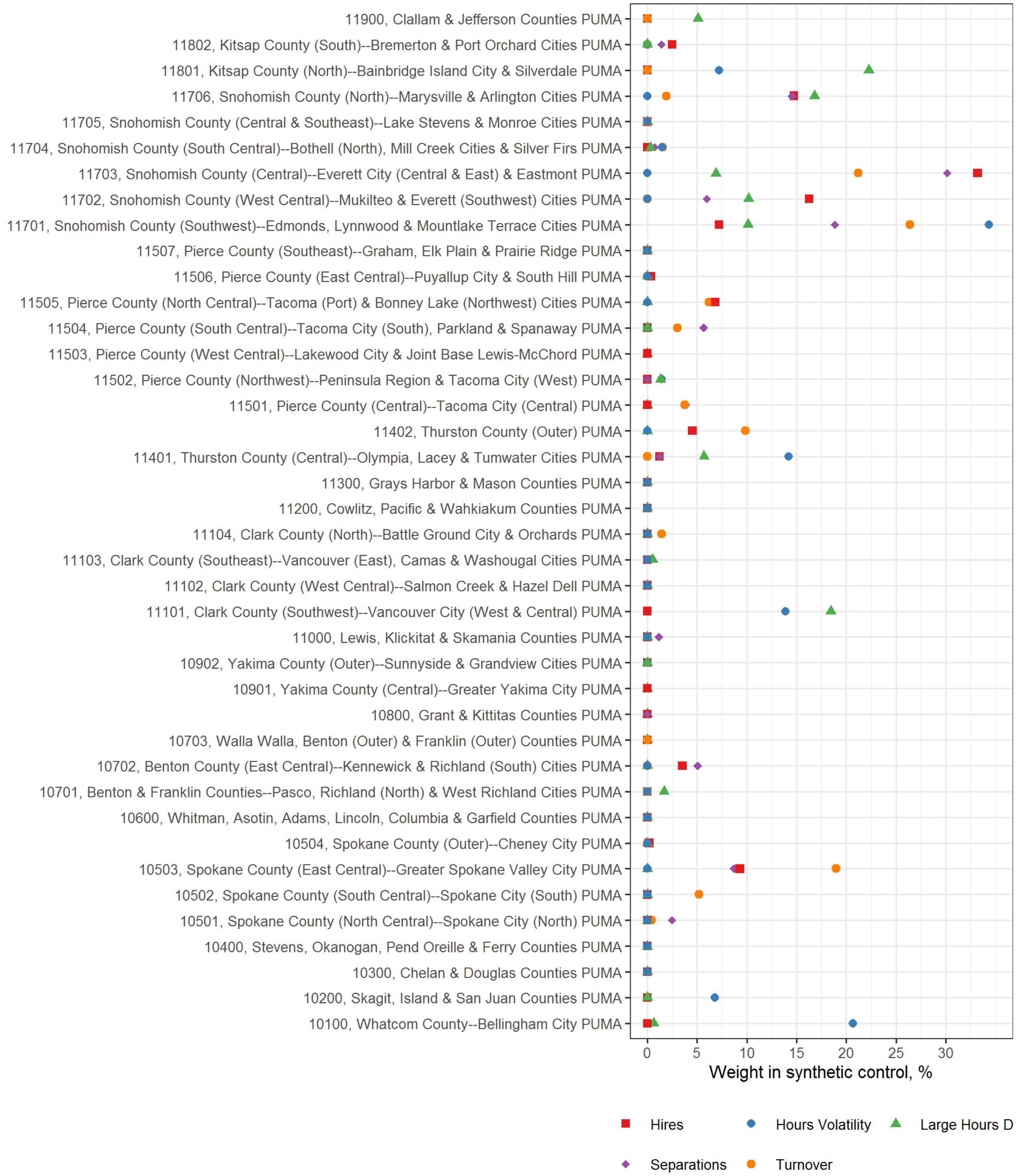

Figure 3.3. Weights chosen for impact estimates using synthetic-control estimator, excluding King County.

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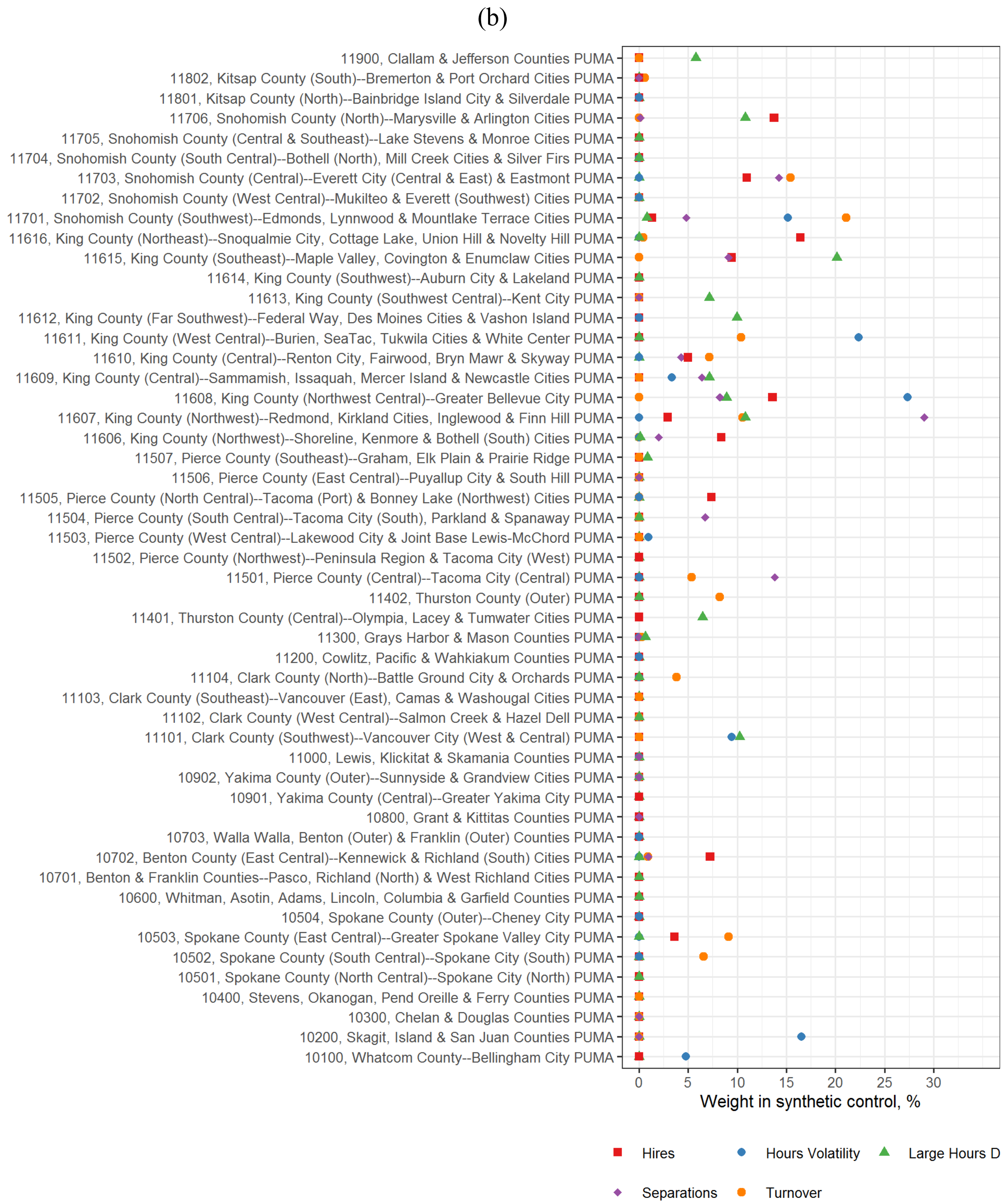
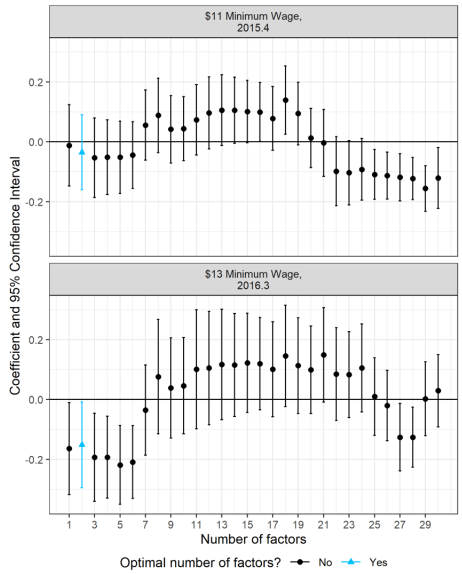


