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# The Effect of Paid Sick Leave Mandates on Access to Paid Leave and Work Absences

Kevin Callison  
*Tulane University*

Michael F. Pesko  
*Georgia State University*

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**The Effect of Paid Sick Leave Mandates  
on Access to Paid Leave and Work Absences**

**Upjohn Institute Working Paper No. 16-265**

Kevin Callison  
*Tulane University*  
E-mail: [kcallison@tulane.edu](mailto:kcallison@tulane.edu)

Michael F. Pesko  
*Georgia State University*  
E-mail: [mpesko@gsu.edu](mailto:mpesko@gsu.edu)

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**ABSTRACT**

We evaluate the impact of paid sick leave (PSL) mandates on access to PSL and work absences for private sector workers in the U.S. By exploiting geographic and temporal variation in PSL mandate enactment, we compare changes in outcomes for workers in counties affected by a PSL mandate to changes for those in counties with no mandate. Additionally, we rely on within-county variation in the propensity to gain PSL following a mandate to estimate policy effects for workers most likely to acquire coverage. Results indicate that PSL mandates lead to increased access to PSL benefits, especially for women and those working in industries where workers historically lacked access to PSL. We also find that PSL laws increase work absences for those most likely to gain coverage, but reduce absences for others.

**JEL Classification Codes:** I18, I12, J21, J23, J32

**Key Words:** Paid sick leave, labor market, absenteeism

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Kevin Callison\*  
*Tulane University*

Michael F. Pesko  
*Georgia State University*

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## ABSTRACT

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\* Callison: Tulane University, School of Public Health and Tropical Medicine & Department of Economics, 1440 Canal St., New Orleans, LA 70112; email [kcallison@tulane.edu](mailto:kcallison@tulane.edu). Pesko: Georgia State University, Department of Economics, P.O. Box 3992, Atlanta, GA 30302; email [mpesko@gsu.edu](mailto:mpesko@gsu.edu).

## 1. Introduction

The United States is one of only two OECD countries with no federal mandate guaranteeing workers access to paid sick leave (PSL) (World Policy Analysis Center, 2016).<sup>1</sup> At the local level, proliferation of PSL mandates has continued since San Francisco enacted the first municipal PSL legislation in 2006, while Connecticut became the first state to require that employers provide PSL benefits in 2012 followed by California, Massachusetts, and Oregon (National Partnership for Women & Families 2016).<sup>2</sup> Renewed efforts to establish federally mandated PSL access in the U.S. culminated in the reintroduction of the Healthy Families Act to Congress in 2015.<sup>3</sup> The legislation, were it to become law, would establish a national standard regulating the provision of PSL benefits for qualified workers. Furthermore, an executive order signed by President Obama that required firms with federal government contracts to provide employees with up to 7 paid annual sick days took effect in 2017. Alternatively, 19 states have passed legislation that preemptively blocks municipalities from enacting local PSL mandates and the expansion of PSL through legislative regulation has become a contentious political issue. Though it is not uncommon for firms to offer employees PSL benefits in the absence of a federal or local mandate, estimates suggest that nearly 40 percent of private sector workers in the U.S. currently lack PSL coverage (Bureau of Labor Statistics, 2016).

While public opinion polls indicate strong support for mandated PSL benefits, opponents argue that PSL regulations are costly to employers and reduce worker hours by encouraging absenteeism (Jones et al., 2014; Nelson, 2014; Pichler and Ziebarth, 2017b).<sup>4</sup> Unfortunately, evidence

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<sup>1</sup> South Korea is the other OECD country that lacks federal PSL legislation.

<sup>2</sup> See Appendix Table 1 for a detailed description of these paid sick leave mandates.

<sup>3</sup> The Healthy Families Act was originally introduced to Congress in 2004 and then again in 2013.

<sup>4</sup> For examples of public opinion polls of paid sick leave support see: Huffington Post/YouGov Paid Sick Leave Poll, June 2013; Lake Research Partners Poll, January 2015; Pew Research Center, March 2017, “Americans Widens Support Paid Family and Medical Leave, but Differ Over Specific Policies”.

to support a causal link between PSL mandates and absenteeism is sparse given that nearly every study of the relationship between PSL access and labor outcomes has analyzed legislation adopted outside the U.S. (Henrekson and Persson, 2004; Puhani and Sonderhof, 2010; Ziebarth and Karlsson, 2010; Ziebarth and Karlsson, 2014). These foreign laws are largely dissimilar to the proposed and recently enacted U.S. statutes and serve as poor models for the effects of expanded PSL generosity in the U.S.

Our paper adds to a small, but growing literature examining the employment effects of PSL mandates in the U.S. Specifically, we use the enactment of local and state level mandates to evaluate the relationship between PSL mandates and access to paid work leave for private sector workers. After establishing that PSL mandates lead to coverage expansions, we then estimate the effect of PSL mandates on illness-related work absences. Our findings make two contributions to the study of the labor market effects of PSL mandates. We provide the first estimates of the coverage effects of PSL mandates at the local and state levels in the U.S. Understanding how PSL mandates translate into increased access to paid leave is a crucial first step for any analysis of the long-run effects of PSL regulations. Secondly, we quantify the impact of increased access to PSL on worker absenteeism in the U.S. The ex-ante effect of access to paid leave on work absences is unclear. The availability of PSL reduces the cost of absenteeism to workers, thus potentially increasing the likelihood of a work absence (Gilleskie, 1998). However, in the presence of a communicable illness, increased access to PSL could also reduce “presenteeism” or attending work while sick (Johns, 2010, Susser and Ziebarth, 2016). For example, fewer sick employees in the workplace could limit the spread of a communicable illness and lead to fewer work absences (Pichler and Ziebarth, 2017a).

We first establish that PSL mandates lead to economically significant increases in access to paid leave using data from the National Health Interview Survey (NHIS) and the Current Population Survey’s Annual Social and Economic Supplement (CPS-ASEC) from 2005 to 2015. Relying on

geographic and temporal variation in mandate adoption, we estimate difference-in-differences (DD) models comparing outcomes for those living in counties affected by PSL mandates to those living in counties with no mandate in place. Since workers often have PSL benefits in the absence of a legislative mandate, we refine our analysis by predicting the probability of gaining PSL after a mandate takes effect and using this predicted probability to estimate a triple-differences (DDD) model comparing within-county changes in work absences after mandate enactment for those with a high probability of gaining coverage to those with a low probability of gaining coverage. This method is an improvement over traditional DD estimates of the effect of PSL mandates that fail to focus on workers targeted by the legislation.

We then turn to estimates of the effect of PSL mandates on illness-related work absences. Our results indicate that workers gaining access to PSL following a mandate exhibit substantial increases in illness-related work absences at both the extensive and intensive margins. We find similar patterns among newly covered workers in both the NHIS and CPS-ASEC samples. However, these absences are largely offset by a reduction in illness-related work absences for populations that traditionally have access to paid leave and are not directly affected by the enactment of a mandate. This finding supports the notion that access to PSL lowers rates of presenteeism for those gaining coverage, limiting the spread of communicable illnesses, and resulting in fewer illness-related work absences for others. These results have significant implications for the current debate over the expansion of PSL benefits to workers in the U.S. While the direct cost to employers of providing paid leave may be large, we show that increased access to PSL generates positive spillovers that act to offset these direct costs and have the potential to improve the health of the workforce.

## 2. Labor Market Effects of Paid Leave Policies

Since PSL mandates are a relatively new phenomenon in the U.S., studies of the labor market effects of mandated leave policies have generally focused on two related areas: paid leave mandates surrounding childbirth and unpaid access to sick leave. Rossin-Slater, Ruhm, and Waldfogel (2012) and Baum and Ruhm (2016) examined changes in maternal and paternal paid leave following the enactment of California's 2004 Paid Family Leave program. Both studies found that access to paid family leave increased leave-taking on the intensive margin, while Das and Polachek (2015) found persistent negative effects on women's employment. We note that the applicability of these studies to the case of PSL for all workers is questionable since work absences related to childbirth are largely planned in advance.

