Can Paid Sick Leave Mandates Reduce Leave-Taking?*

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Abstract

Since 2006, several cities and states have implemented paid sick leave mandates. We examine the effects of paid sick leave mandates in Washington, D.C. (2008) and Connecticut (2011) on leave-taking behavior. After these policies are implemented, there are significant decreases in the aggregate rate of illness-related leave taking, relative to control groups, for both those directly affected and those not directly affected by the policy. We find similar effects when exploiting exogenous variation in access to paid sick leave through changes in local industry composition over time. Our results suggest that mandated sick leave policies can provide large positive public health externalities by allowing sick workers to stay home rather than coming to work and spreading their illness to customers and coworkers.

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1 Introduction

American workers spend fewer days absent from work due to illness than do workers in almost any other OECD country,[1] but the United States fares poorer on many common measures of health than similarly wealthy nations (Woolf and Aron, 2013). One reason for this discrepancy may be the lack of access to paid sick leave (PSL) in the United States. The U.S. is one of the only industrialized countries that does not guarantee workers access to either employer- or government-provided paid sick days (Heymann et al., 2009), and nearly 40 percent of private sector workers currently do not receive paid sick leave from their employer.[2] Low-skilled workers are particularly unlikely to have access to paid leave benefits. Among workers in the bottom quartile of the wage distribution (those making $11.64 per hour or less), only 31 percent accumulate paid sick leave and less than half receive any paid vacation time. Thus, a substantial fraction of the U.S. population cannot take paid time off from work.

Given the low rates of employer-provided leave in the United States, government-mandated paid sick leave policies may have large effects on worker absences as well as overall health. This paper evaluates the effects of the introduction of mandatory paid sick leave policies in Connecticut and Washington, D.C. (hereafter D.C.) on leave-taking behavior. We show that these policies have the potential to generate large positive public health externalities, decreasing aggregate work absences due to illness by up to 0.4 percentage points – about an 18% decrease relative to the mean illness-related absence rate of 2.2% in the U.S.

Access to paid sick leave is important for several reasons. Going to work while sick may prolong illness (Earle and Heymann, 2006; Grinyer and Singleton, 2000), and sick workers are less productive than those at full health (Goetzel et al., 2004; CCH Incorporated, 2003). Some studies even suggest that the productivity losses associated with employees coming to

[1] Statistics calculated from the OECD Health Statistics 2015 database: http://www.oecd.org/els/health-systems/health-data.htm.

[2] U.S. Bureau of Labor Statistics. 2015. Employee Benefits in Private Industry, Table 6: Selected Paid Leave Benefits: Access, accessed September 6, 2015, http://www.bls.gov/news.release/ebs2.t06.htm.

work while sick exceed the costs of illness-related absenteeism (Stewart et al., 2003; Hemp, 2004). Additionally, many workers cannot afford to take unpaid time off from work for illness. According to a 2008 survey, almost half of employed workers report going to work while sick because they were worried about the financial consequences of taking time off.[3] Even those with access to paid vacation or unallocated paid time off may be reluctant to use it when sick, instead preferring to save it for other uses. Sick leave also often allows parents to stay home to care for sick children or elderly relatives. When parents can stay home to care for them, sick children have shorter recovery periods and fewer symptoms (see Heymann (2000) for a review).

Paid sick leave policies also directly affect employee leave-taking behavior. Several economic studies from Europe show that decreasing the generosity of sick leave compensation can lead to lower levels of absenteeism (Dale-Olsen, 2014; De Paola, Scoppa and Pupo, 2014; Ziebarth and Karlsson, 2010; Henrekson and Persson, 2004). This lower level of absenteeism is typically interpreted as a decrease in shirking behavior (i.e., using sick leave for non-illness reasons). Yet, one other important consequence of PSL usage is that contagious diseases and illnesses are less likely to be spread throughout the workplace. Approximately 68 percent of workers report having gone to work with the stomach flu or other contagious illness (Smith, 2008), and 40 percent of workers report contracting the flu from a colleague (Lovell, 2006). Workers who show up to work sick spread their illness to coworkers and customers, resulting in more work absences (Skåtun, 2003; Lovell, 2004). It is possible, therefore, that in aggregate, large scale PSL policies may actually *reduce* illness related work absences by limiting the spread of disease.

Why might this be the case? Only some workers gain access to PSL through the implementation of a mandate. In the United States, for example, many workers already have access to paid sick leave through their employer and thus are not directly affected by such a

[3]NPR/Kaiser Family Foundation/Harvard School of Public Health. 2008. "Health Care and the Economy in Two Swing States: A Look at Ohio and Florida, accessed October 24, 2015, www.npr.org/documents/2008/july/kaiserpoll/toplines.pdf.

policy. The workers who gain access to sick leave should be more likely to stay home from work when ill, but they may also be less likely to get sick through a public health benefit. For these directly affected workers, the effect of PSL on the illness-related leave-taking rate is ambiguous. But workers who do not gain access to sick leave should only receive the public health benefit so the effect on leave-taking should be negative. If this public health effect is large enough, PSL mandates will decrease the aggregate rate of illness-related leave-taking.

To test this, we exploit two PSL mandates passed in Washington, D.C. (2008) and Connecticut (2011). Using a difference-in-differences (DD) identification strategy, we evaluate the effects of PSL policies on a measure of illness-related work absences constructed from the Current Population Survey (CPS). We find that after these PSL policies are implemented, there are significant decreases in aggregate illness-related leave-taking rates in both Connecticut and D.C., relative to a variety of control groups. This decrease exists for both those directly affected and those unlikely to be directly affected by the policy. We find no such impacts on rates of non-illness leave-taking.

We corroborate these findings by exploiting a completely separate source of variation in the provision of paid sick leave, and again find that increased access to PSL decreases the aggregate rate of illness-related leave-taking. We make use of the fact that there is a high degree of variation in access to PSL across industries in the absence of government mandates, and use changes in industry composition over time within metropolitan statistical areas (MSAs) as an alternative source of variation in the provision of PSL. Further, we use a "Bartik instrument" approach that provides an arguably exogenous source of changes in local industry composition by using only the variation in within-MSA industry composition over time that is explained by national industry-specific labor demand shocks (Bartik, 1991; Blanchard and Katz, 1992). The results from this analysis again indicate that increased access to PSL is associated with decreases in the rate of illness-related leave-taking. Both of these identification strategies present evidence that suggests PSL policies have the potential to provide large public health benefits by allowing sick workers to stay home, rather than coming to work sick and spreading their illness to others.

There is good reason to expect that the introduction of PSL in the U.S. would have much larger public health effects than have been documented in other countries. The existing literature examines changes in sick leave policies in countries where nearly all workers already had access to sick leave prior to the implementation of the policy change. Effects on leave-taking behavior are usually identified off of small decreases in the sick leave wage replacement rate, from 100% to a slightly lower level (80-90% depending on the policy). U.S. PSL mandates instead require employers who previously provided no access to paid sick leave to provide full compensation. Additionally, the workers most likely to gain access to sick leave through PSL mandates are employed in service occupations and industries, where one could argue that the risk of spreading contagious illnesses is particularly high.

This is the first paper to identify the effect of access to PSL on leave-taking behavior in the United States. To the best of our knowledge, only three other papers examine the effects of mandated PSL in the U.S. Ahn and Yelowitz (2015) study the impact of the Connecticut policy on employment, and find very small but statistically significant positive impacts on the probability of being unemployed. These effects are concentrated among those age 30 and older, suggesting that employers view older workers as more costly once PSL is available. However, using all of the U.S. sick pay mandates implemented through 2014, Pichler and Ziebarth (2016) find little evidence that PSL significantly affects employment or wages. Most relevant to our work, Pichler and Ziebarth (2017) examine the effect of PSL policies in the U.S. on influenza rates using Google Flu data. Though they do not look at leave taking, they show that PSL reduces influenza rates by about 10 percent, which is consistent with our findings that PSL policies provide large public health externalities.

The remainder of the paper is organized as follows. Section 2 describes PSL policies in the U.S. in more detail, and provides a brief conceptual discussion of the predicted impacts of PSL mandates. Section 3 details the data used to evaluate the effects of PSL on aggregate leave-taking rates. The primary estimation strategy is outlined in Section 4, and Section 5 presents results. In Section 6, we describe and present results from our alternative identification strategy. Finally, Section 7 provides concluding remarks.

2 Paid Sick Leave in the United States

The United States currently does not require private-sector employers to provide PSL to employees. While about 60 percent of workers are eligible for job protection during unpaid leave related to serious illness under the Family and Medical Leave Act of 1993, a substantial fraction of workers are not legally protected from taking even unpaid time off from work when sick (Klerman, Daley and Pozniak, 2012).[4] Growing evidence about the importance of access to sick leave has caused the federal government to take steps to improve access, however. President Obama issued an executive order in September 2015 to require federal contractors to provide employees with PSL, and called on Congress to pass the Healthy Families Act, which would require most private-sector employers to provide PSL as well.[5]

Since 2006, several cities and states have implemented their own PSL policies that require employers to provide certain workers with access to fully paid time off for reasons related to illness, caring for sick family members, and seeking routine or preventative medical care. Many of these laws also cover care related to domestic violence or sexual assault. These policies generally mandate that eligible employees accrue one hour of PSL for every 30-40 hours worked, up to a maximum amount per year. Unused leave can be rolled over to the next year. Unlike the recent Paid Family Leave policies in several states, employers bear the costs of the leave.

San Francisco became the first city to mandate employer-provided sick leave in 2006.

[4] Other policies that provide individuals with compensated time off due to illness or injury include Workers' Compensation and Disability Insurance. Workers' Compensation is administered at the state level, and covers lost income due to a work-related injury or illness. Disability Insurance insures against more permanent labor force withdrawals related to long-term illness or injury. Neither of these programs pertain to the types of illnesses most commonly causing the need for short-term work absences.

[5] Federal contractors are required to provide PSL as of January 1, 2017. The Healthy Families Act proposed that all employers with 15 or more employees to provide each employee with up to 7 days of PSL per year. See https://www.whitehouse.gov/the-press-office/2015/09/07/fact-sheet-helping-middle-class-families-get-ahead-expanding-paid-sick for more information.

The law covers all workers, although employees at small businesses can only accrue up to five instead of nine days per year. This historic legislation led the way for several other cities and states to pass similar laws.[6]

Washington, D.C. passed the Accrued Sick and Safe Leave Act in March 2008, with accrual of sick time beginning on November 13 of the same year. This act covers all employers, but requires larger firms to provide more generous benefits. Specifically, the act requires large employers (over 99 employees) to provide up to 56 hours of paid sick time per year, medium employers to provide up to 40 hours, and small employers (less than 25 employees) to provide up to 24. Additionally, workers in these firm types accrue their benefits at different rates: one hour for every 37, 43, and 87 hours worked in large, medium, and small firms, respectively. Given these parameters, a full-time worker (40 hours per week) in a large firm would have accrued one full day of sick time in mid January 2009. The original act excluded restaurant workers who receive tips (but not those who do not receive tips) from coverage, and workers in their first year of employment were also not eligible to accrue benefits. However, the act was amended in 2014 to cover tipped restaurant workers and to require that all employees start accruing benefits upon hire.[7]

Connecticut became the first state in the nation to pass a PSL mandate in July 2011, and it went into effect on January 1, 2012. In contrast to the PSL policies in San Francisco and D.C., this act specifically applies to service workers, and only to employers with 50 or more employees. Service workers (as defined by a set of occupation codes that match the Standard Occupational Classification system used by the Bureau of Labor Statistics) are typically hourly employees, and accrue one hour of PSL for every 40 hours worked, up to a maximum of 40 hours of PSL per year. Further, employees must have worked for the same employer for 680 hours from either the start of employment or January 1, 2012 (whichever was most recent) to begin accrual. Together, this means that a full-time worker (40 hours

[6]Our main analysis will not use San Francisco because sample sizes in the CPS are too small to obtain precise estimates of the effects in this city. However, we can show that the point estimates of the effects in San Francisco are consistent with what we find in Connecticut and D.C., albeit much noisier.

