# For Better or Worse? The Economic Implications of Paid Sick Leave Mandates*

Turk Al-Sabah** Paige Ouimet***

Current Draft: 11/18/2022
First Draft: 10/30/2021

**Abstract**

Public calls for a national paid sick leave policy continue to mount in the United States. Using the staggered adoption of local and state mandates, we document an average increase of 1.5% in employment following the enactment of a paid sick leave policy. As predicted, workers with ex ante lower access to paid sick leave drive the employment effect. Several mechanisms can explain our findings. Paid sick leave mandates are associated with a decline in labor turnover, an increase in the labor supply, and an increase in household income, which creates positive spillover effects on local markets. Moreover, firms exposed to the mandate experience a significant increase in operating profit – benefits firms may not be able to achieve through voluntary actions, in the absence of a mandate, due to adverse selection.

**JEL Classification:** *G51, I18, J21, J23, J32, J63*.
**Keywords:** paid sick leave, employment, labor turnover, labor productivity, financial health.

*We would like to thank Jonathan Cohn (discussant), Jesse Davis, Cami Kuhnen, Elena Simintzi, and seminar participants at the Labor and Finance Group, Istanbul Finance Seminar Series, SOLE, Nova, Purdue University, University of Houston, University of Iowa, University of Melbourne, and the University of North Carolina, for their valuable comments and feedback.
**Kenan-Flagler Business School, UNC-Chapel Hill. Email: Turk_Alsabah@kenan-flagler.unc.edu
***Kenan-Flagler Business School, UNC-Chapel Hill. Email: Paige_Ouimet@kenan-flagler.unc.edu

# 1. Introduction

The United States stands out as the only wealthy nation that does not provide comprehensive federal paid sick leave.[1] As of 2010, the Bureau of Labor Statistics (BLS) estimated that only 63% of workers in the private sector had access to paid sick leave. In the absence of a federal mandate, multiple states and localities have enacted their own legislation requiring firms to provide paid sick leave coverage to all local employees. These laws have been shown to have positive public health benefits, such as reducing transmission of the seasonal flu (Pichler and Ziebarth, 2017); however, less is known about the economic implications of mandatory paid sick leave, a question we investigate in this paper.

Economic theory suggests that mandatory paid sick leave legislation has an ambiguous impact on labor market outcomes. In a frictionless competitive labor market, mandated benefits, such as paid sick leave, impede the free operation of the labor market by restricting the flexibility of employers and workers to voluntarily choose the optimal compensation package. As a mandate will increase total compensation costs for firms, labor demand may decline, particularly for low-wage workers where minimum wages are more likely to be binding. Labor demand may also be negatively affected to the extent that paid sick leave mandates increase shirking behavior, which could lower worker productivity.

Alternatively, mandatory paid sick leave may improve economic efficiency in the presence of market failures (Summers, 1989; Ruhm, 1998). One important source of market failure is adverse selection under asymmetric information. Workers generally have better information about their health condition and the probability of using sick leave than their employers. Therefore, companies that provide sick leave voluntarily will attract a disproportionate number of workers who are more likely to be sick and use up all of the provided leave, resulting in increased costs for these companies. Mandatory sick leave has the potential to eliminate this type of sorting behavior and improve welfare. As such, mandatory paid sick leave can achieve benefits that firms cannot achieve through

[1]See https://cepr.net/report/contagion-nation-2020-united-states-still-the-only-wealthy-nation-without-paid-sick-leave/

voluntary actions.[2]

Since marginal workers will receive benefits from the mandate, mandatory paid sick leave legislation should also increase the labor supply, especially for groups that value sick leave benefits and were unlikely to have access to them in the absence of a mandate. Moreover, by offering job security to workers who have to miss some time due to a short-term illness, and by decreasing the incentive for workers to switch jobs depending on sick leave availability, sick leave mandates may reduce labor turnover. Therefore, expenses associated with hiring and training replacements might fall for employers. These factors are expected to have positive implications for labor demand.

Mandatory paid sick leave could also improve the health of workers and promote a healthier workplace (Asfaw et al., 2012; Pichler and Ziebarth, 2017; Stearns and White, 2018; Stoddard-Dare et al., 2018). Lastly, demand for local goods and services is expected to rise, following the adoption of a paid sick leave policy, given that low-income households, who have a high propensity to consume, are predicted to benefit more from the mandate.

We investigate the employment effects of local and state mandates using a difference-in-differences design around the implementation of mandatory paid sick leave legislation. We find that following implementation, private sector employment in treated counties increases by 1.5%, on average. This treatment effect is higher when the law mandates more generous sick leave or in counties where a higher percentage of employees is assumed to not have paid sick leave benefits in the absence of a legal mandate. Moreover, we observe a larger positive employment effect in counties where a high percentage of the population reports having poor health ex ante.

While we cannot conclusively rule out non-causal interpretations of our results, due to the endogeneity of the passage of paid sick leave laws, our results are most consistent with a causal relationship between paid sick leave mandates and employment. For

[2] Another type of market failure is imperfect information. In the absence of perfect information, workers and/or employers plausibly value the benefits of paid sick leave imprecisely. If workers and/or employers tend to underestimate these benefits, then mandating sick leave could raise welfare by supplying the amount of sick leave that reflects the actual value of these benefits (Summers, 1989; Baum, 2003).

one, we show the absence of any pre-trends in employment prior to the implementation of paid sick leave mandates and that our results are robust to using a synthetic control estimator as well as estimators that allow for treatment effect heterogeneity. Second, we explore within-county variation by looking at populations of workers for which a paid sick leave mandate is most likely to be binding, lower skill employees with a high school education or less and workers in industries that provide sick leave for less than 60% of their workers. In these specifications, we include county by quarter-year fixed effects, thereby absorbing all time-varying county-specific omitted variables and document stronger treatment effects among the subpopulations most sensitive to the mandate.

Several non-mutually exclusive mechanisms can explain our findings. For one, Dube et al. (2016) show that for a broad class of job ladder models, a shift in compensation leads to a decline in employee turnover. Moreover, separations may be even more sensitive to an increase in paid sick leave, relative to cash compensation if (in the absence of paid sick leave benefits) employees are more likely to quit or be fired after an illness. Consistent with this argument, Hill (2013), using the Medical Expenditure Panel Survey, finds a 25% lower probability of separation among workers with paid sick leave, after controlling for a large set of job and worker characteristics.

We test this prediction and find a 2.5% decrease in the county-level separation rate following the adoption of a paid sick leave mandate. This decrease in the separation rate is most pronounced among workers most likely to be affected by the mandate, low-skill workers and workers who have poor health — a pattern consistent with the heterogeneity in employment effects. Assuming workers learn while on-the-job, then lower turnover will increase labor productivity, potentially impacting labor demand.[3]

Alternatively, mandatory sick leave benefits may increase the labor supply, thus impacting equilibrium employment, if more individuals are willing to enter the labor force

[3]While we can directly observe turnover, it is more difficult to establish the link to employee productivity. However, there are a number of reasons to expect that productivity may increase following a paid sick leave mandate. Stearns and White (2018) provide results which suggest that workers are less likely to work while sick following the implementation of a sick leave mandate, decreasing presenteeism and increasing productivity. In addition, paid sick leave benefits reduce the probability of suffering from an occupational injury (Asfaw et al., 2012) and improve mental health (Stoddard-Dare et al., 2018).

if provided with sick leave benefits. Consistent with this channel, we find an ex-post increase in employment among workers in subpopulations that should be most sensitive to a paid sick leave mandate, including older workers considering retirement, individuals with poor health, and individuals with young children. We also document a modest increase in migration to counties with paid sick leave mandates.

Finally, our results could be driven by shocks to household wealth spilling over into the local economy from continuing to receive wages while sick and experiencing less frequent unemployment spells. Consistent with this argument, we observe a 2% increase in household income, following the adoption of a paid sick leave mandate, with the gains concentrated among low-income households. Moreover, we find decreases in the percentage of individuals with subprime credit ratings, the percentage of individuals who are living in poverty, the percentage of workers who are without health insurance, the aggregate number of bankruptcy filings, and in income inequality within a county.

While we cannot pinpoint one exclusive mechanism as the driver of our results, we can exclude several mechanisms. For example, a potential explanation for the positive employment effects is that firms hire more to cover for workers who are taking sick leave. Maclean et al. (2020) find that the average employee takes 2 more hours of sick leave after a mandate becomes applicable, 0.1% of the typical work hours of a full-time employee, suggesting this mechanism is unlikely to explain the whole effect. Alternatively, firms may compensate for the costs of the mandate by reducing workers' wages. However, we do not find any evidence of a depression on wages, nor do we observe a shift to part-time employment.

The magnitude of the employment effect that we document in this paper is large, especially when compared with the costs to firms from providing paid sick leave. However, from the worker's point of view, the benefits of paid sick leave are large. Without paid sick leave, workers may not be able to schedule routine health visits or care for an ill child. DeRigne et al. (2016) find that workers without paid sick leave are three times more likely to forgo medical care for themselves and 1.6 times more likely to forego medical care for their family. Given the importance of sick leave for the health of workers and their families, it is unsurprising to observe significant effects on workers following a mandate, including lower employee turnover and a higher willingness to supply labor. We also document impacts on workers at the firm-level. We observe a significant improvement in a company's overall rating on Glassdoor, its work/life balance rating, and its culture/values rating, following the implementation of a mandate in the headquarters location.

In the final part of the paper, we turn to firm-level implications. We find that paid sick leave mandates have positive implications for firms, which suggests that the gains from the employee's perspective are not coming at the expense of firms. Following the enactment of a paid sick leave mandate in the headquarters location, firms experienced a 1.8 percentage point increase in ROA. Given that the mandates require firms to provide paid sick leave benefits to all employees within the mandate's jurisdiction, we construct an alternative treatment variable based on the percentage of the firm's employees operating in treated locations and find similar results. Moreover, consistent with a causal interpretation of our results, the increase in ROA is concentrated on firms that do not provide voluntary paid sick leave benefits ex ante.

We relate to three literatures. First, the literature on paid sick leave. The prior literature has documented numerous health benefits associated with paid leave, including decreases in population-level infectious disease rates, as in Stearns and White (2018), better mental health, as in Stoddard-Dare et al. (2018), and fewer workplace injuries, as in Asfaw et al. (2012). In more related work, Ahn and Yelowitz (2015) examine the short-run impacts of Connecticut's paid sick leave legislation and find that the mandate had modest but negative employment effects. Alternatively, Pichler and Ziebarth (2018), using a broader sample of events, find that paid sick leave mandates had an insignificant impact on county employment and wages. Our results build on these earlier papers examining the economic consequences.[4] We document that employment increases, following paid sick leave mandates, with employment increases accruing over time with the peak

[4] Another strand of the literature focuses on paid sick leave laws in Europe. See, for example, Henrekson and Persson (2004), Ziebarth and Karlsson (2014), and De Paola et al. (2014). These studies are less relevant to the U.S. given differences in the generosity of such programs and the availability of government subsidies in Europe.

effect occurring three years after the law became effective. As such, earlier papers, that were limited in the time series of available post-mandate years following these recent laws, show more modest effects. Moreover, we make stronger conclusions in support of a causal relation by using within-county variation and highlighting specific mechanisms through which the effect occurs.

Second, we contribute to the literature on firms and employee health. Almeida et al. (2021) study the effect of Obamacare on firm employment and performance. Cohn and Wardlaw (2016) find that workplace injury rates are higher for financially constrained firms. Following private equity buyouts, Cohn et al. (2021) document a persistent decline in establishment-level workplace injury rates. Bach et al. (2021) studies the impact of M&As on the metal health of employees and show that following a takeover, an incumbent employee's likelihood of being diagnosed with mental illness increases by 3%. Our paper contributes to the literature on firms and employee health by documenting employment gains following the implementation of sick leave mandates, which may be partially driven by improvements in the health of workers and the workplace.

Finally, our paper relates to the literature on parental and family leave in the US. Waldfogel (1999) and Baum (2003) show that the Family and Medical Leave Act (FMLA), which provides unpaid leave, did not have a significant impact on women's employment or wages. Moreover, Bailey et al. (2019) and Bana et al. (2020) find no significant effect of California's 2004 Paid Family Leave Act on women's employment using administrative data and a regression discontinuity design.[5] Bennett et al. (2020) show that paid family leave mandates are associated with better corporate performance, lower turnover, and higher productivity. Our results show that paid sick leave mandates, which apply to any short-term illness and cover more workers but offer a shorter duration of leave, have positive effects on employment.

[5]Studies outside of the US tend to report either a modest positive or insignificant change in female employment following an expansion of paid maternity leave. See Bartel et al. (2018) for a review.

## 2. Paid Sick Leave in the United States

The United States lacks a national mandate that requires firms to provide paid sick leave (PSL). The BLS estimates that 37% (43 million workers) of the US private industry workforce did not have access to paid sick leave as of 2013, the start of our sample, with significant heterogeneity in access across workers. As shown in Figure 1, Panel A, as of 2013, only 31% of workers with wages in the bottom quartile had paid sick leave, while 87% of workers in the top quartile had paid sick leave. Likewise, there is significant variation by industry. As of 2013, around 93% of workers in the information industry received paid sick leave, as compared to only 44% in the construction industry, and just 27% in the leisure and hospitality industry (Panel B). Panel C compares workers in the private and public sectors and finds that paid sick leave is more common among civil servants. Lastly, Panel D compares access by firm size and documents a pattern of greater paid sick leave availability among larger firms.

In response, a number of states and localities have enacted legislation requiring firms to provide a minimum level of paid sick leave. We summarize all local and state laws in Appendix Table A1. Between 2013 and 2019, 10 states and 29 localities have enacted a paid sick leave mandate (Figure 2).[6,7] To assess the extent to which individuals are aware of these laws, in Internet Appendix Figure IA2, we gauge the effect of key mandates on the overall search interest related to "sick leave" using Google trends. In each state, we observe spikes in sick leave searches around the effective date of the state mandate. These increases in interest are most likely due to workers learning about their sick leave rights under the new mandate.

The typical law has workers earn an hour of paid sick leave for every 30-40 hours worked with a cap on total hours that can be accrued in a year. By dictating that sick leave accrues per hour worked, these plans typically cover part-time and temporary workers, although some laws require a modest minimum threshold of hours worked per year.[8]

[6]We begin our analysis in 2013 to avoid confounding effects of the Great Recession of 2008 on our measurement of employment.

[7]Internet Appendix Figure IA1 shows the distribution of all paid sick leave laws as of March 31, 2019.

[8]Sick leave mandates in the following locations had exemptions as defined: workers who work less than

Most laws apply to all firms within the mandate's jurisdiction, however differences in rates of accrual as well as caps may apply to smaller firms with occasional exemptions for the smallest firms (Appendix Table A1). Maclean et al. (2020) find that the average (marginal) employee takes two more hours (two more days) of sick leave after a mandate becomes applicable, costing firms an estimated additional 2.7 cents (21 cents) per hour worked.

Workers in the US may also be eligible for leave under the FMLA or paid family leave laws. However, there are important differences between paid sick leave, the FMLA, and paid family leave.[9] The FMLA is a federal law that requires firms with 50 or more employees to provide up to twelve weeks of unpaid leave to employees with one year or more of tenure. As such, the FMLA does not cover a significant percentage of US workers. The BLS estimates that 22% of workers have less than one year of tenure and 27% of workers are at firms with less than 50 employees.[10,11] Moreover, besides providing only unpaid leave, the FMLA can only be claimed for serious health events (of the worker or a close family member) and does not apply to short-term illnesses. Paid family leave usually operates as an insurance program, in which workers contribute, through taxes, to a government-run fund from which eligible workers can apply for a partial wage replacement during parental leave. Under paid sick leave mandates, employers bear the full cost, where they have to provide workers with full pay for any short-term illness, subject to an annual sick leave cap.

The majority of workers employed in the private sector in the US are "at will" employees. As such, the employer can sever employment for any non-illegal justification. For employees without sick leave, missing too much work due to an illness can be considered a legal reason for termination as long as the illness is not covered by FMLA—in other

40 hours in a year (Philadelphia, PA), less than 80 hours in a year (Morristown, NJ; Tacoma, WA; Chicago, IL; Cook County, IL; Jersey City, NJ; New York, NY; Newark, NJ; Patterson, NJ; Trenton, NJ; Elizabeth, NJ; Minneapolis, MN; St Paul, MN), less than 240 hours in a year (Spokane, WA), less than 30 days in a year (California), less than 2 hours in a week (Oakland, CA; Los Angeles, CA; San Diego, CA), less than 8 hours in a week (Montgomery County, MD), less than 12 hours in a week (Maryland), less than 18 hours in a week (Vermont), and less than 20 hours in a week (New Brunswick, NJ).

[9] See https://www.dol.gov/sites/dolgov/files/oasp/legacy/files/paidleavefinalrulecomparison.pdf

[10] See https://www.bls.gov/news.release/pdf/tenure.pdf

[11] See https://www.bls.gov/web/cewbd/table_f.txt

words it is not sufficiently serious—or if the worker is not covered by the FMLA. This is a concern for workers given that in January 2018 alone, 4.2 million workers had illness-related work absences.[12] Consistent with this argument, Hill (2013), using the Medical Expenditure Panel Survey, finds that workers without paid sick leave have a 25% higher probability of a job separation, even after controlling for observable worker characteristics.

# 3. Data

We use county-level data from a number of sources. We describe each data source individually below.

## 3.1. Quarterly Workforce Indicators

We use the public-use version of the Census's Quarterly Workforce Indicators (QWI) data as our primary source to measure private sector employment, separations, and earnings at the county-level.[13] The QWI is based on the Longitudinal Employer-Household Dynamics (LEHD) linked employer-employee microdata. The data is aggregated from state unemployment insurance records and covers over 95% of US private sector jobs.[14] QWI's employment measure is based on the total count of jobs by place of employment, which is consistent with the applicability of paid sick leave mandates. The data is available at the quarterly frequency.

## 3.2. Quarterly Census of Employment and Wages

To measure average weekly wages and the number of establishments at the county-level, we rely on BLS's Quarterly Census of Employment and Wages (QCEW) data. The QCEW also relies on administrative data from state unemployment insurance programs. The data is available at the quarterly frequency.

[12] See https://www.bls.gov/opub/ted/2018/4-point-2-million-workers-have-illness-related-work-absences-in-january-2018.htm?view_full for more information

[13] The QWI data is also available within-county by industry and worker demographics, which we use in later cross-sectional tests.

[14] Notable exclusions include independent contractors, unincorporated self-employed, and railroad workers covered by the railroad unemployment insurance system. See Abowd et al. (2009) for more information.

### 3.3. Small Area Income and Poverty Estimates

Measures of median household income and the number of individuals in poverty come from the Small Area Income and Poverty Estimates (SAIPE) data. SAIPE data is provided by the Census and is available annually.

### 3.4. Equifax

We use data from Equifax on the estimated subprime credit population in each county. Equifax defines subprime individuals as ones with a credit score below 660. Counties with fewer than 20 people in the sample are not reported for privacy concerns. The estimates are based on a representative sample of the Federal Reserve Bank of New York (FRBNY) Consumer Credit Panel. The representative sample includes only the primary member per household and reflects around 5% of the US credit report population (all US residents with a credit history). The data is available at the quarterly frequency.

### 3.5. Federal Reserve

We obtain data on county-level household debt-to-income ratios from the Federal Reserve. The Federal Reserve assigns each county to one of 10 possible debt-to-income ratio bins. Estimates of aggregate household debt (excluding student loans) and aggregate income are from Equifax and BLS, respectively. The data is available at the quarterly frequency.

