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ABSTRACT

We evaluate the impact of paid sick leave (PSL) mandates on labor market outcomes, the utilization of health care services, and health behaviors for private sector workers in the United States. By exploiting geographic and temporal variation in PSL mandate adoption, we compare changes in outcomes for workers in counties affected by a PSL mandate to changes for those in counties with no mandate. Additionally, we rely on within-county variation in the propensity to gain PSL following a mandate to estimate policy effects for workers most likely to acquire coverage. Results indicate that PSL mandates lead to increased access to PSL benefits, especially for women without a college degree. We find that PSL laws reduce average weekly hours worked and private sector employment, but appear to have no effect on job tenure or labor force participation. PSL mandates are associated with sizable reductions in emergency department utilization and increases in general practitioner visits. Finally, we present suggestive evidence that PSL mandates lead to more days binge drinking.

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

Key Words: Paid sick leave, labor market, health care, health behaviors

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The United States is one of only two OECD countries without a federal mandate requiring employers to provide workers with access to paid sick leave (PSL) (World Policy Analysis Center 2016).[1] Though it is not uncommon for firms to offer employees PSL in the absence of a federal or local mandate, estimates suggest that nearly 40 percent of private sector workers in the United States currently lack access to paid leave (Bureau of Labor Statistics 2016). Despite no national PSL mandate, between 2006 and 2014 San Francisco; Washington, DC; Seattle; New York City; Portland; Newark; and New Jersey City adopted PSL requirements at the municipal level, and Connecticut became the first state to enact PSL legislation (National Partnership for Women & Families 2016).[2] Additionally, renewed efforts to establish federally mandated PSL benefits have culminated in the reintroduction of the Healthy Families Act to Congress in 2015.[3] If signed into law, this legislation would establish a national standard requiring applicable employers to provide their workers with PSL. Moreover, a recent executive order signed by President Obama requires firms with federal government contracts to provide employees with up to 7 paid annual sick days beginning in 2017. The Obama administration argued that PSL would "improve the health and performance of employees of federal contractors," as well as benefit employers through the "recruitment and retention" of workers.[4]

Supporters of PSL legislation have made similar claims, touting increased access to preventive care and reduced emergency department (ED) utilization (U.S. Department of Labor 2015). Conversely, opponents of PSL requirements have argued that the mandates reduce worker hours and wages and have little effect on job turnover, absenteeism, or presenteeism (Nelson

[1] South Korea is the other OECD country that lacks federal PSL legislation.

[2] Connecticut's PSL mandate only covers service industry workers in firms with 50 or more employees (Ahn and Yelowitz 2015).

[3] The Healthy Families Act was originally introduced to Congress in 2004 and then again in 2013.

[4] For additional details, see the 2015 White House Fact Sheet "Helping Middle-Class Families Get Ahead by Expanding Paid Sick Leave." https://www.whitehouse.gov/the-press-office/2015/09/07/fact-sheet-helping-middle-class-families-get-ahead-expanding-paid-sick (accessed November 29, 2016).

2014).[5] A lack of empirical evidence on the effects of PSL mandates on labor market outcomes and health care utilization for affected workers has left policymakers unable to reconcile these various claims.

In this paper, we evaluate the effect of state and municipal PSL laws on labor market outcomes, health care utilization, and health behaviors. Our analysis relies on variation in the timing of PSL mandate adoption to estimate the effect of gaining PSL on several health and labor market outcomes using restricted-use data from the National Health Interview Survey (NHIS) from 2005 to 2014. We begin by estimating difference-in-differences (DD) specifications comparing those living in counties affected by PSL mandates to those living in nonaffected counties. However, since workers often have PSL benefits in the absence of a legislative mandate, we refine our analysis by predicting the probability of gaining PSL after a mandate takes effect. We then use this predicted probability to estimate a triple-differences (DDD) model comparing within-county changes in health and labor force outcomes after PSL adoption for those with a high probability of gaining coverage to those with a low probability of gaining coverage.

After establishing that workers in counties affected by a PSL mandate exhibit higher rates of PSL access than those in nonadopting counties, we focus on changes in labor market outcomes, health care utilization, and health behaviors. We find that mandates reduce average weekly hours worked and private sector employment for those most likely to gain access to PSL benefits, but have no effect on job tenure or labor force participation. PSL adoption appears to have a relatively large effect on ED utilization, reducing the probability of an ED visit by approximately 23 percent. We also report evidence that access to PSL leads to an increase in

[5] Presenteeism is defined as attending work when sick (Johns 2010).

general practitioner physician visits, though we find no effect for dental visits, mental health visits, or visits to a medical specialist. Finally, we find suggestive evidence that PSL mandates affect health behaviors related to alcohol and tobacco consumption; however, our estimates for these outcomes are imprecise and not particularly reliable.

While both paid and unpaid sick leave mandates have been the focus of past research, we address two significant gaps in the literature. First, due to minimal policy variation in the United States, most existing studies focus on PSL mandates in non-U.S. settings. In many cases, these foreign laws are dissimilar to the proposed and recently enacted U.S. statutes and therefore serve as poor models for the effects of expanded PSL generosity in the United States. Second, studies of PSL mandates primarily focus on worker absenteeism, resulting in limited evidence on the effect of PSL access on health care utilization, health behaviors, or other potential labor market changes.

Our results have significant implications for the current debate over the expansion of PSL benefits to workers in the United States. Our estimates of the effects of a PSL mandate on labor outcomes tend to support arguments made by opponents of PSL laws who claim that work hours will fall and unemployment will rise. However, we also find evidence that access to PSL can improve the efficiency of heath care consumption by shifting visits away from more expensive EDs and toward cheaper, and oftentimes more appropriate, primary care settings. This finding is especially striking considering that in 2013, ED expenditures for working-age adults approached $40 billion (Medical Expenditure Panel Survey 2013).