[7]Benefits can be used after 90 days of employment.

per week), working every week for the same employer as of January 1, 2012 would not have accrued a full day of sick time until June 2012. Importantly, many of the individuals covered are in food service, retail sales, and transportation occupations, which all require substantial contact with customers. Although the law does not cover all employees, it targets a group of workers who are both unlikely to have access to PSL in the absence of the law and who may disproportionately spread diseases when going to work while sick.[8]

Several other cities and states have since passed PSL legislation as well. Seattle, Washington's PSL act was passed in 2011, and Portland, Oregon, New York City, and Jersey City, New Jersey followed in 2013 or later. California and Massachusetts both passed statewide laws in 2014 that went into effect in July 2015, as did several other cities in New Jersey and Oregon. Although we would ideally examine all cities and states with PSL laws in our analysis, most are still too recent to analyze. Further, data limitations due to small sample sizes in San Francisco and poorly defined treatment boundaries in Seattle prevent our analysis of these policies, though the imprecise estimates generated for these regions are consistent with our main findings for Connecticut and D.C.[9]

While these PSL policies all have slightly different eligibility requirements and benefit accrual rates, they all substantially expanded workers' access to sick leave. Table 1 shows the proportion of workers in the U.S. with access to sick leave benefits by industry in both 1993 and 2010-2014. Although workers in high skill industries are likely to have sick leave benefits even in the absence of a PSL law, sick leave provision is much lower in service industries. Only 26 percent of workers in the accommodation and food services industry had access to sick leave in the 2010-2014 period, for example. Therefore, these policies may have large direct effects on leave take-up among eligible workers.

PSL policies could affect leave-taking rates in several ways. First, earlier work from

[8]Only about 20% of the Connecticut workforce is covered by the mandate (Pichler and Ziebarth, 2017). A small share of these workers already had access to employer provided sick leave benefits, so the share of workers directly affected by the mandate is likely somewhat smaller than 20%.

[9]In reference to Seattle, we are only able to identify the county (King county) that contains the city of Seattle in our data; this county has more than three times the population of the city itself.

Europe finds evidence that providing workers with more generous sick leave increases shirking behavior (Dale-Olsen, 2014; De Paola, Scoppa and Pupo, 2014; Ziebarth and Karlsson, 2010; Henrekson and Persson, 2004). Workers may take advantage of the compensated time off to stay home from work when they are not actually ill. If PSL increases shirking, it would lead to an increase in leave-taking among those who gain access to the benefit, but it would not affect the leave-taking behavior of other workers.

Second, PSL policies allow sick workers to stay home from work when ill. In the absence of paid sick leave, sick workers face a trade-off between staying home without pay (where they likely recuperate faster, have more access to medical professionals, and reduce the number of interactions with coworkers or customers) and earning a wage by going to work. The benefits of staying home while sick are arguably increasing in the severity of the illness. This means that for many workers, the costs of missing work will exceed the benefits, and workers will show up to work when sick.[10] PSL lowers the monetary cost of missing work, thereby decreasing the share of directly affected workers who go to work when ill. While it is tempting to think that access to paid sick leave should unambiguously increase the leave-taking rate of directly affected workers, this is not necessarily the case. The positive direct effect of PSL on leave-taking could be offset, or even reversed, by a reduction in illness duration (conditional on getting sick) if staying home from work when sick leads to faster recovery. The effect could be even further offset or reversed by a public health effect that reduces the probability of getting sick in the first place.[11]

Third, when sick workers are less likely to go to work, they are less likely to spread contagious illnesses to coworkers and customers. It then follows that PSL policies may reduce the probability that all workers get sick, regardless of whether or not they gain access to PSL. In other words, only directly affected workers will be less likely to go to work when sick, but

[10] There may also be non-monetary costs of missing work. For example, workers may be concerned about the negative career consequences of missing work.

[11] Although outside the scope of this paper, these policies may actually be cost effective for firms if workers who stay home either recuperate faster or are sufficiently less likely to spread their illness to coworkers. Non-illness-related paid absences are unambiguously costly for firms.

potentially all workers will be less likely to get sick to begin with. If these "indirect" public health effects are large enough, PSL may actually lower the spread of contagious disease enough to decrease the illness related leave-taking rate in aggregate. However, whether the direct effects of PSL on leave-taking will be larger or smaller than the indirect effects is an empirical question.

What is clear is that mandated PSL policies (that reduce the cost of illness-related leave taking) will not directly affect workers who already have access to employer-provided paid sick leave, or who are ineligible for the benefits. This means that PSL policies should lead to a decrease in the share of workers who are absent from work due to illness but are not directly affected by the policies, so long as the public health effect is strictly positive. Furthermore, the total policy effect will essentially be a weighted average of the effects for workers who are and are not directly affected by the PSL mandates, weighted by the size of each population. In the U.S. policy settings that we study, the population of workers who do not gain access to sick leave from the mandate tends to be significantly larger than the population who does, and hence we might expect to see PSL reduce aggregate illness-related leave-taking rates.

3 Data

3.1 CPS

Our primary data source is the 2006-2015 Current Population Survey (CPS) basic monthly files. Households participating in the CPS are surveyed on eight separate monthly occasions (four months in the survey, followed by eight months out, and an additional four months in). The basic monthly files contain information collected in all eight surveys. This is in contrast to Annual Social and Economic Supplement (collected in March only) and Outgoing Rotations Files (collected in each household's fourth and eighth survey months only). While the basic monthly files do not contain detailed labor market information in every month (on earnings, for example), they do contain the information needed to construct our measures of leave-taking.[12]

There are two questions in the CPS that form the basis of our measures. First, individuals who report being employed but absent from work in the reference week (i.e., worked zero hours) are asked the main reason for their absence from work. The first column of Table 2 reports the list of reasons why individual may be absent, and the percentage of absent individuals falling into each category.

Second, individuals who are employed and at work during the reference week report both their usual hours worked per week and the number of hours actually worked in the reference week. Those who work less than 35 hours during the reference week but report that they usually work at least 35 hours per week are asked the main reason for working less than normal. Typically, these are full-time workers who took one or more days off in the reference week, yet worked a non-zero number of hours (i.e., were not absent the entire week). It is important to note that this measure does not capture time off for most part-time workers. The second column of Table 2 reports the list of reasons an individual may have worked less than 35 hours, and the percentage of leave-takers falling into each category.

Each of these two questions lists "own illness" as one possible reason for missing work and is the reason given for 18.4% and 19.2% of entire-week and partial-week absences, respectively. Leave-taking for own illness forms the basis of our main outcome, but we also assess whether leave-taking for any other reason is affected by these policies. Specifically, we categorize employed individuals who usually work at least 35 hours per week into one of three categories.

1. "No Leave": employed and at work, and worked at least 35 hours in the reference week.

2. "Sick Leave": employed and at work but worked less than 35 hours in the reference

[12] The main outcome of interest in this analysis is illness-related leave taking. We also show effects on the number of hours absent for illness and other reasons. It is also possible to examine total hours worked, and we have estimated models in which the outcome was specified as such (not shown). Unfortunately, these estimates were insignificant and lacked the precision to draw any meaningful conclusions.

week due to own illness, *OR* employed and absent from work in the reference week due to own illness.[13]

3. "Other Leave": employed and at work but worked less than 35 hours in the reference week due to any other reason, *OR* employed and absent from work in the reference week due to any other reason.

It is important to note that individuals who are not employed or who do not usually work at least 35 hours per week are left out of the analysis. In other words, we consider only individuals who are eligible to answer both questions regarding leave-taking. Unfortunately, this structure of the questionnaire means that we are likely missing a large portion of individuals who are directly affected by sick leave mandates (i.e., part-time workers). When the data are collapsed to the state-month level, the sick leave rate is defined as the percentage of workers on "Sick Leave" as a proportion of the total population of employed individuals working at least 35 hours per week. To be clear, define Total_{st} as the number of employed individuals working at least 35 hours per week in state s and month t; then the sick leave rate, SL_{st}, and the other leave rate, OL_{st}, are defined as follows:

$$\text{SL}_{st} = \frac{\#\ \text{Sick-Leave}_{st}}{\text{Total}_{st}} \qquad \text{OL}_{st} = \frac{\#\ \text{Other-Leave}_{st}}{\text{Total}_{st}}$$

These measures reflect the share of full time workers absent from work for that reason in a given week. Our results are not sensitive to the definition of sick leave and other-leave rates.[14] The other leave rate is used as an outcome in a placebo check.[15]

[13] In an alternative specification of our main estimates, we exclude workers who were absent for the entire week. Our estimates are not sensitive to this exclusion and these results are presented in Table A1.

[14] In particular, we have defined the sick leave and other-leave rates using two alternative denominators. Results are robust to using the total CPS population (rather than the population of employed workers working at least 35 hours) as the denominator, and also to using the number of employed workers who are not on any kind of leave.

[15] While we use other leave-takers as a placebo check, there are a couple of reasons why a PSL mandate could increase leave-taking for non-illness reasons. First, some workers likely shirk and use their paid time off when they are not actually sick. Even if they are using PSL, it is unclear how they would report the reason for their absence in the CPS. Second, our measure of illness-related leave focuses specifically on own illness. The PSL policies we study allow workers to stay home to care for sick children or relatives, which would be considered "other leave" in this analysis.

We also make use of the basic demographic characteristics included in the CPS. We select a standard group of controls including indicators for gender, age (<20, 20-30, 30-40, 40-50, 50-60, >60), race (non-Hispanic white, non-Hispanic black, Hispanic, other race/ethnicity), marital status (married, widowed/divorced/separated, never married), and education (less than high school, high school diploma, some college, college graduate). Finally, we construct a "service occupation" indicator based on the individual's reported occupation that specifies whether they work in one of the service occupations for which the policy in Connecticut applies. All regressions and means are weighted by CPS final weights.[16] Table 3 shows summary statistics for all workers 16-64 as well as for the sample of full-time workers used in the analysis. Over 83 percent of all workers usually work at least 35 hours per week and are included in our sample. Just over 2 percent of workers report being absent from work due to illness during at least part of the reference week, while about 9 percent miss work for any other reason.

Finally, we use data on time-varying state level factors to control for differential economic changes across states during the sample period. These state-level controls include the unemployment rate, per-capita income, the log of the population, and the state minimum wage.[17]

3.2 Quarterly Workforce Indicators

After presenting our main results, Section 6 provides additional evidence that increasing access to PSL can create large positive public health externalities. Instead of using policy driven variation in access to PSL, we exploit MSA-level variation in non-mandated employer-

[16] Because state fixed effects are implicitly included in all specifications, and state-specific variation is one of the main sources of differential sampling probabilities, it could be argued that CPS weights are conditionally exogenous. If this is the case, then weighting provides no benefit in terms of consistency, but may cause losses in efficiency. We follow Solon et al. (2015) and reproduce our main results without weights in Table A2; as expected, the point estimates vary only slightly, and the standard errors are slightly smaller in the estimates without weights.

[17] These data were obtained from: University of Kentucky Center for Poverty Research. 2016. UKCPR National Welfare Data, 1980-2015. Gatton College of Business and Economics, University of Kentucky, Lexington, KY. http://www.ukcpr.org/data.

based PSL provision using a Bartik instrument approach. For this analysis, we combine data from the CPS with employment data from the Quarterly Workforce Indicators (QWI). The QWI provides local labor market statistics on employment and earnings. The data are provided as part of the Longitudinal Employer-Household Dynamics (LEHD) program, which covers over 95 percent of U.S. private sector jobs. The QWI is constructed from Unemployment Insurance earnings data, the Quarterly Census of Employment and Wages, and other Census information and administrative records. One of the main advantages of the QWI is that employment measures can be computed by industry and geographic area, as well as by demographic characteristics of the workers.

States voluntarily report QWI data. Although it began in 1990, few states reported in its early years. We use a balanced panel of MSAs for which data was reported in every quarter between 2003 and 2014.[18] To construct a yearly measure of industry-MSA employment, we take the average over the beginning of quarter employment levels by 2-digit industry code and MSA for each quarter of the year.

4 Empirical Strategy

In our primary identification strategy, we estimate the effect of PSL mandates on illness related leave-taking using a difference-in-differences (DD) approach. With many treatment states, it would be straightforward to pool these together into a single estimating equation. Because we have only two, however, we opt to estimate the DD effects for each treatment location separately. As such, the econometric setting presented here should be interpreted as one in which there is a single treatment group; when calculating estimates for Connecticut, observations from D.C. are dropped from the model, and vice-versa. Consider a standard DD estimator for a single treatment group and multiple time periods:

[18]This excludes Arizona and Washington, D.C., as well as Massachusetts, which never reports to the QWI. We also exclude Connecticut.

$$Y_{ist} = \alpha_0 + \alpha_1 Treated_s * Post_t + \alpha_2 X_{ist} + \delta_s + \phi_t + \varepsilon_{ist} \quad (1)$$

In our setting, Y_{ist} represents the outcome of interest for individual i in region s and month-year t. The coefficient of interest is α_1, where $Treated_s$ is an indicator that is set equal to one for Connecticut or D.C., depending on the regression being estimated, and $Post_t$ indicates month-years after the policy went into effect. The vector X_{ist} contains time-varying covariates (described in Section 3). Finally, δ_s and ϕ_t are state and month-year fixed effects, respectively, and ε_{ist} is the error term.