Evidence of the effect of access to unpaid leave on work absences is mixed. The Family and Medical Leave Act (FMLA) of 1993 guaranteed eligible workers access to unpaid, job-protected employment leave for circumstances including a serious health condition that impedes job performance, childbirth, or the care of a close relative with a serious health condition (U.S. Department of Labor 2016).<sup>5</sup> Waldfogel (1999) analyzed the effect of the FMLA on work absences, employment, and earnings. She found that the FMLA increased instances of leave-taking, but had no effect on changes in employment or wages. Alternatively, using the FMLA and prior state-level unpaid leave mandates, Baum (2003) reported that unpaid leave mandates had no effect on leave-taking for mothers who had recently given birth. Also examining unpaid parental leave, Han, Ruhm, and Waldfogel (2009) found that expansions in access to unpaid leave increased leave-taking for both mothers and fathers. Because the FMLA provides *unpaid* leave and covers less than half of

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<sup>5</sup> An eligible worker is defined as a worker in a firm with 50 or more employees working at least 1,250 hours in the 12 months prior to taking leave.

private sector workers, it is unclear whether these earlier findings of the effects of FMLA on work absences extend to the recent PSL mandates adopted by states and municipalities in the U.S.

Research on the labor market effects of *paid* leave mandates has generally focused on European countries where mandated benefits have been in place for several years and administrative data on paid leave access and work absences is more widely available than in the U.S. Henrekson and Persson (2004) concluded that increases in sick leave generosity in Sweden were related to increased absenteeism over an extended period from 1955 to 1999. Puhani and Sonderhof (2010) investigated German legislation that initially reduced and then later expanded opportunities for PSL. The authors reported that decreasing sick pay from 100 percent to 80 percent of wages resulted in a reduction of 2.4 sick days per year on average. Similarly, examining the same reduction in German sick pay, Ziebarth and Karlsson (2010) reported that the share of workers with zero work absences increased between 6 percent and 8 percent, while Ziebarth and Karlsson (2014) found that restoring German sick pay to 100 percent of wages led to a 10 percent increase in work absences.

Due to the novelty of PSL mandates in the U.S., few studies have examined labor market effects of paid leave access for U.S workers. Using a survey of employers in San Francisco, Colla et al. (2014) found that the share of firms providing PSL coverage to employees increased from 73% to 91% following the enactment of a PSL mandate in 2007. However, it was not clear how many workers were affected by the policy change. Furthermore, San Francisco employers voluntarily offered PSL benefits at a relatively high rate prior to the mandate raising concerns over the applicability of these findings to other regions. Using a structural framework simulation, Gilleskie (1998) found that moving from no PSL coverage to full coverage would increase illness-related work absences by 45% per illness episode, but estimates from a reduced form model deviated substantially when she considered alternative policy scenarios that included combinations of PSL mandates and expanded health insurance coverage.



More recently, Ahn and Yelowtiz (2016) matched U.S. workers with and without access to PSL on various observable characteristics and found that PSL coverage led to approximately 1.2 additional work absences per year. One concern with this identification strategy is that unobserved differences between those with and without access to PSL could bias the authors' estimated effects. Because Ahn and Yelowtiz do not rely on the plausibly exogenous (to the worker) effect of PSL mandate enactment, estimates of within-region differences between those with and without PSL access are especially susceptible to omitted variable bias. In the same study, Ahn and Yelowtiz estimated models that used exogenous measures of regional influenza infection rates to identify the effect of PSL access on absenteeism, which resulted in a smaller effect of 0.9 additional work absences per year. Finally, Pichler and Ziebarth (2017b) found no evidence that U.S. PSL mandates affected employment or wages, though the authors focused on all workers rather than those most likely to gain PSL access following the enactment of a mandate. We show below that this distinction has meaningful implications for the magnitude of the estimated effects of PSL mandates.

### **3. Data**

The primary data source for our analysis of the effect of PSL mandates on PSL access and worker absences is the National Health Interview Survey (NHIS) maintained by the National Center for Health Statistics (NCHS). The objective of the NHIS is to monitor the health of the U.S. population through the collection and analysis of data on a broad range of health and labor market topics. The NHIS is a cross-sectional household survey with continuous sampling and interviewing throughout the year, which follows a multistage area probability sampling design that permits the representative sampling of households and non-institutional group quarters (e.g. college dormitories) in the U.S. Data are collected through a personal household interview conducted by interviewers

employed and trained by the U.S. Census Bureau according to procedures specified by the NCHS (Centers for Disease Control [CDC], 2016a).

We rely on restricted access state- and county-identified NHIS data collected between 2005 and 2015 for our analyses. Our sample is restricted to workers between the ages of 18 and 64 who are employed in the private sector and do not report being self-employed at the time of their interview.<sup>6</sup> To maintain consistency between our treatment counties (those enacting a PSL mandate) and our control counties (those with no PSL mandate), we further restrict our sample to urban counties as defined by the 2013 Urban-Rural Classification (CDC, 2014). The NHIS includes information on gender, race/ethnicity, education, poverty status, marital status, and health insurance coverage that we use to control for observable differences between individuals in our sample. We construct an indicator that is equal to one for those living in counties that enacted a PSL mandate after the mandate became effective and is equal to zero otherwise.<sup>7</sup>

Our outcome measure for PSL access is derived from a question that asks those currently employed, “Do you have paid sick leave on this main job or business?”. We measure work absences using a question that asks, “During the past 12 months, about how many days did you miss work at a job or business because of illness or injury (do not include maternity leave)?”. It is important to note that this question does not include all work absences, but specifically addresses absences due to illness or injury. Finally, to our NHIS sample we merge data on the number of physicians per capita (general practitioners, family practitioners, and all MDs), the number of inpatient days per capita, and the number of outpatient physician visits per capita from the Area Health Resource Files

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<sup>6</sup> Our analysis is limited to private sector workers because nearly all public-sector workers already have access to PSL. We exclude individuals in the following employment categories: looking for work; working, but not for pay, at a family-owned job or business; not working at a job or business and not looking for work; employees of federal/state/local government; and self-employed in own business, professional practice, or farm.

<sup>7</sup> Without more precise geographic information, we measure municipal PSL mandate adoption at the county level so that a county is included in our treatment group if a city within that county has enacted a PSL mandate. We expect the measurement error introduced from this definition of PSL to be small considering that the city/county boundaries for San Francisco, Washington DC, and New York City fully overlap.

(AHRF); county-level Medicare fee-for-service parts A and B per-capita spending from the Centers for Medicare and Medicaid Services; and county-level unemployment rates from the Bureau of Labor Statistics (BLS). We include data on the supply of physicians and on health care expenditures, along with controls for individual insurance coverage, to address concerns with access-related changes and insurance expansions resulting from the implementation of provisions of the Affordable Care Act that occurred contemporaneously with some of the PSL mandates that we study.

While the NHIS has clear advantages for our analysis, one drawback to this particular dataset is its relatively small sample size (approximately 3,700 workers per year remain after sample restrictions). To address issues associated with precision arising from a smaller sample, we conduct a complementary analysis using data from the Current Population Survey's Annual Social and Economic Supplement (CPS-ASEC) between 2005 and 2015. Unfortunately, the CPS has limited available information on access to paid sick leave, but the survey does include questions related to worker absences. Specifically, the CPS-ASEC asks workers who were absent from work the previous week the reason for their absence. We use this variable to construct an indicator for a work absence in the past week and an indicator for a work absence in the past week due to "own illness, injury, or medical problems". We combine the CPS-ASEC sample with the AHRF, Medicare, and BLS data described above.

Table 1 presents descriptive statistics for our NHIS sample separately for those living in counties that enacted a PSL mandate (treatment counties) and those living in counties with no PSL mandate (control counties). Counties that enact PSL mandates tend to have more highly educated populations, more residents earning 400% of the federal poverty level or above, and more residents with Medicaid coverage, while those living in control counties are more likely to lack insurance coverage.

#### 4. Empirical Framework

We begin our empirical analysis by estimating the effect of PSL laws on PSL access and work absences using a difference-in-difference (DD) strategy that exploits the temporal and geographic variation in the timing of the enactment of PSL mandates. The relationship between PSL mandates and our outcomes of interest is formalized as follows:

$$(1) \quad Y_{i,c,t} = \alpha + \gamma PSL_{c,t} + \beta X_{i,c,t} + \theta Z_{c,t} + \delta_c + \tau_t + \delta_c \times \tau_t + \varepsilon_{i,c,t}$$

where  $Y$  is the outcome of interest for person  $i$  in county  $c$  at year-quarter  $t$ ;  $PSL$  is an indicator for the enactment of a PSL mandate in any part of year-quarter  $t$ ;<sup>8</sup>  $X$  is a vector of individual characteristics (sex, race/ethnicity, marital status, education, age, health insurance coverage, and industry of employment);  $Z$  is a vector of time-varying county-level factors (physician supply, inpatient and outpatient days, FFS Medicare spending, and the unemployment rate);  $\delta_c$  is a county fixed effect,  $\tau_t$  is a year-quarter fixed effect, and  $\delta_c * \tau_t$  represents a county-specific linear time trend which is included in our preferred specifications. Equation (1) represents a standard DD analysis where outcomes in our treatment regions (i.e., counties and states enacting a PSL mandate) are compared to control regions that have no PSL laws in place. We cluster our standard errors at the county level in all analyses.