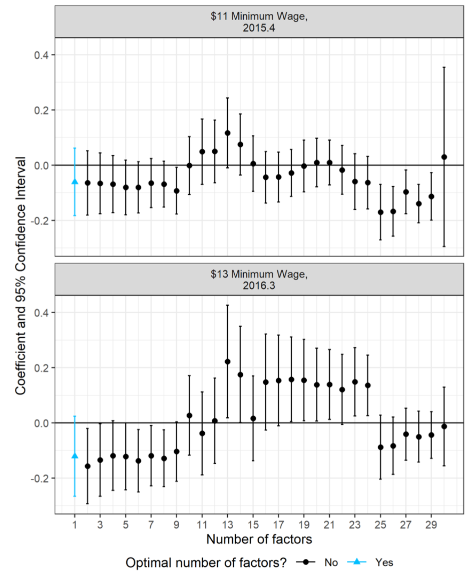
Figure 3.3. *continued*

Source: Author's analysis of Washington state UI program records.

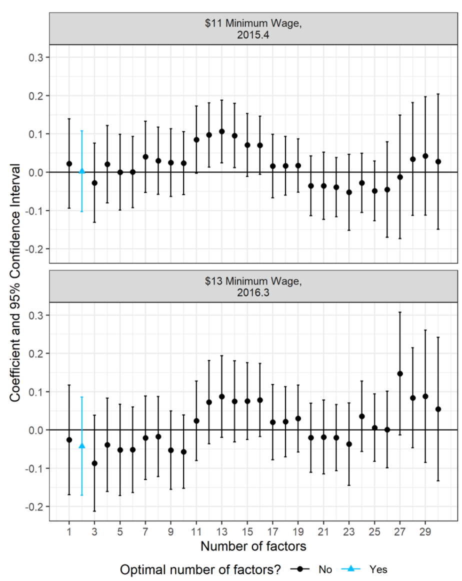
Figures 3.4a-e show the sensitivity of the interactive fixed-effects estimator to the number of factors chosen. The number of factors chosen is determined by the model with the smallest sum of squared residuals, conditional on the number of regions and time observations in the data (Bai & Ng, 2002). Overall the optimal number of factors appears to be low, ranging from one to five factors. The coefficient estimates for the number of factors used to estimate hires and hours volatility takes on a flat U shaped pattern, in which the impact estimates change in directionality after 5-9 factors and then return back to the coefficient estimates similar to that of the main analysis when upwards of 20-25 factors are used. Figures 3.4a and 3.4d suggest some there is some sensitivity to the number of factors used for these outcomes, however only 3.4d reveals sensitivity in precision. The impact estimates for job turnover and the number of large changes, by contrast, are markedly robust to the number of factors chosen. The number of separations is a hybrid of the two trends just described. These impact estimates remain the same up to 15 factors, and after 25 factors, however, the impact estimates change in direction when between 16 and 24 factors are chosen. These impact estimate ranges shown in Figures 3.4a-e show that the impact estimates for large hours declines under the interactive fixed-effect estimator are somewhat sensitive to the number of factors chosen.



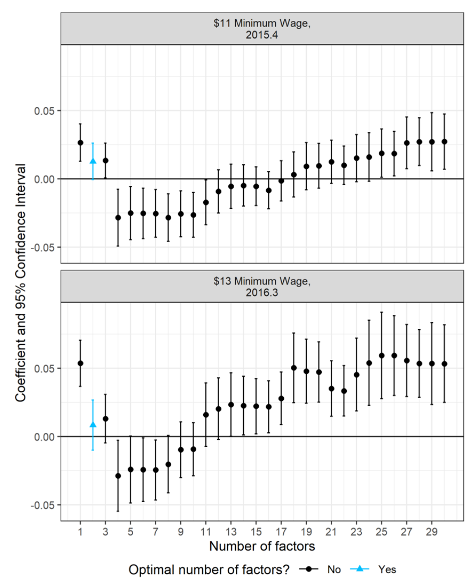
(a) Changes in Hires



(b) Changes in Separations



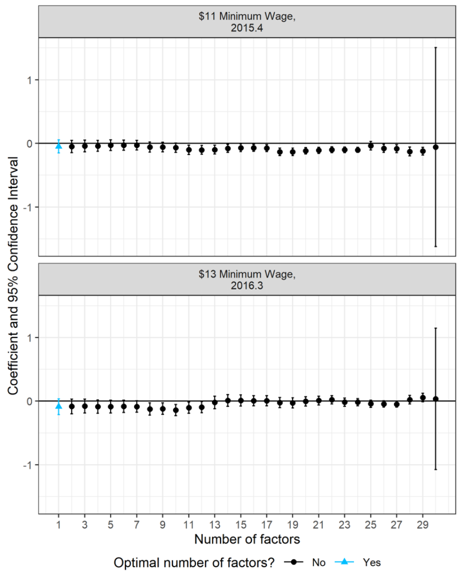
(c) Job Turnover Rate



(d) Hours Volatility

Figure 3.4. Sensitivity of impact estimates of the Seattle Minimum Wage Ordinance on jobs paying <$19 per hour to the number of factors used.