There are a number of potential problems with estimating an equation of this form. The first issue lies in the choice of control group. Equation (1) uses all states other than the treatment state as the control group, but it is unclear whether this is an appropriate choice. Our solution is to show that our DD estimates are not sensitive to the choice of control. We choose three alternatives: all other states, neighboring states, and a synthetic control group. The synthetic control methodology, developed by Abadie et al. (2010), was specifically created for case-control settings such as ours. The synthetic control group is constructed as a weighted average of all other states, where the weights are determined by matching on pre-treatment trends. We match only on pre-treatment trends in the outcome variable in some specifications, and in others we regress out the individual and time-varying state-level controls in Equation (1) and then match on pre-treatment trends in the residuals.

The second issue with Equation (1) is that estimation of standard errors in models such as these typically rely on clustering at the state level (Bertrand et al., 2004). Though there are many states included in this model, there are actually only two *relevant* groups (the treatment state and all others).[19] To account for this, we use the two-step procedure developed by Donald and Lang (2007), henceforth DL. The first step in this procedure is

[19] In total, there are 44 states included in the analysis. First, we exclude all states that had a city adopt a PSL mandate by the end of 2013. These states are California, Oregon, Washington, New York and New Jersey. Second, we exclude Connecticut from the D.C. regressions and vice-versa. Finally, we exclude Alaska and Hawaii from the analysis.

to collapse the data to the treatment/control level; in specifications that include additional covariates, however, we first regress out these controls:

$$Y_{ist} = \gamma_0 + \gamma_1 X_{ist} + \eta_{ist} \tag{2}$$

We then use the residuals from this equation as the outcome in the two-step DL procedure. We collapse the data to the treatment/control level and calculate the mean difference in the outcome of interest (or in the residuals from Equation (2) in models that include covariates) between treatment and control groups for each month, D_t. The second step is to regress this difference on an indicator set equal to one in periods after the policy has gone into effect:

$$D_t = \beta_0 + \beta_1 Post_t + u_t \tag{3}$$

The coefficient of interest is β_1, which is the DD estimate of the effect of PSL mandates. In both Connecticut and D.C., treatment status in the first year of the policy is partial or ambiguous. In each case, there is a significant lag between the implementation of the policy and the time when benefits can be taken since paid sick leave hours can only begin to be accrued after the policy is implemented. Further, there is also the possibility that it takes time for information regarding these benefits to be disseminated to workers, or for workers to grow comfortable enough to actually use their benefits (one could imagine that no worker wants to be the first at their firm to take advantage of sick leave benefits). Because of this ambiguity, we drop observations in the first 6 months following policy implementation in the primary regression specifications.[20] For clarity, Figure 1 displays a timeline of events for both treatment states, including the pre-period, dropped observations, and the post-period.

A final note on the identification strategy concerns externality impacts. If PSL mandates have significant externality impacts, then the impacts of these policies need not be concentrated within a single geographical unit. For example, it is conceivable that reducing the

[20]These regressions are estimated at the monthly level. We also present figures displaying how annual rates of leave taking change over time, and no observations are dropped in these figures.

spread of disease within Connecticut could also lead to a decrease in the spread of disease in other nearby states. To the extent that such cross-state spillovers are important (the importance of which is not possible to measure in our setting), our estimates capture only a lower bound on the total externality impacts of PSL mandates. But as shown below, our results are robust to using a variety of control groups that consist of states of varying geographical distance from Connecticut and D.C. This suggests that any cross-state spillover effects are likely small.

5 Results

5.1 Leave-Taking (CPS)

The main results are summarized in event-study plots, presented in Figure 2. These figures plot the unadjusted sick leave rates over time for Connecticut and D.C., along with the sick leave rates for their respective synthetic control groups. Because leave-taking is highly seasonal, and because of the relatively small monthly sample sizes, leave-taking rates in these plots are aggregated to the annual level. In each case, the vertical line represents the first year in which one day of sick leave could be taken following the implementation of each policy. In both cases, the main result is clear: a decline in the rate of leave-taking for illness in the years following policy implementation relative to the control.

In both cases, this decline in sick leave-taking persists for the at least two years following the policy. This decline does not seem to last beyond the first two years in D.C., as sick leave rates appear to climb back to approximately the level of the control group over time. In Connecticut, this decline persists in all three years in which post-policy data are available. It should be noted that the results for D.C. should be taken cautiously as the implementation of the sick leave policy coincided with the onset of the financial crisis. It is reassuring, however, that the decline in leave-taking for illness following policy implementation occurs for both treatment groups and is of similar magnitude, even though the policies occur in different years.

To check whether these results are specific to sick leave, we conduct the same procedure for non-illness leave-taking. Figure 3 displays these results. For both Connecticut and D.C., the treatment group tracks the synthetic control quite well throughout both the pre- and post-periods, and there appears to be no systematic difference in leave-taking immediately following policy implementation. This is reassuring as PSL policies should be less likely to affect non-illness leave-taking. Furthermore, these results imply that the synthetic control method is doing a good job of estimating the counterfactual, as the trends continue to track together even though they are only matched on the pre-treatment periods. These results lend support to the interpretation of the relative decline in illness-related leave-taking seen in Figure 2 as a causal effect of the PSL policy. In other words, it seems unlikely that diverging trends in general leave-taking that just happen to coincide with the timing of the policy are driving the sick leave results.

The results for sick leave are presented numerically in Tables 4 and 5 for Connecticut and D.C., respectively, using data at the monthly level. Panel A of each table displays the main result from Equation (3), which is the coefficient on a post-policy indicator that represents the entire available period beyond which the policy was implemented in each state. The estimates in Panel A exclude the first six months after policy implementation when a full day sick leave has not yet been accrued. Panel B in table Table 5 (D.C.) restricts the post period to three years beyond policy implementation, which is the available post-period for Connecticut, for an equivalent comparison between the two states. Panel B of Table 4 (CT) and Panel C of Table 5 (D.C.) present the coefficients from an event-study specification in which the coefficients for each year following policy implementation are displayed. In these event-study specifications, we do not drop any months from the sample and as such, the Year

0 estimates should be interpreted as partial treatment.[21] In each table, results are displayed for three sets of control groups (all other states, synthetic control, and neighboring states).

Synthetic control groups are formed by matching the pre-treatment trend in sick leave-taking in the treatment state.[22] This method chooses zero or non-zero weights for each of the possible control states such that the root mean square prediction error is minimized. The weights used to calculate these plots and the regression results below are presented in Table A3, for models with and without covariates regressed out. For Connecticut, in the model with covariates, the synthetic control weight for Maine is 100% (and zero for all other states). For D.C., in the model with covariates, Maine also receives the largest weight (48%), with small contributions by all other states. In the models excluding covariates, the weights are more evenly spread across states for both Connecticut and D.C. (with non-zero weights for all states in both cases). In the tables below, all estimates include both the individual and state-level covariates described in Section 3, and we additionally replicate the main results excluding all covariates, and present these estimates in Table A4.

For Connecticut, the point estimate for the total effect (using a synthetic control group with covariates) is a statistically significant -0.0041. This can be interpreted as a 0.41 percentage point decline in the rate of leave-taking for illness. Relative to a mean rate of leave-taking for illness of 2.2%, this represents an approximate 18 percent decline. The event study results confirm what can be seen graphically in Figure 2: there appears to be no effect in the first year of the policy, which we interpret as a partial-treatment period (labeled "Year 0"), followed by strong negative effects in each of the following three years. Furthermore, all of the estimates for Connecticut are quite robust across choices of control group.

The results for D.C. mirror the results for Connecticut in many ways; this can be seen most easily by looking at the event-study impacts in Panel C, which show no effect in the

[21] Sample sizes represent the number of months included in the sample (since coefficients are obtained from the Donald and Lang (2007) procedure). 120 months are included in the event-study specification in which no monthly observations are dropped; 114 months are included in the main estimates in which the first 6 months following policy implementation are dropped; 78 months are included in the 3-year estimate for D.C. in which the post-period is restricted to three years beyond the implementation.

[22] Results are similar if state-level demographics are included in the matching equation as well.

partial year followed by negative effects in the first two years that are similar in magnitude to the estimates for Connecticut. The impacts presented here vary somewhat with the choice of control group, and the strongest negative impacts are found when the control group is defined as neighboring states. This is not necessarily surprising given that the timing of the policy for D.C. coincided with the onset of the financial crisis, and economic conditions may be more similar.[23] In any case, in the third full year following policy implementation and beyond, these negative and significant effects dissipate or disappear. This is reflected in the total effects, which tend to be close to zero, especially in comparison to the total effects estimated for Connecticut. It is not immediately clear why one would expect these effects to fade over time in D.C., though there are several possibilities. It could be, for example, that once workers become comfortable using PSL they are more likely to use their benefits to shirk rather than for an actual illness. Alternatively, the public health effects of PSL driven by reduced exposure to contagious illness may be less persistent in D.C. than in other states because of the large share of commuters who reside outside of the city. The effects of the policy could have also been attenuated by the economic climate in D.C., as growth stagnated from 2012-2014 relative to other areas.

On balance, the main results for both Connecticut and D.C. exhibit decreases in leave-taking for illness in the years following policy implementation. Analogous results for other leave-taking are shown in Table 6; consistent with the graphical results in Figure 3, there are no significant effects of the PSL mandates on leave-taking for non-illness reasons. The lack of effects on leave-taking for other reasons is reassuring, and indicates that the effects of PSL on illness absences are not driven by differential trends in leave-taking more generally.

Table 7 shows estimates of the effect of the mandates on the number of hours absent due to illness during the reference week. The results are largely consistent with the effects shown

[23] The effects of PSL in D.C. do not appear to be driven by a differential response to the Great Recession. To test for recession effects, Tables A7 and A8 show the differential effects of the 2001 recession on illness and other leave-taking, respectively, in D.C. compared to each of the same three control groups used in the main analysis. We do not find evidence of differential leave-taking responses in D.C. in this earlier recession. Additionally, we find no evidence that the Great Recession is correlated with a decrease in illness-related absences in the U.S. overall.

above. In Connecticut, PSL leads to about a 0.05 reduction in weekly hours absent due to illness. This is a 25 percent effect from the mean of 0.2 hours absent due to illness per week. Given that we find a 18 percent decline in the overall absence rate (any hours absent), our results suggest that PSL reduces 1-2 day absences.[24] If PSL limits the spread of contagious disease, these relatively short absences are the ones that should be prevented.[25]

Delving deeper, we decompose the main effect on illness-related leave-taking by service and non-service workers in Connecticut. Recall that the policy in Connecticut only *directly* applies to service workers in that these are the workers who may have gained access to PSL as a result of the policy. Non-service workers should only be impacted by the policy through *indirect* mechanisms. Theoretically, it is unclear whether we should expect the effect for service workers (directly affected workers) to be larger or smaller than the effect for non-service workers (indirectly affected workers). On one hand, since service workers are those who gained access to leave, the sign of the effect for these workers is ambiguous: the health externalities may or may not be strong enough to offset the additional leave-taking. On the other hand, service workers are more likely to be exposed to other service workers who are now less likely to show up to work sick, so service workers may experience a larger public health benefit compared to non-service workers.[26]

Table 8 shows that we find no significant differences between the two groups; with the negative impacts for the non-service group being slightly larger. That being said, we cannot reject economically meaningful differences in magnitudes given the large standard errors associated with each set of estimates. While the coefficients for service workers are negative

[24]The illness-related absence rate is 2.2 percent. Assuming an 8 hour work day, the effect on hours is consistent with a 28 percent reduction in one day absences ((0.22*8)/0.05) or a 14 percent reduction in two day absences.

[25]We find no significant effects on hours absent for non-illness related reasons. Results are shown in Table A5.

[26]We should also reiterate that part-time workers are left out of this analysis. Many service workers may work part-time, and if part time workers are relatively younger and healthier, then it could be that those who are left out of the analysis are a healthier group on average. While healthier non-service workers are likely more resilient to infectious diseases and thus less likely to be affected by the public health impacts (and the policy in general), the relative effects for healthy versus unhealthy workers in the service industry is ambiguous: they are less likely to be affected by the public health impacts, but also less likely to require the additional sick days in the first place.

and of a similar magnitude to the estimates for non-service workers, these estimates are not statistically different from zero at conventional levels. Of greater interest is that the coefficients for non-service workers (those who should not have gained PSL as a result of the policy) are negative and statistically different from zero across all specifications. Because these workers are not directly affected by the policy, we interpret this negative effect of the PSL policy as a pure externality effect.

The negative and significant impact for non-service workers helps to rule out other explanations for a decrease in leave-taking following the implementation of PSL mandates that would only affect the workers who gain access to PSL as a result of the policy. For instance, one possibility is that in response to a PSL mandate employers discriminate against potential employees that are more prone to illness absence, as proposed in Ahn and Yelowitz (2015). If firms began hiring workers who were less likely to be absent, this would induce a decrease in leave-taking. Another possibility is that in response to PSL mandates, firms begin to more strongly discourage illness absences. In either of these scenarios, however, only illness absences for workers who are directly affected by PSL mandates would be affected.