BACKGROUND AND PREVIOUS LITERATURE

Paid Sick Leave Legislation in the United States

There is no federal regulation currently requiring private employers in the United States to provide PSL to workers. While federal PSL proposals like the Healthy Families Act of 2015 have yet to be enacted, several municipalities have recently adopted PSL legislation. Beginning with San Francisco in 2007, PSL laws were adopted in Washington, DC, in 2008, Seattle in 2012, New York City in 2013, and Portland, Newark, and New Jersey City in 2014. Additionally, in 2012 Connecticut became the first state to mandate PSL for qualifying private employees (National Partnership for Women & Families 2016). In most cases, these ordinances exclude certain categories of workers and require workers to accrue sick pay over time.[6] We utilize mandates enacted in these early-adopting municipalities and states to provide evidence of the effects of PSL mandates on labor and health outcomes for affected U.S. workers. Since 2015, PSL mandates have been adopted in an additional four states, 26 cities, and two counties (National Partnership for Women & Families 2016).

Mandated Leave Access and Labor Market Outcomes

There are several mechanisms through which PSL mandates have the potential to impact labor market outcomes for workers. Primarily, increased access to PSL reduces the cost of absenteeism to workers, thus potentially increasing the likelihood of leave-taking. However, in the presence of a communicable illness, increased access to PSL has the potential to reduce worker absenteeism through a reduction in presenteeism (Pichler and Ziebarth 2016b). Additionally, any wage rigidities that prevent compensating wage differentials would cause

[6] In San Francisco, for example, an eligible worker accrues one hour of PSL for every 30 hours worked (Work and Family Legal Center 2016).

mandated leave policies to increase the cost of labor to employers. Therefore, employers may react to PSL mandates by substituting away from more expensive labor and toward capital production, leading to higher levels of unemployment. Furthermore, the exemption of small employers from PSL mandates creates an incentive for employers to reduce employment or hours of work. Finally, access to PSL benefits could promote entry into the labor market for those who would otherwise remain out of the labor force.

Although the United States currently lacks mandated PSL for all workers, the Family and Medical Leave Act (FMLA) of 1993 guarantees eligible workers access to unpaid, job-protected employment leave for circumstances including a serious health condition that impedes job performance, childbirth, or the care of a close relative with a serious health condition (U.S. Department of Labor 2016).[7] Evidence of the effect of access to unpaid leave on work absences is mixed. Waldfogel (1999) analyzes the effect of the FMLA on work absences, employment, and earnings. She finds that the FMLA increased instances of leave-taking, but finds no effect on changes in employment or wages. Alternatively, using the FMLA and prior state-level unpaid leave mandates, Baum (2003) reports that unpaid leave mandates had no effect on leave-taking for mothers who had recently given birth. Also examining unpaid parental leave, Han, Ruhm, and Waldfogel (2009) find that expansions in access to unpaid leave increased leave-taking for both mothers and fathers.

Because the FMLA provides unpaid leave and covers less than half of private sector workers, it is unclear whether these earlier findings of the effects of FMLA are applicable to the recent PSL mandates adopted by states and municipalities in the United States. Studies of paid leave in the United States are generally focused on mandates surrounding childbirth. Rossin-

[7] An eligible worker is defined as a worker in a firm with 50 or more employees working at least 1,250 hours in the 12 months prior to taking leave.

Slater, Ruhm, and Waldfogel (2012) and Baum and Ruhm (2016) examined changes in maternal and paternal paid leave following the enactment of California's 2004 Paid Family Leave program. Both studies find that access to paid family leave increased leave-taking on the intensive margin, while Slater, Ruhm, and Waldfogel also find increased work hours and higher wages for mothers who had returned to work after giving birth. Focusing on the same California paid leave policy, Das and Polachek (2015) report that, compared to men and older women, younger women experienced an increase in labor force participation and unemployment following the mandate. Byker (2016) studies the California paid leave mandate and a similar policy adopted in New Jersey in 2009 and also finds evidence of increased labor force participation for women. Again, the applicability of these studies to the case of PSL is questionable since work absences related to childbirth are largely planned in advance.

Few studies have examined access to PSL and changes in labor market outcomes in the United States. Ahn and Yelowtiz (2016) utilize data on U.S. workers who reported access to PSL and find that an exogenous health shock led to approximately one additional work absence per year for those with PSL. Finally, Ahn and Yelowitz (2015) and Pichler and Ziebarth (2016a) conduct analyses that most closely match our approach in this paper. Ahn and Yelowitz (2015) examine the effect of Connecticut's 2012 state PSL mandate on labor market outcomes including unemployment and labor force participation. Results indicate that adoption of the PSL mandate was associated with a small decline in employment, but had no effect on labor force participation. One drawback of this study was that the authors had only one year of postadoption data and so were not able to examine the longer run effects of a PSL mandate.[8] Pichler and

[8] This short postperiod is also concerning because it takes a worker two months of full-time work to accrue eight hours of PSL in Connecticut, and workers may not use their PSL benefits until the fourth month after employment commences (Work and Family Legal Center 2016).

Ziebarth (2016a) use a synthetic control approach to analyze changes in employment and wage growth resulting from many of the same U.S. PSL mandates that we examine in this paper. The authors' results suggest that the adoption of a PSL mandate was unrelated to employment rates or wage growth at the county level. However, the study was unable to distinguish differential effects of a mandate for those most likely to gain PSL benefits in adopting counties. Changes in labor market outcomes for this group may differ significantly from the overall effects on adopting counties because firms may offer PSL in the absence of a mandate.

Research has also explored the effects of PSL mandates in other countries, using greater variation in PSL adoption than is currently available in the United States. Henrekson and Persson (2004) conclude that increases in sick leave generosity in Sweden were related to increased absenteeism over an extended period from 1955 to 1999. Puhani and Sonderhof (2010) investigate German legislation that initially reduced and then later expanded opportunities for PSL. The authors report that decreasing sick pay from 100 percent to 80 percent of wages resulted in a reduction of 2.4 sick days on average. Similarly, examining the same reduction in German sick pay, Ziebarth and Karlsson (2010) report that the share of workers with zero work absences increased between 6 percent and 8 percent, while Ziebarth and Karlsson (2014) find that restoring German sick pay to 100 percent of wages led to a 10 percent increase in work absences.