(continued on next page)



(e) Large hours changes

Figure 3.4. *continued*

Source: Author's analysis of Washington state UI program records.

Discussion and Policy Implications

In mandating employers to raise their minimum wages up to $15 over the course of several years, Seattle became a leader in workplace policy, raising the minimum wage by nearly 60 percent. In doing so, the city aimed to advance workplace equity in the low-wage labor market. Jobs which pay low wages are disproportionately jobs with short duration, high turnover rates, and volatile scheduling practices; conditions which research has shown can cause negative health outcomes and economic instability for workers (Bania & Leete, 2009; Morduch & Schneider, 2017; Schneider & Harknett, 2019; Western et al., 2016; Ziliak et al., 2011b). As such, understanding the implications of workplace regulation on employment stability in the low-wage market is critical in advancing workplace equity.

In this study, I assess the impact of the Seattle Minimum Wage Ordinance on employment flows (hires, separations, turnover) and hours-volatility in jobs in the low-wage labor market.

Although the UI data does not capture jobs that are not covered by the UI program, it fully captures jobs subjected to the MWO and allows for a direct examination of low-wage work. I am able to examine the 64 percent of jobs that exist in locatable firms in Seattle, relative to those in the rest of Washington state. In doing so, I contribute a study in which the treatment group spans the entire labor low-wage market and not a proxy for it. I additionally contribute evidence on employment flows from a local minimum wage law. As cities are enacting minimum wage ordinances throughout the country, an in-depth evaluation of the first city to raise its minimum wage up to $15 per hour will provide critical feedback for mayors and city councils around the country considering similar policies.

I find evidence that separations and hires in the low-wage labor market decline following the enactment of the minimum wage, with the most precise reductions occurring in the onset of the second phase-in of the minimum wage. Hires fell by a range of 12.2 to 19.3 percent, and separations fell by 16.9 percent in response to a maximum of a 37 percent increase in the statutory minimum wage. These estimates lead to elasticities ranging from -.31 to -.52 when the denominator is the statutory increase in the minimum wage. Job turnover similarly experienced a less precise but persistent decrease in the second phase-in of the minimum wage (statistically significant estimates show a decline of 8.5 percent). In using the statutory increase in the local minimum wage, the elasticity estimates of hires and separations found in this study are slightly higher to that of prior research, which has documented elasticity estimates for hires and separations to be between -0.23 and -0.37 among teenagers and workers in the restaurant industry (Brochu & Green, 2013; Dube et al., 2016b; Portugal & Cardoso, 2006). In using a more precise treatment group to identify the low-wage labor market, I view my results as an improvement on previous estimates.

Moreover, the estimated wage change from Jardim and coauthors' (2020) analysis on Seattle's MWO indicated that average wages in the low-wage labor market did not increase by 37 percent, but by 3.3 to 3.4 percent, as most jobs were not paid the minimum wage prior to the Ordinance, and not all firms were mandated to increase wages to the maximum amount. Using the estimated wage increase leads to much larger elasticities for hires and separations, ranging from -3.4 to 4.8. Regardless of the denominator chosen, it appears as though Seattle's Minimum Wage Ordinance had a larger impact on job flows than those of state and federal laws (Brochu & Green, 2013; Dube et al., 2016b; Portugal & Cardoso, 2006). The results suggest that employment responses to local minimum wage policy are directionally the same as responses to state and federal minimum wage policies. The difference lies in the magnitude of changes. Local ordinances, with a smaller jurisdiction of coverage, have larger impacts on the employment flows of its jurisdiction, relative to state and federal policies.

As one of the first policy evaluations to estimate the impact of a policy on hours volatility within a job, this study further contributes information on the working conditions within jobs affected by the minimum wage ordinance. I find mixed effects: volatility in hours worked modestly increased in the onset of the second phase-in by 1.5 to 2.8 percent, while the number of large hours drops declined during the second phase-in of the Minimum Wage Ordinance. While the increase in hours volatility may lead to declines in well-being, the decline in large hours drops indicates welfare improvement.

The results imply that workers entering Seattle's low-wage labor market may have a harder time finding employment. The jobs which remain, however, have higher wages, and last slightly longer. These results corroborate evidence on Seattle employment levels, which found a decline in employment over the first and second phase-in and an increase in earnings among workers who remain employed (Jardim et al., 2020). As Seattle City Council's goal was to improve the living standards of workers employed in the city, the evidence points to increased work opportunities for workers in low-wage jobs employed during the Ordinance, which, at higher wages, can be viewed as a welfare improvement.

Future research could further assess the impact of the minimum wage on employment flows by assessing the nonemployment duration of workers and of job transitions to assess whether employment separations resulted in nonemployment or in a new job. A decline in separations to nonemployment, for example, would further the hypothesis that workers are becoming more attached to their Seattle employers. A decline in separations due to quits, however, would indicate that workers are less likely to be able to move freely in the labor market, which could lead to difficulty in garnering wage mobility over time.

In addition, further research should explore the pathways that employment policy can impact employment-volatility within jobs. New data at the weekly and monthly level shows that this scheduling instability can create frequent volatility in earnings and can have deleterious consequences for worker well-being. While some of these employment practices cannot be captured at the quarterly level of the UI data, the results here suggest that overall volatility in hours worked at the quarterly level increased, however the number of large hours drops declined. Employment policy thus has a real potential to affect worker well-being. Future work should examine how employment policy leads to changes in volatility including frequency, directionality and magnitude to provide a more comprehensive picture of employment volatility.

CONCLUSION

The studies presented in this dissertation demonstrate the prevalence of earnings volatility in the low-wage labor market and the effects of local labor regulations on employment and economic stability of workers most likely to experience and be affected by earnings volatility—those earning low wages. Despite affecting all workers (Dynan et al., 2012), earnings volatility is particularly policy-relevant for workers earning low wages. These workers are the most likely to lack the savings, assets, and access to credit that would buffer them against negative income fluctuations. Periods without earnings may affect their ability to pay bills or purchase necessities (Dynarski et al., 1997). Workers earning low wages are also disproportionally susceptible to precarious employment practices and jobs that have uncertain employment practices, such as the practice of scheduling workers for many hours one week and a few hours the next week to meet changing consumer demand (Henly & Lambert, 2014; A. L. Kalleberg, 2012; A. L. Kalleberg & Marsden, 2013; Schneider & Harknett, 2019). The rise in precarious work and the impact that precarious jobs can have on workers' economic security merits a deeper understanding of the relationship between employment instability and earnings volatility, and the degree to which policy affects the low-wage labor market.