Our dependent variable,  $Y_{i,c,t}$  in Equation (1) represents one of several possible outcomes. We initially estimate the effect of PSL laws on access to PSL measured by whether a worker in our sample reports having PSL benefits at their main job. Our hypothesis is that a PSL mandate should

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<sup>8</sup> We define enactment of a PSL mandate as the year-quarter in which the mandate actually took effect.

increase the share of workers reporting access to PSL. However, if PSL mandate adoption is focused in areas where private employers display a high likelihood of offering PSL, then we may find a relatively small effect of the mandate. Additionally, employers may attempt to avoid a mandate by relocating or restructuring their workforce, which would also negate access effects of a mandate.

After examining the effect of PSL mandates on worker access to paid leave, we then estimate the effect of mandates on work absences using both the NHIS and CPS-ASEC data. In the NHIS data, we examine changes in the probability of reporting any work absences due to illness or injury (excluding pregnancy-related absences) in the past 12 months, 2 or more work absences, 5 or more work absences, and 10 or more work absences. The CPS-ASEC does not include information on the intensive margin of work absences, so we are only able to analyze changes in the probability of any work absence in the past week. Another modification we make when examining work absences is to exclude the first 12 months after the mandate took effect. This is because all of the mandates in our study include provisions requiring workers to accrue PSL over an extended period of time (e.g. 1 hour of PSL for every 30 hours worked). We therefore expect any effect of PSL mandates on work absences to be delayed as a result of these accrual periods. This pattern is confirmed in event study estimates presented below. Results for work absences that include the year of adoption are similar to our main results, though the magnitude of the effect is attenuated. These estimates are available upon request.

A potential challenge to the validity of our DD specification is the endogenous adoption of PSL mandates. For example, if a municipality's population demographics (e.g., share of service industry workers or underlying health of the population) are changing over time in unobserved ways, and this change leads to the adoption of a PSL mandate, then our estimates of the effect of PSL legislation on labor market and health outcomes would be biased. To gauge the potential for policy exogeneity we use data from the CPS-ASEC along with information on county-level, unemployment

rates, and proxies for population health to regress PSL adoption on several observable county-level characteristics that would plausibly be related to the adoption of a PSL mandate.<sup>9</sup> Specifically, we examine the association between the enactment of a PSL law and county-level estimates of population age, race/ethnicity, education, and income; per capita hospital inpatient days, per capita outpatient visits, and total Medicare spending for parts A and B;<sup>10</sup> county-level unemployment rates; and industry of employment. While not a definitive test of the exogeneity of the PSL mandates that we study, an inability to explain the variation in the adoption of PSL laws with a rich set of observable population characteristics lends support to our assertion that our identification strategy returns causal estimates of the effect of PSL mandates (Hoynes and Schanzenbach, 2009; Hoynes and Schanzenbach, 2012).

Results of this analysis are presented in Table 2. Column (1), which omits county fixed effects and county time trends, indicates that the share of the population with VA health insurance coverage and the share of the population working in agricultural industries is negatively associated with the enactment of a PSL mandate ( $p < 0.05$ ). The addition of county fixed effects in Column (2) attenuates the explanatory power of our industry indicators and leaves only gender, race, V.A. coverage, and Medicare spending as moderately significant predictors of PSL mandates. Finally, we add county time trends in Column (3) and find that only Medicare spending is associated with PSL mandate adoption. If the positive association between Medicare spending and PSL regulation indicates that counties with unobservably sicker populations were more likely to adopt a PSL mandate, then estimates of the effect of PSL adoption on health care utilization could be biased. However, we control for Medicare spending in our preferred specifications to minimize any

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<sup>9</sup> We choose to use the CPS-ASEC rather than the NHIS sample for this analysis due to the CPS-ASEC's much larger sample size.

<sup>10</sup> We would prefer to use county-level estimates of health care expenditures for the entire population; however, we are unaware of any such data that span the time frame of our analysis. Instead, we use Medicare parts A and B spending as a proxy for total health care expenditures.

potential bias that might arise. We also note that, despite the inclusion of county fixed effects and county time trends in Column (3), the R-squared indicates that much of the variation in PSL mandate adoption is unexplained by demographic and county-level observables and, therefore, suggests substantial random variation in PSL adoption to be leveraged in our estimation strategy.

In addition to policy exogeneity, another necessary assumption for the validity of our DD model is that the treatment and control groups would have followed the same trends (in terms of the outcome variables) had the adoption of a PSL mandate not occurred. This assumption is untestable, as it is impossible to observe the treatment group in the untreated state during the post-treatment period; however, evidence that these two groups followed similar trends in the outcome variables in the pre-PSL period lends credence to our estimation strategy. To assess whether trends in our outcomes were similar in the pre-enactment period for our treatment and control counties, we estimate a model similar to Equation (1) that replaces the *PSL* term with an interaction between an indicator for whether a county ever enacts a PSL mandate and a continuous measure of the year-quarter limiting the sample to the years before PSL mandates went into effect. A lack of statistical significance on this interaction term indicates that no measurable difference in pre-policy trends exists between the treatment and control counties. We report the results of this parallel trends test along with our estimates of Equation (1) below.

## **5. Results & Supplementary Analyses**

Before turning to our DD estimates from Equation (1), we present unadjusted visual evidence of the effect of PSL mandates on PSL access and work absences in Figures 1 and 2. Figure 1 plots the share of workers who report that they have access to PSL at their main job for those in both the treatment and control counties. Figure 1 is centered about the year-quarter of PSL mandate enactment for the treatment counties and a “pseudo-enactment” period for the control counties that

randomly assigns each control county to the year-quarter of mandate enactment for a county in the treatment group. The visual evidence in Figure 1 suggests that workers in counties that would go on to enact a PSL mandate had greater access to PSL in the periods before the mandates took effect. Pre-period trends in PSL access are relatively similar for the treatment and control counties, though the treatment counties show signs of increasing PSL access in the year immediately preceding the enactment of a mandate. Despite this potential evidence of policy anticipation in the unadjusted means, we find no evidence of diverging trends in the pre-period in our regression analyses or in event-study estimates of work absences discussed below. Figure 1 suggests that after the PSL mandates took effect, workers in the treatment counties were more likely to report having access to PSL and that PSL access continued to increase for the first 3 years before a slight downturn between years 3 and 4 post-enactment. Workers in the control counties show no similar increase in PSL access.

Figure 2 plots the share of workers with a work absence in the past 12 months due to an illness or injury. The share of workers reporting at least one work absence in both the treatment and control counties increased between 4 and 2 years prior to PSL mandate enactment, with a larger increase for those in treatment counties, before leveling off in the two years immediately preceding the PSL mandates. Workers in treatment counties show no significant change in work absences in the year following a PSL mandate, but the share of workers with at least one absence over the past 12 months rises steeply in treatment counties after the first year. This pattern is consistent with the requirement that workers must accrue PSL hours over time and motivates our decision to exclude the first 12 months post-enactment in our regression analyses of work absences.

Table 3 presents estimates of the effect of PSL mandate adoption on the probability of reporting access to PSL. Results in the first column in Table 3 are from a specification that omits controls for industry of employment, time-varying county characteristics, and county time trends. Panel A



contains the DD estimates of the effect of PSL mandates while Panel B includes the coefficient on the interaction term in our parallel trends test described earlier. Column (1) indicates that the enactment of a PSL mandate results in an increase in PSL access of 4.1 percentage points compared to counties with no mandate in effect. Based on a mean access rate of 58.5% in our sample, this represents a 7% increase in access to PSL. The specification in Column (2) adds separate controls for the 22 industry categories listed in Appendix Table 2. The inclusion of industry controls has little effect on the magnitude of the estimate. Column (3) adds our county level controls that include per capita measures of physician supply, health care utilization measures, Medicare FFS per capita spending, and the county unemployment rate. Our estimate remains largely unchanged after the addition of these county controls. Finally, Column (4) adds county time trends to further control for potential policy endogeneity or anticipation effects. Here our coefficient estimate is similar to Columns (1) through (3), but the inflated standard error in this specification results in a loss of statistical significance. In all four specifications, our parallel trends test finds no evidence that trends in PSL access were significantly different for treatment and control counties prior to the mandates taking effect.