Mandated Leave Access and Health Care Utilization

Relatively few studies have attempted to link access to PSL to the use of health care services. Conceptually, a reduction in the cost of a work absence could increase health care utilization by facilitating worker access to services typically available during conventional work hours (e.g., doctor visits, dental visits, preventive care services). However, if ED visits substitute for primary care physician visits because of time constraints on workers, then an increase in doctor visits may be accompanied by a reduction in ED utilization. Alternatively, health care utilization could be expected to decrease with PSL through the reduced likelihood of the spread of communicable illnesses in the workplace. Puhani and Sonderhof (2010) find that decreased sick pay in Germany had a particularly large effect on hospitalizations, reducing the average hospital length-of-stay by 30 percent. Peipins et al. (2012) and Wilson, Wang, and Stimpson (2014) find that those with access to PSL were more likely to receive screenings for a variety of cancers. Bhuyan et al. (2016) report that those with PSL were less likely to use the ED. However, in all three papers, the authors' research strategies failed to account for potential unobserved differences between those with and without PSL access that could be related to health care utilization.

Mandated Leave Access and Health Behaviors

In addition to the direct relationship between the introduction of PSL programs and the reduced cost of absenteeism, we are also interested in the effect of PSL on the propensity for workers to engage in risky behaviors that have the potential to affect work attendance. For example, suppose that episodes of binge drinking increase following a PSL mandate, which leads to a higher probability of a work absence.[9] Additionally, any increase in alcohol consumption resulting from PSL access could lead to changes in tobacco use since the two have been found to be compliments (Bask and Melkersson 2004; Decker and Schwartz 2000; Tauchmann et al. 2013). Given that the cost of a work absence is reduced in the presence of PSL, we may expect to see a higher incidence of these risky behaviors following the passage of PSL legislation.

[9] Bouchery et al. (2011) attributed an annual productivity loss of $4.2 billion (in 2006 dollars) to worker absenteeism related to excessive alcohol consumption.

To our knowledge, no other study has attempted to investigate the impact of PSL mandates on behavioral responses related to alcohol and tobacco consumption. However, there is substantial evidence to suggest that lowering the cost of a risky behavior can lead to increased engagement in high-risk activities. For example, Klick and Stratmann (2007) report that mandated coverage of medical treatment for diabetes led to an increase in average body mass index for diabetics. Along the same lines, Margolis et al. (2014) find that, compared to those receiving intensive treatment, individuals who were treated with less intensive measures following a heart attack were less likely to show improvement in physical activity, diet, drinking, and smoking, implying that intensive heart attack treatment sent a stronger signal to the patient about the future costs of risky behaviors. Finally, Dave and Kaestner (2009) and de Preux (2011) both examine behavioral changes accompanying Medicare receipt for those aging into the program at age 65. Dave and Kaestner's results indicated potentially large effects on alcohol consumption and binge drinking, but the relative imprecision of their estimates led to a lack of statistical significance. On the other hand, while de Preux finds little evidence to support Medicare-related changes in alcohol or cigarette consumption, she did observe a reduction in physical activity.

DATA

We received access to state- and county-identified National Health Interview Survey (NHIS) data for years 2005 to 2014 through the National Center for Health Statistics (NCHS). The main objective of the NHIS is to monitor the health of the United States population through the collection and analysis of data on a broad range of health topics. The NHIS is a cross-sectional household interview survey with continuous sampling and interviewing throughout the year. The sampling plan follows a multistage area probability design that permits the representative sampling of households and noninstitutional group quarters (e.g., college dormitories). Data are collected through a personal household interview conducted by interviewers employed and trained by the U.S. Census Bureau according to procedures specified by the NCHS (Centers for Diseast Control [CDC] 2016a). The primary advantage of the NHIS for our purposes is that the survey collects data on worker access to PSL at their current job.

Within the NHIS data, we extract a variety of variables that we use for outcomes and that were consistently collected across the 2005 to 2014 period over which we conduct this study. We are first interested in the effect of PSL mandates on the likelihood of having PSL benefits, hours worked in the past week, private employment, and participation in the labor force. Since individuals may use PSL laws to obtain health care more frequently, we explore the effect of the laws on visits to a dentist, general doctor, mental health professional, medical specialist, or ED over the past year. Finally, we also examine the effect of PSL laws on the number of binge drinking episodes over the past year and the number of cigarettes consumed over the past month.

Using the NHIS data, we create a treatment indicator for all counties exposed to a municipal or state PSL mandate passed in the United States between 2004 and 2014.[10] These mandates include those adopted in San Francisco; Washington, DC; Connecticut; Seattle; and New York City (National Partnership for Women & Families, 2016).[11] Appendix Table A.1 lists effective dates and the scope of coverage of each mandate. To maintain consistency between our sample of treated counties (those adopting a PSL mandate) and control counties (those with no

[10] Without more precise information than county, we count all of the county containing a city PSL law as treated.

[11] Portland passed a PSL mandate that went into effect on January 1, 2014, but was later overridden by a state mandate that went into effect on January 1, 2016. Because we exclude the year after a PSL mandate takes effect, we are unable to include Portland, Newark, and New Jersey City in our treatment group.

PSL mandate), we restrict our sample to large metro areas as defined by the 2013 Urban-Rural Classification (CDC 2014).

Additional merged data sources include state level unemployment rates from the Bureau of Labor Statistics. We also merge cigarette prices and beer prices, which is inclusive of excise taxes, at the state level using retail data from approximately 35,000 grocery, drug, mass merchandiser, liquor, and convenience stores from the Nielsen retail data system (Kilts Center for Marketing 2016). Finally, we also incorporate data on cigarette indoor air laws for restaurants, workplaces, and bars as recorded by the CDC State System (CDC 2016b).

Table 1 presents descriptive statistics for our sample of privately employed individuals aged 18–64.[12] Approximately 51 percent of our sample is male, the average age is 39, and 40 percent are white, non-Hispanic. Over half of our sample has some college education and 68 percent were privately insured. While only 5 percent lived in a city with a PSL law in place, 58 percent of individuals were offered PSL through their employers. This is consistent with prior estimates of the privately employed population with access to PSL (Bureau of Labor Statistics 2016).

EMPIRICAL FRAMEWORK

Our goal is to estimate the effect of recent PSL laws on labor outcomes, health care utilization, and health behaviors. Because existing PSL mandates in the United States vary geographically and were adopted at different times, we choose to employ a DD strategy to estimate the causal impact of PSL legislation on our outcomes of interest.

[12] We excluded individuals in the following categories: looking for work; working, but not for pay, at a family-owned job or business; not working at a job or business and not looking for work; employees of federal/state/local government; and self-employed in own business, professional practice, or farm.