In this dissertation, I contribute new knowledge on the trends and determinants of earnings instability for workers in low-wage jobs, illustrating the implications of the shift in risk in the labor market from firms to workers. Additionally, I expand the breadth of economic experiences beyond what evaluation studies typically consider, including workers' employment and earnings volatility as outcomes of interest. I evaluate the impact of two local policies, Seattle's Paid Sick and Safe Time Ordinance and Seattle's Minimum Wage Ordinance, on workers' employment flows and hours volatility, contributing new estimates on how employment policy affects workers earnings and employment volatility.

Documenting the scope of earnings and employment volatility in the low-wage labor market is important for policy scholars because policymakers have the ability to create policy to protect workers from labor market inequities and risk. These dimensions—measures of employment and earnings volatility—have been shown to have deleterious effects on worker health and well-being (Schneider & Harknett, 2019). Public policy that improves the stability of work, therefore, has the potential to improve worker health and well-being. Local policy is a particularly salient arena to conduct research. Cities and states across the country have enacted employment policies, such as paid sick leave and minimum wage laws, without clear evidence about how these policies will affect the low-wage labor market. Evaluation of these policies will inform policymakers across the country how changes in minimum wages and paid sick leave access can affect the economic security of workers in their jurisdiction.

In chapter 1, I analyzed intra-year measures of earnings volatility using quarterly administrative data for the entire formal workforce in Washington State. While these data will not cover volatility that occurs week-to-week or month-to-month, they are an improvement from survey data, which generally only captures volatility year-to-yearchanges in income. Using administrative data further allowed me to override the issues of non-response, seam bias, and underreporting that can plague volatility estimates from surveys (B. D. Meyer et al., 2015). I found that the vast majority (84.0 percent) of workers in low-wage jobs experienced an earnings change that is greater than 25 percent of their previous quarters' earnings within a year. The share of workers in low-wage jobs experiencing large changes is higher than that found in the prior literature, and this finding should give policymakers pause when evaluating how well the formal labor market is working for workers in low-wage jobs. While intra-year earnings volatility is relatively stable over the period, workers earning low wages are much more likely to experience declines during economic downturns and increases during a period of strong labor market growth. And yet, despite 2011-2016 being characterized as a recovery period defined by economic health, the magnitude of large increases was smaller than those of large decreases.

Further, I found that transitions in and out of UI-covered work accounted for nearly two-thirds of the total intra-year earnings volatility over that period. Of the remaining third, I find that volatility from workers' hours worked has a large, significant, and positive effect on intra-year earnings volatility. An increase in hours volatility by one percentage-point led to a 0.99 percentage-point increase in earnings volatility. The magnitude of this relationship is smaller for workers outside of the arts, entertainment, food, and accommodation industry. These results suggest that within-year variation at workplaces deserves more attention. Policies such as the recent secure scheduling laws and guaranteed minimum hours law can go a long way in reducing the "risk of employment" for workers if the costs of these policies are not passed down to workers. If employers cannot create predictable schedules, policymakers could consider regulations that ensure workers have a right to refuse certain scheduling practices, thereby providing them a degree of control over scheduling their time to work.

Chapter 2 evaluated Seattle's Paid Sick and Safe Time (PSST) policy on employment outcomes for firms and workers. In reducing exposure to infectious disease, the PSST policy has the potential to cultivates healthier, more productive workers, who are more likely to be employed, have longer job duration, and lower job turnover, earnings volatility, and hours volatility. I found that firms affected by the policy experienced a modest increase in hours worked and no change in employment, earnings, hires, separations, or job turnover. Workers newly covered by the PSST mandate experienced no change in their likelihood of remaining employed, their hours worked, or their quarterly earnings in each of the four cohorts studied (2011-2014 cohorts). These results persist for subgroup analysis of part-time and low-earnings workers. Workers did not experience a meaningful change in their employment flows, or in their employment volatility as a result of the PSST policy.

Results from this analysis suggest that the policy accrual rules may be too small to make a meaningful difference in workers' lives. It is also possible that workers may not know about the PSST law or that they have access to paid sick leave time. Previous research on workers' policy knowledge suggests that many workers in low-wage jobs do not know their rights at the time newly mandated wages and benefits are enacted (H. Hill & Wething, 2019). This explanation can also be applied to low-earnings workers' knowledge about their new access to a paid leave benefit. Other factors, such as employee health, reductions in contagion, and employee morale, while not readily evidenced in this study, should be considered as potential mechanisms in future paid sick leave legislation. The evidence here corroborates a larger body of work that paid sick leave policies are not costly to employers to implement and could potentially be strengthened to provide more generous leave for workers in the future.

Finally, chapter 3 evaluates the Seattle Minimum Wage Ordinance, which intended to improve equity in compensation for workers in its low-wage labor market. In this chapter, I assessed the impact of the Seattle Minimum Wage Ordinance on employment flows (hires, separations, turnover) and hours-volatility in the low-wage labor market. Wage increases have the opportunity to provide economic security to affected workers if these increases not curtailed by declines in employment or large increases in hours volatility on the job. I found evidence that workers entering Seattle's low-wage labor market may have a harder time finding a job. Hires and separations in the low-wage labor market declined following the enactment of the minimum wage; hires fell by a range of 12.2 to 19.3 percent, and separations fell by 16.9 percent in response to the maximum of a 37 percent increase in the statutory minimum wage. Job turnover decreased, commensurately by 8.5 percent. I found mixed effects on employment volatility. Volatility in hours worked increased modestly during the onset of the second phase-in by 1.5 to 2.8 percent, however the number of large hours drops declined during the second phase-in of the Minimum Wage Ordinance. The results suggest that jobs which remained post policy last slightly longer, however may exhibit higher levels of hours volatility, leading to mixed effects on whether economics security for workers improved as a result of the MWO.

Taken together, the papers in this dissertation synthesized a diverse body of theory and methods from fields including economics, sociology, and cognitive and behavioral science to identify and respond to theoretically interesting and policy-relevant research questions. The analysis contributed to efforts to develop a nuanced understanding of how intra-year earnings volatility has evolved in low-wage work and how employment policy mitigated or exacerbated this volatility. Two of the papers focused on local public policy, a domain of policy that is growing across the country but has received insufficient attention to date.

The evidence in this dissertation showed that workers in low-wage jobs experienced substantially high levels of intra-year volatility over the 2006 to 2016 decade. Nearly 66 percent of earnings volatility for workers earning low-wage jobs was driven by movements in and out of work. While some of this movement may be voluntary, some if it may not be and future research should investigate the degree to which employment changes are voluntary, and the share of low-wage earnings that is comprised by formal work. Among the 34 % of volatility that existed within work, hours fluctuations drove the majority of it, indicating that workers experiencing hours volatility are not experiencing wage growth, just more complexity in their work. These results suggest that policies that aim to stabilize schedules, like the secure scheduling and fair work week laws could be one way to promote stability within work.