### 3.6. Other County-level Data

We also collect annual county-level data from several additional sources. We use population estimates and income inequality measures from the Census/American Community Survey (ACS), number of personal bankruptcy filings from US courts, and health insurance measures from the Census's Small Area Health Insurance Estimates (SAHIE) data. Finally, we obtain data on county gross domestic product (GDP) from the Bureau of Economic Analysis (BEA). All variable descriptions can be found in Appendix Table A2.

### 3.7. Summary Statistics

Table 1 reports summary statistics for the primary variables used in our analysis. Our timeline starts in Q1-2013 and ends in Q1-2019. We start in 2013 to avoid capturing labor market effects following the Great Recession and end in 2019 to avoid picking up impacts of COVID-19 on employment. Moreover, to best isolate the impact of treatment, we restrict the time series of treated counties to the 16 quarters before and after the mandate became effective.[15]

In Panel A, we report means, medians, standard deviations, and observation counts for quarterly measures at the county-level. On average, we observe over 32,000 jobs and 2,684 establishments per county. The average quarterly separation rate, defined as the ratio of total worker departures to average employment, is around 20%. The median weekly wage is $671, which is close to the national median of $768 reported by the BLS in Q1-2013.[16] On average, 29% of residents have subprime credit ratings, defined as credit scores below 660, and the median county has an aggregate household debt-to-income ratio between 1.58 and 1.82, which represents the sixth bin out of a possible 10.

In Panel B, we report means, medians, standard deviations, and observation counts for annual measures at the county level. The average GDP per capita and median household income is $58,163 and $46,828, respectively. On average, just slightly over 14,000 residents per county are defined as under the poverty threshold, around 10,000 residents under the age of 65 lack access to health insurance, and 267 personal bankruptcy cases are filed in a county. In terms of demographics, average population per county is 99,130, with 85% of residents being white, 13% between 15-24 years old, 37% between 25-54 years old, 14% between 55-64 years old, and 18% are 65 years or older.

# 4. Empirical Methodology

## 4.1. Quarterly-level

Our main empirical specification is a difference-in-differences specification utilizing the staggered adoption of local and state paid sick leave mandates. Specifically, we estimate:

$$z_{c\tau} = \beta PSL_{c\tau} + \theta X_{c\tau-4} + \mu_c + \delta_\tau + \varepsilon_{c\tau} \quad (1)$$

[15]In Internet Appendix Table IA1, we repeat our baseline regression with relaxing this assumption and find similar results.

[16]See https://www.bls.gov/news.release/wkyeng.t01.htm

where $c$ and $\tau$ index county and quarter-year, respectively. $PSL_{c\tau}$ is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate for the entire quarter $\tau$, and zero otherwise. $PSL_{c\tau}$ captures variation in paid sick leave mandates at both the local- and state-level.[17] For counties with local and state paid sick leave mandates, we consider the effective law date as the earlier implementation date of the two laws.[18]

We include the following time-varying county controls ($X_{c\tau-4}$): the natural logarithm of a county's population, population ratios for different age groups, ratio of females, and the ratio of whites. To avoid look-ahead bias and given that our controls are measured at an annual frequency, we lag all control variables by four quarters. County and quarter-year fixed effects are denoted by $\mu_c$ and $\delta_\tau$, respectively. The dependent variable, $z_{c\tau}$, captures county-level characteristics of interest, including employment and separations. Standard errors are clustered at the state-level.

## 4.2. Annual-level

Given that several key outcome variables are measured at an annual frequency, we also estimate an annual version of equation 1:

$$z_{ct} = \beta PSL_{ct} + \theta X_{ct} + \mu_c + \delta_t + \varepsilon_{ct} \tag{2}$$

where $c$ and $t$ index county and year, respectively. $PSL_{ct}$ is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate for at least half of the year $t$, and zero otherwise. We include the same controls ($X_{ct}$) as in equation 1, but do not lag them since control variables and outcomes are end-of-year measures. We also include county ($\mu_c$) and year fixed effects ($\delta_t$) to control for time-invariant county characteristics and year-specific trends.

[17] Azar et al. (2019) apply a similar methodology. They consider minimum wage changes at the federal-, state-, and county-level.

[18] When mandates apply to a sub-county level, we assume that the entire county is treated. In Internet Appendix Table IA2 (Panel B), we instead assume a given county has a paid sick leave mandate only if at least 50% of the population is impacted by the local mandate. The results are robust.

# 5. Impact of Mandatory Paid Sick Leave on Employment

## 5.1. Baseline

We start by showing the change in employment, log transformed, following the adoption of a paid sick leave mandate by estimating equation 1. We report the results in Table 2. The unit of observation is at the county-quarter level.

In column 1, we report a positive and significant relationship between the presence of a paid sick leave mandate and employment, after controlling for total county population as well as county and quarter-year fixed effects. We observe a 1.9% increase in employment, on average, after a paid sick leave mandate becomes effective. This increase is economically important and translates to a rise in employment of approximately 610 jobs for the median county with a pre-treatment employment of 32,000.

In column 2, we add controls for the age distribution of the county and continue to estimate a positive relationship between paid sick leave mandates and employment. Results are also robust to added controls for the percent of the county that is female (column 3) and racial distribution (column 4). Furthermore, in Internet Appendix Table IA2 (Panel A), we show similar results using the sample of just local laws (columns 3-4) and just state laws (columns 5-6).[19]

## 5.2. Identification Concerns

In the following section, we show that our results are robust to allowing for treatment effect heterogeneity and using synthetic controls. In addition, we show that our baseline results are not driven by differential pre-trends in the treated and control groups, as shown by dynamic plots as well as a synthetic controls approach. These results lend further support to a causal interpretation of our results—an argument we further strengthen in the subsequent section (5.3) by using variation in treatment intensity across and within counties.

[19] Pichler and Ziebarth (2018) report a null effect on employment following the implementation of paid sick leave mandates. The difference in the finding appears to be driven by differences in our methodology, allowing us to have more precisely estimated standard errors.

#### 5.2.1. Treatment Effect Heterogeneity

A recent literature highlights the potential pitfalls of two-way fixed effects (TWFE) estimators in difference-in-difference settings with multiple time periods and variation in treatment timing.[20] Biased estimation can occur when there is heterogeneity in treatment effects across treated groups and over time. For example, if the number of firms that voluntarily provide paid sick leave increases over time, then we might expect stronger treatment effects for counties that implemented a paid sick leave mandate earlier in the sample period. In Table 3, we allow for treatment effect heterogeneity by using the group-time average treatment effect estimator proposed by Callaway and Sant'Anna (2021).[21]

In Panel A, we report the coefficient on $PSL$ from the baseline estimate, for comparison. In Panel B, we show the average treatment effect using the Callaway and Sant'Anna (2021) methodology. In column 1, we include county population as the only pre-treatment covariate. In column 2, we include all controls in equation 1 as pre-treatment covariates. Our results are robust in both specifications.

#### 5.2.2. Dynamic Treatment Effects

In this section, we perform an event study analysis to check for any evidence of differential pre-trends. To that end, we build on our baseline two-way fixed effect (TWFE) specification by including indicators for each treatment period. We use a window covering four years before the event and four years after the event.[22] We consider the reference (excluded) period as the year before the implementation year of the paid sick leave mandate.

In settings with variation in treatment timing across treated groups, Sun and Abraham (2021) show that the point estimate of a given treatment period indicator may be contaminated by effects from other periods. To alleviate such concern, we also report results

[20] See for example, Borusyak et al. (2021), Athey and Imbens (2021), de Chaisemartin and D'Haultfœuille (2020), Sun and Abraham (2021), Callaway and Sant'Anna (2021), and Goodman-Bacon (2021).

[21] We also decompose the difference-in-difference average treatment effect, as in de Chaisemartin and D'Haultfœuille (2020), and find that all weights are strictly positive.

[22] In our county-level dataset, some outcomes are observed each quarter (e.g., employment), while others are reported each year (e.g., poverty). Therefore, to be consistent across all outcomes, we perform all event study analyses on a yearly basis.

using the interaction-weighted (IW) estimator proposed by Sun and Abraham (2021). The results are displayed in Figure 3 (Panel A). Estimates in red (black) are computed using the TWFE (IW) method. Regardless of the method, Panel A shows no pre-trends in employment prior to the effective date of the paid sick leave law and a jump in employment ex post. Specifically, we observe the largest increases in employment starting two or more years from the effective date of paid sick leave mandates. Overall, these results lend further support to the causal interpretation of our previous findings.

#### 5.2.3. Synthetic Controls

While we document no significant pre-trend, we acknowledge the limitations of a graphical approach. In this section, we use a synthetic controls approach to further alleviate concerns related to differences in pre-trends between treated and control counties. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010), we create a synthetic control for each treated county, which represents a weighted average of control counties that most closely mimic the pre-intervention dynamics of the labor market and demographics of the treated county. Details of our empirical approach are described in Appendix Section 1.

In Table 3, Panel C (column 1), we show the results when we only include the logged values of pre-intervention employment and average population as predictors. We find an aggregate average intervention effect (AIE) of 1.4%, significant at the 1% level, and consistent with our baseline results. Overall, approximately two-thirds of treated counties have a positive AIE. The average pre-intervention root mean square prediction error (RMSPE) is relatively low at 2.7% of employment, indicating that the synthetic controls provide a good overall fit for the employment dynamics of treated counties. In column 2, we find similar results when we add the following predictors: average weekly wages, population ratios for different age groups, population ratio of females, and population ratio of whites. In columns 3-4, we repeat the same estimation but exclude placebo treatment counties with a poor fit in the pre-intervention period. Similar to Abadie et al. (2010), we omit placebo treatment counties with a pre-RMSPE greater than 5 times the pre-RMSPE of the treated county. The overall results are similar.

## 5.3. Treatment Intensity

### 5.3.1. Between-county Variation

Our analysis, so far, uses a binary measure for paid sick leave mandates. However, there is important cross-sectional variation in these laws due to variation in the number of sick days guaranteed by the law as well as variation in how much of the population is expected to be affected by these laws. We predict larger impacts on employment when treatment intensity is greater. To test this prediction, we modify equation 1 by interacting the paid sick leave indicator (*PSL*) with a treatment intensity variable. We show the results in Table 4.

We start by looking at variation in the maximum number of days of paid sick leave per year that must be provided to full time employees under the relevant paid sick leave law. The laws in our sample provide between 3 days (counties in Rhode Island) and 9 days (Alameda County in California). We report the results in column 1. We include the same controls and fixed effects as in the fully specified baseline regression (equation 1). We find a positive and significant coefficient on the interaction of PSL and number of annual sick days. For example, employment increases by 1.2% (3.6%), on average, following the implementation of a mandate that offers 3 (9) days of annual paid sick leave.

We next look at variation in the percent of the population that is expected to be impacted by the sick leave mandate. We use three different measures. First, we look at a measure of the estimated percentage of new workers who will gain access to paid sick leave. *Mandate Coverage Ratio* is defined as the total number of workers who will gain access to paid sick leave through the mandate divided by county employment, measured four quarters prior to the effective law date.[23] Column 2 reports the results. We find a positive and significant coefficient on the interaction term, consistent with larger treatment effects when more workers in a given county are expected to gain coverage through the laws. Given the positive correlation between access to PSL and wages, another approach to estimating impact is to identify counties with a higher pre-mandate poverty rate. In

[23]We obtain estimates of the number of workers who will gain access to paid sick leave through each law from the National Partnership for Women and Families.

column 3, we show a positive and significant point estimate on the interaction of PSL and pre-mandate poverty rate.

Finally, we look at the underlying health in the county. Paid sick leave will be especially valuable to employees with poor underlying health. *Poor Health Ratio* measures the percentage of adults in a county who consider themselves to be in poor or fair health, as provided by the County Health Rankings and Roadmaps program. Column 4 finds a higher point estimate for treatment among counties with a higher pre-mandate *Poor Health Ratio*. A treated county where 23% of the population are in poor health (90th percentile) is expected to experience increases in employment as high as 2.8% following the implementation of the paid sick leave mandate.

Taken together, the results show a clear pattern of increasing employment following the adoption of a paid sick leave mandate. Moreover, the increase in employment ex post is stronger in counties where we predict treatment to have more effect, such as when the law provides more days of paid sick leave or when a higher percent of the county did not have paid sick leave or had poor health ex ante.

#### 5.3.2. Within-county Variation

The results in the previous section rely on variation in treatment intensity between counties. To further support a causal interpretation of our results, we next explore variation within a county in terms of predicted impact. These results build on Figure 1, where we show variation in the availability of paid sick leave ex ante across wage levels and industries. This setting allows us to add county-quarter-year fixed effects, thereby absorbing all time-varying omitted variables at the county-level. Specifically, to test within-county variation, we estimate the following triple difference model:

$$z_{gc\tau} = \beta_1 PSL_{c\tau} + \beta_2 PSL_{c\tau} \times LowAccess_g + \theta X_{c\tau-4} + \eta_{gc} + \alpha_{g\tau} + \lambda_{c\tau} + \varepsilon_{gc\tau} \tag{3}$$

where $g$, $c$, and $\tau$ denote group, county, and quarter-year. $PSL_{c\tau}$ is the treatment indicator and $LowAccess_g$ is an indicator variable that equals one if group $g$ has low pre-mandate access to paid sick leave, and zero otherwise. $\beta_2$ captures the incremental treatment effect for groups expected to benefit the most from the implementation of sick leave mandates. We also include group-county fixed effects ($\eta_{gc}$), group-quarter-year fixed effects ($\alpha_{g\tau}$), county-quarter-year fixed effects ($\lambda_{c\tau}$), and the same controls ($X_{c\tau-4}$) as in the baseline specification (equation 1). We display the results in Table 5.

Given paid sick leave is less likely to be offered for low-wage workers and the strong correlation between wage and education, in Panel A, we consider within-county variation based on level of education. The unit of observation is at the worker education-county-quarter level. In columns 1-2, we use the full sample and divide observations into groups based on education, dividing at the level of graduating high school. In column 1, we include county times education fixed effects to absorb any difference in baseline employment by education and geography, and education times quarter-year fixed effects to absorb any time varying trends in employment by educational attainment. In column 2, we also add county-quarter-year fixed effects, thereby absorbing any time varying trends at the county-level. Across both specifications, we find a greater increase in employment among low-education workers from paid sick leave mandates. Moreover, the estimates on the interaction terms are stable across both specifications, suggesting time-varying county-level omitted variables are not driving our results.

In columns 3-6, we look separately at these low-education worker groups. We keep all worker groups with above high school education (the control sample), and then add workers with less than high school education (columns 3-4), and workers with high school education (columns 5-6). Consistent with the disparity in access to paid sick leave, we find that sick leave mandates have no impact on employment for workers with some college education or higher. On the other hand, the positive employment effects seem to be concentrated on workers with a high school education or less.

In Panel B, we focus on within-county variation across industries.[24] We repeat the same methodology as in Panel A, but now using observations at the industry-county-quarter level. In columns 1-2, we use all industry-county-quarter observations and group industries into whether or not less than 60% of the industry nationally has access to PSL,

[24] Alternatively, in Internet Appendix Table IA3, we consider subsample regressions based on each industry.

as of 2013.[25] In column 1, we include county times industry fixed effects to absorb any difference in baseline employment by industry and geography, and industry times quarter-year fixed effects to absorb any time varying trends in industry employment. In column 2, we also add county-quarter-year fixed effects, thereby absorbing any time varying trends at the county-level. Across both specifications, we find employment increases more, on average, following the implementation of a sick leave mandate, in industries where less than 60% of workers have access to paid sick leave. We find no significant impact of sick leave mandates on employment in industries with broad ex ante coverage of sick leave. Moreover, we find stable results across both specifications, supporting the causal interpretation of our results.

In columns 3-8, we dig into industries where less than 60% of the employees have access to PSL. In these columns, we keep all industries that provide paid sick leave to 60% or more of their employees as the control group, and then add the low coverage industries one-by-one. In columns 3-4, we add leisure and hospitality industry observations. In columns 5-6, we add construction industry observations. In columns 7-8, we add retail industry observations. Increases in employment are most pronounced in the hospitality and construction sectors (the lowest two sectors in terms of workers' access to paid sick leave ex ante) and we continue to observe stable results with and without county-quarter-year fixed effects.

# 6. Mechanism

In this section, we explore several non-mutually exclusive mechanisms that can explain why we observe employment increases in counties with paid sick leave mandates. We document a decline in employee turnover and an increase in labor productivity following the implementation of a mandatory sick leave policy. We also find results consistent with an increase in the labor supply and spillover effects from increased household income following the adoption of a mandatory sick leave policy. Alternatively, we argue that the results are not driven by firms increasing employment to cover for increased

[25] This group includes the following QWI industries: leisure and hospitality, construction, retail, and other services.

leave taking, nor do we find evidence that firms transfer the costs of these mandates onto workers by lowering wages or disproportionately shifting workers to part-time.

## 6.1. Labor Demand

Under a job ladder model, an increase in compensation will lead to a reduction in separations as the arrival rate of better jobs is lowered (Dube et al., 2016). Moreover, compensation in the form of paid sick leave may be especially important in predicting turnover if employees are more likely to quit or be fired after an illness, in the absence of paid sick leave benefits. Turnover is costly to firms due to the direct costs of replacing a worker as well as lost productivity of replacing experienced workers with trainees. All else equal, reductions in turnover should lead to higher labor productivity and subsequently higher labor demand. In discussing this argument, it is important to note that while voluntarily providing paid sick leave would reduce hiring and training costs, such benefits could be swamped by costs due to adverse selection if peer firms do not provide similar benefits.

In Table 6 (Panel A), we test whether turnover declines by re-estimating the baseline specification (equation 1) using the natural logarithm of the separation rate as the outcome variable. The *Separation Rate* is defined as the ratio of total worker departures to average employment. Following the implementation of a paid sick leave mandate, we observe a 2.5% decrease in a county's separation rate (column 1). In columns 2-5, we interact the paid sick leave indicator with the treatment intensity variables. Consistent with the cross-sectional variation in the employment effects, we find a larger decrease in the separation rate when the law offers more paid sick leave days and covers more workers, and when a county has a higher poverty ratio and a higher percentage of individuals with poor health ex ante.

In Panels B and C, we estimate triple difference models following the same specification in equation 3. We focus first on within-county variation based on educational attainment (Panel B). We find that workers with a high school education or less experience a larger decrease in turnover following the implementation of a paid sick leave mandate. The results are robust to controlling for county-quarter fixed effects, thereby absorbing any time-varying county-level unobservables (columns 2, 4 and 6). In Panel C, we consider within county variation across industries. In column 1, we do not find any effects on the separation rate for industries where more than 60% of the workers have access to paid sick leave. However, the separation rate is 1.5% lower in industries where less than 60% of workers have access to sick leave. Columns 3-8 show that differences in the separation rate are mainly driven by the hospitality and construction sectors.

These findings are consistent with the idea that, following a paid sick leave mandate, workers become more attached to their jobs either by reducing incentives to switch jobs, or by reducing the probability of getting fired after a short-term illness. This increase in job stability is likely to lead to an increase in labor productivity. Paid sick leave mandates could also directly impact labor productivity. In the US, presenteeism—or on-the-job productivity loss that's illness related—costs firms over 150 billion dollars a year,[26] with more than 2.7 million workers going to work sick in a given week (Susser and Ziebarth, 2016). Presenteeism may also have implications for workplace safety. Workers with access to paid sick leave are 28% less likely to suffer from an occupational injury vis-à-vis workers without access (Asfaw et al., 2012).

## 6.2. Labor Supply

Mandatory sick leave policies can also lead to higher employment by increasing the labor supply. By making job characteristics more desirable, workers who otherwise were opting to sit out of the labor market may be more willing to seek employment. We test this mechanism in Table 7.