Motivated by evidence that women are more responsive than men to changes in PSL policies, we explore the possibility of heterogeneous impacts of PSL mandates in Table 4 (Henrekson and Persson, 2004). Here we split the sample into 5 subgroups and examine the effect of PSL mandates separately for: women, men, white non-Hispanics, non-whites or Hispanics, and workers in industries with historically low access to PSL.<sup>11</sup> Regression specifications in Table 4 are analogous to Column (4) in Table 3 and include demographic, industry, and county controls, and county time trends. The results in Table 4 suggest that while the effect of PSL mandates may differ slightly

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<sup>11</sup> We define these low-access industries as those industries in which fewer than the overall mean share of workers report access to PSL prior to the enactment of a PSL mandate. See Appendix Table 2 for a complete listing of low-access industries.

across gender or race/ethnicity, the differences appear to be rather small. The coefficient estimate for PSL mandates is statistically significant at the 5% level for women and is not statistically significant for men, though the magnitude of the coefficient is relatively similar. White, non-Hispanic workers see a 4.9 percentage point increase in PSL access as a result of a mandate compared to a 3.6 percentage point increase for non-white, Hispanic workers. Lastly, Column (5) indicates that, as expected, those working in industries where access to PSL was uncommon prior to a mandate see a large, 8 percentage point gain in PSL access following the enactment of a mandate.

We next turn to the second phase of our analysis, which includes estimates of the effect of PSL mandates on work absences in the past 12 months due to illness or injury in Table 5. The odd numbered columns in Table 5 omit the county time trends, while county time trends are included in the even numbered specifications. We estimate separate regressions for any work loss days, 2 or more work loss days, 5 or more work loss days, and 10 or more work loss days. We generally find no effect of PSL mandates on work loss days in our DD models and note that several of the specifications in Table 5 fail to exhibit parallel trends between the treatment and control counties in the pre-enactment period.

Overall, the results in Tables 3 and 4 consistently indicate that workers in counties that enact PSL mandates report greater access to PSL compared to workers in counties with no mandated PSL requirement. However, since a large share of workers in our sample reported access to PSL in the absence of mandate, our results likely underestimate the gain in PSL access for those most affected by mandatory PSL legislation. This notion is supported by the much larger effect we estimate for workers in low-PSL industries. To shed additional light on the effect of a PSL mandate on PSL access and work absences for those most likely to benefit from the legislation we modify our empirical strategy and estimate a triple-difference (DDD) model that allows the effect of a PSL mandate to differ by the probability that a worker lacks access to PSL prior to the mandate taking

effect.<sup>12</sup> The first step in our DDD analysis is to estimate the probability that a worker in our sample lacks PSL coverage using data from time periods before actual adoption for our treatment counties or before the randomly assigned pseudo-enactment date for control counties. We use a logistic regression model that includes the same individual demographic characteristics found in Equation (1), as well as income, firm size, and codes for specific industry of employment.<sup>13</sup> After obtaining the predicted probability that a worker lacks PSL coverage, we estimate the following DDD specification:

$$(2) \quad Y_{i,c,t} = \alpha + \gamma_1 PSL_{c,t} + \gamma_2 Prob.(PSL)_{i,c,t} + \gamma_3 PSL_{c,t} \times Prob.(PSL)_{i,c,t} + \beta X_{i,c,t} + \theta Z_{c,t} + \delta_c + \tau_t + \delta_c \times \tau_t + \epsilon_{i,c,t}$$

Equation (2) is similar to Equation (1) but allows the effect of PSL mandate enactment to vary by the probability of lacking PSL coverage prior to mandate enactment. Specifically, the coefficient of interest,  $\gamma_3$ , measures the impact of a PSL mandate on an individual who is predicted to gain PSL coverage. The coefficient  $\gamma_1$  represents the effect of mandate enactment on those with PSL coverage prior to the mandate (i.e., predicted probability of lacking PSL access equals zero). The coefficient  $\gamma_2$  is then the difference in outcomes for those with no PSL benefits compared to those with PSL benefits when no mandate is in place. Notably, the coefficients from our DD and DDD models are not directly comparable. The DD coefficient of interest in Equation (1) measures the effect of a PSL

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<sup>12</sup> We choose to conduct a DDD analysis rather than simply examining work absences for those in low-PSL industries for two reasons: first, there may be characteristics besides industry associated with the likelihood of PSL coverage that would be captured by our predication model; and second, limiting our sample to low-PSL industries reduces our sample size by more than half. The DDD model allows us to retain our full sample while focusing specifically on those most likely to be affected by PSL legislation.

<sup>13</sup> We exclude income and firm size from our main analyses because it may be related to the enactment of a PSL mandate. We include both in our probability model since we estimate this model for the time before any PSL mandate. Estimates for this prediction model are reported in Appendix Table 3.

mandate on workers in treatment counties compared to workers in control counties (in other words, this is an intent-to-treat estimate). The DDD coefficient of interest in Equation (2) estimates the effect of a PSL mandate on workers in treatment counties who are more likely to gain PSL compared to those in treatment counties who are less likely to gain PSL (i.e., a treatment-on-the-treated effect).

Table 6 displays results from our DDD model on the effect of PSL mandates on PSL access for those most likely to be affected by a mandate. Column (1) contains results from a specification that includes demographic and county controls, but omits industry controls and county time trends. Column (2) repeats the analysis with the addition of county controls, while Column (3) adds county time trends. All three columns suggest that PSL mandates result in large and statistically significant increases in PSL access for those most likely to gain coverage. In each case, a PSL mandate increases access by more than 7 percentage points. The coefficient on the *Prob.(PSL)* term clearly indicates that our predication model is successful at identifying individuals who, in the absence of a mandate, would be especially likely to lack PSL coverage, while the statistically insignificant coefficient estimates on *PSL Mandate* are expected given that this represents the effect of a mandate on those with PSL coverage already in place. Estimates in panel B provide evidence of parallel pre-period trends in our DDD specifications.<sup>14</sup>

After confirming that PSL mandates increase access to paid leave and that the effect is larger for those most likely to gain coverage, we again turn to the effect of PSL mandates on work absences by re-estimating our DDD model in Equation (2) using work loss days in the past 12 months for illness or injury as the dependent variable. Results are included in Table 7. Columns (1) and (2) contain estimates for the effect of PSL mandates on reporting any work loss days in the past 12 months due

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<sup>14</sup> The parallel trends test for the DDD model is similar to that described for the DD model except that we now interact the indicator for whether a county ever adopts a PSL mandate with both the continuous year measure and with the *Prob.(PSL)* variable from Equation (3). We also include all two-way interactions between these variables.

to illness or injury. When county time trends are omitted in Column (1), we find that those who are most likely to lack PSL coverage are less likely to report any work loss days in the absence of a PSL mandate. This finding is consistent with earlier research indicating those with no access to PSL are less likely to miss work when ill (DeRigne et al., 2016; Susser and Ziebarth, 2016). Estimates of the interaction term in Columns (1) and (2) show no statistically significant increase in the probability of experiencing a work loss day after the enactment of a PSL mandate for those most likely to be affected, though the coefficient is positive. Interestingly, the coefficient on the indicator for whether a county has enacted a PSL mandate is negative and statistically significant. This estimate represents the impact of a PSL mandate on those who already have PSL coverage in place. While we expected this coefficient to be zero in regressions of PSL mandates on PSL access, in this case that expectation would no longer hold. If access to PSL reduces instances of presenteeism and the spread of communicable disease, then those with existing PSL access could plausibly benefit from a mandate. We return to this point below, but note that others have found evidence that PSL mandates reduce presenteeism through disease transmission rates (Pichler and Ziebarth, 2017a). Our estimate in Column (1) suggests that the enactment of a PSL mandate reduces the likelihood of a work absence due to illness or injury by 4.2 percentage points, a reduction of approximately 10%. When we add county time trends to our specification in Column (2), this effect is no longer statistically significant, but the sign of the coefficient is still negative and similar in magnitude.

Moving to Columns (3) and (4), it appears that PSL mandates increase the probability that those most likely to gain PSL access report at least 2 work loss days due to illness or injury by approximately 4.6 percentage points. This effect is quite large in relative terms, representing a 13.4% increase off of the sample mean, and is largely unaffected by the inclusion of county time trends in the regression model. Columns (5) and (6) suggest that the positive relationship between PSL mandates and work loss days for the targeted population continues when moving to 5 or more work

loss days. Here the effect of the mandate is still large in magnitude, though only borderline statistically significant. Finally, Columns (7) and (8) contain estimates for 10 or more work loss days. Coefficient estimates are now quite small and statistically insignificant, indicating no effect of PSL mandates on 10 or more work loss days.