This dissertation also served as a proof of concept that local employment policies can affect earnings and employment volatility, suggesting that policy researchers should consider expanding their employment outcomes to include these metrics. In assessing the employment and earnings effects of local policy, evidence from this dissertation suggests that the design and marginal cost of policy implementation can make a difference in its impact on affected employment and earnings. In the PSST policy, employees accrued one hour of paid sick time for every 30-40 hours worked (a marginal cost of up to 3.3 percent if all hours of paid sick leave are used). Under this design, the policy didn't affect employment and earnings outcomes for firms and workers right around the coverage threshold. By contrast, the MWO did not rely on an accrual policy and raised the wages of low-wage jobs substantially and automatically by up to 37.3 percent. This larger marginal cost, coupled with a straightforward implementation pathway, led to substantial impacts on the employment flows among low-wage jobs. Careful attention to the ways in which workers and firms differentially respond to employment policy within and between work is necessary to identify and address these gaps and improve outcomes for both workers and firms.

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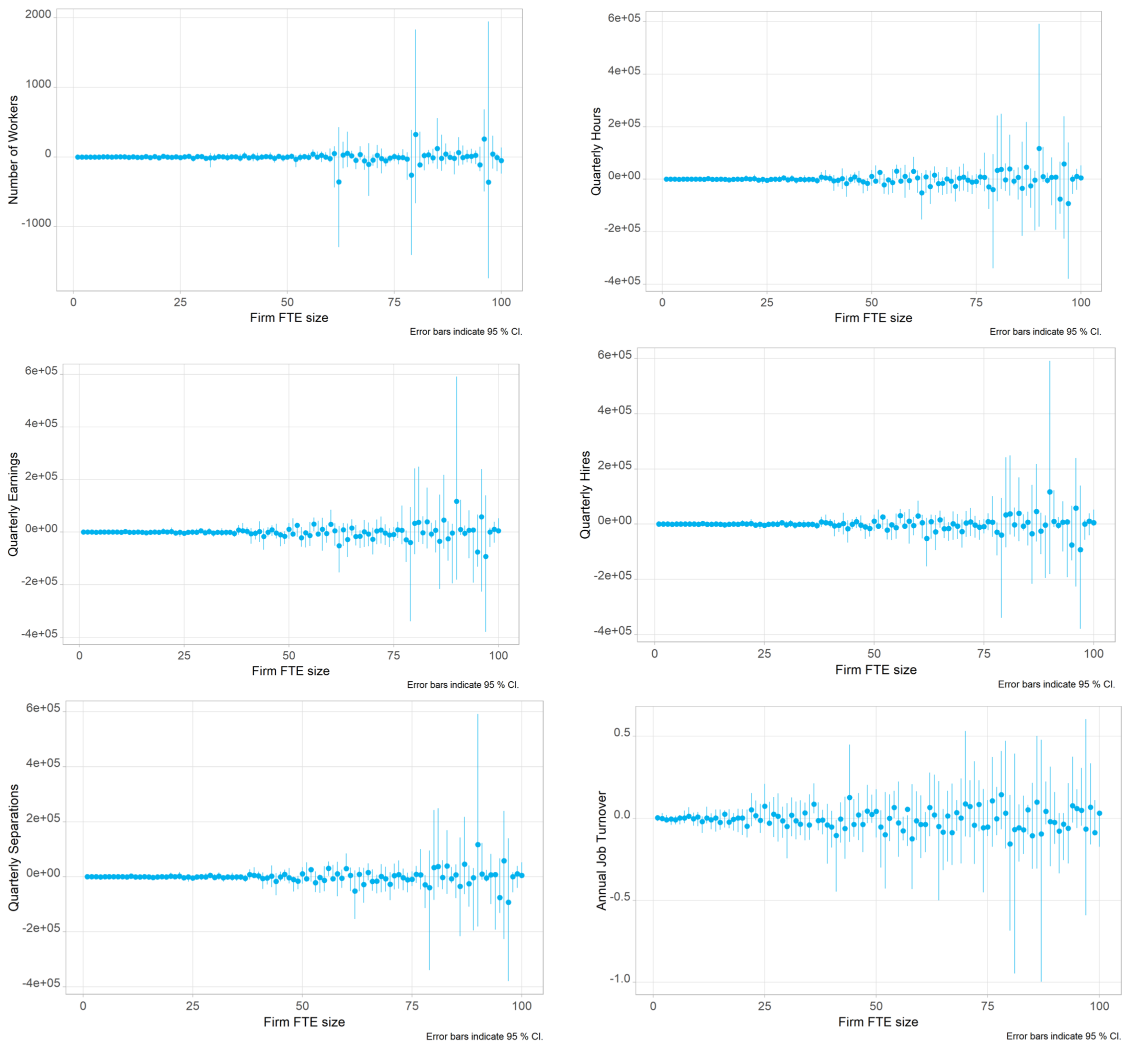
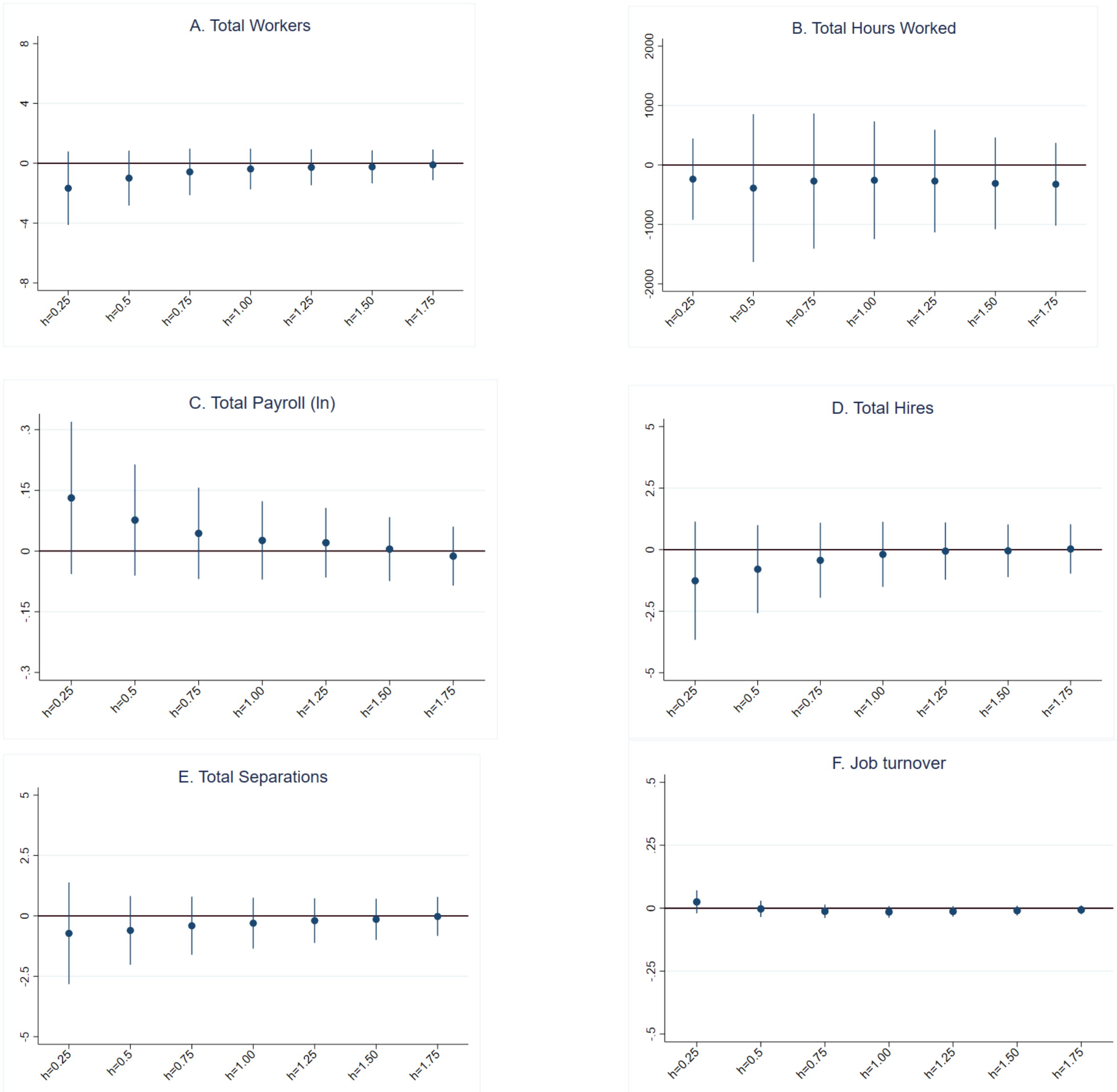
APPENDIX A