Given we observe equilibrium employment, it is difficult to disentangle supply and demand effects. However, to gain insight into changes in labor supply around a paid sick leave mandate, we start by looking at subsamples of workers that should be most sensitive to the new policy. Given that health typically declines with age, older workers may be especially responsive to a paid sick leave mandate. In Panel A (columns 1-2), we observe a decrease in the number of retirees, which suggests that some workers may delay the timing of their retirement following the implementation of a paid sick leave policy. For the average county, this translates to an increase in employment of 192 workers. Next, we

[26] See https://hbr.org/2004/10/presenteeism-at-work-but-out-of-it

consider changes in migration to counties with paid sick leave mandates. We find an increase in the net migration rate (columns 3-4), suggesting the average county grows by 800 people.[27] Finally, in columns 5-6, we examine the labor force participation (LFP) - a measure of all workers who wish to be employed, including people looking for jobs. We find an average increase of 1% in a county's labor force participation (LFP) rate, after the implementation of a paid sick leave mandate.

In Panel B, we test whether individuals in poor health or with young kids are more likely to be in the labor force following the enactment of a paid sick leave mandate. Presumably, paid sick leave should have a greater impact on the supply of workers with poorer health or workers with younger children. We test these predictions using individual-level data from the Annual Social and Economic supplement (ASEC) of the March Current Population Survey (CPS).[28] To preserve the confidentiality of respondents, the CPS does not identify counties for all respondents (approximately 45% of individuals reside in a county that is identified). To avoid bias related to the exclusion of certain counties, we only consider state-level paid sick leave mandates in these tests. We create two treatment intensity indicators. In the survey, individuals self-report their health status based on 5 choices: excellent, very good, good, fair, or poor. Our first intensity variable, *Poor Health*, equals one if an individual reports having fair or poor health, and zero otherwise. The second intensity indicator is *Has Young Children*, which equals one if an individual has at least one child under the age of 5, and zero otherwise. In each regression, we include the treatment intensity indicator, the paid sick leave indicator, and the interaction of the treatment intensity with the paid sick leave indicator.

In columns 1-2, we find that following a paid sick leave mandate, individuals with poor health are significantly more likely to answer "yes" to the question of whether or not they are employed. In contrast, we do not find a significant change in the probability of employment for individuals who report having better health. Similarly, columns 3-4 show that respondents with young children have a significantly higher probability of employment after the adoption of a sick leave mandate. Overall, these results suggest that

[27] Note, this cannot explain our findings directly as we control for population in all employment regressions.
[28] Refer to Section 9.3 for full details of the individual-level specification.

paid sick leave mandates encourage individuals highly affected by these laws to supply labor, which could lead to higher levels of employment.

### 6.3. Spillover Effects

Finally, our results could be driven by shocks to household wealth spilling over into the local economy. Households are expected to benefit from continuing to receive wages while sick and from experiencing less frequent unemployment spells. We examine these financial health implications in Table 8 using our baseline county-level specification. In Panel A, we focus on household income. Column 1 shows that following a paid sick leave mandate, median household income increases by 2%, on average. For a county with a median household income of $50,000, this translates to an average increase of around $1,000.

Median household income will increase in treated counties even if the income of all households increases by an equivalent amount. Based on earlier results, however, we know that paid sick leave mandates have a greater impact on low earners. Consistent with our results, in column 2, we observe an average decrease of 3.2% in the number of individuals in poverty. Moreover, in columns 3-5, we find that the share of aggregate household income increases for individuals earning below the 40th percentile, and individuals earning between the 40th-60th percentile, while it decreases for individuals earning above the 60th percentile.[29] The shift in the relative share of total income from top earning households to lower income households is consistent with paid sick leave laws reducing income inequality. We explore this possibility directly in column 6, using the Gini index to proxy for income inequality. The Gini index measures the deviation between the observed cumulative income distribution and a perfectly equal income distribution. It ranges from 0 (perfect equality) to 1 (perfect inequality). Following the implementation of a paid sick leave mandate, we observe a decrease of 0.2 percentage points in the county-level Gini index, on average.

We also consider other financial health implications in Panel B. Following a sick leave

[29]The Census/American Community Survey (ACS) reports the share of aggregate household income for each group of earners and the Gini index for counties with populations exceeding 65,000. Therefore, the number of observations drops to 4,859.

mandate, we document a decrease in the population of subprime borrowers (column 1), the debt-to-income ratio bin (column 2), the population of individuals without health insurance (column 3), and the total number of personal bankruptcy filings (column 4). Overall, the results in Table 8 are consistent with the positive employment effects that we document. Paid sick leave mandates increase job stability, leading to longer spells of employment, which ultimately result in overall improvements in financial health. While we cannot directly measure the impact of these gains in household income on jobs, the well documented high propensity to consume of lower wage workers suggests that this should translate into increased demand for local goods and services.

### 6.4. Alternative Mechanisms

While we cannot pinpoint one exclusive mechanism as the driver of our results, we can exclude several mechanisms. For example, a potential explanation for the positive employment effects is that firms hire more to cover for workers who are taking sick leave. Maclean et al. (2020) find that the average employee takes 2 more hours of sick leave after a mandate becomes applicable, 0.1% of the typical work hours of a full-time employee, suggesting this mechanism is unlikely to explain the whole effect.

Another potential mechanism involves direct cost transfers. To deal with the increased compensation costs imposed by the sick leave mandate, firms may respond by transferring these costs directly onto the workers' wages. We explore this possibility in Internet Appendix Table IA4 (Panel A). We estimate equation 1 using the logged value of average weekly wages (columns 1-2) and average monthly earnings (columns 3-4) as outcome variables. Whether we only control for population or for additional county demographics, we do not find any evidence of a depression on wages. Maclean et al. (2020) also finds no offsetting effects of a PSL mandate on cash wages or other benefits.

Finally, we do not find evidence that firms shift to more part-time work following the adoption of a sick leave mandate. Even though most sick leave mandates have a very low threshold for eligibility in terms of days worked per year (10 working days for most laws), several local and state mandates set a higher bar. Firms operating in these jurisdictions might be tempted to rely more on part-time workers. To the extent that our results are influenced heavily by counties with mandates that set a higher threshold, we should observe an increase (decrease) in part-time (full-time) employment.

We test this channel in Panel B using individual-level data from the March CPS.[30] In columns 1-2, the outcome variable, *Part-time*, is an indicator variable that equals one if an employee worked less than 35 hours in a week. In columns 3-4, the outcome variable is the logged value of the number of hours worked in a week. Across all columns, the results are inconsistent with the prediction. As a matter of fact, they go in the opposite direction. Following the implementation of a sick leave mandate, we observe an increase in the number of hours worked and a decrease in part-time employment. In sum, our results are inconsistent with this mechanism.

# 7. Discussion of Magnitudes

We have documented an increase in county-level employment of 1.5%, on average, following the enactment of a paid sick leave mandate. This is a large increase, especially in light of the modest direct costs to firms from providing paid sick leave. Maclean et al. (2020) estimate that compensation costs rise by 21 cents/hour for the marginal employee following the enactment of a PSL mandate.[31] Indirect costs, such as replacement staffing and costs from the business disruption following a last minute employee absence, will add to this cost. However, given modest estimates of incremental sick leave use post-mandate, it is likely that these costs will remain modest.[32]

But while these costs are likely to be modest from the firm's perspective, the benefits are large from an employee's point of view. Without paid sick leave – work will often be in conflict with the worker's own health or the health of their family. Workers without paid sick leave may not be able to schedule routine health visits because they can't get time off work. If they are sick, they may still have to go to work. These workers may not be able

[30] Refer to Section 9.3 for full details of the individual-level specification.

[31] Another way to think about this is in regards to the average employee. Given a paid sick leave mandate is expected to increase PSL coverage by 13 percentage points, on average, this translates to an average increase in compensation of 2.7 cents per hour. We prefer to think in terms of marginal as firms typically provide PSL to all employees or no employees. As such, few firms will experience the estimated "average" effect.

[32] Maclean et al. (2020) find that the marginal employee takes 2 more days of sick leave following the passage of a paid sick leave mandate.

to take a day off to attend a funeral or to stay home with a sick child. DeRigne et al. (2016) find that workers without PSL were 3 times more likely to forgo medical care for themselves and 1.6 more likely to forego medical care for the family, as compared to workers with paid sick leave. Moreover, Earle and Heymann (2011) find that access to paid sick leave is associated with better mental health, as measured by the Mental Health Inventory (MHI-5), and better self-reported physical health. Given these impacts on health for the worker and her family from access to paid sick leave, it is not surprising that we observe significant changes in workers behavior in the form of lower turnover and greater willingness to supply labor following a mandate. Moreover, in the next section, we show that PSL mandates also positively impact the relationship between workers and their firm.

# 8. Impact of Paid Sick Leave Mandates on Firms

The previous results document employment gains following the adoption of PSL mandates. These results suggest positive economic implications for workers, however, such gains could come at the expense of firm rents. To investigate this question, we now turn to firm-level implications.

To construct our firm-level sample, we start with all non-financial and non-utility firms in Compustat for the years 2013 to 2019 (the sample period of our baseline county-level analysis). Next, we exclude all firm-years where historical information on the company's headquarters location is unavailable or the company is headquartered outside the US. Our main treatment variable is *PSL HQ*, which is an indicator variable that equals 1 if a firm is headquartered in a location with an active PSL mandate, and 0 otherwise. Given that many firms in Compustat tend to be geographically dispersed with operations spanning multiple states, we construct an alterative treatment variable, *PSL EstabEmp*, which is based on establishment employment and location data from Infogroup. *PSL EstabEmp* is an indicator variable that equals 1 if more than 50% of the firm's employees operate in locations with an active PSL mandate, and 0 otherwise. The final step in constructing our sample involves matching Compustat data with Glassdoor, a large crowd-sourcing company that provides data on company ratings (e.g., overall, culture, and work/life balance) and the availability of different non-wage benefits, including PSL.[33]

To assess the implications of mandatory PSL for firms, we estimate the following regression:

$$z_{jkt} = \beta_1 PSL_{jkt} + \theta X_{jkt-1} + \mu_j + \delta_{kt} + \varepsilon_{jkt} \tag{4}$$

where the outcome variable $z_{jkt}$ represents either ROA, defined as operating income scaled by lagged assets, or Glassdoor rating for firm $j$ in industry $k$ during fiscal year $t$. $PSL_{jkt}$ is the treatment variable (*PSL HQ* or *PSL EstabEmp*). We include firm fixed effects ($\mu_j$) and industry (2-digit SIC) by year fixed effects ($\delta_{kt}$), along with the following firm-level controls ($X_{jkt-1}$): the natural logarithm of total assets, book leverage, cash/assets, and asset tangibility (PPE/assets). Variables are winsorized at the 1st and 99th percentile level. Standard errors are clustered at the headquarters state-level. We show the results in Table 9.

In Panel A, we assess the impact on firm performance. Odd-numbered columns include firm and year fixed effects, while even-numbered columns include firm and industry by year fixed effects. Focusing first on columns 1-4, the coefficients on *PSL HQ* and *PSL EstabEmp* are positive and statistically as well as economically significant across all specifications. Columns 1-2 show that following the enactment of a PSL mandate in the headquarters location, firms experienced a 1.8 to 2.7 percentage point increase in ROA, on average. If firms do not offer PSL to all employees nationwide once the headquarters location has a mandate in place, then *PSL EstabEmp* may better capture the overall treatment effect. Similar to columns 1-2, we find an increase of 1.5 to 2.1 percentage points in a firm's ROA, on average, following high exposure to the mandates i.e. more than 50% of workers operate in locations with an active PSL law (columns 3-4).

One potential concern with the earlier results is that many Compustat firms are likely to have offered voluntary PSL benefits and hence are not expected to be affected by the mandates. To distinguish between firms where the mandates are expected to be binding

[33] While Glassdoor provides some aggregate information publicly, we have access to confidential micro-data from Glassdoor that allows us to observe the detailed breakdown of survey responses on the availability and rating of PSL, as well as detailed information on the employees providing these responses.

(i.e. did not have PSL ex-ante) versus firms that already provided PSL voluntarily, we use data on the availability of PSL for firms that appear both in Glassdoor and Compustat.[34] We construct the variable, *Low Access*, which is an indicator variable that equals 1 if less than 75% (national average) of total survey responses indicate that PSL is available, and 0 otherwise.[35]

In columns 5-8, we modify equation 4 by adding an interaction term between the firm-level treatment variables and *Low Access*. Consistent with a causal interpretation of our results, columns 5-8 show that the operating performance of high-access firms is unaffected by the PSL mandates. However, the increase in ROA is concentrated on firms with low access to PSL. Using *PSL HQ* (*PSL EstabEmp*) as the treatment variable, the increase in ROA is 1.6 (2.2) percentage points higher for low-access PSL firms, on average.

In Panel B, we turn to company ratings on Glassdoor, which are measured on a 1-5 scale, where 5 (1) is the highest (lowest) score. We consider 3 ratings: overall (columns 1-2), culture (columns 3-4), and work/life balance (columns 5-6). Odd-numbered columns include firm and year fixed effects, while even-numbered columns include firm and industry by year fixed effects. Across all specifications, the coefficient on *PSL HQ* is positive and statistically significant. Following the enactment of a PSL mandate in the headquarters location, we find a 4.85%, 6.7%, and 6.03% standard deviation increase in a company's overall, culture, and work/life balance rating, respectively, on average. Overall, the firm-level results are consistent with our previous arguments. PSL positively impacts culture and improves employee's work/life balance, which has positive implications for firm performance.

[34] Glassdoor reviews on the availability of PSL start in 2014, but are not time stamped. We, thus, must use all reviews to proxy for PSL coverage throughout our sample period (2013-2019). We argue that any look-ahead bias should work against us by adding noise to our estimates as well as concerns that firms that have been more profitable would be more likely to add additional benefits, such as PSL, biasing us against finding a result.

[35] To improve the accuracy of our proxy for PSL availability, we require that a firm has at least 10 survey responses. The results are robust to using other thresholds.

# 9. Robustness

In the following section, we consider a number of robustness tests. We show that the employment effects are robust to 1) controlling for contemporaneous changes in minimum wages and unemployment insurance, 2) matching by county employment, population, GDP, and number of establishments, 3) limiting the control pool to counties in coastal states, 4) using weighted regressions based on county population, 5) excluding the largest treated states from the sample, and 6) using alternative measures of employment. We also show zero economic effects when using placebo events using different time periods for the true treated sample or using nearest neighbor control counties. Finally, we use the group-time average treatment effect estimator for other outcome variables and show similar results when using city-level and individual-level data, when available.

## 9.1. County-level

During our sample period, several localities and states also changed their minimum wage. Although the minimum wage literature does not provide a consensus on the impact of minimum wage on employment, in Internet Appendix Table IA5, we verify that minimum wage changes are not driving the positive employment effects. Columns 1-2 show that paid sick leave mandates lead to an average increase of 1.6%-1.9% even after controlling for the effective minimum wage at the county-level. Similarly, in columns 3-4, we control for average unemployment benefits and find similar results. Overall, adding these controls has a modest impact on the treatment effect we estimate, suggesting our findings are not driven by contemporaneous changes to other laws.

Next, we explore the extent to which our findings can be attributed to differences in characteristics between counties with paid sick leave mandates and counties without any mandates. We report in Internet Appendix Table IA6 (Panel A) differences between treated and control counties, estimated four quarters before the implementation of the mandate. On average, treated counties are larger in terms of employment, population, and number of establishments. To ensure that our results are not driven by these compositional differences, we create four different matched samples by performing a one to three nearest neighbor matching, with replacement, based on employment, population, GDP, and number of establishments.[36] We then re-estimate our baseline model using the matched samples in Internet Appendix Table IA7 (Panel A).

Across all matched samples, we find a significant increase in employment ranging from 1.2% to 1.8%, following the implementation of paid sick leave mandates. These results are consistent with our baseline findings, which makes it less likely that the increases in employment are due to differences in county characteristics and further supports our causal interpretation.[37] In Internet Appendix Figure IA2, we examine the geographical dispersion of US paid sick leave mandates. We observe that most of the mandates are enacted in coastal states. To ensure that we are not picking up trends specific to coastal states, we restrict the pool of controls to counties located in coastal states that did not have a sick leave mandate, and repeat the same estimation as in Panel A. We show the results in Panel B. Across all matched samples, we find similar results, which alleviates concerns related to trends in coastal states.

In our baseline specification, each county-quarter receives the same weight in the regression. Alternatively, in Internet Appendix Table IA9, we re-estimate the baseline model using weighted least squares. We use county population as the weighting variable. Across all specifications, we observe similar increases in employment ranging from 1.4% to 1.9% following implementation of sick leave mandates. Moreover, to ensure that our results are not driven entirely by sick leave mandates that were enacted in larger states i.e., states that dominate our sample in terms of the number treated counties, we repeat the baseline estimation in Internet Appendix Table IA10, but exclude from the sample counties in California (column 1), Washington (column 2), Oregon (column 3), and counties in all 3 states (column 4). Regardless of which state(s) we exclude from the sample, we obtain similar treatment effects, arguing our results are not disproportionately influenced by a small number of mandates passed in larger states.

[36]In each matched sample, the mean of the matching variable is not statistically different between treated and control counties. See Panels B, C, D, and E of Internet Appendix Table IA6.

[37]In support of these results, we find in Internet Appendix Table IA8 that local economic determinants do not seem to matter for the enactment of paid sick leave laws. However, sick leave mandates are more likely to be enacted in counties with a large percentage of individuals 65 years or older and in counties with a relatively smaller percentage of whites.

QWI provides several measures of employment in each quarter. Our baseline results are based on full quarter employment. In Internet Appendix Table IA11, we consider other measures of employment. We use QWI's beginning of quarter employment (column 1), QWI's end of quarter employment (column 2), QCEW's beginning of quarter employment (column 3), LAUS's measure of employment (column 4), and BEA's measure of employment (column 5). Other than QCEW, where the magnitude is slightly higher (2.4%), we find similar treatment effects across all measures relative to our baseline.

Next, we perform several placebo tests to verify that our results are robust. The results are displayed in Internet Appendix Table IA12. In panel A, we alter the treatment date and assume treatment starts 16 (column 1), 12 (column 2), 8 (column 3), or 4 (column 4) quarters before the true implementation date. We assume treatment lasts until the true implementation date of the sick leave law. Therefore, the time series of treated counties ends in the quarter the mandate became effective. Using any of these "false" treatment dates, we fail to find any evidence of increases in employment. Instead of changing the treatment date, in Panel B, we drop all "truly" treated counties and assume that the nearest neighbor control county is "falsely" treated and inherits the same treatment dates. We determine the nearest neighbor control county based on differences in employment (column 1), population (column 2), GDP (column 3), and number of establishments (column 4). Consistent with the results in Panel A, we do not find any treatment effects.

Finally, we consider the robustness of the results related to labor turnover and financial health outcomes. In Panels B, C, and D of Figure 3, we plot the dynamic treatment effects of paid sick leave mandates for separations, household income, and poverty, respectively. We do not observe significant pre-trends. However, in post-treatment periods, we find a sharp decline in the separation rate and in the number of individuals in poverty, and a significant increase in median household income.

To ensure the robustness of these results further, we allow for treatment effect heterogeneity by using the group-time average treatment effect estimator (Callaway and Sant'Anna, 2021). We show the results in Internet Appendix Table IA13. The first row represents the baseline estimates, whereas the second row denotes the average treatment effect using the Callaway and Sant'Anna (2021) estimator. Focusing first on turnover, we find that the separation rate decreases by 1.5% following the sick leave mandate (column 1). Consistent with our initial results, in columns 2-4, we also observe significant improvements in financial health. Overall, the results in this section lend further support to the causal interpretation of our previous findings.