While our findings indicate that PSL mandates increase work absences for those gaining access to PSL coverage, the relatively small sample sizes in the NHIS mean that our estimates are somewhat imprecise. To address this concern, we supplement our estimates from the NHIS with data from the CPS-ASEC, which affords us a much larger sample for our analysis. While the CPS-ASEC significantly increases our sample size, the survey contains only very limited information on access to PSL.<sup>15</sup> Therefore, we use the NHIS to calculate the mean rate of PSL access in the absence of a mandate for 30 separate groups defined by age (18-24, 25-34, 35-44, 45-54, and 55-64), education (high school or less, some college, and college graduate), and gender. We then estimate the following version of Equation (3) using the CPS-ASEC sample and the PSL access rates for each of the 30 groups:

$$(3) \quad Y_{i,c,t} = \tilde{\alpha} + \tilde{\gamma}_1 PSL_{c,t} + \tilde{\gamma}_2 PSL_{c,t} \times Prob.(PSL)_g + \tilde{\beta} X_{i,c,t} + \tilde{\theta} Z_{c,t} + \tilde{\delta}_c + \tilde{\tau}_t + \lambda_g + \tilde{\delta}_c \times \tilde{\tau}_t + \tilde{\varepsilon}_{i,c,t}$$

Where  $Prob.(PSL)_g$  is the mean rate of PSL access for each of the 30 groups,  $\lambda_g$  is an indicator for each of the 30 groups, and all of the remaining variables are as previously defined. In this specification,  $\tilde{\gamma}_2$ , measures the effect of a PSL mandate on those gaining PSL coverage. To calculate the dependent variable in Equation (3) we follow the BLS definition of a work absence, which

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<sup>15</sup> The CPS-ASEC asks whether a worker who was absent from work in the past week was paid for their absence. It is not possible to discern whether a worker who was not absent from work in the past week had access to PSL.

includes “instances when persons who usually work 35 or more hours per week worked less than 35 hours during the reference week”. Additionally, the CPS-ASEC asks the reason for this reduction in work hours and we use this information to construct a measure of work absences due to “illness or health/medical limitation”.

Results for reporting any illness-related work absence using the CPS-ASEC sample are presented in Table 8. The first column includes coefficient estimates from a DD model similar to Equation (1), while Columns 2 through 5 contain estimates from the DDD model described in Equation (3). DD estimates in Column (1) indicate a small negative effect of PSL mandates on work absences for workers in treatment counties compared to those in control counties. The coefficient is marginally statistically significant, though small in magnitude, and suggests that the overall effect of mandate enactment on work absences is negligible. The DDD estimates in Table 8, however, show strong effects of mandates for both those gaining access and those with existing PSL coverage in the absence of a mandate. Our estimates suggest that PSL mandates increase the likelihood of an illness-related work absence in the past week by 2.5 percentage points for those gaining coverage, a relative increase of nearly 140%, compared to those with prior PSL coverage. This finding is similar to our earlier estimates of the effect of PSL mandates on work absences using the NHIS data reported in Columns (1) and (2) of Table 7. In addition, estimates of the coefficient on the PSL mandate term in Table 8 indicate that the enactment of a mandate affects the probability of a work absence for those with existing PSL coverage. Following a PSL mandate, those unaffected workers see a decline in the likelihood of reporting an illness-related work absence of nearly 2 percentage points. Though identifying the mechanism for this spillover effect is beyond the scope of this paper, we note that these results are consistent with evidence that PSL mandates reduce presenteeism and the transmission of contagious illnesses (Pichler and Ziebarth, 2017a).

Finally, Figure 3 plots event study estimates of the effect of a PSL mandate on the likelihood of an illness-related work absence in the past week using the CPS-ASEC data. We estimate an event study model similar to Equation (3), but include individual interactions with years immediately preceding and following a county's adoption of a PSL mandate. We designate period 0 as the year in which the mandate took effect and omit the preceding year so that coefficient estimates can be interpreted as the change in work absences compared to the year before the enactment of a PSL mandate. The coefficients are then plotted for the three years following the mandate's enactment and three years preceding enactment. The coefficient estimates in Figure 3 represent the effect of PSL mandates on illness-related work absences for those most likely to gain PSL coverage compared to those with PSL coverage in place prior to a mandate. Figure 3 shows no evidence of a change in illness-related work absences for those gaining coverage in the periods before a mandate took effect. However, we see a slight increase in the likelihood of an illness-related work absence occurring in the year that the mandate took effect (period 0) and a large increase in the first full year following the mandates enactment (period 1). Compared to those unaffected by the mandate, those gaining coverage increase their probability of reporting an illness-related work absence by approximately 5 percentage points in period 1. The increase in work absences for the affected group falls in the second year after the mandate becomes effective and then rises again in the third year. We highlight two additional features of the event-study model that relate to our previous analyses. First, a lack of a pre-period effect on work absences indicates that firms are not increasing their PSL coverage in anticipation of mandate enactment and, secondly, we appear to be justified in omitting the year of enactment from our earlier models. Since PSL accrues over time, many workers will not have access to paid leave days for several months following the enactment of a mandate.



## 6. Discussion

Legislative mandates for paid sick leave (PSL) benefits are a relatively recent phenomenon in the U.S. Beginning with San Francisco in 2007, several cities, counties and states now require employers to provide their workers with paid leave for illness or injury (National Partnership for Women & Families 2016). Despite strong public support for expanding PSL coverage, legislators in 19 states have banned municipal-level PSL mandates citing concerns over costs to businesses and rising rates of employee absenteeism (Pichler and Ziebarth, 2017b). Evidence to support these concerns, however, is minimal as the relationship between PSL mandates in the U.S. and labor market outcomes remains unclear.

Our goal in this paper was to first establish a link between the enactment of a PSL mandate and access to PSL coverage. Since many employers provide PSL benefits in the absence of a mandate, the effect on access to coverage is not evident ex-ante and represents a crucial first-step to quantifying the labor market effects of PSL mandates. Furthermore, if firms respond to costly mandates by altering their workforce or relocating, then any change in access to PSL benefits would be minimal. Our results indicate that, overall, PSL mandates increase access to paid leave by 4.1 percentage points compared to counties where no mandate exists. This represents a relative increase in PSL coverage of approximately 7 percent and is concentrated among women, white non-Hispanics, and those working in industries with low levels of PSL coverage prior to the mandate. The finding that mandates are associated with small gains in PSL coverage can explain why studies of the supply-side labor market effects of PSL mandates in the U.S. have found minimal impacts on employment and wages (Ahn and Ylowitz, 2015; Pichler and Ziebarth, 2017b). However, when we shift our focus to those most affected by a mandate, we find that the effect of a PSL mandate on access to coverage nearly doubles to 7.4 percentage points; a relative increase of 13 percent. This

much more substantial effect highlights the importance of concentrating on the population targeted by the mandates.

We then turn to the relationship between PSL access and the likelihood that a worker reports an illness-related work absence. Overall, our difference-in-differences models suggest that PSL mandates are associated with a small decline in illness-related work absences, though some specifications fail to satisfy the assumption of parallel trends in the pre-period. In an effort to refine our analysis, we estimate a triple-difference model that allows us to observe the effects of PSL mandates on those gaining coverage *and* on those with a high likelihood of PSL coverage prior to mandate enactment. Our application of this strategy is novel to the literature on PSL and provides a unique perspective on potential spillovers to populations not directly affected by a PSL mandate. Using two distinct datasets, we find strikingly similar results: workers gaining access to PSL benefits following the enactment of a mandate are approximately 2.5 percentage points more likely to report a recent illness-related work absence. Estimates from an event-study model suggest that much of the effect on absences occurs in the calendar year following the enactment of a mandate, which is consistent with an initial accrual period; a feature that is common to all of the mandates that we study. When examining intensive margin changes in absenteeism, we find particularly large effects at the thresholds of reporting at least 2 work absences and at least 5 work absences. This effect declines monotonically from at least 2 work absences and becomes small and statistically insignificant at 10 or more work absences.

As expected, we find no access effect for those with a high probability of PSL coverage prior to a mandate. However, our results indicate that a PSL mandate is associated with fewer illness-related work absences for those with prior coverage. The magnitude of the estimated effect is similar in size to the increase in work absences for those gaining PSL access. Though we are not able to directly test the mechanism that drives these reductions in illness-related work absences, we note that this

finding is consistent with recent work on PSL mandates and presenteeism (Pichler and Ziebarth, 2017a). For example, Susser and Ziebarth (2016) found presenteeism to be especially prevalent among low-income females; a group of workers we show experiencing larger gains in access to PSL following the enactment of mandate. Taken together, our results indicate that while there is some justification for concerns expressed over PSL mandates and increased work absences, there is also evidence to support a key claim made by proponents of PSL mandates, that increased access to PSL can improve population health.