A potential challenge to the validity of our DD specification is the endogenous adoption of PSL mandates. For example, if a municipality's population demographics (e.g., share of service industry workers and underlying health of the population) are changing over time in unobserved ways, and this change leads to the adoption of a PSL mandate, then our estimates of the effect of PSL legislation on labor market and health outcomes would be biased.

In order to gauge the potential for policy exogeneity we use data from the Current Population Survey's basic monthly files along with information on county-level poverty rates, unemployment rates, and proxies for population health to regress PSL adoption on several county-level characteristics that would plausibly be related to the adoption of a PSL mandate. Specifically, we examine the association between the enactment of a PSL law and county-level estimates of population age, race/ethnicity, education, and income; per capita hospital inpatient days, per capita outpatient visits, and total Medicare spending for parts A and B;[13] county-level poverty and unemployment rates; and the share of workers employed in retail and service industries.[14]

Results of this analysis are presented in Table 2. Column (1), which omits county fixed effects and county time trends, indicates that adopting counties tend to have younger populations with more college graduates. The addition of county fixed effects in column (2) has little effect on our estimates for age and education, but we do see that coefficient estimates on Medicare spending, per capita hospital days, and poverty rates are now statistically significant at the 10 percent level. Finally, we add a county time trend in column (3) and find that the share of the

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

[14] See Table A.2 for a description of industry codes used to categorize retail and service sector employment.

population over age 65, Medicare spending, and county-level unemployment rates are associated with PSL mandate adoption. If the positive association between Medicare spending and PSL regulation indicates that counties with sicker populations were more likely to adopt a PSL mandate, then estimates of the effect of PSL adoption on health care utilization could be biased. We note that the R-squared in each specification suggests that much of the variation in PSL mandate adoption is unexplained by demographic and county-level observables; therefore, there is a substantial random variation in PSL adoption that we are able to leverage in our estimation strategy in order to isolate the causal effect of PSL mandates. While not definitive evidence of policy exogeneity, the relative lack of predictive power in these models strengthens the validity of our research design.

To further explore this issue of endogenous policy adoption we conduct a series of event study analyses, described in detail below, that examine the timing of the changes in our outcomes of interest to determine whether there is any evidence of anticipatory behavior prior to the enactment of a PSL mandate. We include the results of these event studies alongside our estimates of the average effects of PSL mandate adoption.

We begin our main analysis by estimating the effect of PSL adoption for individuals between the ages of 18 and 64 that are privately employed but not self-employed, and that reside in large metro areas. We exclude data from the first year after PSL laws were passed because PSL benefits generally accrue over time. The relationship between PSL and our outcomes of interest is formalized as follows:

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

where Y is the outcome of interest for person i in county c at year-quarter t, *Adopt* is an indicator for one year after the adoption of PSL legislation in any part of year-quarter t, X is a vector of individual characteristics (sex, race/ethnicity, marital status, education, age, and health insurance coverage), Z is a vector of time-varying state-level factors (unemployment rate, beer and cigarette prices, and cigarette indoor air laws for bars, restaurants, and private workplaces),[15] δ_c is a county fixed effect, τ_t is a year-quarter fixed effect, and $\delta_c \times \tau_t$ represents a county-specific linear time trend.[16] Equation (1) represents a standard DD analysis where outcomes in our treatment states (i.e., those passing PSL legislation) are compared to control states that have no PSL laws in place. We cluster our standard errors at the county level in all analyses.[17]

Our dependent variable, $Y_{i,c,t}$ in Equation (1), represents one of several possible outcomes. We initially estimate the effect of PSL laws on the reported probability of having PSL directly. Our hypothesis is that a PSL mandate should increase the share of workers reporting access to PSL. However, if PSL mandate adoption is focused in areas where private employers display a high likelihood of offering PSL, then we may find a relatively small effect of the mandate. We then estimate the effect of PSL laws on labor market outcomes, including years on the job, hours worked last week, private employment, and labor force participation. For the private employment outcome, we add to our sample of privately employed workers individuals without employment who report looking for work. For labor force participation, we then add individuals who are not looking for work (but who remain between the ages of 18 and 64) to our sample. In our third set of results, we estimate the effect of PSL laws on health care utilization

[15] Unemployment rate is controlled for in all models. Cigarette prices, cigarette indoor air laws, and beer prices are only controlled for in substance use models.

[16] One of our outcomes, binge drinking, changed in year 2014 so that females were now asked if they drank four or more drinks on any one setting rather than five or more drinks, which was used in prior years. We control for this wording change in this question in year 2014 by including an interaction term for female and year 2014.

[17] Our sample of large metro areas contains 68 counties.

measures, which include having visited a dentist, general doctor, mental health professional, medical specialist, or ED in the past year. In our final set of results, we estimate the effect of PSL laws on health behaviors, including cigarettes consumed over the past month and the number of binge drinking episodes over the past year.

In addition to policy exogeneity, another necessary assumption for the validity of our DD model is that the treatment and control groups would have followed the same trends (in terms of the outcome variables) had the adoption of a PSL mandate not occurred. This assumption is untestable, as it is impossible to observe the treatment group in the untreated state during the posttreatment period; however, evidence that these two groups followed similar trends in the outcome variables in the pre-PSL period lends credence to our estimation strategy. We test for this assumption by performing an event study in which we add to Equation (1) four PSL lag and lead variables: 1) 1–3 years before PSL laws, 2) 0–1 year before PSL laws, 3) 1–3 years after PSL laws, and 4) ≥3 years after PSL laws. The year immediately following PSL law enactment continues to be excluded. Our event study takes the following form:

$$(2)\ Y_{ict} = \tilde{\alpha} + \gamma_1 Adopt_{c,t-3} + \gamma_2 Adopt_{c,t-1} + \gamma_3 Adopt_{c,t+1} + \gamma_4 Adopt_{c,t+3} + X_{i,c,t}\tilde{\beta} + Z_{c,t}\tilde{\theta} + \tilde{\delta}_c + \tilde{\tau}_t + \tilde{\delta}_c \times \tilde{\tau}_t + \varepsilon_{i,c,t}$$

Statistically significant estimates of the lead terms would be consistent with the case in which PSL policies are adopted in response to changes in the outcome measures, and therefore we would be unable to assign a causal interpretation to the PSL coefficient from Equation (1). The event study is also useful to evaluate possible heterogeneity over time in the post-PSL period. Because PSL takes time to accrue, effects may not be immediately apparent.