### 9.2. City-level

In our baseline specification (county-level), when local paid sick leave mandates apply at the subcounty-level, we had to make certain assumptions about whether the overall county is treated. To avoid making these assumptions, in this section, we use city-level data to examine the effects of mandatory paid sick leave on employment and other financial health outcomes.[38] We obtain annual data on employment, population, and other economic characteristics from BLS's Local Area Unemployment Statistics (LAUS)[39], the Census, and the ACS, respectively. In Internet Appendix Table IA14, we show the results of estimating a city-level version of equation 2.

Following the implementation of a city, county, or state sick leave mandate, we observe an average increase of 1.2% in city-level employment (column 1). The magnitude of the employment effect is similar to our earlier findings at the county-level. Moreover, consistent with our initial results, we document overall improvements in financial health at the city-level. On average, paid sick leave mandates lead to an increase in median household income (column 2), and a decrease in the poverty count (column 3), Gini index (column 4), and the number of individuals who lack access to health insurance (column 5).[40]

### 9.3. Individual-level

In the last robustness section, we test whether our results hold when we use individual-level data. Our individual-level analysis is based on data from the March Current Population Survey (CPS). We focus primarily on the Annual Social and Economic supple-

[38]While city-level data allows us to obtain more precise estimates, we use the county-level specification as our baseline specification since we can obtain more comprehensive data at the county-level.

[39]LAUS provides employment data for cities with a population of at least 25,000.

[40]The Census/ACS provides yearly estimates of key economic variables for cities with a population exceeding 65,000. Therefore, the number of observations drops to 3,227 in columns 2-5.

ment (ASEC) of the CPS, which provides data on employment, number of job transitions, household income, and earnings from the longest job held in a year.

To preserve the confidentiality of respondents, the CPS does not identify counties for all respondents (approximately 45% of individuals reside in a county that is identified). To avoid bias related to the exclusion of certain counties, we modify equation 1 when using individual-level data and only consider state-level paid sick leave mandates. We estimate the following:

$$z_{ist} = \beta PSL_{ist} + \theta X_{ist} + \mu_s + \delta_t + \varepsilon_{ist} \tag{5}$$

where $i$, $s$, and $t$ index individual, state, and year, respectively. Individual-level outcomes at time $t$ are captured by $z_{ist}$, and $PSL_{ist}$ is an indicator variable that equals one if an individual lives in a state with an effective paid sick leave mandate at time $t$, and zero otherwise. We include the following individual controls ($X_{ist}$): age, an indicator variable for females, an indicator for whites, and indicators for ethnicity and different educational attainments. State and year fixed effects are denoted by $\mu_s$ and $\delta_t$, respectively. Standard errors are clustered at the state-level. We show the results in Internet Appendix Table IA15.

In Panel A, we focus on employment and job changes. *Employed* is an indicator variable that equals one if an individual is employed, and zero otherwise; *Job Change* is an indicator variable that equals one if a worker went through at least one job change in the survey year, and zero otherwise. Columns 1-2 show that, following a state paid sick leave mandate, the probability of employment increases by 0.6 percentage points, on average. In columns 3-4, we find that state sick leave mandates lead to an average decrease of approximately 1 percentage point in the probability of a job transition.

Next, we consider other financial health measures in Panel B. Following a state sick leave mandate, we observe an increase in household income, an increase in earnings from the longest job held in a year, a decrease in the probability of being in poverty, and a decrease in the probability of lacking access to private health insurance. In summary, the individual-level results in this section support our findings at the county-level.

# 10. Conclusion

The United States remains as the only industrialized nation without a national paid sick leave policy. In the absence of a federal mandate, several localities and states passed their own paid sick leave policies. In this paper, we examine the economic implications of paid sick leave mandates by documenting an average increase of 1.5% in county-level employment following the implementation of such policies. Consistent with a causal interpretation of our results, workers expected to benefit most from the mandate drive the employment effect. Moreover, our results do not appear to be driven by the endogenous timing of the adoption of these laws, as we observe the within-county-quarter effect is concentrated among workers most likely to be affected by the mandate.

Several non-mutually exclusive mechanisms can explain our findings. Following the implementation of a mandatory paid sick leave policy, we find a decline in labor turnover which has implications for labor productivity. We also find results consistent with an increase in the labor supply. Lastly, paid sick leave mandates appear to increase household due to fewer unemployment spells, which likely translates into increased demand for local goods and services.

These gains from the employee's perspective do not come at the expense of firms. Firms exposed to sick leave mandates experience a significant increase in operating performance, with the effect concentrated in firms that did not provide voluntary paid sick leave ex ante. This result emphasizes the point that firms may not be able to realize the same benefits from voluntary actions due to selection concerns. Finally, the recent pandemic (COVID-19) serves as a good reminder on the importance of providing workers with access to sick leave benefits.

# References


Abadie, Alberto, Alexis Diamond, and Jens Hainmueller, 2010, Synthetic control methods for comparative case studies: Estimating the effect of California's Tobacco control program, *Journal of the American Statistical Association* 105, 493–505.

Abadie, Alberto, and Javier Gardeazabal, 2003, The Economic Costs of Conflict: A Case Study of the Basque Country, *American Economic Review* 93, 113–132.

Abowd, John M, Bryce E Stephens, Lars Vilhuber, Fredrik Andersson, Kevin L McKinney, Marc Roemer, and Simon Woodcock, 2009, 5. the lehd infrastructure files and the creation of the quarterly workforce indicators, in *Producer dynamics*, 149–234 (University of Chicago Press).

Acemoglu, Daron, Simon Johnson, Amir Kermani, James Kwak, and Todd Mitton, 2016, The value of connections in turbulent times: Evidence from the united states, *Journal of Financial Economics* 121, 368–391.

Ahn, Thomas, and Aaron Yelowitz, 2015, The short-run impacts of Connecticut's paid sick leave legislation, *Applied Economics Letters* 22, 1267–1272.

Almeida, Heitor, Ruidi Huang, Ping Liu, and Yuhai Xuan, 2021, How does health insurance affect firm employment and performance? evidence from obamacare, *Evidence from Obamacare (January 15, 2021)* .

Asfaw, Abay, Regina Pana-Cryan, and Roger Rosa, 2012, Paid sick leave and nonfatal occupational injuries, *American Journal of Public Health* 102.

Athey, Susan, and Guido W. Imbens, 2021, Design-based analysis in Difference-In-Differences settings with staggered adoption, *Journal of Econometrics* .

Azar, José, Emiliano Huet-Vaughn, Ioana Marinescu, Bledi Taska, and Till von Wachter, 2019, Minimum Wage Employment Effects and Labor Market Concentration, NBER Working Papers 26101, National Bureau of Economic Research, Inc.

Bach, Laurent, Ramin Baghai, Marieke Bos, and Rui Silva, 2021, How do acquisitions affect the mental health of employees?, *Available at SSRN 3947957* .

Bailey, Martha J, Tanya S Byker, Elena Patel, and Shanthi Ramnath, 2019, The long-term effects of california's 2004 paid family leave act on women's careers: Evidence from us tax data, Technical report, National Bureau of Economic Research.

Bana, Sarah H, Kelly Bedard, and Maya Rossin-Slater, 2020, The impacts of paid family leave benefits: regression kink evidence from california administrative data, *Journal of Policy Analysis and Management* 39, 888–929.

Bartel, Ann P, Maya Rossin-Slater, Christopher J Ruhm, Jenna Stearns, and Jane Waldfogel, 2018, Paid family leave, fathers' leave-taking, and leave-sharing in dual-earner households, *Journal of Policy Analysis and Management* 37, 10–37.

Baum, Charles L, 2003, The effect of state maternity leave legislation and the 1993 family and medical leave act on employment and wages, *Labour Economics* 10, 573–596.

Bennett, Benjamin, Isil Erel, Léa H Stern, and Zexi Wang, 2020, Paid leave pays off: The effects of paid family leave on firm performance, Technical report, National Bureau of Economic Research.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, 2021, Revisiting Event Study Designs: Robust and Efficient Estimation. *Working Paper* .

Callaway, Brantly, and Pedro H.C. Sant'Anna, 2021, Difference-in-Differences with multiple time periods, *Journal of Econometrics* 225, 200–230.

Cohn, Jonathan, Nicole Nestoriak, and Malcolm Wardlaw, 2021, Private Equity Buyouts and Workplace Safety, *The Review of Financial Studies* 34, 4832–4875.

Cohn, Jonathan B., and Malcolm I. Wardlaw, 2016, Financing Constraints and Workplace Safety, *Journal of Finance* 71, 2017–2058.

de Chaisemartin, Clément, and Xavier D'Haultfœuille, 2020, Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects, *American Economic Review* 110, 2964–96.

De Paola, Maria, Vincenzo Scoppa, and Valeria Pupo, 2014, Absenteeism in the italian public sector: The effects of changes in sick leave policy, *Journal of Labor Economics* 32, 337–360.

DeRigne, LeaAnne, Patricia Stoddard-Dare, and Linda Quinn, 2016, Workers without paid sick leave less likely to take time off for illness or injury compared to those with paid sick leave, *Health Affairs* 35, 520–527, PMID: 26953308.

Dube, Arindrajit, William Lester, and Michael Reich, 2016, Minimum wage shocks, employment flows, and labor market frictions, *Journal of Labor Economics* 34, 663–704.

Earle, Alison, and Jody Heymann, 2011, Protecting the health of employees caring for family members with special health care needs, *Social Science Medicine* 73, 68–78.

Goodman-Bacon, Andrew, 2021, Difference-in-differences with variation in treatment timing, *Journal of Econometrics* 225, 254–277.

Henrekson, Magnus, and Mats Persson, 2004, The effects on sick leave of changes in the sickness insurance system, *Journal of Labor Economics* 22, 87–113.

Hill, Heather D., 2013, Paid Sick Leave and Job Stability, *Work and Occupations* 40, 143–173.

Maclean, Johanna Catherine, Stefan Pichler, and Nicolas R. Ziebarth, 2020, Mandated Sick Pay: Coverage, Utilization, and Welfare Effects, NBER Working Papers 26832, National Bureau of Economic Research, Inc.

Pichler, Stefan, and Nicolas R. Ziebarth, 2017, The pros and cons of sick pay schemes: Testing for contagious presenteeism and noncontagious absenteeism behavior, *Journal of Public Economics* 156, 14–33.

Pichler, Stefan, and Nicolas R. Ziebarth, 2018, Labor Market Effects of U.S. Sick Pay Mandates, *Journal of Human Resources* 55, 0117–8514R2.

Ruhm, Christopher J, 1998, The economic consequences of parental leave mandates: Lessons from europe, *The quarterly journal of economics* 113, 285–317.

Stearns, Jenna, and Corey White, 2018, Can paid sick leave mandates reduce leave-taking?, *Labour Economics* 51, 227–246.

Stoddard-Dare, Patricia, LeaAnne DeRigne, Cyleste C Collins, Linda M Quinn, and Kimberly Fuller, 2018, Paid sick leave and psychological distress: An analysis of us workers., *American Journal of Orthopsychiatry* 88, 1.

Summers, Lawrence H, 1989, Some simple economics of mandated benefits, *The American Economic Review* 79, 177–183.

Sun, Liyang, and Sarah Abraham, 2021, Estimating dynamic treatment effects in event studies with heterogeneous treatment effects, *Journal of Econometrics* 225, 175–199.

Susser, Philip, and Nicolas R. Ziebarth, 2016, Profiling the U.S. Sick Leave Landscape: Presenteeism among Females, *Health Services Research* 51, 2305–2317.

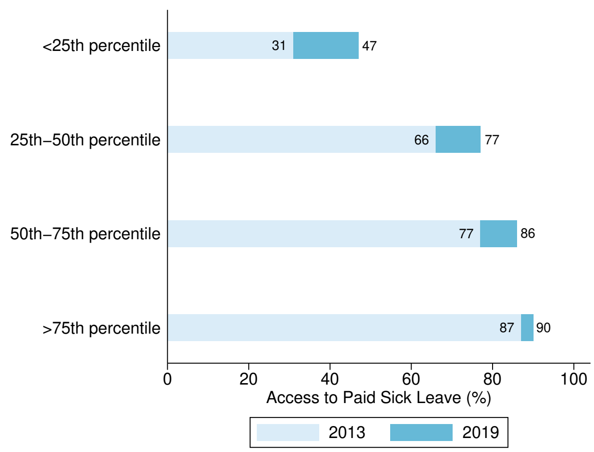
Waldfogel, Jane, 1999, The impact of the family and medical leave act, *Journal of Policy Analysis and Management* 18, 281–302.

Ziebarth, Nicolas R, and Martin Karlsson, 2014, The effects of expanding the generosity of the statutory sickness insurance system, *Journal of Applied Econometrics* 29, 208–230.

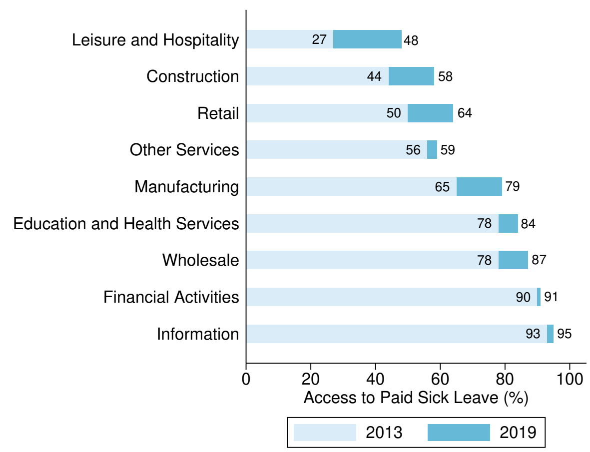
**Figure 1: Access to Paid Sick Leave**

Figure 1 shows the percentage of workers with access to paid sick leave in the US. Access to paid sick leave is displayed by average hourly wage (Panel A), industry (Panel B), type of employment (Panel C), and firm size (Panel D). Data on access to paid sick leave is from the BLS.

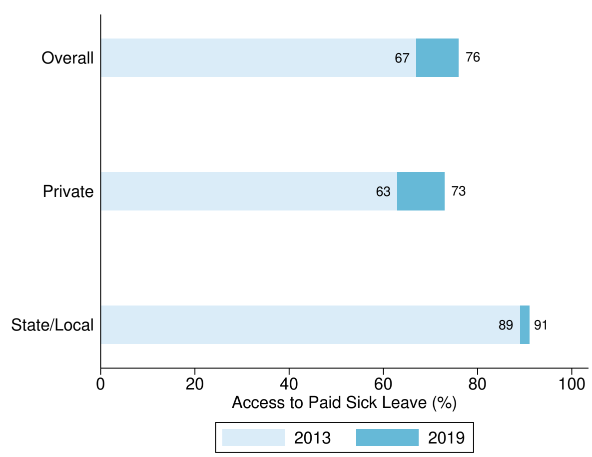
**(A)** *Average Hourly Wage*



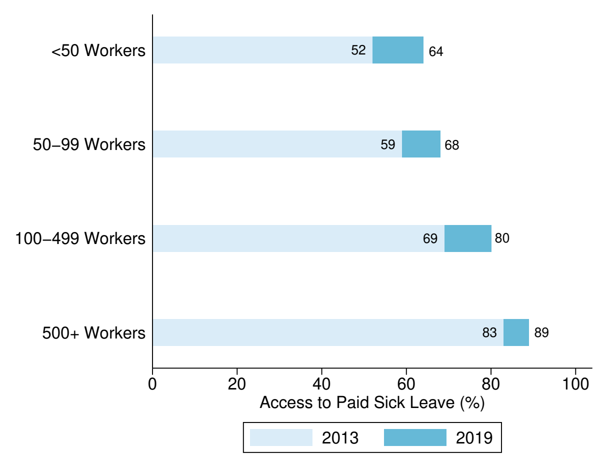

**(B)** *Industry*




**(C)** *Private vs. Public*



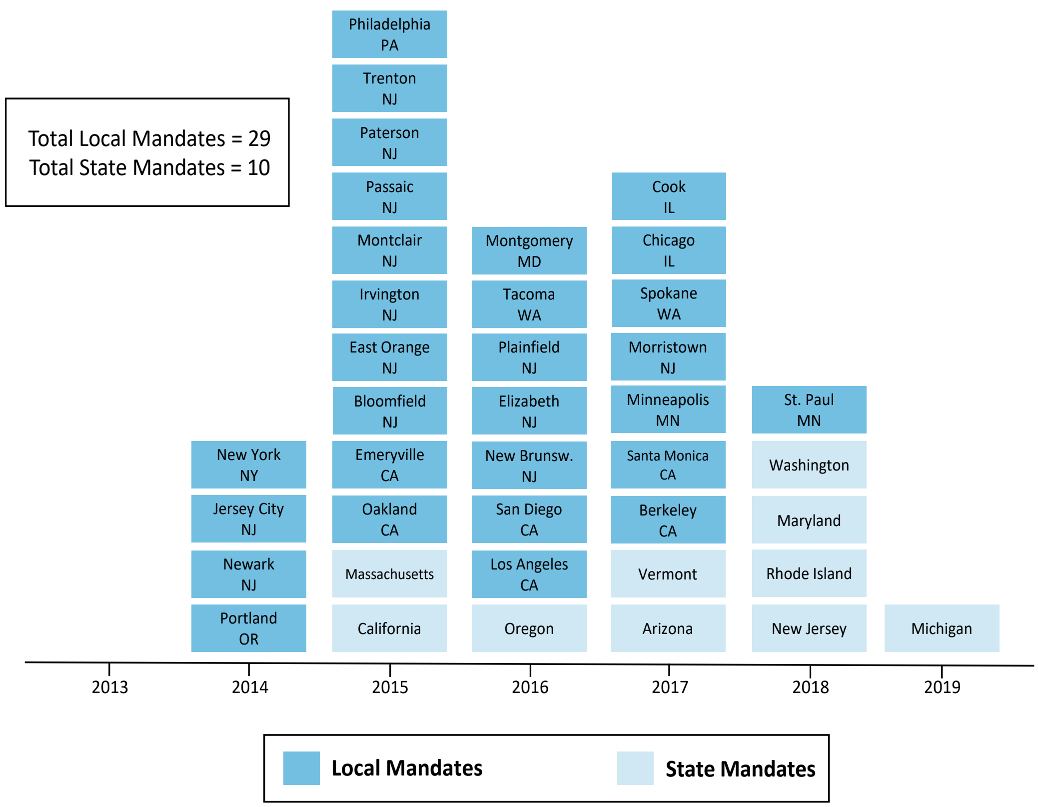

**(D)** *Firm Size*



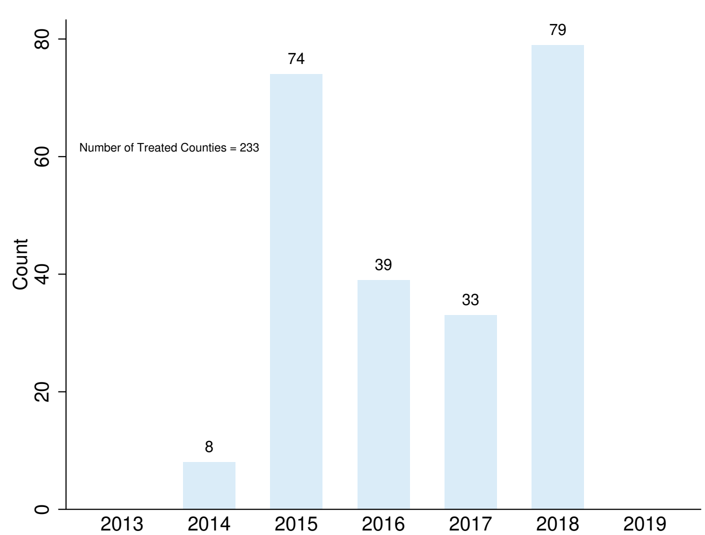
## Figure 2: Timeline of Paid Sick Leave Mandates

Figure 2 presents a timeline of US paid sick leave mandates that were passed between Q1-2013 and Q1-2019. In Panel A, we list newly enacted local and state paid sick leave mandates in each year during the sample period. Dates are based on the effective law date. In Panel B, we plot the number of newly treated counties in each year over the sample period. A county is considered treated if a local or state paid sick leave mandate is effective for the entire quarter-year.