Finally, as another test of the health externality mechanism, we test whether there are seasonal differences in the treatment effect. If the reduction in leave-taking operates through a public health externality, then one would expect the effect to be stronger in periods when contagious illnesses are more likely to be circulating. Because contagious illnesses are more common in winter months, we show the differential effect of PSL in the winter compared to summer in Table 9, where winter is defined as November through March and summer is all other months. The DL approach to calculating standard errors will not work when the independent variable of interest varies within groups, as is the case here. As such, we conduct inference by using the bootstrapping procedure proposed by Ferman and Pinto (2015) to produce p-values, shown in brackets under each coefficient.[27] For both Connecticut and D.C., the point estimates on the interaction terms are negative and economically significant

[27] This method is designed to work well when there is only one treated group and multiple control groups. It also accounts for heteroskedasticity in the errors generated by differences in group size.

(the winter impacts range from 54% larger to 113% larger compared to the summer impacts), indicating that the policies have a larger negative effect on leave-taking in the winter months. That being said, the winter effects are not statistically different from the summer effects at conventional levels.

5.2 Additional Robustness Checks

While a number of robustness checks have already been discussed (e.g., effects for non-illness related leave-taking, unweighted estimates and estimates without covariates), two additional robustness checks are presented in this section. First, we present estimates for a variety of alternative control groups; second, we conduct a permutation test.

The main estimates are presented for three control groups: all other states, a synthetic control group, and neighboring states. In each case, the synthetic control group ends up having non-zero weights for all other states. As such, there is ultimately little difference between the all states and synthetic control specifications. Table A6 presents a test of the robustness of the results to a variety of truly distinct control groups. Specifically, we examine the Northeast and South census regions, as these contain Connecticut and D.C., respectively. For each census region, estimates are presented using all states in these regions as the control group, and a synthetic control (where the donor pool is limited to these regions). As such, four new control groups are presented for both Connecticut and D.C. While there is certainly some variation in the point estimates depending on the control group chosen, this table shows that the estimates are generally quite insensitive to the choice of the control group, reflecting the idea that is shown in the synthetic control plots (Figure 2), that in both Connecticut and D.C. there is a substantial dip in leave-taking following the implementation of the policies, whereas leave-taking evolves relatively smoothly over time in other states.

To demonstrate that the observed decreases in illness-related leave-taking are much more likely to occur in the treatment locations in the years after the policy went into effect, we conduct a permutation test. In this test, we estimate placebo difference-in-differences regressions with leave-taking for illness as the outcome for each state in the contiguous U.S. We estimate a series of regressions for each state, allowing the placebo PSL policy to go into effect beginning in each month from November 2008 to January 2012 (the D.C. and Connecticut implementation months, respectively). All estimates are calculated using a synthetic control, and we match on pre-treatment trends in the outcome using data from all months between January 2006 (the beginning of the sample frame) through the month prior to the assigned policy implementation. The post-period is defined as the 3 years following policy implementation, excluding the first 6 months (which is the length of the post-period available for Connecticut). Finally, we conduct this entire exercise with and without control variables. With 42 states, 38 treatment months, and regressions with and without controls, we estimate a total of 3,192 placebo estimates.[28] The results of this test are summarized in Figure 4, which plots the histogram of placebo t-statistics associated with the test that the DD coefficient from each separate regression is equal to zero. For regressions with and without covariates, the histogram of placebo t-statistics is shown twice (for a total of four plots), each overlayed with the t-statistics from the estimated DD effects in Connecticut and D.C.

The histogram shows that the distribution of placebo t-statistics is centered around zero, with the tests for Connecticut and D.C. lying in the left tail of the distribution. Our primary argument in conducting this test is that it is highly unlikely that we would observe treatment effects in the left tail of the distribution for PSL mandates instituted in two different states at two different time periods if our estimates were due to a spurious correlation. In the next section, we use a completely different source of variation (i.e., not policy driven) to provide additional evidence on the impacts of increased access to paid sick leave on leave-taking.

[28] There are 42 states because of the following exclusions: Connecticut and D.C., Hawaii and Alaska (not in the contiguous U.S.), California, New Jersey, New York, Oregon, and Washington (states with localities adopting PSL mandates by the end of 2013).

6 MSA-level Analysis

The state-level sick leave mandates provide nice quasi-experiments that allow us to estimate the effect of relatively large changes in sick leave availability. However, the case-control setting makes it difficult to rule out the possibility that the effects on illness related leave-taking rates are driven (or mitigated) by other simultaneous state-level changes that would also affect employment or leave-taking. Furthermore, it is possible that the negative relationship between access to sick leave and leave taking observed in Connecticut and D.C. is unique to those settings if, for example, the types of workers who gained access to sick leave were especially likely to spread contagious illnesses by going to work sick. As such, a more general analysis of the relationship between access to sick leave and leave-taking can provide evidence on the external validity of the estimates from the prior section.

In this section, we provide some corroborating evidence that suggests increasing the provision of PSL within an MSA over time is associated with a lower rate of illness related leave-taking in the aggregate. To show this relationship, we combine information on industry-specific PSL provision with data on industry composition at the MSA level to construct a measure of local PSL access. We find a negative relationship between access to sick leave and illness-related leave-taking that is consistent with the results presented above, and that again suggests the presence of large public health externalities. However, it is important to note that the direction and magnitude of the effect of increased access to PSL on aggregate leave-taking likely depends on the base level of access to leave, the degree of interactions with other workers, health of the community, and the degree to which workers use PSL to shirk. For this reason, the average effect we identify may mask substantial heterogeneity across communities.

6.1 Identification

Because workers in certain industries are much more likely to have access to employer-provided sick leave than workers in other industries, we use variation in MSA industry composition over time (from 2004-2014) to identify the effects of access to sick leave on the aggregate MSA-level illness-related leave-taking rate. The national variation in paid sick leave availability by industry is from the 1993 CPS Survey of Employee Benefits and is shown in Table 1. The share of workers with access to PSL in a given MSA is:

$$PSLshare_{mt} = \frac{\sum_{j=1}^{J} PSLrate_j * emp_{jmt}}{\sum_{j=1}^{J} emp_{jmt}} \tag{4}$$

where $PSLshare$ is the share of workers with access to paid sick leave in MSA m in year t. The variable $PSLrate$ is the share of full-time workers in industry j with access to employer-provided PSL.[29] The variable emp_{jmt} is the total number of employed workers in an industry, MSA, and year.

We are therefore interested in estimating the effect of $PSLshare$ on the sick leave rate:

$$Y_{mt} = \beta_0 + \beta_1 PSLshare_{mt} + \theta X_{st} + \delta_m + \phi_t + \varepsilon_{mt} \tag{5}$$

where Y_{mt} is the share of workers absent from work due to illness (from the CPS), X_{st} is a vector of time-varying state-level controls including the unemployment rate, minimum wage, log population, and per capita income, and δ_m and ϕ_t are MSA and year fixed effects, respectively. The strategy described in Equation (5) uses changes in industry composition to generate variation in access to PSL within an MSA over time; this variation in industry composition, however, may come from either changes in labor supply or labor demand. Ideally, the variation in industry composition observed at the local level would not be driven by un-

[29] One limitation is that we do not have a reliable time-varying measure of access to employer-provided sick leave by industry during this period. However, we do not think this is a major issue, as the variation in access to PSL across industries has remained fairly stable over time. Results are similar if we use information on access to employer-provided sick leave by industry from the 2010-2014 Employee Benefits Survey instead.

observed changes in worker characteristics at the local level (i.e., local labor supply factors). To alleviate these concerns, we use instead a "Bartik instrument" approach that provides an arguably exogenous source of variation in local industry composition that is driven by industry-specific labor demand shocks at the national level (Bartik, 1991; Blanchard and Katz, 1992). We first predict local industry-specific employment shares using national industry employment growth. We then use these predicted employment shares combined with the industry-specific measure of PSL access to generate a measure of predicted PSL access that is unrelated to changes in local labor supply. Following Wozniak (2010), we create this Bartik measure of employment and the new PSL share variable according to the following:

$$\widehat{emp}_{jmt} = \frac{N_{jt}}{N_{jt-1}} emp_{jmt-1} \tag{6}$$

$$\widehat{PSLshare}_{mt} = \frac{\sum_{j=1}^{J} PSLrate_j * \widehat{emp}_{jmt}}{\sum_{j=1}^{J} \widehat{emp}_{jmt}} \tag{7}$$

where N_{jt} is national employment in industry j and year t, excluding employment in MSA m. The instrument $\widehat{PSLshare}$ captures variation in the predicted share of workers with access to employer-provided PSL in a given MSA and year due to national labor demand shocks. For this method to yield a consistent estimate of the effect of access to PSL on leave-taking rates, it must be the case that changes in industry-level employment at the national level are uncorrelated with MSA-level labor supply shocks. Additionally, we must assume the share of workers with access to sick leave in a given industry is not changing differentially across MSAs over time. The industry-level share of workers with access to employer provided PSL must also be uncorrelated with the labor supply shocks. Because this measure is not time or location-varying, this is likely to be a reasonable assumption. In fact, there is no significant correlation between the industry-level PSL rate and MSA-specific employment shocks. Furthermore, the instrument is not significantly correlated with other measures of local employment conditions, including the employment rate and usual hours worked. This lends support to the idea that the exclusion restriction is satisfied.

An additional threat to the validity of this instrument is that the predicted employment measure could affect illness-related leave-taking through other channels. For example, industries that provide higher amounts of paid sick leave may provide healthier or safer work environments as well, making it less likely that employees get sick. While we cannot rule this out entirely, there is no significant relationship between industry-level access to leave and the average industry-level rate of illness-related leave-taking in our data.[30] Finally, if employers who provide PSL are more likely to also provide other forms of paid time off, our instrument will capture changes in access to both types of leave. If workers are less likely to shirk as their access to other types of paid time off increases, then any negative effect of our instrument on the illness-related leave-taking rate could reflect a decrease in shirking rather than a public health effect.[31] However, if this were the case then there should be a positive relationship between the share of workers with access to PSL and absences for other reasons. As we show below, our instrument does not identify a significant effect on the share of workers absent due to other reasons. Furthermore, the correlation between industry sick leave access and the industry's rate of leave-taking for non-illness related reasons is only 0.082.

6.2 Results

The first column of Table 10 shows the OLS estimate of the predicted share of workers with access to PSL on the share of workers absent from work due to illness. The coefficient is negative, but not statistically significant. The magnitude of this estimate indicates that a one percentage point increase in the share of workers with access to PSL is correlated with a 0.06 percentage point decrease in the aggregate illness related leave-taking rate. The second column shows the same effect using the Bartik instrument to predict employment.[32] The

[30] More specifically, a one percent increase in the share of workers with access to leave in an industry increases the illness related leave-taking rate by 0.000657 percentage points and is not statistically significant.

[31] For this to be true, workers who use paid sick leave to shirk would have report being absent due to illness in the CPS, which is not necessarily the case.

[32] The first stage is strong with a coefficient estimate of 0.72 and F-statistic of 1450.

estimate is larger in magnitude and statistically significant at the 10 percent level, indicating that the OLS estimate may be attenuated. A one percentage point increase in the share of workers with access to PSL causes a 0.15 percentage point decrease in the aggregate illness related leave-taking rate. This is a sizable effect: a one standard deviation increase in the share of workers with access to PSL would decrease the share of workers absent due to illness by 0.4 percentage points, or 16 percent. Reassuringly, there are no significant effects on the share of workers absent due to other reasons (the third and fourth columns show the OLS and IV effects, respectively), usual hours worked, or employment.

An important concern with this analysis is that initial industry composition is correlated with future changes in PSL share. Table 11 shows that the results do not appear to be driven by serial correlation in our instrument. First, the first two columns show that the OLS and IV results are robust to using a long difference model, where we regress the change in our outcome between 2004 and 2014 on the change in the instrument and other controls. The last column shows the results of a placebo test where we regress the leave-taking rate on a placebo instrument of PSL share that is constructed from initial industry composition and *future* industry-level employment growth. Reassuringly, we do find any significant effects of PSL share on illness-related leave-taking using this placebo IV estimate.[33] This suggests that the results in Table 10 are not driven by secular trends.

Although our results suggest that increased access to PSL reduces the illness-related leave-taking rate, there are several reasons to believe that there may be substantial heterogeneity in the size of the effects across MSAs. Additionally, we are likely underestimating the variation over time in the share of workers with access to PSL, and the 95% confidence intervals for the point estimates shown in Table 10 do not rule out very small local average treatment effects in either direction. The primary purpose of this additional analysis is to provide additional evidence that increased access to PSL can reduce the aggregate rate of illness related leave-taking.

[33] Because this method requires us to drop the last year of data from the analysis, we also present the OLS estimate using the same sample in the third column.