RESULTS

Table 3 presents estimates of the effect of PSL mandate adoption on the probability of reporting access to PSL. In the first column of Panel A, we estimate the impact of PSL legislation at the county level on the probability that a privately employed worker reports PSL coverage. Our results indicate a sizable increase of 6.9 percentage points in PSL coverage in response to passage of a PSL mandate. This represents an increase in PSL coverage of approximately 12 percent for adopting counties compared to counties with no PSL law in place. As expected, the event study in Panel B suggests that the gain in PSL access comes in the first few years after the mandate takes effect, and we find no indication of policy anticipation.

Prior evidence on the response to an increase in PSL generosity indicated that women are more responsive to changes in PSL policies than men (Henrekson and Persson 2004). We explore this possibility in the remaining columns of Table 3. Columns (2) and (3) contain estimates from Equation (1) with the sample limited to men and women, respectively. We find no effect of PSL mandates on men but an 8.4 percentage point increase in PSL access for women. Finally, when we further restrict our sample to workers with no college education, we find that the effect for women doubles to 16.5 percentage points (a 37 percent increase in access to PSL for this group).

The fact that gains in PSL access following adoption of a mandate vary by gender and education motivates us to further explore heterogeneity in the impacts of PSL mandates. To do so, we first estimate the predicted probability of lacking access to PSL using data from the 2005 and 2006 waves of the NHIS.[18] We use a logistic regression model that includes the same

[18] We choose these years because they predate the first municipal PSL policy passed by San Francisco in 2007.

individual demographic characteristics found in Equation (1), as well as income and codes for specific industry of employment.[19] We then use this predicted probability of lacking PSL in a DDD model that takes the following form:

$$(3)\ Y_{i,c,t} = \acute{\alpha} + \acute{\gamma}_1 Adopt_{c,t} + \acute{\gamma}_2 Prob.(PSL)_{i,c,t} + \acute{\gamma}_3 Adopt_{c,t} \times Prob.(PSL)_{i,c,t} + X_{i,c,t}\acute{\beta} + Z_{c,t}\acute{\theta} + \acute{\delta}_c + \acute{\tau}_t + \acute{\delta}_c \times \acute{\tau}_t + \varepsilon_{i,c,t}$$

Equation (3) is similar to Equation (1) but allows the effect of PSL mandate adoption to vary by the probability of lacking PSL access. Specifically, the coefficient of interest, $\acute{\gamma}_3$, measures the impact of a PSL mandate on an individual who is predicted to gain access to PSL benefits. The coefficient $\acute{\gamma}_1$represents the effect of PSL adoption on those with PSL access prior to a mandate (i.e., predicted probability of lacking PSL access equals zero). The coefficient $\acute{\gamma}_2$is then the difference in outcomes for those with no PSL benefits compared to those with PSL benefits when no mandate is in place (i.e., predicted probability of lacking PSL access equals one compared to a predicted probability of lacking PSL access equal to zero). It is important to keep in mind that the coefficients from our DD and DDD models are not directly comparable. The DD coefficient of interest in Equation (1) measures the effect of a PSL mandate on workers in adopting counties compared to workers in nonadopting counties (in other words, this is an intent-to-treat estimate). The DDD coefficient of interest in Equation (3) estimates the effect of a PSL mandate on workers in adopting counties who are more likely to gain PSL compared to those in adopting counties who are less likely to gain PSL (i.e., a treatment-on-the-treated effect).

[19] We exclude income from our main analyses because it may be related to the enactment of a PSL mandate. We include income in our probability model since we estimate this model for the time before any PSL mandate.

Table 4 displays results from our DDD model on the effect of PSL mandates on access to PSL. Our coefficient estimate on the interaction term is positive, indicating that those who lack access to PSL prior to a mandate are 5.6 percentage points more likely to benefit from adoption of the PSL law. While the point estimate is large and the result is intuitive, the estimate is not statistically significant. Taken together, this estimate and the estimate from Table 3, column (1) suggest a cumulative effect of more than a 12 percent increase in PSL access for individuals predicted to lack PSL prior to mandate adoption.

Table 5 reports both DD and DDD estimates of the effect of PSL mandates on labor market outcomes. We examine number of years at the current place of employment, number of hours worked in the past week, the probability of being employed as a private sector worker, and the overall labor force participation rate. As we described previously, we would expect PSL to have a negative effect on hours worked as workers use their PSL benefits and as employers adjust to the added expense of providing PSL. The theoretical impact of PSL on employment and job tenure is ambiguous, as PSL is likely to attract additional workers to the private sector but also increase costs to employers. If individuals value the added PSL benefit, then we expect the labor force participation rate to rise. Panel A of Table 5 displays results from the DD model in Equation (1). We find no measurable effect of PSL mandates on any of our labor market outcomes. Event study models in Panel B indicate a small increase in job tenure occurring in the first three years after a PSL mandate takes effect, while hours worked in the past week appear to decline after the third year that the policy has been in place.

Next we turn to the DDD estimates reported in Panel C of Table 5. If the lack of evidence on the labor market effects of PSL adoption reported in Panel A were due to the availability of PSL benefits in the absence of a mandate, then the DDD models that allow for within-county heterogeneity may be more informative. The coefficients on the *Prob. (PSL)* variable in columns (1) through (4) suggest that workers predicted to lack access to PSL have nearly five fewer years of job tenure, work 11.9 fewer hours per week, are 5.2 percentage points less likely to be employed, and are 4.1 percentage points less likely to participate in the labor force. Estimates in columns (1) and (4) indicate that workers predicted to lack PSL access see no differential gains to tenure or labor force participation from PSL mandates. Estimates in columns (2) and (3), however, indicate that those gaining access to PSL work fewer hours per week and are less likely to be employed in the private sector following the adoption of a PSL mandate. The observed reduction in employment coupled with no measurable effect on labor force participation is consistent with the findings of Ahn and Yelowitz (2015).[20] Weekly hours worked fall for this group by approximately 8 percent and employment rates decline by nearly 7 percent. The event study models in Panel D show that both of these effects tend to occur more than three years after the enactment of PSL legislation, suggesting a delayed effect in early-adopting municipalities (San Francisco and Washington, DC). We also note that the coefficient estimate on the second lead term for the hours worked outcomes is statistically significant at the 10 percent level, indicating the potential for policy endogeneity or anticipatory behavior. Were this an employer effect, we would expect this estimate to be negative rather than positive, so this could be an indication that workers increased their weekly hours in anticipation of the PSL mandate.