**(A)** *Local and State Paid Sick Leave Mandates*



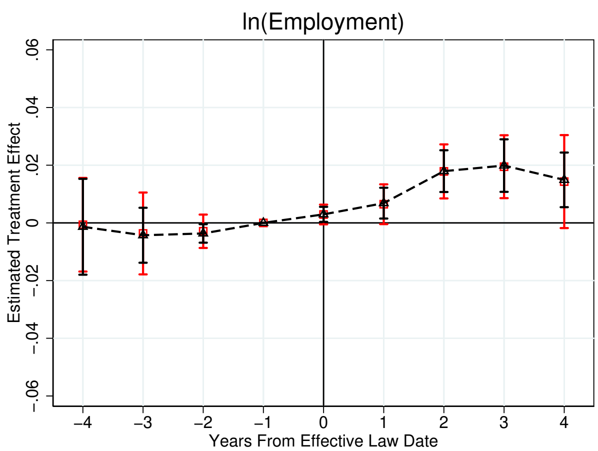

**(B)** *Treated Counties*



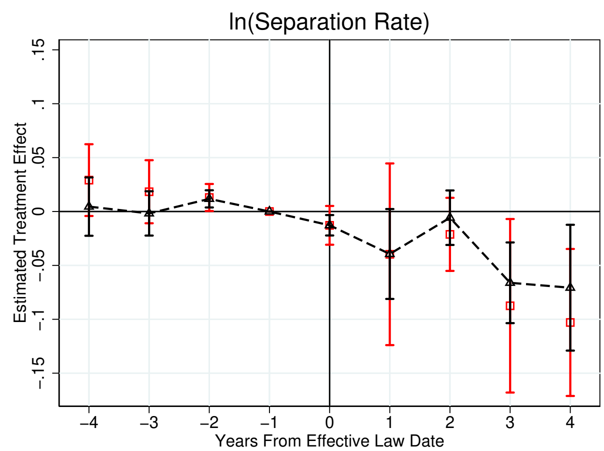
## Figure 3: Dynamic Treatment Effects

The following graphs show event studies based on extending the baseline specification (equation 2). We include time indicators for each treatment period and use a window covering four years before and after the implementation of the paid sick leave mandate. The reference year is given by t=-1. All models include county and year fixed effects. Estimates in black are computed using the interaction weighted (IW) estimator proposed by Sun and Abraham (2021), while estimates in red are from the baseline two-way fixed effects model (TWFE). The bars represent the 95% confidence intervals where the standard errors are clustered at the state level.

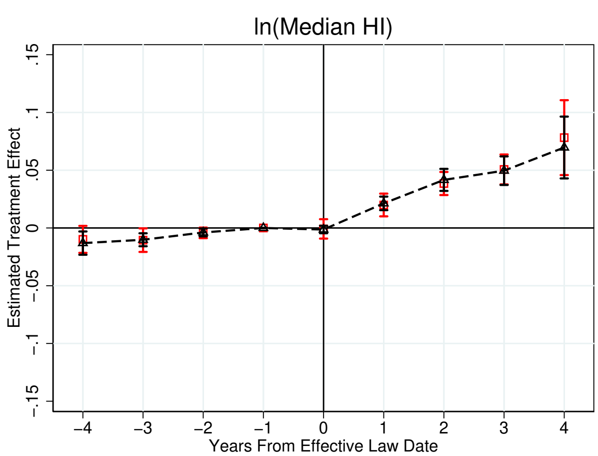
**(A)** *Employment*



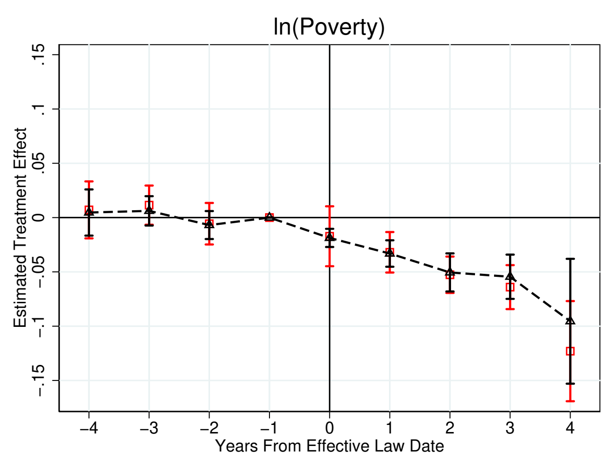

**(B)** *Separations*




**(C)** *Household Income*




**(D)** *Poverty*



## Table 1: Summary Statistics

Table 1 shows the summary statistics of key variables. Panel A displays the results of quarterly measures, while Panel B tabulates the statistics of annual measures. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave mandate. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. *Average Wages/Week*, *GDP Per Capita*, and *Median HI* are expressed in 2012 dollars. Variable descriptions can be found in Table A2 of the Appendix.

| | N | Mean | Median | Std.Dev |
|---|---|---|---|---|
| ***Panel A: Quarterly Measures*** | | | | |
| Employment | 76,803 | 32,095 | 5,366 | 122,926 |
| Establishments | 76,795 | 2,684 | 569 | 11,382 |
| Separation Rate | 76,785 | 0.20 | 0.18 | 0.07 |
| Average Wages/Week | 76,795 | 704 | 671 | 188 |
| Subprime Ratio | 76,596 | 0.29 | 0.28 | 0.09 |
| Debt/Income Bin | 76,747 | 5.48 | 6.00 | 2.82 |
| ***Panel B: Annual Measures*** | | | | |
| GDP Per Capita | 21,445 | 58,163 | 36,547 | 581,239 |
| Median HI | 18,683 | 46,828 | 44,994 | 12,043 |
| Poverty | 18,683 | 14,184 | 4,050 | 51,162 |
| Uninsured | 18,683 | 10,012 | 2,445 | 41,826 |
| Bankruptcy Filings | 18,223 | 267 | 57 | 1,045 |
| Population | 21,816 | 99,130 | 25,522 | 323,344 |
| Age 15-24 Ratio | 21,816 | 0.13 | 0.12 | 0.03 |
| Age 25-54 Ratio | 21,816 | 0.37 | 0.37 | 0.04 |
| Age 55-64 Ratio | 21,816 | 0.14 | 0.14 | 0.02 |
| Age 65+ Ratio | 21,816 | 0.18 | 0.18 | 0.05 |
| Female Ratio | 21,816 | 0.50 | 0.50 | 0.02 |
| White Ratio | 21,816 | 0.85 | 0.92 | 0.16 |

## Table 2: Employment

In the following table, we examine the impact of mandatory paid sick leave laws on employment. The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave mandate. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

| | ln(Employment) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.019*** | 0.016** | 0.015** | 0.015** |
| | (0.006) | (0.006) | (0.006) | (0.006) |
| ln(Population) | 0.996*** | 1.079*** | 1.084*** | 1.086*** |
| | (0.102) | (0.086) | (0.086) | (0.085) |
| Age 15-24 Ratio | | 1.578*** | 1.698*** | 1.704*** |
| | | (0.544) | (0.546) | (0.546) |
| Age 25-54 Ratio | | 1.577*** | 1.756*** | 1.755*** |
| | | (0.451) | (0.474) | (0.473) |
| Age 55-64 Ratio | | 2.313*** | 2.349*** | 2.345*** |
| | | (0.615) | (0.596) | (0.592) |
| Age 65+ Ratio | | 2.523*** | 2.532*** | 2.520*** |
| | | (0.495) | (0.486) | (0.474) |
| Female Ratio | | | 0.732 | 0.726 |
| | | | (0.442) | (0.442) |
| White Ratio | | | | 0.102 |
| | | | | (0.161) |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,803 | 76,803 | 76,803 |
| Within $R^2$ | 0.08 | 0.10 | 0.10 | 0.10 |

**Table 3: Alternative Estimators**

In the following table, we examine the impact of mandatory paid sick leave laws on employment using alternative estimators. In Panel A, we report the average treatment effect using the baseline two-way fixed effects estimator (Table 2). In Panel B, we allow for treatment effect heterogeneity by using the group-time average treatment effect estimator (Callaway and Sant'Anna, 2021). We include all of the control variables in Table 2 as pre-treatment covariates. In Panel C, we use the synthetic control estimator. To construct each synthetic control, we use the following predictors: ln(employment) (16,13,9,5, and 1 quarter before the implementation of the mandate), average weekly wages, average population, and averages of other controls. Other controls include population ratios for different age groups, population ratio of females, and population ratio of whites. Averages are computed over the entire 16 quarters before the effective law date. We restrict the treated counties' time series to 16 quarters before and after the effective date of the sick leave mandate. Confidence intervals are based on random draws of 5,000 different combinations of placebo treatment counties. In each draw, we randomly select one placebo from the control group of each treated county and then compute the aggregate average intervention effect (AIE) for the 233 placebos. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | ln(Employment) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ***Panel A: Two-way Fixed Effects*** | | | | |
| TWFE estimate of PSL | 0.019*** | 0.015** | | |
| | (0.006) | (0.006) | | |
| ***Panel B: C&S (2021) Approach*** | | | | |
| C&S (2021) estimate of PSL | 0.016*** | 0.013*** | | |
| | (0.002) | (0.003) | | |
| ***Panel C: Synthetic Controls*** | | | | |
| Aggregate AIE of PSL | 0.014*** | 0.011** | 0.014*** | 0.011** |
| Confidence Interval (2.5%) | 0.007 | 0.002 | 0.007 | 0.002 |
| Confidence Interval (97.5%) | 0.020 | 0.016 | 0.019 | 0.014 |
| Number of (+) AIEs | 148 | 147 | | |
| Number of (−) AIEs | 85 | 86 | | |
| Average Pre-RMSPE | 0.027 | 0.022 | | |
| Average Post-RMSPE | 0.045 | 0.041 | | |
| Average RMSPE Ratio | 2.416 | 2.736 | | |
| County Population | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | | ✓ |
| Exclude High Pre-RMSPE Placebos | | | ✓ | ✓ |
| Number of Treated Counties | 233 | 233 | 233 | 233 |

## Table 4: Treatment Intensity (between-county variation)

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. Pre-mandate *poverty ratio* and *poor health ratio* are measured 1 year prior to the effective mandate date. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | ln(Employment) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL × Number of Days | 0.004** | | | |
| | (0.001) | | | |
| PSL × Mandate Coverage Ratio | | 0.046** | | |
| | | (0.018) | | |
| PSL × Pre-mandate Poverty Ratio | | | 0.096** | |
| | | | (0.037) | |
| PSL × Pre-mandate Poor Health Ratio | | | | 0.121*** |
| | | | | (0.038) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,803 | 76,803 | 76,603 |
| Within $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

**Table 5: Treatment Intensity (within-county variation)**

In the following panels, we estimate triple difference models (equation 3). The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In panel A, we examine differences in treatment based on the educational level. The unit of observation is at the education-county-quarter level. There are 4 education bins: less than high school, high school, some college, bachelors+. In panel B, we examine differences in treatment across industries. The unit of observation is at the industry-county-quarter level. In 2013, less than 60% of workers have access to paid sick leave in the following industries: leisure and hospitality (27%), construction (44%), retail (50%), and other services (56%). We exclude natural resources and mining and public administration as BLS does not provide paid sick leave data for these industries. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Education*

| | ln(Employment) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL | 0.003 | | 0.004 | | 0.003 | |
| | (0.006) | | (0.006) | | (0.005) | |
| PSL × High School or Less | 0.025*** | 0.025*** | | | | |
| | (0.004) | (0.004) | | | | |
| PSL × Less than High School | | | 0.019*** | 0.019*** | | |
| | | | (0.006) | (0.006) | | |
| PSL × High School | | | | | 0.030*** | 0.030*** |
| | | | | | (0.007) | (0.007) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Education × County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Education × Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County × Quarter-Year FE | | ✓ | | ✓ | | ✓ |
| N | 307,060 | 307,059 | 230,257 | 230,211 | 230,346 | 230,345 |

*Panel B - Industry*

| | ln(Employment) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| PSL | -0.003 | | -0.002 | | -0.003 | | -0.001 | |
| | (0.007) | | (0.007) | | (0.007) | | (0.007) | |
| PSL × <60% Access | 0.038*** | 0.039*** | | | | | | |
| | (0.009) | (0.010) | | | | | | |
| PSL × Leisure & Hospitality | | | 0.018*** | 0.021*** | | | | |
| | | | (0.007) | (0.007) | | | | |
| PSL × Construction | | | | | 0.086*** | 0.087*** | | |
| | | | | | (0.021) | (0.022) | | |
| PSL × Retail | | | | | | | 0.007 | 0.009* |
| | | | | | | | (0.005) | (0.005) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry × County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry × Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County × Quarter-Year FE | | ✓ | | ✓ | | ✓ | | ✓ |
| N | 849,070 | 848,796 | 622,998 | 622,207 | 634,944 | 634,289 | 636,063 | 635,585 |

## Table 6: Labor Turnover

In the following panels, we examine the effect of paid sick leave mandates on labor turnover. The outcome variable in all regressions is the natural logarithm of the separation rate. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In Panel A, the unit of observation is at the county-quarter level. Pre-mandate *poverty ratio* and *poor health ratio* are measured 1 year prior to the effective mandate date. In Panels B and C, we estimate triple-difference models where the unit of observation is at the education-county-quarter and the industry-county-quarter level, respectively. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Overall*

| | ln(Separation Rate) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| PSL | -0.025* | | | | |
| | (0.015) | | | | |
| PSL × Number of Days | | -0.006* | | | |
| | | (0.003) | | | |
| PSL × Mandate Coverage Ratio | | | -0.079 | | |
| | | | (0.052) | | |
| PSL × Pre-mandate Poverty Ratio | | | | -0.176* | |
| | | | | (0.091) | |
| PSL × Pre-mandate Poor Health Ratio | | | | | -0.181* |
| | | | | | (0.093) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 76,781 | 76,781 | 76,781 | 76,781 | 76,581 |
| Within $R^2$ | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |

*Panel B - Education*

| | ln(Separation Rate) | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL | -0.019 | | -0.019 | | -0.019 | |
| | (0.014) | | (0.014) | | (0.014) | |
| PSL × High School or Less | -0.008** | -0.009** | | | | |
| | (0.003) | (0.003) | | | | |
| PSL × Less than High School | | | -0.012*** | -0.013*** | | |
| | | | (0.004) | (0.005) | | |
| PSL × High School | | | | | -0.005* | -0.005* |
| | | | | | (0.003) | (0.003) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Education × County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Education × Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County × Quarter-Year FE | | ✓ | | ✓ | | ✓ |
| N | 305,577 | 305,483 | 228,978 | 228,732 | 229,393 | 229,286 |

*Panel C - Industry*

| | ln(Separation Rate) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| PSL | -0.001 | | -0.002 | | -0.002 | | -0.001 | |
| | (0.011) | | (0.011) | | (0.011) | | (0.011) | |
| PSL × <60% Access | -0.015** | -0.018*** | | | | | | |
| | (0.006) | (0.006) | | | | | | |
| PSL × Leisure & Hospitality | | | -0.011 | -0.017* | | | | |
| | | | (0.011) | (0.010) | | | | |
| PSL × Construction | | | | | -0.022* | -0.025** | | |
| | | | | | (0.012) | (0.012) | | |
| PSL × Retail | | | | | | | 0.005 | -0.001 |
| | | | | | | | (0.013) | (0.014) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry × County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Industry × Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County × Quarter-Year FE | | ✓ | | ✓ | | ✓ | | ✓ |
| N | 765,752 | 765,225 | 547,652 | 546,016 | 562,459 | 561,156 | 565,013 | 563,989 |

## Table 7: Labor Supply

In the following panels, we examine the impact of paid sick leave mandates on the labor supply. In Panel A, the unit of observation is at the county-year level. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. In Panel B, the unit of observation is at the individual-state-year level. *Employed* is an indicator variable that equals one if an individual is employed, and zero otherwise. The variable *PSL* is an indicator variable that equals one if an individual lives in a state with an effective paid sick leave mandate in a given year. We create two treatment intensity indicators. The variable *Poor Health* is an indicator variable that equals one if an individual reported having fair or poor health, and zero otherwise. The variable *Has Young Children* is an indicator variable that equals one if an individual has children under the age 5, and zero otherwise. In each regression, we include the treatment intensity indicator, the paid sick leave indicator, and the interaction of the treatment intensity with the paid sick leave indicator. To conserve space, we do not report the coefficient on *Poor Health* (columns 1-2) and *Has Young Children* (columns 3-4). We include the following controls: age, and indicators for female, white, ethnicity, less than high school, high school, and some college education. The sample period is from March 2013 to March 2019. We restrict the data for individuals in treated states to 4 years before and after the effective date of the paid sick leave law. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

*Panel A - Labor Force Participation, Migration, and Retirement (county-level)*

| | ln(Retired Workers) | | Net Migration Rate | | ln(LFP Rate) | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL | -0.006** | | 0.002** | | 0.010* | |
| | (0.003) | | (0.001) | | (0.005) | |
| PSL × Number of Days | | -0.002*** | | 0.001* | | 0.002** |
| | | (0.001) | | (0.000) | | (0.001) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 18,686 | 18,686 | 18,693 | 18,693 | 21,809 | 21,809 |

*Panel B - Health and Family (individual-level)*

| | Employed? | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.002 | | 0.004* | |
| | (0.003) | | (0.002) | |
| PSL × Poor Health | 0.024** | 0.024** | | |
| | (0.011) | (0.011) | | |
| PSL × Has Young Children | | | 0.013* | 0.013* |
| | | | (0.007) | (0.007) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| State FE | ✓ | | ✓ | |
| Year FE | ✓ | | ✓ | |
| State-Year FE | | ✓ | | ✓ |
| N | 938,561 | 938,561 | 938,561 | 938,561 |

## Table 8: Financial Health

In the following panels, we analyze the impact of paid sick leave mandates on financial health. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter/year, and zero otherwise. In Panel A, The unit of observation is at the county-year level. In Panel B, the unit of observation is at the county-quarter (county-year) level in columns 1-2 (3-4). Controls include all of the control variables in Table 2. For quarterly outcomes, all control variables are lagged by one period. For quarterly (annual) outcomes, the sample period is from Q1-2013 to Q1-2019 (2013-2018). We restrict the treated counties' time series to 16 quarters/4 years before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Income, Poverty, and Inequality*

| | **Household Income** | **Poverty** | **Share of Aggregate HI** | | | **Inequality** |
|---|---|---|---|---|---|---|
| | ln(Median HI) (1) | ln(Poverty) (2) | <40% (3) | 40%-60% (4) | >60% (5) | Gini (6) |
| PSL | 0.020*** | -0.032*** | 0.001* | 0.001** | -0.002** | -0.002** |
| | (0.005) | (0.010) | (0.000) | (0.000) | (0.001) | (0.001) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 18,683 | 18,683 | 4,859 | 4,859 | 4,859 | 4,859 |