7 Conclusion

Paid sick leave policies provide many potential benefits to workers, especially in the United States where access to employer-provided leave is relatively low. Despite the proliferation of city- and state-wide PSL mandates in recent years, these policies remain relatively understudied. In this paper, we provide the first empirical evidence that PSL mandates in the U.S. may actually decrease the aggregate rate of illness related leave-taking. We use the introduction of mandatory PSL coverage for a subset of workers in Connecticut and Washington, D.C. to show that after these policies are implemented, work absences due to illness decrease by up to 18 percent. In Connecticut, where the policy applies to a specific and identifiable group of workers, we show that this decrease exists both for workers who are directly affected by the policy and for workers who are unlikely to be directly affected by the policy. This effect likely comes through a reduced probability of getting sick. When workers have access to PSL, they are less likely to go to work sick, and therefore less likely to spread their illness to coworkers and customers.

Additionally, using variation in industry composition in MSAs over time, we show that the negative relationship between the share of workers with access to PSL and the illness-related leave-taking rate holds more broadly. This suggests that our results are not specific to the environment in Connecticut and D.C., and that PSL mandates in other U.S. cities and states would likely have large public health benefits as well.

These findings have important policy implications. The presence of positive public health externalities associated with the provision of PSL implies that firms under-provide access to PSL relative to what a welfare-maximizing social planner would provide. Even if increased access to PSL increases shirking, as has been found in the European context, the evidence we show is consistent with the idea that the public health benefits of PSL policies in the U.S. could far exceed the wage and productivity costs that they impose on employers. But the magnitude of these public health effects may be a function of the relatively low rate of access to PSL in the U.S., which could explain why the effects of PSL that we find are so different from those found in Europe. It is plausible that the public health effects of PSL are decreasing in the pre-reform PSL rate. In other words, these policies may not effectively reduce the spread of contagious disease when most workers already are able to stay home from work when sick. More research is still needed to better quantify the magnitude of the effects described in this paper and to further explore the mechanisms driving the decreases in illness-related leave-taking. As data becomes available to study the substantial number of additional U.S. sick leave mandates passed in recent years, it will be possible to do so, as well as to utilize heterogeneity in policy design to better understand how to maximize public health benefits while minimizing productivity losses.

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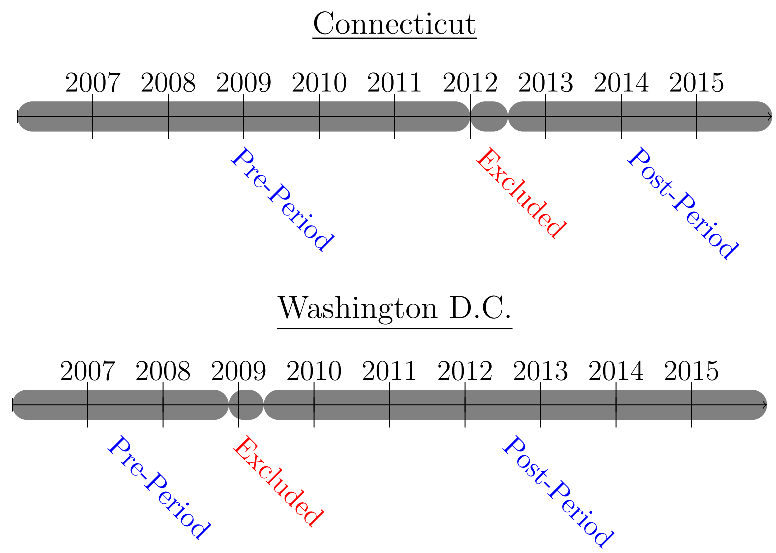
Wozniak, Abigail. 2010. "Are college graduates more responsive to distant labor market opportunities?" *Journal of Human Resources*, 45(4): 944–970.

Ziebarth, Nicolas R, and Martin Karlsson. 2010. "A natural experiment on sick pay cuts, sickness absence, and labor costs." *Journal of Public Economics*, 94(11): 1108–1122.

Figure 1: Timeline



Note: These timelines display the sample frame (2006-2014). Years mark the beginning of each year. The policy in CT was implemented January 2012, and the timeline shows that the first 6 months following policy implementation are dropped from the analysis (through June 2012). Similarly for D.C., the policy was implemented in November 2008, and months through April 2009 are dropped from the analysis.

Figure 2: Synthetic Control - Sick Leave

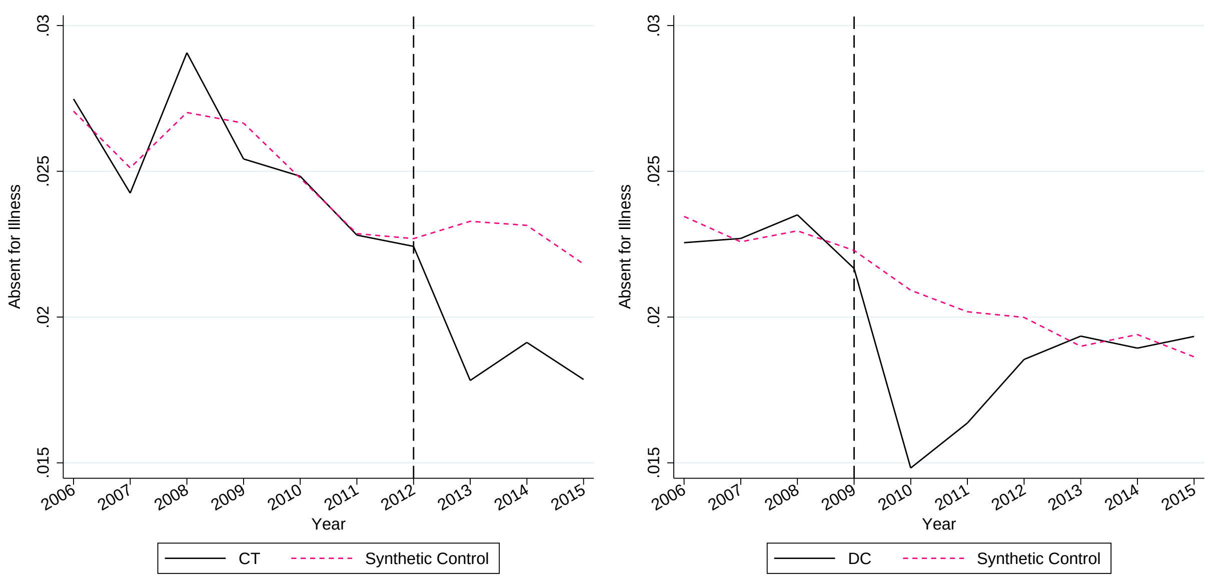


Figure 3: Synthetic Control - Other Leave

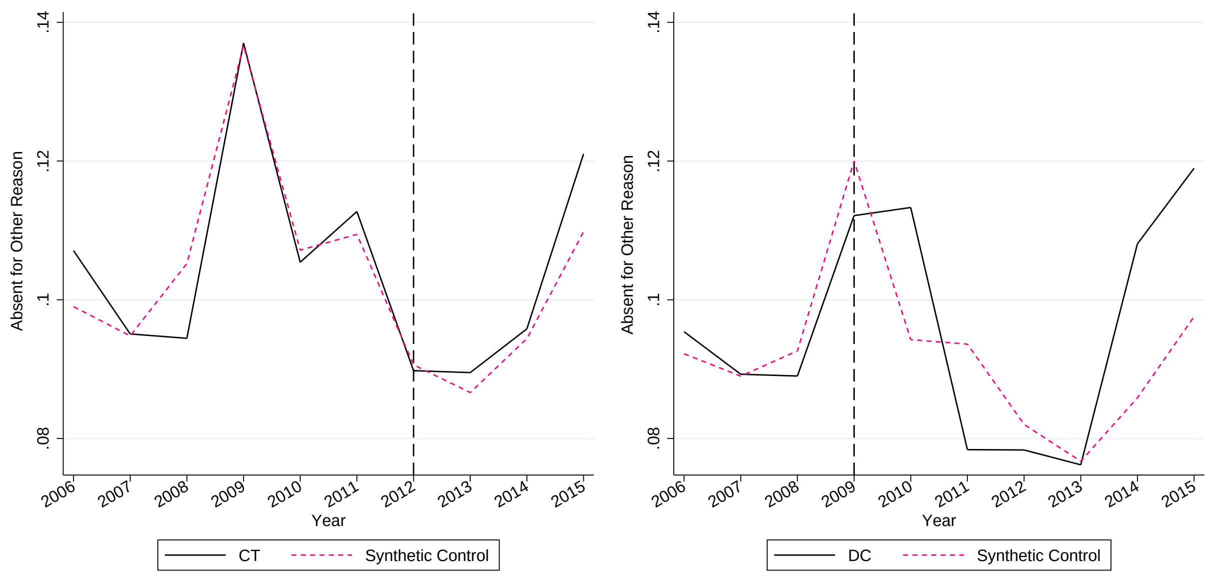
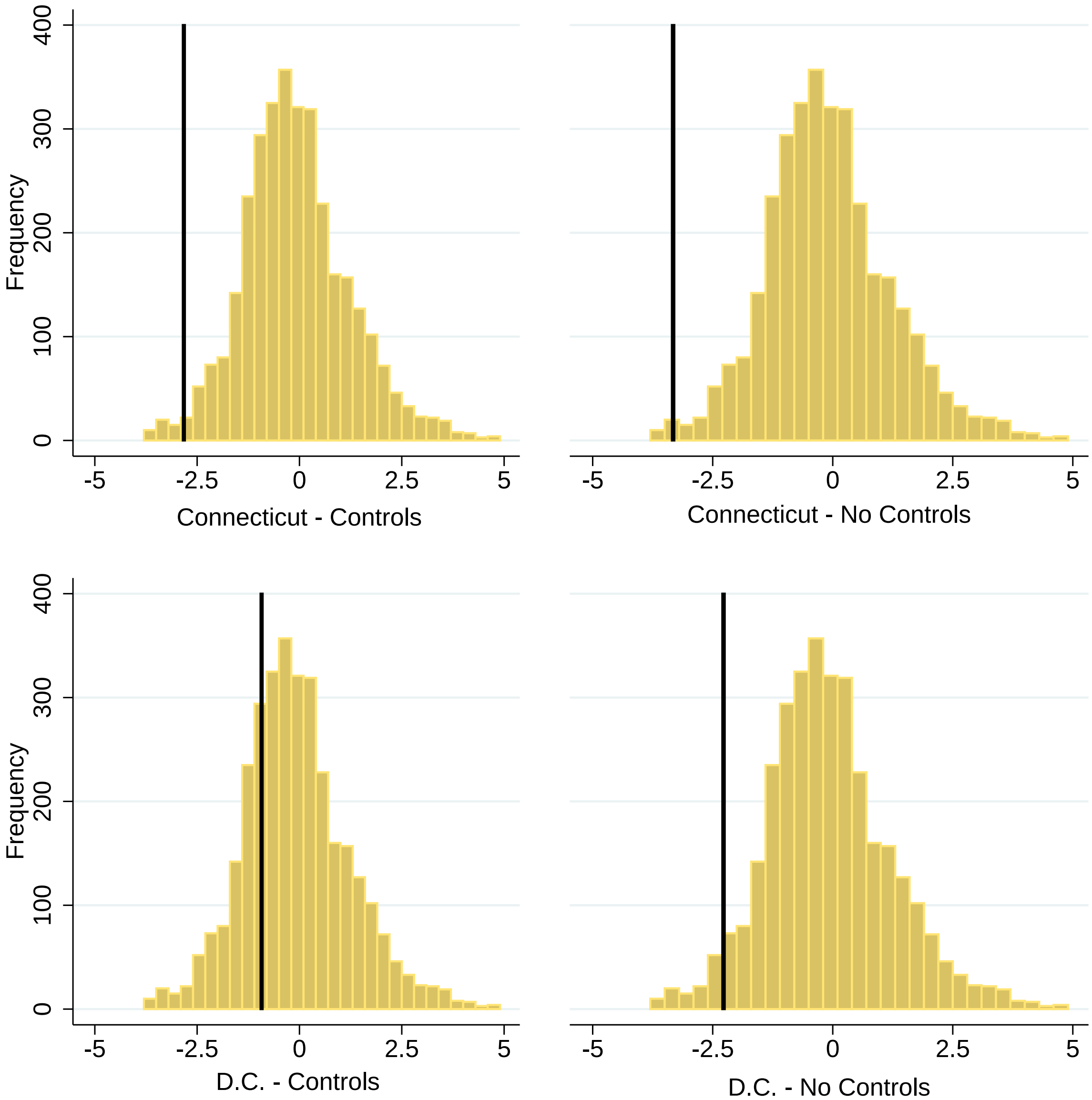


Figure 4: Permutation Test



Note: The histograms display the t-statistics corresponding the hypotheses that the difference-in-differences coefficient estimates for all states other than Connecticut and D.C. are equal to zero, assuming a counter-factual policy implementation each month beginning November 2008 and ending January 2012. Estimates are calculated using a synthetic control specific to each state and represent the effect in the first 48 months following the policy, excluding the first 6 months (this is the post-treatment period available for Connecticut).