In Table 6, we shift our focus to estimates of the effect of PSL mandates on various measures of health care utilization. Since PSL reduces the cost of a work absence, it may facilitate increased interactions with health care providers, as workers are more likely to take time off work to seek care. We therefore expect dental visits, visits to general practitioners, and

[20] However, Ahn and Yelowitz (2015) arrived at those conclusions using a DD methodology, and we only find these effects in our DDD specification.

mental health visits to increase following PSL adoption, because demand for these services may be relatively elastic with respect to the cost of a work absence. We also examine visits to a medical specialist in the past year, which should be unlikely to respond to changes in PSL availability, as demand for these services would be more inelastic. Finally, we include visits to the ED in the past year for which the effect of PSL would be ambiguous. The reduced cost of a work absence could lead to higher rates of ED utilization, but the increased ability to receive care in more appropriate settings during normal business hours may reduce the likelihood of an ED visit.

Panel A of Table 6 contains the DD estimates of the effect of a PSL mandate on health care utilization. Only the probability of an ED visit in column (5) appears to respond to an increase in the availability of PSL; mandate adoption leads to a 6.1 percentage point reduction in ED utilization, a decline of nearly 23 percent. Notably, the event study estimates for ED utilization in Panel B suggest an initial drop in ED visits in the first three years after mandate adoption, followed by an increase after three years. Event study estimates also indicate a slight increase in mental health visits compared to the prior year after PSL adoption and a decrease in specialist visits three years after adoption.

DDD estimates of the effect of PSL mandates on health care utilization are reported in Panel C of Table 6. Individuals with a high probability of lacking PSL access exhibit fewer visits to a dentist, general practitioner, and medical specialist, but have higher rates of mental health visits and ED utilization. Only general practitioner visits, reported in column (2), appear to change for this group following a PSL mandate compared to those likely to have had access to PSL benefits. PSL adoption is associated with a 20 percent increase in general practitioner visits for those who benefit most from PSL mandates. However, we should be cautious interpreting this as a causal effect of PSL adoption given the statistical significance of the second lead term in the event study in Panel D. Despite finding a strong effect of PSL adoption on ED visits in our DD model, we find no differential effect by likelihood of gaining PSL in our DDD specification. This pattern could be explained by a situation where PSL minimizes externalities associated with communicable illnesses. For example, Pichler and Ziebarth (2016b) find that PSL access reduced influenza rates, a benefit that would accrue to those gaining PSL, as well as those working in jobs that already provide PSL access. Reductions in communicable illnesses could reduce ED visits regardless of access to PSL. We also note that our event study estimates for ED utilization follow a similar pattern in both our DD and DDD specifications; we see a large immediate reduction in ED utilization after the mandate takes effect, followed by an uptick in the likelihood of an ED visit after three years.

Lastly, Table 7 contains estimates of the effect of PSL mandate adoption on the health behaviors of binge drinking and cigarette smoking. Because of the potential for ex ante moral hazard associated with PSL benefits, we might expect instances of binge drinking and cigarette consumption to increase after the enactment of PSL legislation. However, PSL may also increase the ability to seek treatment for substance use disorders, in which case we would find a reduction in both behaviors.

Estimates of the effect of PSL on the number of days binge drinking in the past year are presented in column (1) and the number of cigarettes smoked in the past month in column (2). DD estimates in Panel A find no statistically significant effect of a PSL mandate on either outcome. We do, however, concede that these estimates and the remaining estimates in Table 7 suffer from a lack of precision. Event study models in Panel B suggest an increase in binge drinking and a reduction in smoking three years after PSL adoption compared to the first three years after a mandate took effect. DDD estimates in Panel C show that those predicted to lack access to PSL have higher rates of both binge drinking and smoking and appear to increase binge drinking episodes following a PSL mandate. This finding is consistent with the notion that the moral hazard effect associated with PSL access trumps any benefit arising from the increased availability of alcohol abuse treatment.

DISCUSSION

Those on either side of the mandated SL discussion in the United States have had little evidence to support their claims. Primarily, this paucity of evidence stems from the fact that, until recently, no mandate for the provision of PSL benefits existed for private sector workers in the United States. Beginning with San Francisco in 2007, several cities, counties and states have now legislated worker access to PSL. Our goal in this paper was to first establish a link between early-adopting PSL mandates and access to PSL benefits, and then to explore the impact of increased access to PSL on various health and labor market outcomes.

Our preliminary analysis indicated that adoption of a PSL mandate leads to a 12 percent increase in access to PSL compared to counties with no such mandate in place. This effect is concentrated among women and is especially pronounced for women with no college degree, who experience a 37 percent increase in PSL access. We next examined changes in labor market outcomes, health care utilization, and health behaviors associated with gaining PSL benefits. We found that both average weekly hours worked and private sector employment fall for those most likely to gain access to PSL following mandate adoption. PSL has no effect on job tenure or labor force participation rates.

We were also interested in the potential for PSL benefits to affect health care utilization and health behaviors. Anecdotal evidence has suggested that PSL mandates in the United States have led to higher levels of health care utilization, but causal estimates of the effect of PSL on the consumption of health services is lacking. While we found no evidence that PSL mandates affects dental visits, mental health visits, or medical specialist visits, our results do show an increase in general doctor visits and a large decrease in ED visits corresponding to the adoption of a PSL mandate. Finally, we also examined changes in health behaviors, including binge drinking and cigarette consumption. Our results suggest that PSL mandates were associated with higher rates of binge drinking; however, our estimates of these effects are imprecise.

Our study represents the first effort to provide causal estimates of the effect of gaining PSL on health care utilization and health behaviors for private sector workers in the United States and adds to studies that have examined changes in labor market outcomes. Our study also provides the first evidence that PSL mandates in the United States reduce employment and hours worked for individuals with high probabilities of gaining PSL benefits. We believe that our results highlight the potential trade-offs involved with the expansion of PSL benefits and, as more cities and states push to establish PSL mandates, can help inform the discussion going forward. As of October 2016, PSL mandates have been passed by five states, two counties, and 29 municipalities (National Partnership for Women & Families 2016). While insufficient time has elapsed for us to study these newer mandates, we believe that the results from our study will be instructive to policymakers evaluating these and other future PSL expansions.