*Panel B - Other Financial Health Measures*

| | ln(Subprime Ratio) (1) | ln(Debt/Income Bin) (2) | ln(Uninsured) (3) | ln(Bankruptcy Filings) (4) |
|---|---|---|---|---|
| PSL | -0.039*** | -0.031** | -0.114* | -0.160*** |
| | (0.011) | (0.014) | (0.066) | (0.054) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | | |
| Year FE | | | ✓ | ✓ |
| N | 76,572 | 76,746 | 18,683 | 18,149 |

**Table 9: Impact of Paid Sick Leave Mandates on Firms**

The unit of observation is at the firm-year level. *PSL HQ* is a binary variable that equals 1 if a firm is headquartered in a location with an active PSL mandate, and 0 otherwise. *PSL EstabEmp* is a binary variable that equals 1 if more than 50% of the firm's employees operate in locations with an active PSL mandate, and 0 otherwise. For each firm, *Low Access* is a binary variable that equals 1 if less than 75% of total survey responses indicate that PSL is available, and 0 otherwise. In Panel A, the outcome variable is ROA, defined as operating income scaled by lagged assets. In Panel B, the outcome variable is the firm's overall rating (columns 1-2), culture/values rating (columns 3-4), and work/life balance rating (columns 5-6). Each rating is measured on a 1-5 scale, where 5 (1) is the highest (lowest) score. The sample period is 2013-2019. We restrict the treated firms' time series to 4 years before and after the effective date of the PSL law. Firm-level controls include the natural logarithm of total assets, book leverage, cash/assets, and fixed assets/assets. All control variables are lagged by one period. Continuous variables are winsorized at the 1st and 99th percentile level. Industries are defined using 2-digit SIC codes. Data on PSL availability and ratings is from Glassdoor. Data on establishment employment and location is from Infogroup. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the headquarters state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Profitability*

| | ROA | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| PSL HQ | 0.027*** | 0.018** | | | 0.004 | 0.004 | | |
| | (0.009) | (0.009) | | | (0.006) | (0.005) | | |
| PSL EstabEmp | | | 0.021*** | 0.015** | | | 0.003 | -0.001 |
| | | | (0.007) | (0.007) | | | (0.008) | (0.007) |
| PSL HQ × Low Access | | | | | 0.012* | 0.016** | | |
| | | | | | (0.007) | (0.006) | | |
| PSL EstabEmp × Low Access | | | | | | | 0.021*** | 0.022*** |
| | | | | | | | (0.005) | (0.008) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | | ✓ | | ✓ | | ✓ | |
| Industry × Year FE | | ✓ | | ✓ | | ✓ | | ✓ |
| N | 18,601 | 18,601 | 15,519 | 15,511 | 4,404 | 4,365 | 4,381 | 4,342 |
| $R^2$ | 0.85 | 0.85 | 0.83 | 0.84 | 0.79 | 0.82 | 0.80 | 0.82 |

*Panel B - Glassdoor Ratings*

| | Overall | | Work/Life | | Culture | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL HQ | 0.026* | 0.029* | 0.041** | 0.039* | 0.033** | 0.029** |
| | (0.014) | (0.016) | (0.017) | (0.019) | (0.013) | (0.014) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Firm FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | | ✓ | | ✓ | |
| Industry × Year FE | | ✓ | | ✓ | | ✓ |
| N | 5,217 | 5,179 | 5,217 | 5,179 | 5,217 | 5,179 |
| $R^2$ | 0.73 | 0.75 | 0.75 | 0.77 | 0.77 | 0.79 |

# Appendix

## 1. Synthetic Controls

We have the following setup. First, we assign each county to the treatment or control group based on whether it had an effective paid sick leave mandate at any point in time throughout the sample period. The control group consists of approximately 2,800 counties. Since it is infeasible to construct a synthetic control using all 2,800 counties, in the second step, we narrow down the potential pool of controls for each treated county. Similar to Pichler and Ziebarth (2018), we rank all counties based on measures of employment, population, and average weekly wages four quarters before the implementation of the sick leave mandate. We then keep all controls that fall within +/- 500 ranks from the treated county on all three dimensions. This pre-selection procedure generates about 200 potential controls for each treated county.[41] Then, we construct a synthetic control for each treated county by solving the following optimization problem:

$$\forall i \in TreatmentGroup$$

$$\{w_j^{i*}\}_{j \in ControlGroup} = \underset{\{w_j^i\}_{j \in ControlGroup}}{\arg\min} \sum_{p=1}^{P} v_p^i \left( X_{ip} - \sum_{j \in ControlGroup} w_j^i X_{jp} \right)^2 \tag{6}$$

$$s.t. \quad \sum_{j \in ControlGroup} w_j^i = 1 \quad \text{and} \quad \forall j \in ControlGroup \quad w_j^i \geq 0$$

To increase the likelihood of matching treated counties with controls that have similar labor markets and demographics, we include the following pre-intervention predictors ($X_p$): the logged value of employment 16, 13, 9, 5, and 1 quarter prior to the implementation date of the sick leave mandate along with average weekly wages and averages of the control variables in equation 1. We compute the averages over the entire 16 quarters before the effective law date (pre-intervention period). The nonnegative constant $v_p^i$ represents the weight assigned to each predictor $X_p$ based on its predictive power on the outcome. Following Abadie and Gardeazabal (2003) and Abadie et al. (2010), we choose

[41] If more than 200 controls fall within +/500 ranks from the treated county on all three dimensions, we keep the closest 200 based on the employment count four quarters before the implementation date of the sick leave mandate.

$\boldsymbol{V^i} = (v_1^i, ..., v_p^i)$ such that the synthetic control $(\{w_j^{i*}\}_{j \in ControlGroup})$ minimizes the pre-intervention mean squared prediction error (MSPE):

$$MSPE = \frac{1}{T_0} \sum_{\tau=1}^{T_0} \left( y_{i\tau} - \sum_{j \in ControlGroup} w_j^i(\boldsymbol{V^i}) y_{j\tau} \right)^2 \tag{7}$$

where $T_0$ is the length of the pre-intervention period and $y$ denotes the natural logarithm of employment. After constructing the synthetic control for each treated county, we compute the average intervention effect (AIE):

$$AIE_i = \frac{\sum\limits_{\tau > T_0} (y_{i\tau} - \hat{y}_{i\tau})}{T - T_0} \qquad \text{where} \qquad \hat{y}_{i\tau} = \sum_{j \in ControlGroup} w_j^{i*} y_{j\tau}$$

Then, we aggregate and estimate the simple average of AIEs:[42]

$$Aggregate\ \ AIE = \frac{\sum\limits_{i \in TreatmentGroup} AIE_i}{\sum\limits_{i \in TreatmentGroup} i}$$

To conduct inference, we follow a similar method to Acemoglu et al. (2016) and construct 95% confidence intervals based on random draws of 5,000 different combinations of placebo treatment counties. In each draw, we randomly select one placebo from the control group of each treated county, compute its AIE, and then finally estimate the aggregate AIE.

[42]In unreported analysis, we find similar results when we estimate an aggregate weighted average intervention effect (WAIE) where the AIE of each treated county is weighted by the goodness of the match (Acemoglu et al., 2016).

$$Aggregate\ \ WAIE = \frac{\sum\limits_{i \in TreatmentGroup} (\frac{1}{\hat{\sigma}_i} \times AIE_i)}{\sum\limits_{i \in TreatmentGroup} \frac{1}{\hat{\sigma}_i}} \qquad \text{where} \qquad \hat{\sigma}_i = \sqrt{\frac{\sum\limits_{\tau=1}^{T_0} (y_{i\tau} - \hat{y}_{i\tau})^2}{T_0}}$$

## 2. State and Local Paid Sick Leave Mandates

### Table A1: Summary of Paid Sick Leave Mandates

The following panels provide a summary of US paid sick leave mandates enacted between Q1-2013 and Q1-2019. Given that we restrict our sample to the post financial crisis period, we exclude laws passed in San Francisco (2006), Washington D.C. (2008), Seattle (2011), and Connecticut (2011). In Internet Appendix Table IA1 (Panel B), we consider all paid sick leave mandates that were enacted between Q1-2004 and Q1-2019.

*Panel A - State Paid Sick Leave Mandates*

| **State** | **Enactment Date** | **Effective Date** | **Accrual Rate (Annual Cap)** |
|---|---|---|---|
| California | Sep 19, 2014 | Jul 1, 2015 | 1 hour for every 30 hours worked (24 hours) |
| Massachusetts | Nov 4, 2014 | Jul 1, 2015 | >10 employees: 1 hour for every 30 hours worked (40 hours)<br>≤10 employees: 1 hour of unpaid sick leave for every 30 hours worked (40 hours) |
| Oregon | Jun 12, 2015 | Jan 1, 2016 | ≥10 employees: 1 hour for every 30 hours worked (40 hours)<br><10 employees: 1 hour of unpaid sick leave for every 30 hours worked (40 hours) |
| Vermont | Mar 9, 2016 | Jan 1, 2017 | 1 hour for every 52 hours worked (24 hours in 2018 and 40 hours starting in 2019) |
| Arizona | Nov 8, 2016 | Jul 1, 2017 | ≥15 employees: 1 hour for every 30 hours worked (40 hours)<br><15 employees: 1 hour for every 30 hours worked (24 hours) |
| Washington | Nov 9, 2016 | Jan 1, 2018 | 1 hour for every 40 hours worked (no explicit cap) |
| Maryland | Apr 5, 2017 | Feb 11, 2018 | ≥15 employees: 1 hour for every 30 hours worked (40 hours)<br><15 employees: 1 hour of unpaid sick leave for every 30 hours worked (40 hours) |
| Rhode Island | Sep 19, 2017 | Jul 1, 2018 | ≥18 employees: 1 hour for every 35 hours worked (24 hours in 2018, 32 hours in 2019, and 40 hours thereafter)<br><18 employees: 1 hour of unpaid sick leave for every 35 hours worked (24 hours in 2018, 32 hours in 2019, and 40 hours thereafter) |
| New Jersey | May 2, 2018 | Oct 29, 2018 | 1 hour for every 30 hours worked (40 hours) |
| Michigan | Dec 14, 2018 | Mar 29, 2019 | ≥50 employees: 1 hour for every 35 hours worked (40 hours) |

*Panel B - Local Paid Sick Leave Mandates*

| City | County | Enactment Date | Effective Date | Accrual Rate (Annual Cap) |
|---|---|---|---|---|
| Portland, OR | Multnomah | Mar 13, 2013 | Jan 1, 2014 | $>$5 employees: 1 hour for every 30 hours worked (40 hours)<br>$\leq$5 employees: 1 hour of unpaid sick leave for every 30 hours worked (40 hours) |
| Jersey City, NJ | Hudson | Mar 13, 2013 | Jan 1, 2014 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours) |
| New York, NY | New York, Kings, Bronx, Richmond, Queens | Jun 26, 2013 | Apr 1, 2014 | $\geq$5 employees: 1 hour for every 30 hours worked (40 hours)<br><br>$<$5 employees: 1 hour of unpaid sick leave for every 30 hours worked (40 hours) |
| Newark, NJ | Essex* | Jan 29, 2014 | Jun 21, 2014 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours) |
| Paterson, NJ | Passaic* | Sep 2, 2014 | Jan 1, 2015 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours) |
| Oakland, CA | Alameda* | Nov 4, 2014 | Mar 2, 2015 | $\geq$10 employees: 1 hour for every 30 hours worked (72 hours)<br>$<$10 employees: 1 hour for every 30 hours worked (40 hours) |
| Trenton, NJ | Mercer | Nov 4, 2014 | Mar 4, 2015 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours) |
| Philadelphia, PA | Philadelphia | Feb 12, 2015 | May 13, 2015 | $\geq$10 employees: 1 hour for every 40 hours worked (40 hours)<br>$<$10 employees: 1 hour of unpaid sick leave for every 40 hours worked (40 hours) |
| New Brunswick, NJ | Middlesex | Dec 17, 2015 | Jan 6, 2016 | $\geq$10 employees: 1 hour for every 35 hours worked (40 hours)<br>5-9 employees: 1 hour for every 35 hours worked (24 hours) |
| Tacoma, WA | Pierce | Jan. 27, 2015 | Feb 1, 2016 | 1 hour for every 30 hours worked (40 hours) |

| City | County | Enactment Date | Effective Date | Accrual Rate (Annual Cap) |
|---|---|---|---|---|
| Elizabeth, NJ | Union* | Nov 3, 2015 | Mar 2, 2016 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours per year)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours per year) |
| Los Angeles, CA | Los Angeles* | Jun 1, 2016 | Jul 1, 2016 | 1 hour for every 30 hours worked (48 hours per year) |
| San Diego, CA | San Diego | Jun 7, 2016 | Jul 11, 2016 | 1 hour for every 30 hours worked (40 hours per year) |
| Montgomery, MD | Montgomery | Jun 24, 2015 | Oct 1, 2016 | $\geq$5 employees: 1 hour for every 30 hours worked (56 hours per year)<br>$<$5 employees: 1 hour for every 30 hours worked (32 hours per year) |
| Spokane, WA | Spokane | Jan 26, 2016 | Jan 1, 2017 | 1 hour for every 30 hours worked (40 hours per year) |
| Morristown, NJ | Morris | Sep 13, 2016 | Jan 11, 2017 | $\geq$10 employees: 1 hour for every 30 hours worked (40 hours per year)<br>$<$10 employees: 1 hour for every 30 hours worked (24 hours per year) |
| Minneapolis, MN | Hennepin | May 27, 2016 | Jul 1, 2017 | $>$5 employees: 1 hour for every 30 hours worked (48 hours per year)<br>$\leq$5 employees: 1 hour of unpaid sick leave for every 30 hours worked (48 hours per year) |
| Chicago, IL** | Cook | Jun 22, 2016 | Jul 1, 2017 | 1 hour for every 40 hours worked (40 hours per year) |
| St. Paul, MN | Ramsey | Sep 7, 2016 | Jan 1, 2018 | 1 hour for every 30 hours worked (48 hours per year) |

*There are other cities within the county that enacted similar mandates: Essex (Bloomfield, East Orange, Irvington, Montclair), Passaic (Passaic, Paterson), Alameda (Berkeley, Emeryville), Union (Plainfield), and Los Angeles (Santa Monica).

**Cook county enacted a similar mandate on October 5, 2016.

## 3. Variable Descriptions

**Table A2: Variable Descriptions**

| County-level Variable | Description and Source |
|---|---|
| **PSL** | Indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given time period, and zero otherwise. For quarterly (annual) regressions, a county has an effective mandate if the law is effective for the entire quarter (for at least half of the year). For counties with local and state mandates, we consider the effective law date as the earlier implementation date of the two mandates. |
| **Employment** | The number of stable jobs in a quarter. It represents the number of jobs that are held on both the first and last day of the quarter with the same employer. *Source:* QWI |
| **Establishments** | The total count of establishments in a given quarter. *Source:* QCEW |
| **Separation Rate** | A measure of separations which is calculated as follows: $\left(\frac{2 \times Separations_{c\tau}}{Beg.Employment_{c\tau} + End.Employment_{c\tau}}\right)$ where $Separations_{c\tau}$ is the number of workers whose job with a given employer ended in a given quarter, while $Beg.Employment_{c\tau}$ and $End.Employment_{c\tau}$ represent beginning-of-quarter and end-of-quarter employment counts, respectively. *Source:* QWI |
| **Average Wages/Week** | Average weekly wages in a given quarter (expressed in 2012 dollars). *Source:* QCEW |
| **Average Earnings/Month** | Average monthly earnings of employees who worked for the same employer throughout a given quarter (expressed in 2012 dollars). *Source:* QWI |
| **Subprime Ratio** | Estimate of the percentage of the population with a credit score below 660. *Source:* Equifax |
| **Debt/Income Bin** | Household debt-to-income ratio bin. The Federal Reserve constructs 10 bins: debt/income ratios between 0-0.78, 0.78-1.01, 1.01-1.19, 1.19-1.37, 1.37-1.58, 1.58-1.82, 1.82-2.16, 2.16-2.63, 2.63-3.46, and >3.46. *Source:* Federal Reserve |
| **GDP Per Capita** | Estimate of the real gross domestic product per capita expressed in 2012 dollars. *Source:* BEA |
| **GDP Per Job** | Estimate of the real gross domestic product per job expressed in 2012 dollars. *Source:* BEA |
| **Median HI** | Real median household income expressed in 2012 dollars. *Source:* SAIPE |
| **Share of Aggregate HI <40%** | The fraction of aggregate household income held by households earning below the 40th percentile. *Source:* Census/ACS |
| **Share of Aggregate HI 40%-60%** | The fraction of aggregate household income held by households earning between the 40th and 60th percentile. *Source:* Census/ACS |
| **Share of Aggregate HI >60%** | The fraction of aggregate household income held by households earning above the 60th percentile. *Source:* Census/ACS |

| County-level Variable | Description and Source |
|---|---|
| **Gini** | The Gini index is a summary measure of income inequality. It summarizes the dispersion of income across the entire income distribution. The Gini coefficient ranges from 0, indicating perfect equality (where everyone receives an equal share), to 1, perfect inequality (where only one recipient or group of recipients receives all the income). *Source:* Census/ACS |
| **Poverty** | Estimate of the total number of people in poverty. *Source:* SAIPE |
| **Uninsured** | Estimate of the total number of people under the age of 65 who lack access to health insurance. *Source:* SAHIE |
| **Bankruptcy Filings** | Total number of personal bankruptcy filings. *Source:* US Courts |
| **Population** | Estimate of the total population in a given year. *Source:* Census |
| **Age 15-24 Ratio** | Percentage of the population between the ages 15 and 24. *Source:* Census |
| **Age 25-54 Ratio** | Percentage of the population between the ages 25 and 54. *Source:* Census |
| **Age 55-64 Ratio** | Percentage of the population between the ages 55 and 64. *Source:* Census |
| **Age 65+ Ratio** | Percentage of the population above the age of 65. *Source:* Census |
| **Female Ratio** | Percentage of the population that is female. *Source:* Census |
| **White Ratio** | Percentage of the population that is white. *Source:* Census |
| **Number of Days** | Estimate of the maximum number of paid sick leave days that must be provided under the mandate for a full-time employee who works for 250 days in a year. |
| **Mandate Coverage Ratio** | Estimate of the percentage of workers who will gain access to paid sick leave through the mandate. It is defined as the number of workers who are expected to gain access through the law divided by overall employment when the mandate was enacted. *Source:* National Partnership for Women & Families |
| **Poverty Ratio** | Estimate of the percentage of people in poverty. *Source:* SAIPE |
| **Poor Health Ratio** | The percentage of adults reporting fair or poor health in a given year. This measure is age-adjusted. *Source:* County Health Rankings & Roadmaps |
| **LFP Rate** | The percentage of the civilian noninstitutional population 16 years and older that is working or actively looking for work. *Source:* LAUS |
| **Net Migration Rate** | The difference between the inflow and outflow of people divided by total county population. *Source:* Census |
| **Retired Workers** | The count of individuals receiving retirement benefits. *Source:* Social Security Administration |
| **ROA** | Operating income scaled by lagged assets. *Source:* Compustat |
| **PSL HQ** | Indicator variable that equals 1 if a firm is headquartered in a location with an active PSL mandate, and 0 otherwise. *Source:* Compustat |
| **PSL EstabEmp** | Indicator variable that equals 1 if more than 50% of the firm's employees operate in locations with an active PSL mandate, and 0 otherwise. *Source:* Infogroup |
| **Low Access** | Indicator variable that equals 1 if less than 75% of total survey responses indicate that PSL is available, and 0 otherwise. *Source:* Glassdoor |