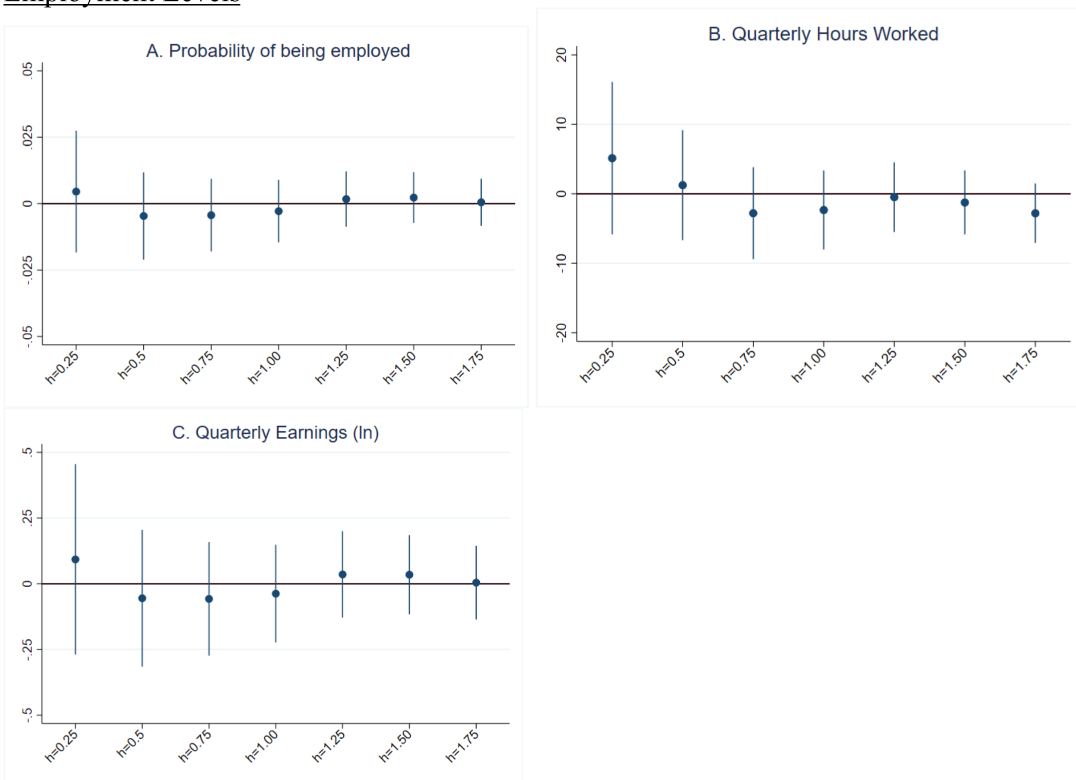


Appendix Figure 1Aa-f. Local average treatment effects on firm-level outcomes using a regression discontinuity design for cutoff points ranging from 1 FTE to 200 FTE, 2013-2014
Source: Author's analysis of Washington state UI program records.
Notes: The graphic shows the treatment effects and the 95 percent confidence interval for each impact estimate, for cutoff point ranging from 1 FTE to 100 FTEs. Treatment effects are estimated using a local linear estimator, weighted with a triangular kernel, and using bandwidths chosen manually (h=2).