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Table 1 Descriptive Statistics

	N	Mean	Std. Dev.
Control variables			
Female	41,221	0.490	0.500
Age	41,221	38.648	11.913
White, non-Hispanic	41,221	0.397	0.489
Hispanic	41,221	0.318	0.466
Black, non-Hispanic	41,221	0.183	0.387
Other race, non-Hispanic	41,221	0.102	0.303
Family income \$0–\$34,999	41,221	0.335	0.472
Family income \$35,000–\$74,999	41,221	0.314	0.464
Family income \$75,000 or higher	41,221	0.263	0.440
Family income missing	41,221	0.089	0.284
Less than high school	41,221	0.154	0.361
High school graduate or GED	41,221	0.220	0.414
Some college or associate's degree	41,221	0.297	0.457
Bachelor's degree	41,221	0.222	0.416
Graduate or professional degree	41,221	0.101	0.302
Education missing	41,221	0.006	0.077
Married	41,221	0.405	0.491
Widowed	41,221	0.015	0.123
Divorced	41,221	0.117	0.321
Separated	41,221	0.039	0.194
Never married	41,221	0.343	0.475
Living with a partner	41,221	0.076	0.264
Marital status missing	41,221	0.004	0.064
Private medical insurance	41,221	0.680	0.466
Medicaid	41,221	0.045	0.206
Military medical insurance	41,221	0.012	0.109
State medical insurance	41,221	0.019	0.137
Other government medical insurance	41,221	0.014	0.118
Single service insurance (e.g., vision, dental)	41,221	0.033	0.178
Medical insurance missing	41,221	0.004	0.066
Dependent variables			
Paid sick leave	40,632	0.577	0.494
Years on the job	40,511	5.906	7.008
Hours worked last week	40,591	39.841	11.612
Privately employed (as percent of the labor force)	47,051	0.876	0.329
In labor force	64,319	0.733	0.443
Dentist in past year	40,526	0.601	0.490
General doctor in past year	40,638	0.571	0.495
Mental health professional in past year	40,660	0.059	0.235
Medical specialist in past year	40,644	0.170	0.375
Emergency department visits in past year	40,636	0.269	0.876
Number of days with 5+ drinks in past year	39,886	8.483	32.081
Number of cigarettes smoked in the past month	40,744	50.291	153.543
Merge data			
Paid sick leave law	41,221	0.050	0.219
Cigarette prices	36,549	5.192	1.410
Beer prices	36,549	0.070	0.009
Unemployment rate	41,221	7.356	2.451
Restaurant smoking partial ban	41,221	0.375	0.484
Restaurant smoking full ban	41,221	0.429	0.495
Private workplace smoking partial ban	41,221	0.330	0.470
Private working smoking full ban	41,221	0.446	0.497
Bar private workplace smoking partial ban	41,221	0.274	0.446
Bar private workplace full ban	41,221	0.324	0.468

NOTE: Multiple forms of health insurance can be held by the same individual. Price data is not available for Alaska or Hawaii. The primary sample used (N = 41,221) is for individuals between the ages of 18 and 64 that are privately employed but not self-employed and that reside in large metro areas. For private employment outcomes, we add individuals who are looking for work back into our sample. For labor force outcomes, we add individuals not privately employed and either looking or not looking for work to our sample.
SOURCE: Authors' calculations.

Table 2 Determinants of Paid Sick Leave Adoption

	(1)	(2)	(3)
Age 18–34	Omitted	Omitted	Omitted
Age 35–44	−0.133	0.100	0.143
	(0.134)	(0.200)	(0.165)
Age 45–54	−0.399**	−0.122	−0.040
	(0.188)	(0.144)	(0.150)
Age 55–64	−0.474***	−0.434**	−0.072
	(0.171)	(0.181)	(0.117)
Age 65+	−0.091	−0.058	0.228**
	(0.082)	(0.103)	(0.109)
White	Omitted	Omitted	Omitted
Black	0.030	−0.003	−0.066
	(0.066)	(0.146)	(0.217)
Hispanic	−0.040	−0.059	0.075
	(0.026)	(0.072)	(0.109)
Asian	0.272	−0.257	−0.218
	(0.214)	(0.398)	(0.260)
Other race/ethnicity	−0.205	0.388	0.404
	(0.270)	(0.249)	(0.307)
High school or less	Omitted	Omitted	Omitted
Some college	−0.289***	−0.136	−0.116
	(0.088)	(0.089)	(0.132)
College or greater	0.219**	0.255*	0.096
	(0.087)	(0.136)	(0.101)
Family income <$25,000	Omitted	Omitted	Omitted
Family income $25,000–$49,999	−0.043	0.059	0.001
	(0.069)	(0.075)	(0.089)
Family income $50–$74,999	−0.137	−0.037	0.092
	(0.114)	(0.103)	(0.074)
Family income >$75,000	−0.105	0.129	0.149
	(0.108)	(0.166)	(0.147)
Medicare spending	−0.002	0.033*	0.033**
	(0.004)	(0.017)	(0.017)
Per capita hospital days	0.001	−0.135*	−0.013
	(0.012)	(0.080)	(0.044)
Per capita outpatient visits	0.001	−0.009	−0.001
	(0.002)	(0.007)	(0.008)
Poverty rate	0.001	−0.005*	0.005
	(0.001)	(0.003)	(0.003)
Unemployment rate	0.005	0.002	0.008*
	(0.003)	(0.004)	(0.004)
Share of retail and service employment	0.079	−0.075	−0.017
	(0.227)	(0.110)	(0.145)
County fixed effects	No	Yes	Yes
County time trend	No	No	Yes
R^2	0.13	0.46	0.64
Observations	34,495	34,495	34,495

NOTE: * p<0.10; ** p<0.05; *** p<0.01. Observations are at the county-month level for years 2005 through 2014. Regressions contain year and month fixed effects and are weighted by county population. See Appendix Table 2 for a list of industry codes included in the Retail and Service Employment designation. Standard errors (in parentheses) are clustered at the county level.
SOURCE: Authors' calculations.