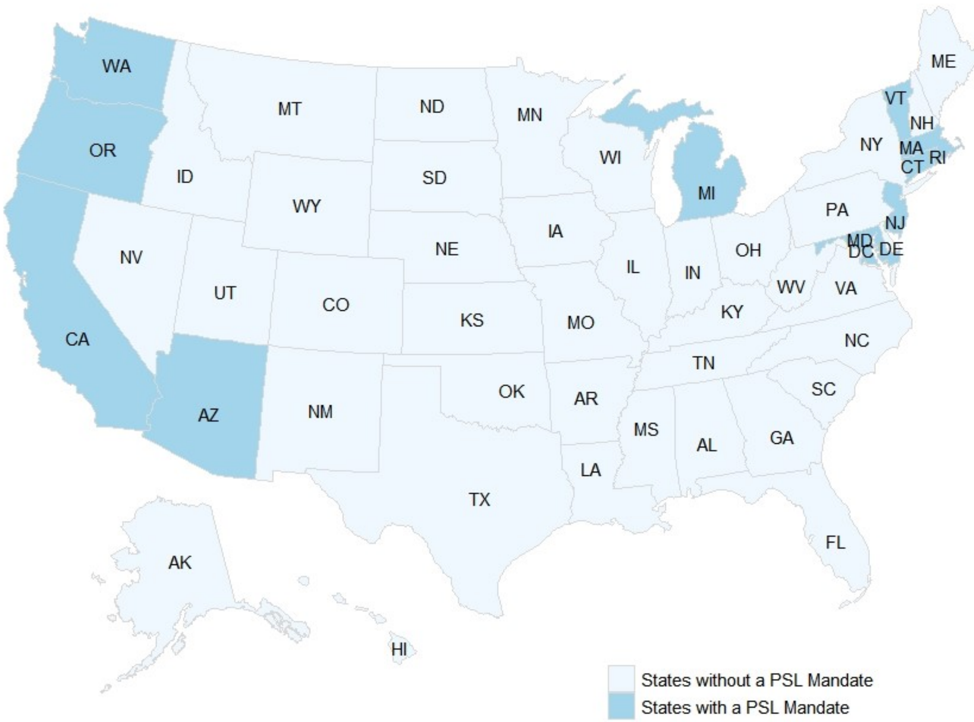
# Internet Appendix:

# "For Better or Worse? The Economic Implications of Paid Sick Leave Mandates"

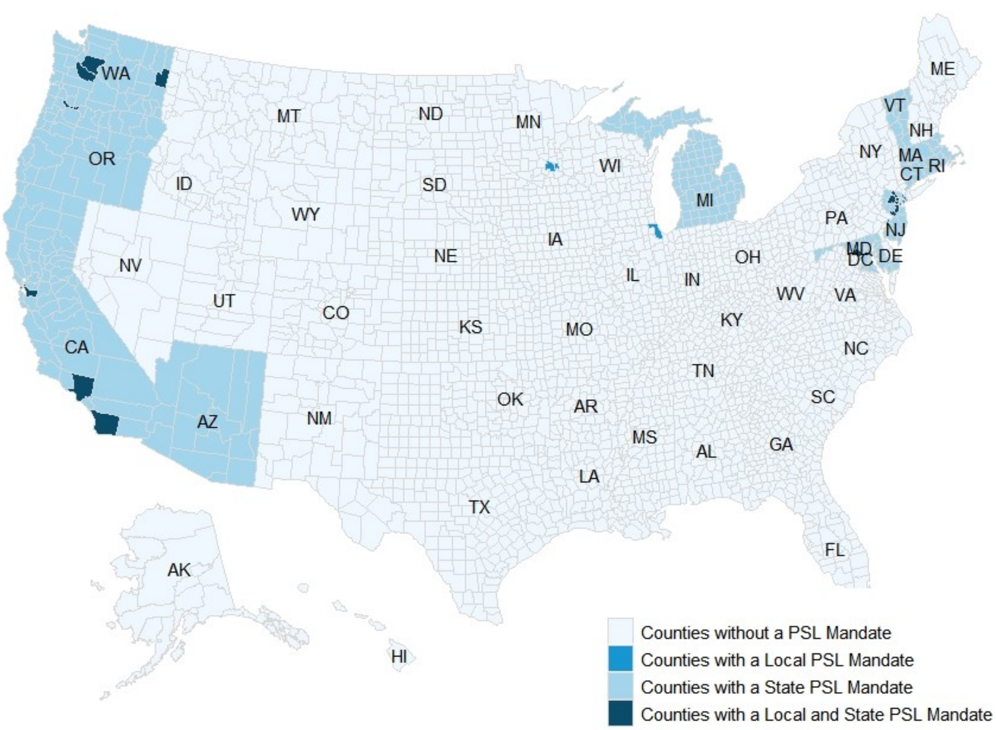
Turk Al-Sabah and Paige Ouimet

**Figure IA1: Paid Sick Leave Mandates in the U.S.**

**(A)** *Paid Sick Leave Mandates by State*



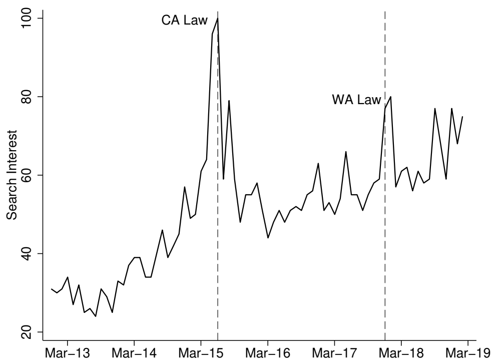

**(B)** *Paid Sick Leave Mandates by County*



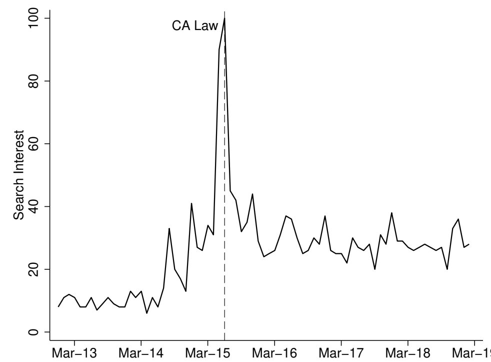
## Figure IA2: Sick Leave Search Trends

Figure IA2 plots the search interest for the keyword "sick leave" from Q1-2013 to Q1-2019. Search interest represents the number of times the keyword "sick leave" was searched. It is normalized by the peak number of searches over time and multiplied by 100. In Panel A, we examine overall search interest in the US where the dotted blue spikes represent the dates when California and Washington's paid sick leave mandates became effective, respectively. In Panels B-F, we consider search interest in several states where the dotted blue spikes represent the implementation date of the respective state paid sick leave mandate. Search activity data is from Google trends.

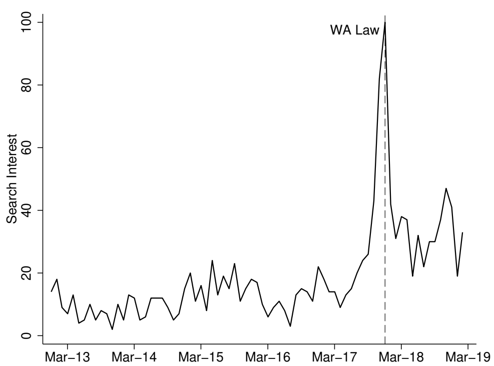
**(A)** *Overall*



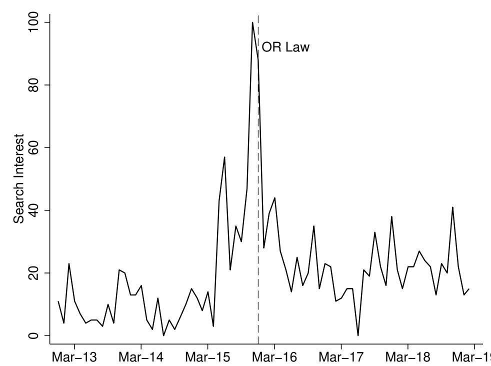

**(B)** *California*



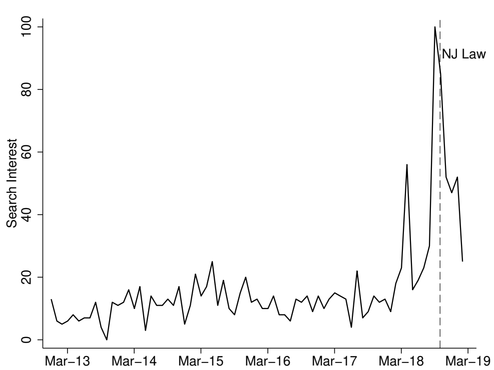

**(C)** *Washington*



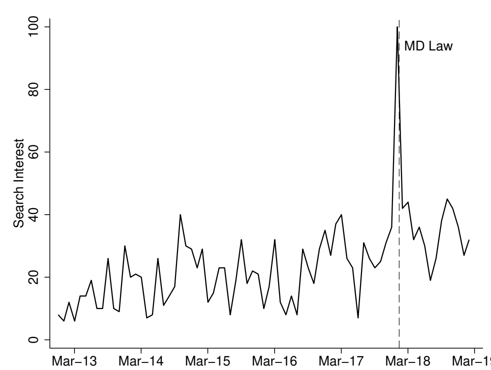

**(D)** *Oregon*




**(E)** *New Jersey*




**(F)** *Maryland*



## Table IA1: Sample Period and Treatment Window

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In Panel A, we focus on our baseline sample and consider different treatment windows. In column 1 (baseline), we restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. In columns 2-3, we use a treatment window of 12 and 8 quarters, respectively. In column 4, we keep all pre- and post-treatment periods. In all columns, we drop counties with active paid sick leave mandates that were implemented before Q1-2013. In Panel B, we repeat the same exercise, but use the full sample, which includes the financial crisis (Q1-2004 to Q1-2019). Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Baseline Sample (2013-2019)*

| | **-16/+16 Quarters** | **-8/+8 Quarters** | **-4/+4 Quarters** | **All Periods** |
|---|---|---|---|---|
| | ln(Employment) (1) | ln(Employment) (2) | ln(Employment) (3) | ln(Employment) (4) |
| PSL | 0.015** | 0.016*** | 0.012*** | 0.015** |
| | (0.006) | (0.006) | (0.004) | (0.006) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,165 | 74,939 | 77,268 |
| Within $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

*Panel B - Full Sample (2004-2019)*

| | **-16/+16 Quarters** | **-8/+8 Quarters** | **-4/+4 Quarters** | **All Periods** |
|---|---|---|---|---|
| | ln(Employment) (1) | ln(Employment) (2) | ln(Employment) (3) | ln(Employment) (4) |
| PSL | 0.029*** | 0.024*** | 0.018*** | 0.032*** |
| | (0.007) | (0.006) | (0.004) | (0.009) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 182,082 | 180,883 | 179,385 | 190,190 |
| Within $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

## Table IA2: Variation in Paid Sick Leave Mandates

In the following panels, we consider variation in paid sick leave mandates. In Panel A, the unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. Columns 1-2 represent the baseline specifications. In columns 3-4, we focus on local paid sick leave mandates. We exclude the following: 1) counties with state paid sick leave mandates 2) counties with state and local paid sick leave mandates, but where the state mandate preceded the local paid sick leave mandate. In columns 5-6, we consider state paid sick leave mandates. We exclude the following: 1) counties with local paid sick leave mandates 2) counties with state and local paid sick leave mandates, but where the local mandate preceded the state paid sick leave mandate. In Panel B, we repeat the same exercise, but exclude local mandates which apply to less than 50% of the total county population. Other controls include population ratios for different age groups, population ratio of females, and population ratio of whites. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

*Panel A - Overall*

| | **Local & State** | | **Local** | | **State** | |
|---|---|---|---|---|---|---|
| | ln(Employment) | | ln(Employment) | | ln(Employment) | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL | 0.019*** | 0.015** | 0.023*** | 0.025*** | 0.018*** | 0.015** |
| | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) | (0.006) |
| County Population | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | | ✓ | | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,803 | 71,934 | 71,934 | 76,312 | 76,312 |
| Within $R^2$ | 0.08 | 0.10 | 0.08 | 0.10 | 0.08 | 0.10 |

*Panel B - Population Coverage >50%*

| | **Local & State** | | **Local** | | **State** | |
|---|---|---|---|---|---|---|
| | ln(Employment) | | ln(Employment) | | ln(Employment) | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PSL | 0.019*** | 0.015** | 0.024*** | 0.028*** | 0.018*** | 0.015** |
| | (0.006) | (0.006) | (0.009) | (0.010) | (0.006) | (0.006) |
| County Population | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | | ✓ | | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 76,754 | 76,754 | 71,716 | 71,716 | 76,312 | 76,312 |
| Within $R^2$ | 0.08 | 0.10 | 0.08 | 0.10 | 0.08 | 0.10 |

## Table IA3: Regressions by Industry

In te following table, we estimate the baseline specification (equation 1) for each major industry group. The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. We obtain data on access to paid sick leave by industry from the BLS. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| **Industry** | **Access to PSL (%)** | **Treatment Effect** | |
|---|---|---|---|
| | | ln(Employment) | N |
| Leisure and Hospitality | 0.27 | 0.016**<br>(0.007) | 75,228 |
| Construction | 0.44 | 0.076***<br>(0.020) | 75,050 |
| Retail Trade | 0.50 | 0.010<br>(0.006) | 76,169 |
| Other Services | 0.56 | 0.043<br>(0.026) | 74,853 |
| Manufacturing | 0.65 | -0.002<br>(0.009) | 70,996 |
| Professional and Business Services | 0.69 | -0.003<br>(0.013) | 74,486 |
| Transportation and Warehousing | 0.73 | 0.010<br>(0.016) | 72,270 |
| Educational Services | 0.75 | -0.049<br>(0.040) | 43,215 |
| Health Care and Social Assistance | 0.78 | 0.017<br>(0.019) | 74,885 |
| Wholesale Trade | 0.78 | 0.014<br>(0.009) | 73,138 |
| Financial Activities | 0.90 | 0.018**<br>(0.008) | 74,460 |
| Information | 0.93 | 0.011<br>(0.024) | 62,503 |
| Utilities | 0.93 | -0.025*<br>(0.013) | 43,186 |

## Table IA4: Alternative Mechanisms

In the following panels, we consider alternative channels behind the positive employment effects. In Panel A, the unit of observation is at the county-quarter level. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. Other controls include population ratios for different age groups, population ratio of females, and population ratio of whites. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. In Panel B, we use individual-level data from the CPS. In columns 1-2, the outcome variable is a dummy variable which equals one (zero) if an individual is a part-time (full-time) employee. In columns 3-4, the outcome variable is the natural logarithm of the average number of hours worked per week. The unit of observation is at the individual-state-year level. The variable *PSL* is an indicator variable that equals one if an individual lives in a state with an effective paid sick leave mandate in a given year. Even columns include the following controls: age, and indicators for female, white, ethnicity, less than high school, high school, and some college education. The sample period is from March 2013 to March 2019. We restrict the data for individuals in treated states to 4 years before and after the effective date of the paid sick leave law. We also exclude individuals in states with active paid sick leave mandates that were implemented before March 2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Wages*

| | ln(Average Wages/Week) | | ln(Average Earnings/Month) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.010** | 0.012*** | 0.012** | 0.012** |
| | (0.004) | (0.004) | (0.005) | (0.005) |
| County Population | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,451 | 76,451 | 76,803 | 76,803 |

*Panel B - Part-time Employment and Hours of Work*

| | Part-time | | ln(Hours/Week) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | -0.013*** | -0.012*** | 0.013*** | 0.012*** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| Controls | | ✓ | | ✓ |
| State FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 367,803 | 367,803 | 367,803 | 367,803 |

**Table IA5: Minimum Wage and Unemployment Benefits**

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. Other controls include population ratios for different age groups, population ratio of females, and population ratio of whites. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

| | ln(Employment) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.019*** | 0.016*** | 0.020*** | 0.017** |
| | (0.006) | (0.006) | (0.007) | (0.006) |
| ln(Effective Minimum Wage) | -0.005 | -0.005 | | |
| | (0.019) | (0.015) | | |
| ln(Avg Unemp. Benefit) | | | -0.089*** | -0.055** |
| | | | (0.030) | (0.027) |
| County Population | ✓ | ✓ | ✓ | ✓ |
| Other Controls | | ✓ | | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,803 | 76,803 | 76,803 |
| Within $R^2$ | 0.08 | 0.10 | 0.08 | 0.10 |

## Table IA6: Treated vs. Control Counties

The following panels show the summary statistics for treated and control counties. Treated counties are counties that had an effective paid sick leave mandate in place at some point in time during the sample period. Control counties are counties that never had an effective paid sick leave mandate throughout the sample period. We exclude counties with active paid sick leave mandates that were implemented before Q1-2013. In Panel A, we consider the overall sample. For a given treated county $c$, the control group represents all control counties 4 quarters before county $c$ effective law date. We also perform a 1:3 nearest neighbor matching with replacement based on employment (Panel B), population (Panel C), real GDP (Panel D), and number of establishments (Panel E). The sample period is from Q1-2013 to Q1-2019. Variable descriptions can be found in Table A2 of the Appendix.