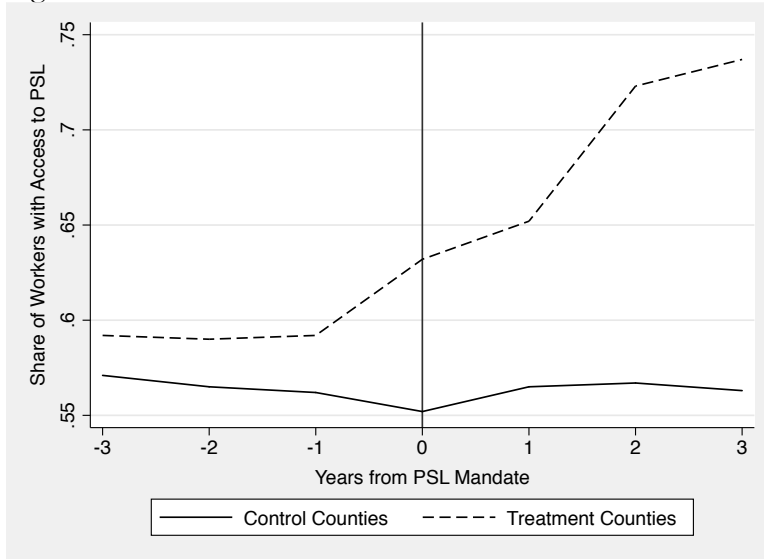
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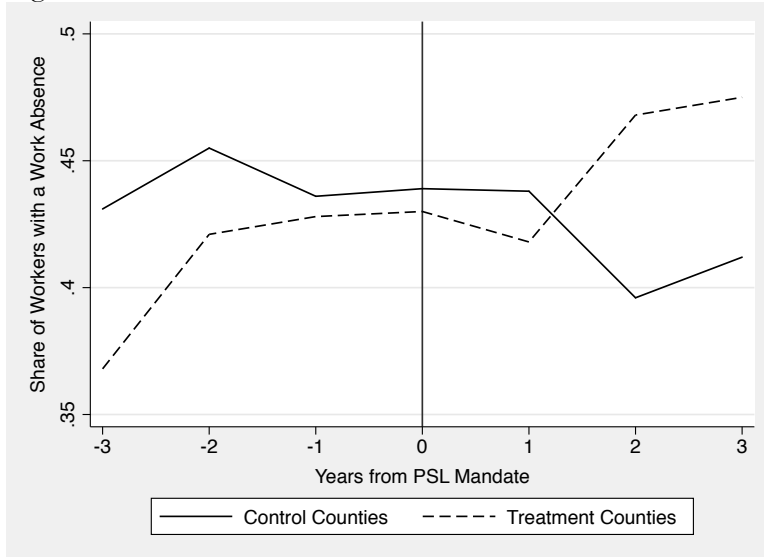
Figure 1: Paid Sick Leave Access Relative to Mandate Enactment



Notes:

1. Year 0 represents the quarter and year that the PSL mandate took effect in treatment counties. Control counties are assigned a “pseudo-enactment” period matched to the quarter and year of enactment for a random treatment county.
2. Access to PSL is calculated as the unadjusted mean rate of workers in the NHIS sample who report access to paid sick leave.

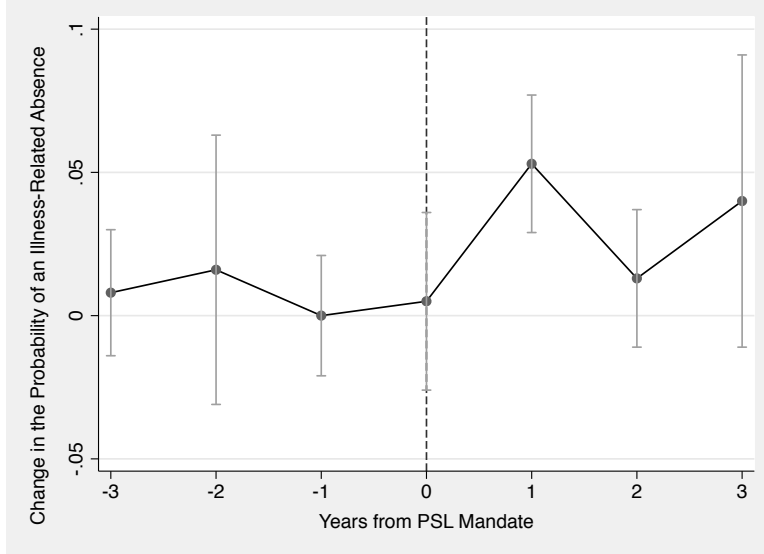
Figure 2: Work Absence over the Past 12 Months due to Illness or Injury



Notes:

1. Year 0 represents the quarter and year that the PSL mandate took effect in treatment counties. Control counties are assigned a “pseudo-enactment” period matched to the quarter and year of enactment for a random treatment county.
2. A work absence is defined as a work loss day to an injury or illness and is calculated as the unadjusted mean rate of workers in the sample who report a work loss day in the past 12 months.

Figure 3: Triple Difference Event Study Estimates of the Effect of Paid Sick Leave Mandates on Any Work Absence due to Illness or Health/Medical Limitation – CPS Sample



Notes:

1. Year 0 represents the year that the PSL mandate took effect in treatment counties.
2. Absences are defined as instances when persons who usually work 35 or more hours per week worked less than 35 hours during the reference week because of an illness or health/medical limitation.



Table 1: Descriptive Statistics for NHIS Sample

	Treatment Counties	Control Counties
Female	0.510	0.495
Age	38.4	38.6
White, non-Hispanic	0.410	0.449
Black, non-Hispanic	0.205	0.212
Hispanic	0.245	0.279
Other race, non-Hispanic	0.139	0.059
Federal Poverty Level < 1.0	0.108	0.103
Federal Poverty Level 1.0 – 1.99	0.140	0.167
Federal Poverty Level 2.0 – 2.99	0.123	0.155
Federal Poverty Level 3.0 – 3.99	0.097	0.118
Federal Poverty Level 4.0+	0.365	0.297
Federal Poverty Level Missing	0.168	0.162
Less than High School	0.125	0.135
High School Graduate or GED	0.194	0.234
Some College or Associate’s Degree	0.233	0.321
Bachelor’s Degree	0.285	0.213
Graduate or Professional Degree	0.154	0.092
Education Missing	0.009	0.004
Married	0.343	0.400
Widowed	0.014	0.016
Divorced	0.092	0.133
Separated	0.038	0.038
Never Married	0.436	0.332
Living with a Partner	0.071	0.076
Marital Status Missing	0.005	0.003
Private Health Insurance	0.693	0.690
Medicaid Health Insurance	0.091	0.034
Military Health Insurance	0.004	0.015
Other Health Insurance	0.063	0.070
Health Insurance Missing	0.004	0.004
No Health Insurance	0.170	0.229
Observations	7,390	26,951

Notes:

1. Data are from the 2005 through 2015 waves of the NHIS.
2. Treatment counties include those enacting a PSL mandate between 2007 and 2014, while control counties are those with no PSL mandate in place over this time period.

Table 2: Determinants of Paid Sick Leave Adoption

	(1)	(2)	(3)
Female	0.017 (0.024)	0.020* (0.012)	0.001 (0.008)
Age 18-34	Omitted	Omitted	Omitted
Age 35-44	-0.051* (0.028)	0.007 (0.011)	0.012 (0.012)
Age 45-54	-0.038 (0.039)	0.003 (0.014)	0.015 (0.016)
Age 55-64	-0.060 (0.047)	-0.036 (0.025)	-0.006 (0.015)
White	Omitted	Omitted	Omitted
Black	-0.006 (0.029)	-0.023 (0.035)	-0.007 (0.013)
Hispanic	-0.006 (0.016)	-0.025 (0.017)	-0.006 (0.012)
Asian	0.222 (0.154)	0.082* (0.048)	0.046 (0.040)
Other Race/Ethnicity	-0.059 (0.049)	0.005 (0.022)	-0.006 (0.022)
Less than High School	Omitted	Omitted	Omitted
High School	-0.062 (0.048)	-0.008 (0.017)	-0.013 (0.019)
Some College	-0.095* (0.056)	-0.013 (0.016)	-0.012 (0.014)
College or Greater	-0.012 (0.049)	0.006 (0.023)	-0.024 (0.019)
Family Income < \$25k	Omitted	Omitted	Omitted
Family Income \$25k-\$49,999	0.030 (0.024)	0.011 (0.011)	0.006 (0.011)
Family Income \$50k-\$74,999	0.033 (0.025)	0.018 (0.012)	0.020 (0.015)
Family Income > \$75k	0.020 (0.028)	0.022 (0.020)	0.010 (0.011)
Privately Insured	Omitted	Omitted	Omitted
Medicare Coverage	-0.146 (0.091)	-0.047 (0.055)	0.019 (0.051)
Medicaid Coverage	0.038 (0.067)	-0.005 (0.024)	-0.005 (0.019)
V.A. Coverage	-0.073** (0.029)	-0.037* (0.021)	0.008 (0.015)
Uninsured	-0.010 (0.017)	-0.009 (0.008)	0.004 (0.005)
Medicare Spending (thousand \$)	-0.001 (0.003)	0.021* (0.011)	0.025** (0.012)
Per Capita Hospital Days	0.008 (0.008)	-0.064 (0.049)	-0.014 (0.016)
Per Capita Outpatient Visits	0.002	-0.002	0.000