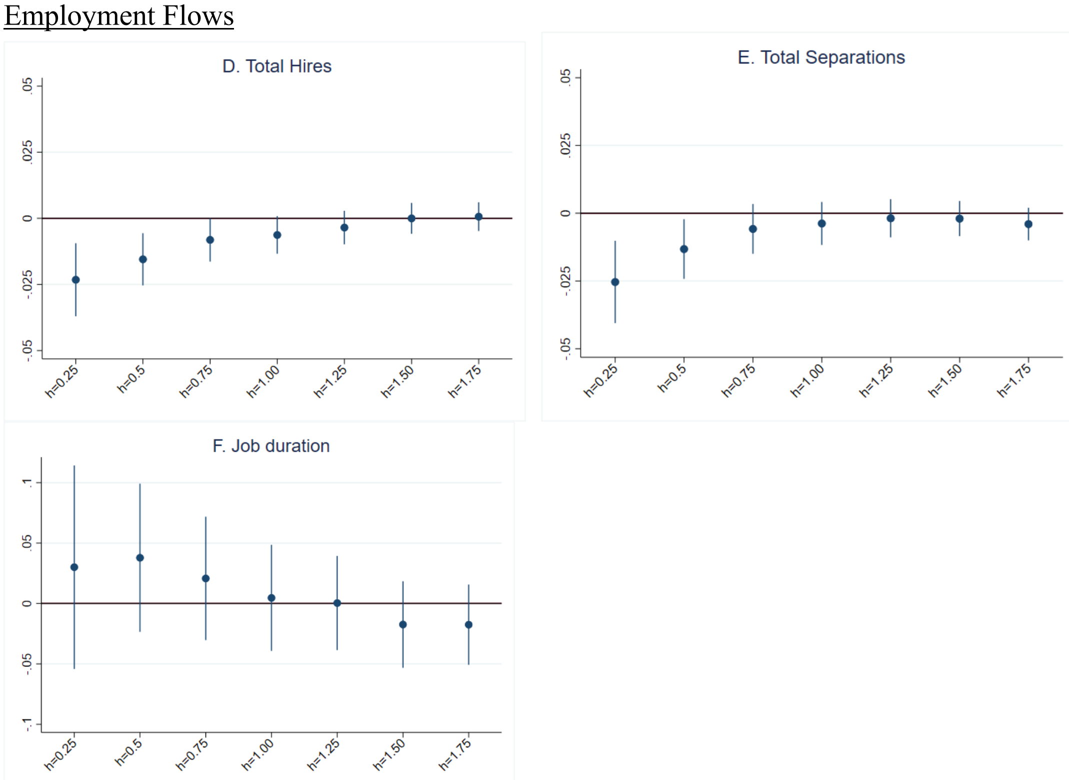


Appendix Figures 2Aa-f. Local average treatment effects for firm-level outcomes using a regression discontinuity design with various sizes of bandwidths, 2013-2014.
Source: Author's analysis of Washington State UI program records.
Notes: Treatment effects are estimated using a local linear estimator, weighted with a triangular kernel, and using bandwidths that are manually chosen to range from 0.25 FTE to 1.75 FTE employees on either side of the threshold. Each dot illustrates the coefficient and 95 percent confidence interval of the estimates at various bandwidth ranges.

Employment Levels



Employment Flows



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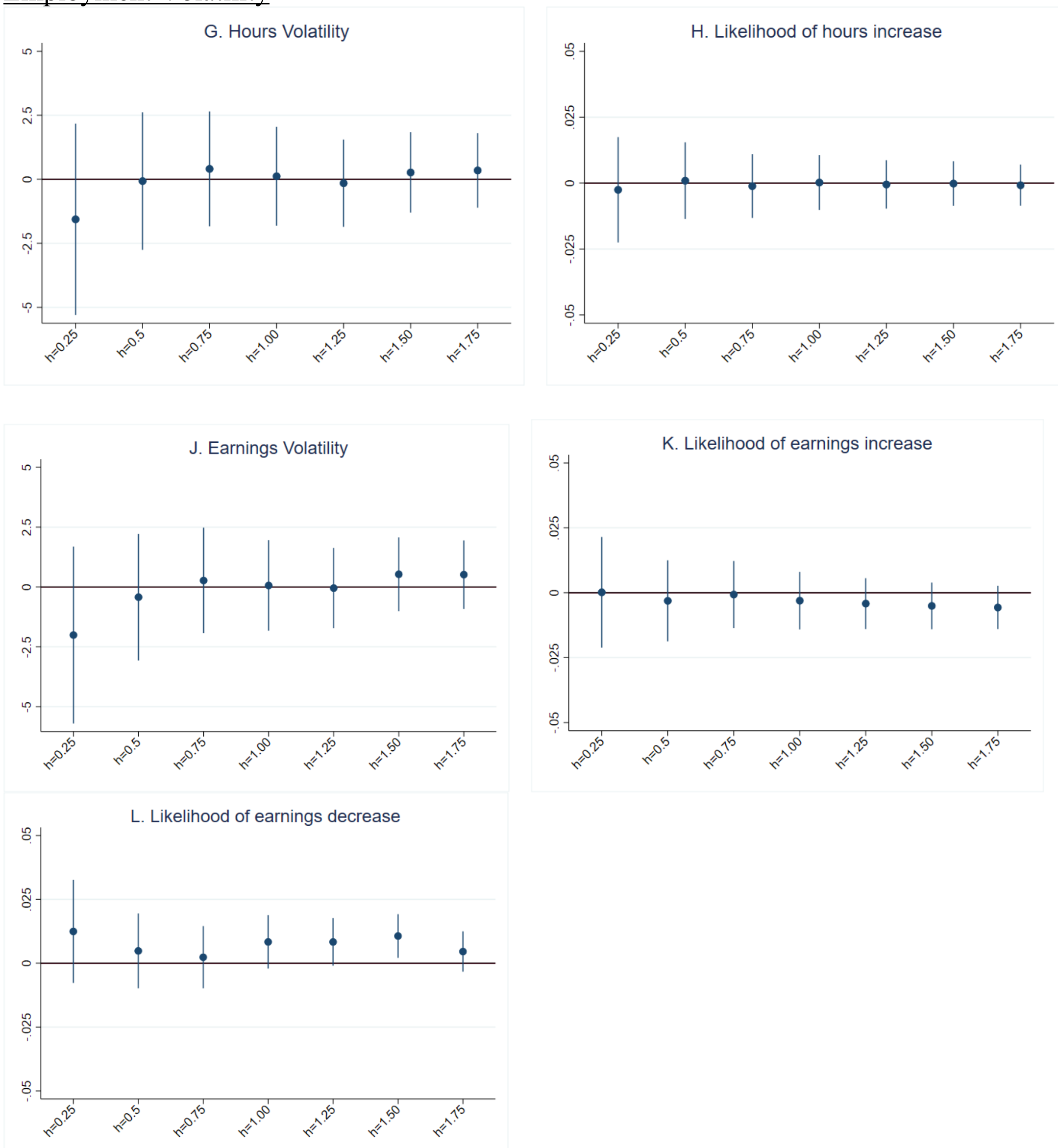
<u>Employment Volatility</u>



Appendix Figure 3Aa-l. Treatment effects for worker-level outcomes using a local linear regression, 2014 cohort

Source: Author's analysis of Washington State UI program records.