*Panel A - Overall Sample*

| | **Treated** | **Control** | **Difference in Means** | |
|---|---|---|---|---|
| | Mean | Mean | Difference | Std.Error |
| Employment | 131,033 | 23,944 | 107,089*** | (21,883) |
| Establishments | 11,940 | 1,907 | 10,032*** | (2,211) |
| Separation rate | 0.20 | 0.19 | 0.006 | (0.005) |
| Average wages/week | 791 | 687 | 104*** | (17) |
| Subprime credit ratio | 0.25 | 0.30 | -0.049*** | (0.004) |
| Debt/Income Bin | 6.79 | 5.45 | 1.344*** | (0.162) |
| GDP Per Capita | 49,887 | 60,361 | -10,474 | (10,802) |
| Median HI | 56,214 | 46,484 | 9,731*** | (1,046) |
| Poverty | 60,214 | 10,757 | 49,457*** | (10,438) |
| Bankruptcy Filings | 1,045 | 207 | 838*** | (204) |
| Uninsured | 39,180 | 7,802 | 31,378*** | (7,898) |
| Population | 403,980 | 75,864 | 328,116*** | (59,975) |
| Age 15-24 ratio | 0.13 | 0.13 | 0.002 | (0.002) |
| Age 25-54 ratio | 0.37 | 0.36 | 0.006* | (0.003) |
| Age 55-64 ratio | 0.14 | 0.14 | 0.002 | (0.002) |
| Age 65+ ratio | 0.18 | 0.18 | -0.001 | (0.004) |
| Female ratio | 0.50 | 0.50 | 0.004** | (0.001) |
| White ratio | 0.83 | 0.85 | -0.017 | (0.009) |

*Panel B - Matched Sample (Employment)*

| | Treated | Control | Difference in Means | |
|---|---|---|---|---|
| | Mean | Mean | Difference | Std.Error |
| Employment | 131,033 | 112,725 | 18,308 | (27,824) |
| Establishments | 11,940 | 8,596 | 3,344 | (2,569) |
| Separation rate | 0.20 | 0.19 | 0.009 | (0.005) |
| Average wages/week | 791 | 770 | 20 | (19) |
| Subprime credit ratio | 0.25 | 0.29 | -0.047*** | (0.006) |
| Debt/Income Bin | 6.79 | 5.10 | 1.685*** | (0.195) |
| GDP Per Capita | 49,887 | 54,028 | -4,142 | (5,191) |
| Median HI | 56,214 | 51,188 | 5,026*** | (1,213) |
| Poverty | 60,214 | 44,805 | 15,410 | (13,051) |
| Bankruptcy Filings | 1,045 | 894 | 151 | (254) |
| Uninsured | 39,180 | 40,233 | -1,052 | (11,923) |
| Population | 403,980 | 311,244 | 92,736 | (74,333) |
| Age 15-24 ratio | 0.13 | 0.13 | -0.003 | (0.003) |
| Age 25-54 ratio | 0.37 | 0.38 | -0.009** | (0.003) |
| Age 55-64 ratio | 0.14 | 0.13 | 0.008*** | (0.002) |
| Age 65+ ratio | 0.18 | 0.17 | 0.015*** | (0.004) |
| Female ratio | 0.50 | 0.50 | -0.002 | (0.001) |
| White ratio | 0.83 | 0.83 | 0.003 | (0.011) |

*Panel C - Matched Sample (Population)*

| | Treated | Control | Difference in Means | |
|---|---|---|---|---|
| | Mean | Mean | Difference | Std.Error |
| Employment | 131,033 | 124,985 | 6,048 | (29,039) |
| Establishments | 11,940 | 9,635 | 2,305 | (2,664) |
| Separation rate | 0.20 | 0.18 | 0.011* | (0.005) |
| Average wages/week | 791 | 768 | 22 | (19) |
| Subprime credit ratio | 0.25 | 0.30 | -0.053*** | (0.006) |
| Debt/Income Bin | 6.79 | 5.41 | 1.376*** | (0.203) |
| GDP Per Capita | 49,887 | 45,702 | 4,185 | (2,609) |
| Median HI | 56,214 | 51,966 | 4,249*** | (1,236) |
| Poverty | 60,214 | 52,571 | 7,644 | (13,705) |
| Bankruptcy Filings | 1,045 | 1,072 | -27 | (276) |
| Uninsured | 39,180 | 47,904 | -8,723 | (12,758) |
| Population | 403,980 | 354,376 | 49,603 | (76,919) |
| Age 15-24 ratio | 0.13 | 0.14 | -0.007* | (0.003) |
| Age 25-54 ratio | 0.37 | 0.38 | -0.012*** | (0.003) |
| Age 55-64 ratio | 0.14 | 0.13 | 0.011*** | (0.002) |
| Age 65+ ratio | 0.18 | 0.16 | 0.021*** | (0.004) |
| Female ratio | 0.50 | 0.51 | -0.003* | (0.001) |
| White ratio | 0.83 | 0.82 | 0.013 | (0.011) |

*Panel D - Matched Sample (Real GDP)*

| | Treated | Control | Difference in Means | |
|---|---|---|---|---|
| | Mean | Mean | Difference | Std.Error |
| Employment | 131,033 | 124,718 | 6,315 | (28,577) |
| Establishments | 11,940 | 9,330 | 2,610 | (2,627) |
| Separation rate | 0.20 | 0.18 | 0.012* | (0.005) |
| Average wages/week | 791 | 788 | 3 | (20) |
| Subprime credit ratio | 0.25 | 0.30 | -0.050*** | (0.006) |
| Debt/Income Bin | 6.79 | 4.98 | 1.805*** | (0.198) |
| GDP Per Capita | 49,887 | 60,209 | -10,322* | (5,166) |
| Median HI | 56,214 | 51,884 | 4,330*** | (1,269) |
| Poverty | 60,214 | 47,706 | 12,509 | (13,279) |
| Bankruptcy Filings | 1,045 | 984 | 61 | (265) |
| Uninsured | 39,180 | 42,705 | -3,524 | (12,200) |
| Population | 403,980 | 330,276 | 73,703 | (75,178) |
| Age 15-24 ratio | 0.13 | 0.13 | -0.004 | (0.003) |
| Age 25-54 ratio | 0.37 | 0.38 | -0.012*** | (0.003) |
| Age 55-64 ratio | 0.14 | 0.13 | 0.011*** | (0.002) |
| Age 65+ ratio | 0.18 | 0.16 | 0.019*** | (0.004) |
| Female ratio | 0.50 | 0.50 | -0.002 | (0.001) |
| White ratio | 0.83 | 0.82 | 0.012 | (0.011) |

*Panel E - Matched Sample (Number of Establishments)*

| | Treated | Control | Difference in Means | |
|---|---|---|---|---|
| | Mean | Mean | Difference | Std.Error |
| Employment | 131,033 | 130,020 | 1,013 | (30,294) |
| Establishments | 11,940 | 10,010 | 1,930 | (2,664) |
| Separation rate | 0.20 | 0.19 | 0.010* | (0.005) |
| Average wages/week | 791 | 781 | 10 | (19) |
| Subprime credit ratio | 0.25 | 0.29 | -0.048*** | (0.005) |
| Debt/Income Bin | 6.79 | 5.28 | 1.511*** | (0.206) |
| GDP Per Capita | 49,887 | 52,743 | -2,856 | (5,099) |
| Median HI | 56,214 | 52,660 | 3,555** | (1,242) |
| Poverty | 60,214 | 53,009 | 7,205 | (13,860) |
| Bankruptcy Filings | 1,045 | 1,054 | -9 | (254) |
| Uninsured | 39,180 | 47,402 | -8,222 | (13,328) |
| Population | 403,980 | 363,388 | 40,592 | (79,567) |
| Age 15-24 ratio | 0.13 | 0.13 | -0.004 | (0.003) |
| Age 25-54 ratio | 0.37 | 0.38 | -0.013*** | (0.003) |
| Age 55-64 ratio | 0.14 | 0.13 | 0.009*** | (0.002) |
| Age 65+ ratio | 0.18 | 0.16 | 0.017*** | (0.004) |
| Female ratio | 0.50 | 0.51 | -0.003* | (0.001) |
| White ratio | 0.83 | 0.82 | 0.015 | (0.011) |

## Table IA7: Matched Samples

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In Panel A, we consider all US counties and perform a 1:3 nearest neighbor matching (with replacement) by employment (column 1), population (column 2), GDP (column 3), and number of establishments (column 4). We repeat the same exercise in Panel B, but restrict the control pool to all counties located in coastal states. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

*Panel A - All States*

| | **Employment** | **Population** | **GDP** | **Establishments** |
|---|---|---|---|---|
| | ln(Employment)<br>(1) | ln(Employment)<br>(2) | ln(Employment)<br>(3) | ln(Employment)<br>(4) |
| PSL | 0.012**<br>(0.005) | 0.018***<br>(0.005) | 0.015**<br>(0.006) | 0.016***<br>(0.005) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 22,699 | 22,651 | 22,715 | 22,727 |
| Within $R^2$ | 0.12 | 0.19 | 0.16 | 0.17 |

*Panel B - Coastal States*

| | **Overall** | **Employment** | **Population** | **GDP** | **Establishments** |
|---|---|---|---|---|---|
| | ln(Employment)<br>(1) | ln(Employment)<br>(2) | ln(Employment)<br>(3) | ln(Employment)<br>(4) | ln(Employment)<br>(5) |
| PSL | 0.012**<br>(0.005) | 0.015***<br>(0.005) | 0.013**<br>(0.006) | 0.017***<br>(0.006) | 0.012***<br>(0.004) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 46,151 | 22,759 | 22,759 | 22,739 | 22,767 |
| Within $R^2$ | 0.12 | 0.18 | 0.15 | 0.15 | 0.20 |

## Table IA8: Determinants of Paid Sick Leave Mandates

The unit of observation is at the county-quarter level. The outcome variable in all regressions is *PSL Enactment*. The variable *PSL Enactment* is an indicator variable that equals one if a local or state paid sick leave mandate was enacted in a given county-quarter, and zero otherwise. All predictors are lagged by one period. The sample period is from Q1-2013 to Q1-2019. We exclude counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

| | PSL Enactment | | | | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ln(Employment) | 0.008 | 0.009 | 0.009 | 0.008 | 0.007 | 0.006 |
| | (0.005) | (0.007) | (0.007) | (0.006) | (0.006) | (0.006) |
| ln(GDP) | | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 |
| | | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) |
| ln(Establishments) | | -0.005 | -0.005 | -0.008 | -0.009 | -0.008 |
| | | (0.018) | (0.018) | (0.021) | (0.021) | (0.021) |
| Poverty Ratio | | | 0.003 | 0.005 | 0.012 | 0.010 |
| | | | (0.023) | (0.023) | (0.023) | (0.023) |
| ln(Median HI) | | | -0.001 | -0.003 | -0.003 | -0.002 |
| | | | (0.007) | (0.008) | (0.008) | (0.008) |
| ln(Population) | | | | 0.021 | 0.040 | 0.034 |
| | | | | (0.028) | (0.031) | (0.030) |
| Age 15-24 ratio | | | | | -0.168 | -0.173 |
| | | | | | (0.116) | (0.113) |
| Age 25-54 ratio | | | | | 0.024 | 0.048 |
| | | | | | (0.068) | (0.083) |
| Age 55-64 ratio | | | | | 0.020 | 0.035 |
| | | | | | (0.177) | (0.177) |
| Age 65+ ratio | | | | | 0.309** | 0.340** |
| | | | | | (0.144) | (0.154) |
| Female ratio | | | | | | 0.109 |
| | | | | | | (0.095) |
| White ratio | | | | | | -0.300** |
| | | | | | | (0.149) |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 74,462 | 73,151 | 73,138 | 73,138 | 73,138 | 73,138 |
| $R^2$ | 0.111 | 0.112 | 0.112 | 0.112 | 0.112 | 0.113 |

## Table IA9: Weighted Regressions

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In all of the regressions we weight each observation by county population. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | ln(Employment) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.019*** | 0.014*** | 0.014*** | 0.014*** |
| | (0.004) | (0.003) | (0.003) | (0.003) |
| ln(Population) | 1.069*** | 1.022*** | 1.019*** | 1.023*** |
| | (0.057) | (0.057) | (0.057) | (0.062) |
| Age 15-24 ratio | | 1.692** | 1.745** | 1.761*** |
| | | (0.649) | (0.670) | (0.656) |
| Age 25-54 ratio | | 2.933*** | 2.979*** | 2.968*** |
| | | (0.553) | (0.565) | (0.577) |
| Age 55-64 ratio | | 3.079*** | 3.084*** | 3.096*** |
| | | (0.643) | (0.643) | (0.632) |
| Age 65+ ratio | | 2.853*** | 2.835*** | 2.815*** |
| | | (0.577) | (0.575) | (0.606) |
| Female ratio | | | 0.424 | 0.420 |
| | | | (0.540) | (0.538) |
| White ratio | | | | 0.073 |
| | | | | (0.247) |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 76,803 | 76,803 | 76,803 | 76,803 |

## Table IA10: Excluding Large Treated States

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. We exclude counties in California (column 1), Washington (column 2), Oregon (column 3), and all 3 states (column 4). Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

| | **California** | **Washington** | **Oregon** | **All 3 States** |
|---|---|---|---|---|
| | ln(Employment) (1) | ln(Employment) (2) | ln(Employment) (3) | ln(Employment) (4) |
| PSL | 0.016** | 0.018*** | 0.010** | 0.011** |
| | (0.008) | (0.006) | (0.004) | (0.005) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 75,378 | 75,997 | 75,907 | 73,676 |
| Within $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

## Table IA11: Measures of Employment

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. The variable *PSL* is an indicator variable that equals one if a county has an effective local or state paid sick leave mandate in a given quarter, and zero otherwise. In columns 1 and 2, we use QWI beginning and end of quarter employment, respectively. In column 3, we use beginning of quarter employment provided by the QCEW. In columns 4-5, we rely on the employment measure provided by the BLS-LAUS and BEA, respectively. Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | **Beg.Quarter** | **End.Quarter** | **QCEW** | **LAUS** | **BEA** |
|---|---|---|---|---|---|
| | ln(Employment)<br>(1) | ln(Employment)<br>(2) | ln(Employment)<br>(3) | ln(Employment)<br>(4) | ln(Employment)<br>(5) |
| PSL | 0.015**<br>(0.007) | 0.013*<br>(0.007) | 0.024**<br>(0.010) | 0.016**<br>(0.007) | 0.016***<br>(0.006) |
| Controls | ✓ | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ | |
| Year FE | | | | | ✓ |
| N | 76,989 | 76,803 | 77,427 | 77,785 | 18,375 |
| Within $R^2$ | 0.09 | 0.08 | 0.09 | 0.20 | 0.34 |

## Table IA12: Placebo Tests

The unit of observation is at the county-quarter level. The outcome variable in all regressions is the natural logarithm of employment. In Panel A, we assume that treatment starts 16 quarters (column 1), 12 quarters (column 2), 8 quarters (column 3), or 4 quarters (column 4) before the true implementation date. We also assume that treatment lasts until the true implementation date of the paid sick leave mandate. In Panel B, we drop all "truly" treated counties and assume that the nearest neighbor control county is "falsely" treated and inherits the same treatment dates. We determine the nearest neighbor control county based on differences in employment (column 1), population (column 2), GDP (column 3), and number of establishments (column 4). Controls include all of the control variables in Table 2. All control variables are lagged by one period and are measured at an annual frequency. The sample period is from Q1-2013 to Q1-2019. We restrict the treated counties' time series to 16 quarters before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

*Panel A - Treatment Date*

| | **-16 Quarters** | **-12 Quarters** | **-8 Quarters** | **-4 Quarters** |
|---|---|---|---|---|
| | ln(Employment) | ln(Employment) | ln(Employment) | ln(Employment) |
| | (1) | (2) | (3) | (4) |
| PSL (placebo) | 0.000 | 0.002 | 0.005 | 0.005 |
| | (0.004) | (0.006) | (0.005) | (0.004) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 73,435 | 73,729 | 74,877 | 74,827 |
| Within $R^2$ | 0.10 | 0.10 | 0.10 | 0.10 |

*Panel B - Treated Counties*

| | **Employment** | **Population** | **GDP** | **Establishments** |
|---|---|---|---|---|
| | ln(Employment) | ln(Employment) | ln(Employment) | ln(Employment) |
| | (1) | (2) | (3) | (4) |
| PSL (placebo) | 0.003 | -0.003 | 0.004 | -0.004 |
| | (0.005) | (0.005) | (0.005) | (0.004) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| County FE | ✓ | ✓ | ✓ | ✓ |
| Quarter-Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 71,017 | 71,015 | 71,021 | 71,000 |
| Within $R^2$ | 0.10 | 0.09 | 0.09 | 0.09 |

## Table IA13: Treatment Effect Heterogeneity

In the following table, we allow for treatment effect heterogeneity by using the group-time average treatment effect estimator (Callaway and Sant'Anna, 2021). We aggregate across groups. The results are displayed in row 2. For comparison, we also show our baseline results in row 1. In column 1 (2-4), the unit of observation is at the county-quarter (county-year) level. A county is treated if it has an effective local or state paid sick leave mandate in a given time period. We include all of the control variables in Table 2 as pre-treatment covariates. For quarterly (annual) outcomes, the sample period is from Q1-2013 to Q1-2019 (2013-2018). We restrict the treated counties' time series to 16 quarters/4 years before and after the effective date of the paid sick leave law. We also drop counties with active paid sick leave mandates that were implemented before Q1-2013. Variable descriptions can be found in Table A2 of the Appendix. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | **Aggregated Treatment Effect Estimates** | | | |
|---|---|---|---|---|
| | ln(Separation Rate) | ln(Median HI) | ln(Poverty) | ln(Bankruptcy Filings) |
| | (1) | (2) | (3) | (4) |
| TWFE estimate of PSL | -0.025* | 0.020*** | -0.032*** | -0.160*** |
| | (0.015) | (0.005) | (0.010) | (0.054) |
| C&S (2021) estimate of PSL | -0.015*** | 0.013*** | -0.029*** | -0.088*** |
| | (0.006) | (0.003) | (0.005) | (0.021) |

## Table IA14: City-level Analysis

The unit of observation is at the city-year level. The variable *PSL* is an indicator variable that equals one if a city has an effective local or state paid sick leave mandate in a given year, and zero otherwise. The outcome variables are city-level employment (column 1), median household income (column 2), poverty count (column 3), Gini index (column 4), and total count of individuals under the age of 65 who lack access to health insurance (column 5). We obtain city-level data on employment and other economic characteristics from BLS-LAUS and the American Community Survey (ACS), respectively. Given that we observe quarterly employment, the sample period in column 1 is from 2013 to 2019. For other outcomes, which we observe annually, the sample period is from 2013 to 2018. We restrict the treated counties' time series to 4 years before and after the effective date of the paid sick leave law. We also drop cities with active paid sick leave mandates that were implemented before 2013. Standard errors are clustered at the state level. (*** $p<0.01$, ** $p<0.05$, * $p<0.10$)

| | ln(Employment) (1) | ln(Median HI) (2) | ln(Poverty) (3) | Gini (4) | ln(Uninsured) (5) |
|---|---|---|---|---|---|
| PSL | 0.012*** | 0.030*** | -0.023* | -0.002* | -0.202** |
| | (0.003) | (0.007) | (0.012) | (0.001) | (0.086) |
| City Population | ✓ | ✓ | ✓ | ✓ | ✓ |
| City FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ | ✓ |
| N | 11,102 | 3,227 | 3,227 | 3,227 | 3,227 |
| Within $R^2$ | 0.76 | 0.02 | 0.00 | 0.00 | 0.08 |

## Table IA15: Individual-level Analysis

In the following panels, we use individual-level data from the ASEC supplement of the CPS. The unit of observation is at the individual-state-year level. *Employed* is an indicator variable that equals one if an individual is employed, and zero otherwise. *Job Change* is an indicator variable that equals one if a worker went through at least one job change in the previous calendar year, and zero otherwise. *Household income* represents the total monetary income of all adult household members, while *Earnings-Longest Job* is the total earnings from the job held for the longest time during the previous calendar year. Lastly, *Poverty* and *Uninsured* are indicators of individuals in poverty and individuals without access to private health insurance, respectively. The variable *PSL* is an indicator variable that equals one if an individual lives in a state with an effective paid sick leave mandate in a given year. All regressions include the following controls: age, and indicators for female, white, ethnicity, less than high school, high school, and some college education. The sample period is from March 2013 to March 2019. We restrict the data for individuals in treated states to 4 years before and after the effective date of the paid sick leave law. We also exclude individuals in states with active paid sick leave mandates that were implemented before March 2013. Standard errors are clustered at the state level. (*** p<0.01, ** p<0.05, * p<0.10)

*Panel A - Employment and Job Stability*

| | Employed? | | Job Change? | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.006* | 0.006** | -0.009*** | -0.009*** |
| | (0.004) | (0.002) | (0.003) | (0.003) |
| Controls | | ✓ | | ✓ |
| State FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 938,561 | 938,561 | 600,571 | 600,571 |
| $R^2$ | 0.01 | 0.16 | 0.00 | 0.02 |

*Panel B - Financial Health*

| | ln(Household Income) | ln(Earnings)-Longest Job | Poverty | Uninsured |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| PSL | 0.020*** | 0.040*** | -0.006** | -0.013* |
| | (0.006) | (0.006) | (0.003) | (0.008) |
| Controls | ✓ | ✓ | ✓ | ✓ |
| State FE | ✓ | ✓ | ✓ | ✓ |
| Year FE | ✓ | ✓ | ✓ | ✓ |
| N | 445,068 | 598,582 | 938,561 | 805,315 |
| $R^2$ | 0.17 | 0.22 | 0.05 | 0.11 |