	(0.001)	(0.002)	(0.001)
Unemployment Rate	0.003*	-0.001	-0.001
	(0.002)	(0.001)	(0.001)
Agriculture Industry	Omitted	Omitted	Omitted
Mining Industry	0.203**	-0.044	-0.005
	(0.088)	(0.039)	(0.024)
Construction Industry	0.113**	0.023	0.001
	(0.051)	(0.023)	(0.015)
Manufacturing Industry	0.123**	0.002	-0.011
	(0.057)	(0.018)	(0.018)
Transportation & Utilities	0.102*	-0.023	-0.021
	(0.061)	(0.023)	(0.019)
Wholesale Trade Industries	0.080	0.001	-0.019
	(0.050)	(0.028)	(0.020)
Retail Trade Industries	0.136**	0.004	-0.002
	(0.066)	(0.017)	(0.014)
Finance, Insurance, and Real Estate	0.155**	-0.006	0.009
	(0.066)	(0.027)	(0.021)
Business and Repair Services	0.187*	0.038	-0.014
	(0.098)	(0.026)	(0.021)
Personal Services	0.152*	-0.009	0.004
	(0.078)	(0.038)	(0.021)
Entertainment and Recreation Services	0.080	-0.015	0.016
	(0.066)	(0.029)	(0.019)
Professional and Related Services	0.166**	-0.013	0.005
	(0.079)	(0.023)	(0.012)
County Fixed Effects	No	Yes	Yes
County Time Trend	No	No	Yes
R <sup>2</sup>	0.077	0.326	0.596
Observations	3,489	3,489	3,489

Notes:

1. Observations are at the county-year level for years 2005 through 2015 using data from the CPS-ASEC.
2. All regressions include year fixed effects and are weighted by county population.
3. Standard errors (in parentheses) are clustered at the county level.
4. \*  $p < 0.10$ ; \*\*  $p < 0.05$ ; \*\*\*  $p < 0.01$

Table 3: Difference-in-difference Estimates of the Effect of Paid Sick Leave Mandates on Access to Paid Sick Leave

	(1)	(2)	(3)	(4)
Panel A:				
PSL Mandate	0.041** (0.021)	0.038* (0.020)	0.041* (0.022)	0.046 (0.030)
Panel B:				
Parallel Trends Test	0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	0.001 (0.001)
Industry Controls	No	Yes	Yes	Yes
County Controls	No	No	Yes	Yes
County-Time Trend	No	No	No	Yes
Mean PSL Access	0.585	0.585	0.585	0.585
Observations	33,679	33,679	33,679	33,679

Notes:

1. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, and county and year fixed effects.
2. Industry controls include indicators for 22 separate industry categories (see Appendix Table 2 for details).
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Standard errors are in parentheses and are clustered at the county level.
5. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 4: Difference-in-difference Estimates of the Effect of Paid Sick Leave Mandates on Access to Paid Sick Leave by Subgroup

Subgroup	(1)	(2)	(3)	(4)	(5)
	Women	Men	White, non-Hispanic	Non-White or Hispanic	Low PSL Industries
Panel A: PSL Mandate	0.042** (0.019)	0.038 (0.029)	0.049* (0.029)	0.036* (0.022)	0.080** (0.040)
Panel B: Parallel Trends Test	0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	-0.001 (0.001)	-0.003 (0.002)
Industry Controls	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes
County-Time Trend	Yes	Yes	Yes	Yes	Yes
Mean PSL Access	0.597	0.573	0.656	0.528	0.406
Observations	16,817	16,862	14,901	18,778	14,270

Notes:

1. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, and county and year fixed effects.
2. Industry controls include indicators for 22 separate industry categories (see Appendix Table 2 for details).
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Low PSL industries are defined as those industries where fewer than the overall mean share of workers report access to paid sick leave prior to the enactment of a PSL mandate.
5. Standard errors are in parentheses and are clustered at the county level.
6. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 5: Difference-in-difference Estimates of the Effect of Paid Sick Leave Mandates on Work Absences Due to Illness or Injury

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any Work Loss Days		2 or More Work Loss Days		5 or More Work Loss Days		10 or More Work Loss Days	
Panel A:								
PSL Mandate	-0.033** (0.016)	-0.022 (0.026)	-0.004 (0.015)	-0.001 (0.022)	0.011 (0.012)	0.002 (0.018)	-0.003 (0.009)	-0.001 (0.011)
Panel B:								
Parallel Trends Test	0.002** (0.001)	0.005** (0.002)	0.002** (0.001)	0.002 (0.002)	0.001 (0.001)	-0.004*** (0.001)	0.000 (0.000)	-0.001 (0.001)
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Time Trend	No	Yes	No	Yes	No	Yes	No	Yes
Mean PSL Access	0.433	0.433	0.344	0.344	0.136	0.136	0.060	0.060
Observations	30,831	30,831	30,831	30,831	30,831	30,831	30,831	30,831

Notes:

1. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, and county and year fixed effects.
2. Industry controls include indicators for 22 separate industry categories (see Appendix Table 2 for details).
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Standard errors are in parentheses and are clustered at the county level.
5. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 6: Triple Difference Estimates of the Effect of Paid Sick Leave Mandates on Access to Paid Sick Leave

	(1)	(2)	(3)
Panel A:			
PSL Mandate x p(PSL)	0.072*** (0.018)	0.073** (0.018)	0.074*** (0.018)
PSL Mandate	0.012 (0.020)	0.012 (0.020)	0.017 (0.027)
p(PSL)	-1.056*** (0.035)	-1.118*** (0.045)	-1.113*** (0.045)
Panel B:			
Parallel Trends Test	-0.000 (0.004)	-0.000 (0.004)	-0.000 (0.004)
Industry Controls	No	Yes	Yes
County Controls	Yes	Yes	Yes
County-Time Trend	No	No	Yes
Mean PSL Access	0.585	0.585	0.585
Observations	33,679	33,679	33,679

Notes:

1. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, and county and year fixed effects.
2. Industry controls include indicators for 22 separate industry categories (see Appendix Table 2 for details).
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Standard errors are in parentheses and are clustered at the county level.
5. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 7: Triple Difference Estimates of the Effect of Paid Sick Leave Mandates on Work Absences Due to Illness or Injury

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Any Work Loss Days		2 or More Work Loss Days		5 or More Work Loss Days		10 or More Work Loss Days	
Panel A:								
PSL Mandate x	0.025	0.025	0.047**	0.046**	0.029*	0.028*	0.007	0.007
p(PSL)	(0.029)	(0.029)	(0.022)	(0.023)	(0.016)	(0.017)	(0.017)	(0.017)
PSL Mandate	-0.042**	-0.032	-0.022	-0.019	0.000	-0.010	-0.005	-0.004
p(PSL)	(0.017)	(0.027)	(0.014)	(0.022)	(0.013)	(0.019)	(0.012)	(0.015)
	-0.285***	-0.287***	-0.206***	-0.205***	-0.040	-0.040	0.020	0.020
	(0.067)	(0.067)	(0.063)	(0.063)	(0.041)	(0.041)	(0.025)	(0.025)
Panel B:								
Parallel Trends Test	0.002	0.002	0.002	0.001	-0.001	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Industry Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Time Trend	No	Yes	No	Yes	No	Yes	No	Yes
Mean Worker Share	0.433	0.433	0.344	0.344	0.136	0.136	0.060	0.060
Observations	30,831	30,831	30,831	30,831	30,831	30,831	30,831	30,831

Notes:

1. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, and county and year fixed effects.
2. Industry controls include indicators for 22 separate industry categories (see Appendix Table 2 for details).
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Standard errors are in parentheses and are clustered at the county level.
5. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$



Table 8: Estimates of the Effect of Paid Sick Leave Mandates on Any Work Absence due to Illness or Health/Medical Limitation – CPS Sample