Table 1: Access to Sick Leave by Industry, U.S.

Industry	Proportion of Workers with Access	
	1993	2010-2014
Utilities	93.0%	92.4%
Construction	25.4%	39.2%
Manufacturing	64.9%	62.6%
Retail Trade	51.3%	49.4%
Wholesale Trade	63.1%	75.8%
Transportation/Warehousing	65.1%	72.0%
Information	86.9%	90.2%
Finance and Insurance	82.3%	91.0%
Real Estate and Leasing	65.5%	76.8%
Professional and Technical	64.4%	82.4%
Educational Services	89.1%	76.0%
Health Care and Social Services	79.2%	76.0%
Accommodation and Food Services	**26.3%**	**26.0%**
Other Services	36.3%	52.4%

Note: Data in Column (1) are from the 1993 CPS Survey of Employee Benefits and calculated for full-time workers. Data in Column (2) are derived from the Employee Benefits Survey, 2010-2014; reported estimates are averages over these five survey years.

Table 2: Reasons for Leave-Taking

Reason	Absent Entire Week	Partial Absence
Slack Work/Business Conditions	-	13.0%
Seasonal Work	-	1.0%
Job Started or Ended During Week	-	1.1%
Holiday (Legal or Religious)	-	10.5%
Vacation/Personal Day(s)	52.2%	37.1%
Own Illness/Injury/Medical Appointment	**18.4%**	**19.2%**
Child Care Problems	0.5%	0.7%
Other Family/Personal Obligations	5.0%	6.9%
Labor Dispute	0.1%	0.1%
Weather Affected Job	2.7%	5.3%
School/Training	2.4%	0.7%
Civic/Military Duty	0.2%	0.2%
Other Reason	13.1%	4.4%
Maternity/Paternity Leave	5.6%	-
Sum	100%	100%

Note: "Absent Entire Week" refers to the question asked of individuals who report being employed but not at work in the reference week, and "Partial Absence" refers to the question asked of individuals reporting being employed and at work, who usually work more than 35 hours per week and report working less than 35 hours during the reference week. While the category of primary interest ("Own Illness") overlaps between these two questions, the full list of possible responses does not perfectly overlap.

Table 3: CPS Summary Statistics

	All Workers	Sample of Full-time Workers		
		Connecticut	D.C.	Other States
Male	0.527	0.572	0.504	0.567
	(0.499)	(0.495)	(0.500)	(0.495)
Age	39.989	42.480	38.405	40.953
	(12.512)	(11.584)	(11.617)	(11.785)
Married	0.572	0.613	0.323	0.599
	(0.495)	(0.487)	(0.468)	(0.490)
Less than High School	0.102	0.052	0.064	0.087
	(0.302)	(0.222)	(0.245)	(0.282)
High School	0.284	0.268	0.154	0.289
	(0.451)	(0.443)	(0.361)	(0.453)
Some College	0.292	0.238	0.137	0.282
	(0.455)	(0.426)	(0.344)	(0.450)
White (Not Hispanic)	0.680	0.766	0.462	0.675
	(0.466)	(0.424)	(0.499)	(0.468)
Hispanic	0.148	0.094	0.102	0.150
	(0.355)	(0.292)	(0.303)	(0.357)
Black	0.110	0.084	0.390	0.112
	(0.313)	(0.277)	(0.488)	(0.315)
Service Worker	0.287	0.227	0.225	0.248
	(0.452)	(0.419)	(0.418)	(0.432)
Hours Worked Last Week	37.833	41.190	41.697	41.320
	(14.252)	(12.928)	(12.053)	(12.304)
Full-time Worker	0.834	1	1	1
	(0.372)	(0.000)	(0.000)	(0.000)
Absent for Illness	0.022	0.026	0.022	0.022
	(0.148)	(0.159)	(0.146)	(0.148)
Absent for Other Reason	0.087	0.104	0.093	0.086
	(0.281)	(0.305)	(0.291)	(0.281)
N	7,843,192	145,413	101,183	6,270,827

Standard deviations in parentheses. All statistics weighted by CPS person weights. "Full-time" indicates usually working at least 35 hours per week.

Table 4: Sick-Leave – Connecticut

	All States	Synthetic	Neighbors
Panel A: All Years			
Post (Jul. 2012 - Dec. 2015)	-0.0037**	-0.0041**	-0.0060**
	(0.0011)	(0.0015)	(0.0014)
N	114	114	114
Panel B: By Year			
Year 0 (2012)	-0.0007	0.0011	-0.0000
	(0.0018)	(0.0023)	(0.0021)
Year 1 (2013)	-0.0050**	-0.0071**	-0.0099**
	(0.0018)	(0.0023)	(0.0021)
Year 2 (2014)	-0.0038*	-0.0049*	-0.0054*
	(0.0018)	(0.0023)	(0.0021)
Year 3 (2015)	-0.0040*	-0.0038	-0.0070**
	(0.0018)	(0.0023)	(0.0021)
N	120	120	120

Each coefficient in Panel A represents a separate regression, and each column in Panel B represents a separate regression. The estimates in Panel A do not include the first 6 months after policy implementation; this period is included in "Year 0" of the event-study specification (Panel B). All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table 5: Sick-Leave – Washington D.C.

	All States	Synthetic	Neighbors
Panel A: All Years			
Post (May 2009 - Dec. 2015)	0.0003	0.0004	-0.0023
	(0.0012)	(0.0012)	(0.0014)
N	114	114	114
Panel B: First Three Years			
Post (May 2009 - Dec. 2012)	-0.0011	-0.0010	-0.0029+
	(0.0013)	(0.0014)	(0.0015)
N	78	78	78
Panel C: By Year			
Year 0 (Nov. 2008 - Dec. 2009)	0.0001	-0.0000	-0.0011
	(0.0017)	(0.0018)	(0.0022)
Year 1 (2010)	-0.0046*	-0.0045*	-0.0044+
	(0.0018)	(0.0019)	(0.0023)
Year 2 (2011)	-0.0018	-0.0014	-0.0039+
	(0.0018)	(0.0019)	(0.0023)
Year 3 (2012)	0.0003	0.0004	-0.0042+
	(0.0018)	(0.0019)	(0.0023)
N	120	120	120

Each coefficient in Panel A and Panel B represent a separate regression, and each column in Panel C represents a separate regression. The estimates in Panel A and Panel B do not include the first 6 months after policy implementation; this period is included in "Year 0" of the event-study specification (Panel C). All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01

Table 6: Other Leave

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	-0.0002	0.0031	-0.0048	0.0054	0.0025	0.0029
	(0.0065)	(0.0059)	(0.0068)	(0.0099)	(0.0103)	(0.0048)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	0.0003	-0.0030	0.0016
				(0.0120)	(0.0124)	(0.0051)
N				78	78	78
Panel C: By Year						
Year 0	-0.0064	-0.0017	0.0048	-0.0039	-0.0062	-0.0017
	(0.0103)	(0.0093)	(0.0108)	(0.0150)	(0.0157)	(0.0075)
Year 1	-0.0038	0.0029	0.0026	0.0196	0.0130	0.0024
	(0.0103)	(0.0093)	(0.0108)	(0.0158)	(0.0166)	(0.0079)
Year 2	-0.0056	-0.0028	-0.0104	-0.0154	-0.0184	-0.0039
	(0.0103)	(0.0093)	(0.0108)	(0.0158)	(0.0166)	(0.0079)
Year 3	0.0067	0.0086	-0.0079	-0.0052	-0.0100	0.0069
	(0.0103)	(0.0093)	(0.0108)	(0.0158)	(0.0166)	(0.0079)
N	120	120	120	120	120	120

These estimates replicate Tables 4 and 5, except that the outcome is leave for any reason other than illness. The years and months represented by the "Post" indicators (Panels A and B), as well as each "Year" indicator (Panel C), are equivalent to the definitions provided in Tables 4 and 5, for Connecticut and D.C. respectively. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table 7: Sick-Leave – Effect on Hours Absent Due to Illness

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	-0.0505**	-0.0460**	-0.0660**	-0.0107	-0.0087	-0.0119
	(0.0140)	(0.0141)	(0.0183)	(0.0151)	(0.0150)	(0.0186)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	-0.0211	-0.0206	-0.0200
				(0.0169)	(0.0167)	(0.0204)
N				78	78	78
Panel C: By Year						
Year 0	-0.0142	-0.0174	0.0055	0.0032	-0.0027	0.0028
	(0.0221)	(0.0222)	(0.0286)	(0.0229)	(0.0227)	(0.0284)
Year 1	-0.0487*	-0.0439+	-0.0992**	-0.0462+	-0.0523*	-0.0535+
	(0.0221)	(0.0222)	(0.0286)	(0.0242)	(0.0240)	(0.0301)
Year 2	-0.0721**	-0.0642**	-0.0730*	-0.0150	-0.0273	-0.0253
	(0.0221)	(0.0222)	(0.0286)	(0.0242)	(0.0240)	(0.0301)
Year 3	-0.0507*	-0.0470*	-0.0645*	0.0015	-0.0113	-0.0222
	(0.0221)	(0.0222)	(0.0286)	(0.0242)	(0.0240)	(0.0301)
N	120	120	120	120	120	120

The outcome is hours absent in the reference week due to illness. Each coefficient in Panel A and Panel B represent a separate regression, and each column in Panel C represents a separate regression. The estimates in Panel A and Panel B do not include the first 6 months after policy implementation; this period is included in "Year 0" of the event-study specification (Panel C). All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table 8: Sick-Leave by Occupation – Connecticut

	All States	Synthetic	Neighbors
Panel A: Service Occupations			
Post	-0.0030	-0.0058	-0.0037
	(0.0025)	(0.0035)	(0.0033)
N	114	114	114
Panel B: Non-Service Occupations			
Post	-0.0041**	-0.0038*	-0.0068**
	(0.0012)	(0.0016)	(0.0015)
N	114	114	114

The estimates replicate those in Panel A of Table 4, except that Panel A here represents only workers in service occupations (directly affected), and Panel B represents only workers in non-service occupations (indirectly affected). All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01

Table 9: Treatment Effect Seasonality

	Connecticut		Washington D.C.	
	All States	Neighbors	All States	Neighbors
Policy (Summer Effect)	-0.0031	-0.0027	-0.0024	-0.0023
	[0.207]	[0.470]	[0.456]	[0.718]
Policy×Winter (Differential Effect)	-0.0028	-0.0025	-0.0013	-0.0026
	[0.196]	[0.110]	[0.643]	[0.372]
N	114	114	114	114

"Winter" is an indicator representing months November through March. The coefficients in each column are from a separate regression. p-values are reported in brackets and are obtained using the bootstrapping procedure described in Ferman and Pinto (2015). Estimates are provided using all other states and neighboring states as control groups; synthetic control estimates have not been included as it is not straightforward to provide synthetic control estimates while using the Ferman and Pinto (2015) procedure. All estimates use all other states as the control group, exclude the first 6 months following policy implementation and include the three years thereafter (through 2015 for Connecticut and through 2012 for D.C.). + p<0.10; * p<0.05; ** p<0.01

Table 10: Effect of Predicted Share of Workers with PSL on Sick Leave Rate

	Sick Leave		Other Leave	
	OLS	IV	OLS	IV
PSLshare	-0.0597	-0.147+	-0.0949	-0.265
	(0.0676)	(0.0758)	(0.142)	(0.208)
Observations	2,343	2,343	2,343	2,343
R-squared	0.362	0.361	0.510	0.510
Individual/State Covariates?	Yes	Yes	Yes	Yes

Cluster-robust standard errors in parentheses, clustered at the MSA level. + p<0.10; * p<0.05; ** p<0.01

Table 11: Robustness of Bartik Analysis

	Outcome: Sick Leave Rate			
	Long Difference		Placebo Check	
	OLS	IV	OLS	IV
PSLshare	-0.213+	-0.270*	-0.0564	0.0089
	(0.118)	(0.123)	(0.073)	(0.121)
Observations	213	213	2,130	2,130
R-squared	0.023	0.021	0.367	0.367
Individual/State Covariates?	Yes	Yes	Yes	Yes

Cluster-robust standard errors in parentheses, clustered at the MSA level. Columns (1) and (2) are from a long difference model, where all variables are measured as the difference between 2014 and 2004. Columns (3) and (4) show the effects using a placebo instrument constructed using future employment growth to predict current employment shares. + p<0.10; * p<0.05; ** p<0.01

Appendix

7.1 Theoretical Framework

To provide a theoretical framework for framing our results, we build upon the model in Pichler and Ziebarth (2017) and modify it to examine the effects of PSL policies on leave-taking rates.[34] The foundation for this model is a standard labor supply model, though it is modified here such that utility is derived from consumption, c_t, and recuperation, r_t.[35] Further, workers are sick with probability p_t, and when they are sick they receive a draw, σ_t, from a probability distribution that represents the severity of their illness. The utility function is linear in consumption and recuperation such that the worker either chooses to report to work or stay home on a given day, t. Utility, U_t, is weighted such that workers value only consumption when healthy ($\sigma_t = 0$), and that sick workers value consumption relatively less compared to recuperation as the severity of their illness increases.

$$U_t = (1 - \sigma_t)c_t + \sigma_t r_t \qquad \text{with} \quad \sigma_t \in [0, 1]$$

The worker spends all their time, H, either working (h) or in recuperation. Further, workers consume their entire income which is determined by their wage rate, w_t, and the rate at which they are compensated through sick leave, $\alpha_t w_t$, resulting in the following budget constraint:

$$c_t = w_t h_t + \alpha_t w_t r_t$$

One can think of α_t as representing the inverse cost of taking sick leave. This cost may be very high (and α_t low) if the worker has no access to PSL. Even with full wage replacement, we assume $0 \leq \alpha_t < 1$, such that taking sick leave may be costly in non-monetary terms

[34] Pichler and Ziebarth (2017) model the trade-off between shirking and presenteeism behavior, while our model focuses on changing the costs of sick leave across different types of workers.