Notes: Each estimate reflect the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter using FTE employment sizes ranging from 0.25 FTEs to 1.75 FTEs on either side of the threshold. Each dot illustrates the coefficient and 95 percent confidence interval of the estimate at various bandwidth ranges. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

Appendix Table 1A. Treatment effects for worker-level outcomes using a difference-in-differences design, 2011-2014 cohorts, restricting to workers' who remain employed in Seattle

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0.007	-0.004	-0.003	-0.003
	(0.006)	(0.006)	(0.006)	(0.006)
Quarterly hours worked	2.076	-3.846	-1.982	-3.093
	(2.801)	(2.715)	(2.738)	(3.01)
Quarterly earnings (log)	0.105	-0.066	-0.049	-0.034
	(0.093)	(0.09)	(0.09)	(0.1)
B. Employment Flows				
Likelihood of being hired this quarter	0	0.011***	0.007**	-0.005
	(0.003)	(0.004)	(0.004)	(0.004)
Likelihood of separating next quarter	-0.002	0.004	-0.001	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Job duration (quarters)	-0.026	-0.022	-0.006	0.005
	(0.021)	(0.021)	(0.021)	(0.022)
C. Employment Volatility				
Arc percent change (abs.) in hours worked	-0.974	1.821**	0.358	-0.091
	(0.968)	(0.928)	(0.951)	(1.004)
Likelihood of an hours increase	0.001	-0.007	0.003	-0.002
	(0.005)	(0.005)	(0.005)	(0.005)
Likelihood of an hours decrease	0.008	-0.001	-0.004	0.015***
	(0.005)	(0.006)	(0.005)	(0.005)
Arc percent change (abs.) in earnings	-0.2	2.168**	0.542	0.016
	(0.951)	(0.915)	(0.933)	(0.984)
Likelihood of an earnings increase	0.007	0.011**	0.007	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Likelihood of an earnings decrease	0	-0.001	-0.004	0.004
	(0.006)	(0.006)	(0.005)	(0.006)
Quarter FE	Y	Y	Y	Y
Worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	88,536	90,048	88,608	87,248
Persons	11,067	11,256	11,076	10,906

Source: Authors' analysis of Washington state UI program records.

Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

Appendix Table 2A. Treatment effects for worker-level outcomes using a difference-in-differences design, 2011-2014 cohorts, restricting to workers who remained assigned treatment and comparison FTE employment size

	(1) 2011	(2) 2012	(3) 2013	(4) 2014
	Policy passage	Policy enactment	Post-policy	Post-Policy
A. Employment Levels				
Likelihood of being employed	0	0.005	-0.009**	0.001
	(0.005)	(0.005)	(0.005)	(0.005)
Quarterly hours worked	2.228	2.405	-1.493	1.405
	(2.92)	(2.76)	(2.788)	(3.099)
Quarterly earnings (log)	0.019	0.066	-0.136*	0.007
	(0.077)	(0.077)	(0.072)	(0.081)
B. Employment Flows				
Likelihood of being hired this quarter	0.001	0.005	-0.002	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)
Likelihood of separating next quarter	0	0.002	-0.007	-0.001
	(0.004)	(0.004)	(0.005)	(0.005)
Job duration (quarters)	0.01	-0.02	0.031*	0.004
	(0.015)	(0.016)	(0.017)	(0.017)
C. Employment Volatility				
Arc percent change (abs.) in hours worked	-1.489	0.58	1.112	-0.907
	(1.131)	(1.097)	(1.101)	(1.129)
Likelihood of an hours increase	-0.012	0	-0.007	-0.007
	(0.008)	(0.009)	(0.008)	(0.009)
Likelihood of an hours decrease	0.009	0.012	0.006	0.014
	(0.009)	(0.009)	(0.009)	(0.009)
Arc percent change (abs.) in earnings	-1.076	0.423	0.779	-0.48
	(1.112)	(1.098)	(1.082)	(1.126)
Likelihood of an earnings increase	-0.006	0.006	-0.009	0.003
	(0.009)	(0.009)	(0.009)	(0.009)
Likelihood of an earnings decrease	-0.008	0.012	0	0.001
	(0.009)	(0.009)	(0.008)	(0.009)
quarter FE	Y	Y	Y	Y
worker FE	Y	Y	Y	Y
S.E.s clustered by worker	Y	Y	Y	Y
Observations	29,032	29,968	29,536	27,400
Persons	3,629	3,746	3,692	3,425

Source: Authors' analysis of Washington state UI program records.

Notes: Each estimate reflects the interaction of the post-policy period of each cohort with the interaction of being employed in a firm with more than for FTE employees in each cohort's baseline quarter. Models include person and quarter fixed effects. Standard errors are clustered at the worker level.

APPENDIX B

Appendix Table 1B. Falsification Test: Impact of the Seattle Minimum Wage Ordinance on jobs paying > $19 per hour using a difference-in-differences strategy

Quarter	Quarters after Passage/ Implementation	Change in Hires	Change in Separations	Job turnover Rate	APC Quarterly Hours	Change in large hours declines
2014.3	1	0.063***	0.006	0.003*	0.009***	-0.068***
		[0.022]	[0.86]	[0.096]	[0.000]	[0.000]
2014.4	2	-0.123***	-0.066**	-0.006***	0.011***	0.017
		[0.000]	[0.047]	[0.003]	[0.000]	[0.241]
2015.1	3	-0.273***	-0.015	-0.003	0.006***	-0.037***
		[0.000]	[0.65]	[0.103]	[0.000]	[0.008]
2015.2	4/1	-0.051*	-0.036	0.006***	0.015***	-0.087***
		[0.066]	[0.276]	[0.003]	[0.000]	[0.000]
2015.3	5/2	-0.003	-0.071	0.004	0.02***	-0.03
		[0.963]	[0.265]	[0.326]	[0.000]	[0.287]
2015.4	6/3	-0.062	-0.015	-0.003	0.024***	0.03
		[0.25]	[0.82]	[0.436]	[0.000]	[0.294]
2016.1	7/4	0.306***	-0.069	-0.009***	0.024***	-0.009
		[0.000]	[0.276]	[0.024]	[0.000]	[0.741]
2016.2	8/5	-0.038	-0.285***	0.002	0.029***	-0.119***
		[0.482]	[0]	[0.561]	[0.000]	[0.000]
2016.3	9/6	0.045	-0.012	0.009	0.041***	0.043
		[0.599]	[0.904]	[0.124]	[0.000]	[0.323]
R2		0.83	0.77	0.85	0.97	0.91
Number of Observations		82	82	82	82	82

Source: Author's analysis of Washington state UI program records.

Notes: Quarter numbers denote the time post-passage/ time post implementation. APC quarterly hours refers to the quarterly arc percent change in hours worked within a job. Number of large declines refers to the number of hours declines greater than 25 percent within a job.

***, **, and * denote statistically significance using a two-tailed test with $p \leq 0.01$, 0.05, and 0.10, respectively.

VITA

Hilary Wething is a Ph.D. candidate in Public Policy and Management, with a graduate certificate in Demographic Methods from the Center for Studies in Demography and Ecology, at the Daniel J. Evans School of Public Policy and Governance at the University of Washington. Her research examines the relationship between economic volatility and labor market policy, household decision-making, and social safety-net programs. Her dissertation investigates the degree to which workers' employment characteristics affect their earnings volatility and assesses whether public employment policy, such as minimum wage or paid sick leave policy, can mitigate or exacerbate earnings volatility. Wething brings an inter-disciplinary theoretical frame to these questions and conducts quantitative and qualitative research using innovative administrative, survey, and interview data. She has undergraduate degrees in Mathematics and Economics from Creighton University and spent three years as a research assistant at the Economic Policy Institute in Washington, DC.