	(1)	(2)	(3)	(4)	(5)
	DD	DDD	DDD	DDD	DDD
Panel A:					
PSL Mandate x p(PSL)	-	0.025*** (0.005)	0.025*** (0.005)	0.025*** (0.005)	0.025*** (0.004)
PSL Mandate	-0.003* (0.002)	-0.021*** (0.004)	-0.021*** (0.004)	-0.021*** (0.003)	-0.019*** (0.004)
Industry Controls	Yes	No	Yes	Yes	Yes
County Controls	Yes	No	No	Yes	Yes
County-Time Trend	Yes	No	No	No	Yes
Mean Worker Share	0.018	0.018	0.018	0.018	0.018
Observations	298,809	298,809	298,809	298,809	298,809

Notes:

1. Data are from the CPS-ASEC between the years of 2005 and 2015 and define a work absence due to illness or health/medical limitation as instances when persons who usually work 35 or more hours per week worked less than 35 hours during the reference week because of an illness or health/medical limitation.
2. All regressions include controls for gender, race, marital status, education, age, health insurance coverage, U.S. born, county and year fixed effects. DDD models include group fixed effects.
3. County controls include number of medical doctors, family doctors, and general practitioners per capita, per capita inpatient days and outpatient visits, and Medicare Parts A and B expenditures, and the county unemployment rate.
4. Standard errors are in parentheses and are clustered at the county level.
5. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Appendix Table 1: Municipal and State Paid Sick Leave Mandates, 2005 – 2014

Municipality or State	Effective Date	Scope of Coverage	Accrual Period
San Francisco, CA	2/5/2007	All workers	1 hour for every 30 hours worked
Washington DC	11/13/2008	All workers except independent contractors, students, certain health care workers, certain unpaid volunteers, and casual babysitters.	<ul style="list-style-type: none"> <li>Firms with 24 or fewer workers: 1 hour for every 87 hours worked</li> <li>Firms with 25-99 workers: 1 hour for every 43 hours worked</li> <li>Firms with 100 or more workers: 1 hour for every 37 hours worked</li> </ul>
Connecticut	1/1/2012	Hourly workers in the service sector working for firms with 50 or more employees.	1 hour for every 40 hours worked
Seattle, WA	9/1/2012	Workers in firms with more than 4 employees completing more than 240 annual hours of work.	<ul style="list-style-type: none"> <li>Firms with more than 4, but fewer than 250 workers: 1 hour for every 40 hours worked</li> <li>Firms with more than 250 workers: 1 hour for every 30 hours worked</li> </ul>
New York, NY	6/26/2013	Workers in firms with more than 5 employees completing more than 80 annual hours of work with certain exemptions.	1 hour for every 30 hours worked
Portland, OR	1/1/2014	Workers in firms with more than 5 employees completing 240 or more annual hours of work.	1 hour for every 30 hours worked up to a maximum of 40 accrued hours
Jersey City, NJ	1/22/2014	Private sector workers completing 80 or more annual hours of work and working for a minimum of 90 days.	<ul style="list-style-type: none"> <li>Firms with fewer than 10 workers: 1 hour for every 30 hours worked up to a maximum of 24 hours</li> <li>Firms with 10 or more workers: 1 hour for every 30 hours worked up to a maximum of 40 hours</li> </ul>
Newark, NJ	1/29/2014	Same as Jersey City	Same as Jersey City

Notes:

1. We rely on information provided by the Work and Family Legal Center (2016), the National Partnership for Women and Families (2016), and Pichler and Ziebarth (2017b) for the information in this table.
2. Philadelphia, PA; Oakland, CA; and the states of California and Massachusetts all passed a PSL mandates in 2015, however because we exclude the first year after the enactment of a PSL mandate for our analysis of work absences, we do not include these mandates in our sample.

Appendix Table 2: NHIS Industry Classifications

*Agriculture, Forestry, Fishing, and Hunting Industries
Mining Industries
Utilities Industries
*Construction Industries
Manufacturing Industries
Wholesale Trade Industries
Retail Trade Industries
Transportation and Warehousing Industries
Information Industries
Finance and Insurance Industries
Real Estate and Rental and Leasing Industries
Professional, Scientific, and Technical Services Industries
Management of Companies and Enterprises Industries
*Administrative and Support and Waste Management and Remediation
Education Services Industries
Health Care and Social Assistance Industries
*Arts, Entertainment, and Recreation Industries
*Accommodation and Food Services Industries
*Other Services (except Public Administration Industries)
Public Administration Industries
Armed Forces
Industry Unknown

Notes:

1. \* indicates that the industry is included in the “Low PSL” sample in Table 4. These are industries in which fewer than the overall mean share of workers report access to PSL prior to the enactment of a mandate.

Appendix Table 3: Characteristics Associated with Lacking Access to Paid Sick Leave

	Coefficient	Standard Error
<i>Demographics</i>		
Intercept	-0.876**	0.4322
<50% FPL	0.8608***	0.1089
50-74% FPL	0.7387***	0.1192
75 to 99% FPL	0.463***	0.0996
100-124% FPL	0.1787*	0.0913
125-149% FPL	0.2585***	0.0909
150-174% FPL	0.2292**	0.0901
175-199% FPL	-0.0593	0.0886
200-249% FPL	-0.0973	0.0710
250-299% FPL	-0.1964***	0.0724
300-349% FPL	-0.2062***	0.0774
350-399% FPL	-0.4464***	0.0842
400-449% FPL	-0.3727***	0.0867
450-499% FPL	-0.2769***	0.0926
>=500% FPL	-0.5121***	0.0597
Missing FPL	Omitted	Omitted
Married	0.5161*	0.2921
Widowed	0.3952	0.3179
Divorced	0.3704	0.2945
Separated	0.2363	0.3020
Never Married	0.3682	0.2928
Living with a Partner	0.5403*	0.2977
Marital Status Missing	Omitted	Omitted
White, non-Hispanic	0.2402***	0.0759
Black, non-Hispanic	-0.0643	0.0747
Hispanic	-0.1072	0.0806
Other race, non-Hispanic	Omitted	Omitted
Less Than High School	0.5062***	0.0842
High School Graduate or GED	0.2784***	0.0744
Some College or Associates Degree	0.2985***	0.0710
Bachelor's Degree	-0.0063	0.0717
Graduate or Professional Degree	Omitted	Omitted
Female	-0.0069	0.0363
US Born	-0.1306***	0.0481
<i>Health Insurance</i>		
Private Health Insurance	-1.0946***	0.1874
Medicaid Health Insurance	0.4119**	0.1969
Military Health Care	0.1913	0.1984
State-sponsored Health Plan	0.4753**	0.2150
Other Government Plan	0.3613*	0.2115
Single Service Plan	-0.1512	0.1008
No Coverage of Any Type	0.6898***	0.1912
Health Insurance Missing	-0.4833	0.3272
<i>Industry</i>		

Agriculture, Forestry, Fishing, and Hunting	1.2259**	0.5414
Mining	-0.2310	0.2712
Utilities	-0.6408**	0.2674
Construction	1.1155***	0.1141
Manufacturing	0.0117	0.1072
Wholesale Trade	-0.1627	0.1328
Retail Trade	0.2408**	0.1036
Transportation and Warehousing	0.1026	0.1159
Information	-0.3657**	0.1425
Finance and Insurance	-0.6672***	0.1202
Real Estate and Rental and Leasing	0.3576***	0.1354
Professional, Scientific, and Technical Services	-0.1238	0.1121
Management of Companies and Enterprises	-0.6577	0.5889
Administrative and Support and Waste Management	0.7943***	0.1147
Education Services	0.0993	0.1295
Health Care and Social Assistance	-0.2885***	0.1035
Arts, Entertainment, and Recreation	0.7396***	0.1361
Accommodation and Food Services	0.9632***	0.1087
Other Services (except Public Administration)	0.6411***	0.1158
Public Administration	-1.2393***	0.4544
Armed Forces	-8.7753	118.0000
Unknown	Omitted	Omitted
Employer Size	0.0005	0.0008
<i>County controls</i>		
Per Capita Physicians	-31.6312**	15.2807
Per Capita General Practitioners	-955.9000	947.3000
Per Capita Family Practitioners	2101.8**	1041.8000
Per Capita Hospital Days	0.0302	0.0638
Per Capita Outpatient Days	0.0403*	0.0206
Medicare Spending	0.0000	0.0000
Unemployment Rate	0.0209***	0.0081
Observations	21,705	

Notes:

1. Estimates are from a logistic regression model with lacking access to paid sick leave as the dependent variable.
2. Regression includes individual age dummies which we omit from the table to conserve space.
3. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$