[35] Individual subscripts are omitted for notational simplicity.

(e.g., the opportunity cost of not being able to take sick leave in the future if the number of hours of sick leave is limited, or due to concerns about the negative repercussions of missing work). The worker is indifferent between working and being absent when the slope of the budget constraint equals the marginal rate of substitution. One can derive the level of sickness associated with this indifference point as follows:

$$\sigma_t^* = \frac{(1-\alpha_t)w_t}{1+(1-\alpha_t)w_t} \qquad \text{and} \quad \frac{\partial \sigma_t^*}{\partial \alpha_t} < 0$$

If $\sigma_t > \sigma_t^*$, the worker will choose to be absent from work; otherwise they will choose to work. The key point here is that as the cost of sick leave decreases (α_t increases), the sickness threshold σ_t^* at which workers choose to stay home decreases. We can think of the PSL mandates as a way of exogenously adjusting the cost of sick leave for some types of workers. Since many workers in the U.S. already have access to PSL (or are otherwise ineligible for mandated sick leave benefits), we consider two types of workers: workers who gain sick leave benefits as a result of a mandate and those who are unaffected. We refer to workers who are directly affected by the policy as "Service Workers" (indexed by s) and workers who are not directly affected as "Non-Service Workers" (indexed by n). This is in part because of the eligibility requirement in Connecticut, where only workers in service occupations are eligible for the benefit. In general, however, workers in service occupations are less likely to have access to PSL and are thus more likely to benefit from any policy mandate.

Each worker type has a different cost of sick leave (α_{st} and α_{nt}) and thus, for a given wage, a different sickness threshold (σ_{st}^* and σ_{nt}^*). Define A_{st} and A_{nt} as the shares of service and non-service workers who are absent from work due to illness, and π_{st} and π_{nt} as the shares of service and non-service workers who are at work and ill.

$$A_{st} = p_{st} \int_{\sigma^*_{st}}^{1} f(\sigma) d\sigma$$
$$A_{nt} = p_{nt} \int_{\sigma^*_{nt}}^{1} f(\sigma) d\sigma$$

$$\pi_{st} = p_{st} \int_{0}^{\sigma^*_{st}} f(\sigma) d\sigma$$
$$\pi_{nt} = p_{nt} \int_{0}^{\sigma^*_{nt}} f(\sigma) d\sigma$$

In each equation, $f(\sigma)$ is a density function and p_{st} and p_{nt} represent the probabilities of being ill for service and non-service workers, respectively. We assume that the probability of being ill is increasing in the share of own-type workers who are at work and ill in the previous period *and* the share of other-type workers who are at work and ill in the previous period: $p_{st} = g(\pi_{st-1}, \pi_{nt-1})$, $p_{nt} = g(\pi_{nt-1}, \pi_{st-1})$. This assumption implies that workers do not only interact with workers of their same type; this is especially likely to be true if we consider interactions outside of the workplace (e.g., non-service workers eating at a restaurant). Additionally, this implies some level of dynamics in the processes that determine both p_{st} and π_{st} (and likewise for non-service workers). Indeed, there may be multiple lagged effects of the probability of illness on the share of workers who are ill; we assume that this effect dissipates over time ($\frac{\partial p_{st}}{\partial \pi_{st-1}} > \frac{\partial p_{st}}{\partial \pi_{st-2}} > ... > \frac{\partial p_{st}}{\partial \pi_{st-h}}$) and that for some arbitrary h there is no longer an effect ($\frac{\partial p_{st}}{\partial \pi_{st-h}} = 0$).[36]

[36]Note that we are not directly interested in the illness dynamics. We choose to let the probability of illness be a function of the *lagged* share of contagious workers rather than the contemporaneous share in part to reflect reality, but also to reduce concerns over simultaneity in these equations if the contemporaneous share were used.

Policy Predictions

We are primarily interested in evaluating the change in the share of workers absent from work due to illness when the cost of leave-taking is reduced. Further, we are interested in how this effect differs for workers who are directly affected by the policy and for workers who are not directly affected. Let us first consider the case of service workers. Suppose the inverse cost of taking sick leave for service workers, α_{st}, increases between periods 0 and T, but α_{nt} does not change. In other words, suppose that the cost of leave-taking is reduced between these two time periods for service workers only. Also assume that there is sufficient time between these periods such that the illness dynamics described in the previous section do not carry from 0 to T (i.e., we are interested in the pre- and post-policy equilibria rather than the time immediately following intervention). An increase in α_s decreases the sickness level at which a service worker is willing to be absent from work ($\sigma^*_{sT} < \sigma^*_{s0}$). This in turn lowers the share of service workers who are at work and contagious ($\pi_{sT} < \pi_{s0}$), and thus the probability of illness for both worker types ($p_{sT} < p_{s0}$; $p_{nT} < p_{n0}$). The outcome of primary interest to us is the change in the share of workers absent from work among service workers, non-service workers, and the total effect. First, the change in the share of service workers absent from work is:

$$\begin{aligned} A_{sT} - A_{s0} &= p_{sT} \int_{\sigma^*_{sT}}^{1} f(\sigma) d\sigma - p_{s0} \int_{\sigma^*_{s0}}^{1} f(\sigma) d\sigma \\ &= \underbrace{p_{sT} \int_{\sigma^*_{sT}}^{\sigma^*_{s0}} f(\sigma) d\sigma}_{\text{direct effect (+)}} + \underbrace{(p_{sT} - p_{s0}) \int_{\sigma^*_{s0}}^{1} f(\sigma) d\sigma}_{\text{indirect effect (-)}} \end{aligned} \tag{8}$$

The first term in Equation (8) represents the increase in absences due to reducing the cost of leave-taking (the "direct effect"). The second term represents the decrease in absences due to the reduced probability of being ill (the "indirect effect"). The direction of the overall effect depends on the relative size of these two components and is therefore ambiguous.[37]

Let us now consider non-service workers; note that the cost of leave-taking for these workers was not reduced, and thus σ_n^* is constant across time periods ($\sigma_{n0}^* = \sigma_{nT}^* = \sigma_n^*$).

$$
\begin{aligned}
A_{nT} - A_{n0} &= p_{nT} \int_{\sigma_n^*}^{1} f(\sigma) d\sigma - p_{n0} \int_{\sigma_n^*}^{1} f(\sigma) d\sigma \\
&= \underbrace{(p_{nT} - p_{n0}) \int_{\sigma_n^*}^{1} f(\sigma) d\sigma}_{\text{indirect effect (-)}}
\end{aligned}
\tag{9}
$$

For non-service workers, the change in the share of workers absent from work due to illness is unambiguously negative as this effect only operates through the indirect effect.

Finally, the total change in the share absent from work (service plus non-service workers) depends on the sizes of the populations of service and non-service workers, N_s and N_n, which we assume do not change over time:[38]

$$
A_T - A_0 = \frac{N_s (A_{sT} - A_{s0}) + N_n (A_{nT} - A_{n0})}{N_s + N_n} \tag{10}
$$

Given the ambiguity of Equation (8), it is clear that the sign of Equation (10) will be ambiguous as well. It is worth noting, however, that the total policy effect is essentially a weighted average of the policy effects for service and non-service workers, weighted by the size of each population. In the U.S. policy settings that we study, the population of workers who do not gain from the mandate tends to be significantly larger than the population who does, and hence the total effect will more closely resemble the indirect effect of the policy.

[37] We are assuming a constant wage within each group of workers such that the threshold value σ_t^* is the same for all workers of a given type.

[38] This model assumes that PSL does not cause employer responses, either through the number of people employed or wages. We find no evidence of these responses empirically. However, allowing for small employment effects would not change the predictions of the model. Instead negative employment effects for service workers would cause N_s to decrease relative to N_n. Then the total effect in Equation (10) would be weighted differently, but the sign is still ambiguous.

Another question of interest is whether the effect of the policy is theoretically larger for service or non-service workers; while the answer is ambiguous, this question is explained more thoroughly in the following section.

To recap, our theoretical framework yields two primary predictions regarding the effect of mandating PSL (reducing the cost of leave-taking for illness) on the share of workers absent due to illness:

1. Mandating PSL will lead to a decrease in the share of workers absent due to illness for workers who are not directly affected by the policy.
2. Mandating PSL can lead to either an increase or a decrease in the share of workers absent due to illness for workers who are directly affected by the policy and thus the total effect (i.e., the effect on all workers) is ambiguous as well.

Relative Magnitudes of PSL Effects for Service and Non-Service Workers

This section concerns the theoretical prediction for the relative sizes of the policy effects for service versus non-service workers. Following on the work presented in Section 7.1, we are interested in the following inequality:

$$p_{sT} \int_{\sigma^*_{sT}}^{\sigma^*_{s0}} f(\sigma) d\sigma + (p_{sT} - p_{s0}) \int_{\sigma^*_{s0}}^{1} f(\sigma) d\sigma \lesseqgtr (p_{nT} - p_{n0}) \int_{\sigma^*_{n}}^{1} f(\sigma) d\sigma \tag{11}$$

The direction of the inequality described in Equation (11) depends on the relative sizes of the changes in the probability that a worker is sick for service and non-service workers, and on the sickness level at which each worker type is indifferent between working and absence (before and after the policy change).[39]

[39] We are assuming a constant wage within each group of workers such that the threshold value σ^*_t is the same for all workers of a given type. When the size of the service and non-service worker groups are equal, the sum of effects for service and non-service workers gives the aggregate effect on leave-taking.

Recall that the probability of illness is an increasing function of the share of service workers who are sick and at work (which is directly affected by the policy). Though we don't model this explicitly, if service workers are more exposed to other service workers relative to non-service workers, the public health benefit of this policy is likely to be greater for these workers (i.e., if $\frac{\partial p_{st}}{\partial \pi_{st}} > \frac{\partial p_{nt}}{\partial \pi_{st}}$).[40] It is thus reasonable to think that the change in the probability of illness would be larger for service workers compared to non-service workers. If this is the case, then even though the direct effect for service workers is positive, the total effect for service workers could either be larger in magnitude (and negative) or smaller than the total effect for non-service workers. In short, the inequality described in Equation (11) remains ambiguous under reasonable assumptions.

[40] As an example, imagine a situation in which there is very little contact between service workers and non-service workers, but a large amount of contact between own-type workers. In this situation, the public health benefit for non-service workers is still negative, but minimal; the public health benefit for service workers, however, could be very large.

Appendix Tables

Table A1: Excluding Absent Entire Week

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	-0.0044**	-0.0022	-0.0062**	-0.0005	0.0005	-0.0025*
	(0.0010)	(0.0014)	(0.0011)	(0.0010)	(0.0012)	(0.0012)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	-0.0013	-0.0006	-0.0027*
				(0.0011)	(0.0013)	(0.0013)
N				78	78	78
Panel C: By Year						
Year 0	-0.0020	-0.0013	-0.0020	-0.0006	-0.0006	-0.0012
	(0.0015)	(0.0023)	(0.0017)	(0.0015)	(0.0018)	(0.0019)
Year 1	-0.0046**	-0.0048*	-0.0080**	-0.0042**	-0.0035+	-0.0041*
	(0.0015)	(0.0023)	(0.0017)	(0.0016)	(0.0019)	(0.0020)
Year 2	-0.0049**	-0.0025	-0.0055**	-0.0020	-0.0009	-0.0035+
	(0.0015)	(0.0023)	(0.0017)	(0.0016)	(0.0019)	(0.0020)
Year 3	-0.0055**	-0.0022	-0.0081**	0.0008	0.0013	-0.0028
	(0.0015)	(0.0023)	(0.0017)	(0.0016)	(0.0019)	(0.0020)
N	120	120	120	120	120	120

These estimates replicate Tables 4 and 5, except that illness-related leave taking is defined only by partial-week absences. The years and months represented by the "Post" indicators (Panels A and B), as well as each "Year" indicator (Panel C), are equivalent to the definitions provided in Tables 4 and 5, for Connecticut and D.C. respectively. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01

Table A2: Sick-Leave – No Weights

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	-0.0037**	-0.0037**	-0.0051**	0.0001	0.0002	-0.0021
	(0.0011)	(0.0014)	(0.0013)	(0.0012)	(0.0013)	(0.0014)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	-0.0016	-0.0014	-0.0030*
				(0.0013)	(0.0014)	(0.0015)
N				78	78	78
Panel C: By Year						
Year 0	-0.0005	0.0020	0.0016	-0.0001	-0.0003	-0.0008
	(0.0017)	(0.0022)	(0.0020)	(0.0018)	(0.0019)	(0.0022)
Year 1	-0.0051**	-0.0069**	-0.0091**	-0.0049*	-0.0048*	-0.0047*
	(0.0017)	(0.0022)	(0.0020)	(0.0019)	(0.0020)	(0.0023)
Year 2	-0.0033+	-0.0037+	-0.0042*	-0.0027	-0.0023	-0.0043+
	(0.0017)	(0.0022)	(0.0020)	(0.0019)	(0.0020)	(0.0023)
Year 3	-0.0043*	-0.0039+	-0.0061**	0.0000	0.0006	-0.0039+
	(0.0017)	(0.0022)	(0.0020)	(0.0019)	(0.0020)	(0.0023)
N	120	120	120	120	120	120

These estimates replicate Tables 4 and 5, except that sampling weights are not used. The years and months represented by the "Post" indicators (Panels A and B), as well as each "Year" indicator (Panel C), are equivalent to the definitions provided in Tables 4 and 5, for Connecticut and D.C. respectively. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01

Table A3: Synthetic Control Weights

State	CT - No Controls	DC - No Controls	CT - Controls	DC - Controls
Alabama	0.016	0.019	0	0.013
Arizona	0.016	0.019	0	0.015
Arkansas	0.018	0.024	0	0.016
Colorado	0.015	0.017	0	0.015
Delaware	0.011	0.011	0	0.006
Florida	0.008	0.009	0	0.009
Georgia	0.010	0.011	0	0.008
Idaho	0.021	0.022	0	0.015
Illinois	0.010	0.012	0	0.013
Indiana	0.010	0.010	0	0.007
Iowa	0.020	0.020	0	0.015
Kansas	0.022	0.027	0	0.016
Kentucky	0.021	0.026	0	0.013
Louisiana	0.013	0.011	0	0.007
Maine	0.334	0.136	1	0.48
Maryland	0.020	0.023	0	0.014
Massachusetts	0.011	0.012	0	0.013
Michigan	0.013	0.014	0	0.014
Minnesota	0.017	0.021	0	0.015
Mississippi	0.020	0.021	0	0.014
Missouri	0.017	0.018	0	0.015
Montana	0.024	0.027	0	0.015
Nebraska	0.016	0.016	0	0.011
Nevada	0.011	0.013	0	0.009
New Hampshire	0.013	0.016	0	0.011
New Mexico	0.018	0.015	0	0.01
North Carolina	0.011	0.012	0	0.01
North Dakota	0.020	0.021	0	0.012
Ohio	0.018	0.019	0	0.016
Oklahoma	0.018	0.024	0	0.016
Pennsylvania	0.016	0.017	0	0.016
Rhode Island	0.017	0.020	0	0.015
South Carolina	0.012	0.013	0	0.009
South Dakota	0.020	0.022	0	0.013
Tennessee	0.013	0.015	0	0.012
Texas	0.017	0.016	0	0.016
Utah	0.021	0.020	0	0.015
Vermont	0.022	0.050	0	0.011
Virginia	0.012	0.012	0	0.011
West Virginia	0.018	0.017	0	0.011
Wisconsin	0.014	0.017	0	0.014
Wyoming	0.023	0.135	0	0.015

Table A4: Sick-Leave – No Controls

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	-0.0039**	-0.0038**	-0.0064**	-0.0013	-0.0014	-0.0029*
	(0.0011)	(0.0012)	(0.0014)	(0.0012)	(0.0012)	(0.0014)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	-0.0025+	-0.0028*	-0.0033*
				(0.0013)	(0.0013)	(0.0015)
N				78	78	78
Panel C: By Year						
Year 0	-0.0008	-0.0003	-0.0005	-0.0005	-0.0006	-0.0012
	(0.0018)	(0.0018)	(0.0021)	(0.0017)	(0.0018)	(0.0022)
Year 1	-0.0050**	-0.0055**	-0.0099**	-0.0061**	-0.0061**	-0.0049*
	(0.0018)	(0.0018)	(0.0021)	(0.0018)	(0.0019)	(0.0023)
Year 2	-0.0039*	-0.0041*	-0.0066**	-0.0036*	-0.0038*	-0.0046*
	(0.0018)	(0.0018)	(0.0021)	(0.0018)	(0.0019)	(0.0023)
Year 3	-0.0043*	-0.0040*	-0.0067**	-0.0010	-0.0014	-0.0043+
	(0.0018)	(0.0018)	(0.0021)	(0.0018)	(0.0019)	(0.0023)
N	120	120	120	120	120	120

These estimates replicate Tables 4 and 5, except that no additional covariances are included. The years and months represented by the "Post" indicators (Panels A and B), as well as each "Year" indicator (Panel C), are equivalent to the definitions provided in Tables 4 and 5, for Connecticut and D.C. respectively. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01

Table A5: Other Leave – Effect on Hours Absent for Other Reasons

	Connecticut			Washington D.C.		
	All States	Synthetic	Neighbors	All States	Synthetic	Neighbors
Panel A: All Years						
Post	0.0391	0.0580	-0.0470	0.0771	0.0771	0.1143
	(0.0675)	(0.0660)	(0.0744)	(0.2765)	(0.2765)	(0.1186)
N	114	114	114	114	114	114
Panel B: First Three Years						
Post	-	-	-	-0.0343	-0.0343	0.0191
				(0.3555)	(0.3555)	(0.1358)
N				78	78	78
Panel C: By Year						
Year 0	-0.0371	-0.0155	0.0586	-0.1290	-0.1290	-0.0288
	(0.1065)	(0.1042)	(0.1172)	(0.4171)	(0.4171)	(0.1810)
Year 1	0.1015	0.1348	0.1285	0.6679	0.6679	0.1670
	(0.1065)	(0.1042)	(0.1172)	(0.4410)	(0.4410)	(0.1914)
Year 2	-0.0665	-0.0433	-0.1030	-0.4566	-0.4566	-0.1175
	(0.1065)	(0.1042)	(0.1172)	(0.4410)	(0.4410)	(0.1914)
Year 3	0.1101	0.1149	-0.1708	-0.2484	-0.2484	0.0492
	(0.1065)	(0.1042)	(0.1172)	(0.4410)	(0.4410)	(0.1914)
N	120	120	120	120	120	120

Outcome is hours absent in the reference week due to other (non-illness related) reasons. Each coefficient in Panel A and Panel B represent a separate regression, and each column in Panel C represents a separate regression. The estimates in Panel A and Panel B do not include the first 6 months after policy implementation; this period is included in "Year 0" of the event-study specification (Panel C). All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table A6: Sick-Leave – Alternative Control Groups

	Connecticut				Washington D.C.			
	NE All	NE Synth.	South All	South Synth.	NE All	NE Synth.	South All	South Synth.
Panel A: All Years								
Post	-0.0047**	-0.0041*	-0.0038**	-0.0025	0.0010	0.0014	-0.0002	0.0003
	(0.0013)	(0.0016)	(0.0012)	(0.0016)	(0.0013)	(0.0013)	(0.0012)	(0.0013)
N	114	114	114	114	114	114	114	114
Panel B: First Three Years								
Post	-	-	-	-	0.0005	0.0006	-0.0020	-0.0014
					(0.0015)	(0.0015)	(0.0013)	(0.0013)
N					78	78	78	78
Panel C: By Year								
Year 0	-0.0004	0.0017	-0.0014	-0.0029	0.0006	0.0003	-0.0003	0.0007
	(0.0020)	(0.0025)	(0.0018)	(0.0025)	(0.0021)	(0.0020)	(0.0018)	(0.0018)
Year 1	-0.0065**	-0.0068**	-0.0051**	-0.0031	-0.0028	-0.0036+	-0.0052**	-0.0042*
	(0.0020)	(0.0025)	(0.0018)	(0.0025)	(0.0022)	(0.0021)	(0.0019)	(0.0019)
Year 2	-0.0049*	-0.0057*	-0.0033+	-0.0016	-0.0004	0.0007	-0.0027	-0.0019
	(0.0020)	(0.0025)	(0.0018)	(0.0025)	(0.0022)	(0.0021)	(0.0019)	(0.0019)
Year 3	-0.0058**	-0.0034	-0.0043*	-0.0031	0.0022	0.0027	-0.0011	-0.0012
	(0.0020)	(0.0025)	(0.0018)	(0.0025)	(0.0022)	(0.0021)	(0.0019)	(0.0019)
N	120	120	120	120	120	120	120	120

Estimates are provided using states in the Northeast (NE) and South census regions as the control group, using either all states in these regions or a synthetic control. For each synthetic control regression, the top three synthetic control weights are reported as follows. **CT/NE:** Maine (100%); **CT/South:** Kentucky (80.1%), Texas (2.8%), Mississippi (2.7%); **DC/NE:** Maine (41.3%), Vermont (23.4%), Pennsylvania (12.3%); **DC/South:** Kentucky (21.5%), Oklahoma (8.2%), Arkansas (8.1%). The years and months represented by the "Post" indicators (Panels A and B), as well as each "Year" indicator (Panel C), are equivalent to the definitions provided in Tables 4 and 5, for Connecticut and D.C. respectively. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table A7: Recession Placebo – Sick Leave – Washington D.C.

	All States	Synthetic	Neighbors
Panel A: All Years			
Post (Sept 2001. - Dec. 2007)	-0.0007	-0.0008	-0.0005
	(0.0016)	(0.0016)	(0.0018)
N	114	114	114
Panel B: First Three Years			
Post (Sept. 2001 - Dec. 2004)	-0.0013	-0.0014	-0.0012
	(0.0019)	(0.0019)	(0.0021)
N	78	78	78
Panel C: By Year			
Year 0 (Mar. 2001 - Dec. 2001)	0.0007	0.0002	-0.0019
	(0.0029)	(0.0029)	(0.0032)
Year 1 (2002)	-0.0025	-0.0023	-0.0030
	(0.0027)	(0.0027)	(0.0030)
Year 2 (2003)	0.0015	0.0010	0.0028
	(0.0027)	(0.0027)	(0.0030)
Year 3 (2004)	-0.0045+	-0.0043	-0.0030
	(0.0027)	(0.0027)	(0.0030)
N	120	120	120

These estimates replicate the regression specifications in Table 5, but use the 2001 recession as a placebo treatment period. Sample includes years 1998-2007. Recession occurs Mar. 2001, and that is defined as the "treatment" date. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + $p<0.10$; * $p<0.05$; ** $p<0.01$

Table A8: Recession Placebo – Other Leave – Washington D.C.

	All States	Synthetic	Neighbors
Panel A: All Years			
Post (Sept 2001. - Dec. 2007)	0.0018	0.0012	0.0086
	(0.0069)	(0.0069)	(0.0060)
N	114	114	114
Panel B: First Three Years			
Post (Sept. 2001 - Dec. 2004)	0.0010	0.0009	0.0074
	(0.0080)	(0.0080)	(0.0073)
N	78	78	78
Panel C: By Year			
Year 0 (Mar. 2001 - Dec. 2001)	0.0117	0.0123	0.0170
	(0.0126)	(0.0127)	(0.0110)
Year 1 (2002)	-0.0078	-0.0077	-0.0026
	(0.0118)	(0.0118)	(0.0103)
Year 2 (2003)	0.0010	0.0011	0.0105
	(0.0118)	(0.0118)	(0.0103)
Year 3 (2004)	0.0009	-0.0001	0.0063
	(0.0118)	(0.0118)	(0.0103)
N	120	120	120

These estimates replicate the regression specifications in Table 5, but use the 2001 recession as a placebo treatment period. Sample includes years 1998-2007. Recession occurs Mar. 2001, and that is defined as the "treatment" date. All estimates are calculated by regressing monthly differences in the outcome between treatment and control groups on post-policy indicators (Donald and Lang, 2007). + p<0.10; * p<0.05; ** p<0.01