Table 3 Estimates of the Effect of Paid Sick Leave Mandates on Access to Paid Sick Leave

	(1) Full sample	(2) Men	(3) Women	(4) Men, no college	(5) Women, no college
Panel A—DD model					
PSL mandate	0.069**	0.049	0.084**	-0.000	0.165**
	(0.031)	(0.039)	(0.043)	(0.048)	(0.081)
Panel B—Event study					
1–3 years before PSL	−0.033	−0.033	−0.037	0.021	−0.088
	(0.032)	(0.027)	(0.054)	(0.053)	(0.081)
<1 year before PSL	0.004	−0.006	0.012	0.020	−0.042
	(0.027)	(0.034)	(0.034)	(0.068)	(0.041)
1–3 years after PSL	0.057**	0.041	0.068*	−0.003	0.156
	(0.026)	(0.037)	(0.039)	(0.061)	(0.095)
≥ 3 years after PSL	0.003	−0.011	−0.002	−0.096	−0.114
	(0.017)	(0.019)	(0.018)	(0.129)	(0.069)
Mean of dependent variable	0.576	0.562	0.590	0.398	0.445
Observations	40,100	20,389	19,711	8,161	6,840

NOTE: * p<0.10; ** p<0.05; *** p<0.01. Analyses exclude the year immediately following the enactment of mandatory paid sick leave (PSL). Each regression includes demographic controls (sex, race, marital status, education, age, and health insurance coverage), state-level unemployment rates, county fixed effects, county-specific linear time trends, and a set of year-by-quarter indicators. Event study coefficients can be interpreted as the change compared to the prior period. The omitted category is >3 years before PSL. Standard errors (in parentheses) are clustered at the county level.
SOURCE: Authors' calculations.

Table 4 Triple-Difference Estimates of the Effect of Paid Sick Leave Mandates on Access to Paid Sick Leave

	Full sample
Panel A: DDD model	
PSL mandate × prob. (PSL)	0.056
	(0.038)
PSL mandate	0.036
	(0.023)
Prob. (PSL)	−1.067***
	(0.031)
Panel B: DDD event study	
1–3 years before PSL × prob. (PSL)	−0.041
	(0.058)
<1 year before PSL × prob. (PSL)	0.035
	(0.069)
1–3 years after PSL × prob. (PSL)	0.055
	(0.104)
≥ 3 years after PSL × prob. (PSL)	0.010
	(0.085)
Mean of dependent variable	0.576
Observations	40,100

NOTE: * $p<0.10$; ** $p<0.05$; *** $p<0.01$. Paid sick leave (PSL) probability is determined using a logistic regression to estimate the probability of paid sick leave access conditional on demographic characteristics, income, and industry of employment. The interaction coefficient represents the effect of a PSL mandate for those gaining access to PSL. Analyses exclude the year immediately following the enactment of mandatory paid sick leave. Each regression includes demographic controls (sex, race, marital status, education, age, and health insurance coverage), state-level unemployment rates, county fixed effects, county-specific linear time trends, and a set of year-by-quarter indicators. Event study coefficients can be interpreted as the change compared to the prior period. The omitted category is >3 years before PSL. Standard errors (in parentheses) are clustered at the county level.

SOURCE: Authors' calculations.

Table 5 Estimates of the Effect of Paid Sick Leave Mandates on Labor Market Outcomes

	(1) Years on the job	(2) Hours worked last week	(3) Private employment	(4) Labor force participation
Panel A—DD model				
PSL mandate	0.425	0.213	0.021	−0.012
	(0.358)	(0.903)	(0.018)	(0.013)
Panel B—Event study				
1–3 years before PSL	0.411	−1.238	−0.023	0.006
	(0.262)	(0.771)	(0.020)	(0.021)
<1 year before PSL	−0.050	−0.320	−0.019	−0.023**
	(0.203)	(0.438)	(0.020)	(0.012)
1–3 years after PSL	0.579*	−0.068	0.023	0.000
	(0.311)	(1.005)	(0.022)	(0.016)
≥3 years after PSL	0.165	−1.555***	0.027	0.006
	(0.243)	(0.354)	(0.020)	(0.015)
Panel C—DDD model				
PSL mandate × prob. (PSL)	0.349	−3.155***	−0.061*	0.021
	(0.424)	(0.766)	(0.035)	(0.026)
PSL mandate	0.232	1.338	0.051***	−0.020
	(0.391)	(1.039)	(0.018)	(0.014)
Prob. (PSL)	−5.159***	−11.922***	−0.052***	−0.041**
	(0.386)	(0.853)	(0.019)	(0.019)
Panel D—DDD event study				
1–3 years before PSL × prob. (PSL)	−0.630	−1.578	0.049	0.000
	(0.514)	(1.972)	(0.070)	(0.041)
<1 year before PSL × prob. (PSL)	1.561*	3.010*	−0.038	−0.014
	(0.926)	(1.621)	(0.051)	(0.045)
1–3 years after PSL × prob. (PSL)	−1.340	−2.679	−0.008	0.055
	(1.570)	(1.738)	(0.043)	(0.057)
≥ 3 years after PSL × prob. (PSL)	1.460*	−3.742*	−0.113*	−0.038
	(0.784)	(1.909)	(0.063)	(0.073)
Mean of dependent variable	5.90	39.84	0.876	0.732
Observations	39,972	40,054	46,423	57,956

NOTE: * $p<0.10$; ** $p<0.05$; *** $p<0.01$. Paid sick leave (PSL) probability is determined using a logistic regression to estimate the probability of paid sick leave access conditional on demographic characteristics, income, and industry of employment. The interaction coefficient represents the effect of a PSL mandate for those gaining access to PSL. Column (3) expands the sample to include individuals looking for work and column (4) adds individuals who are not looking for work. Analyses exclude the year immediately following the enactment of mandatory paid sick leave. Each regression includes demographic controls (sex, race, marital status, education, age, and health insurance coverage), state-level unemployment rates, county fixed effects, county-specific linear time trends, and a set of year-by-quarter indicators. Event study coefficients can be interpreted as the change compared to the prior period. The omitted category is >3 years before PSL. Standard errors (in parentheses) are clustered at the county level.
SOURCE: Authors' calculations.

Table 6 Estimates of the Effect of Paid Sick Leave Mandates on Healthcare Utilization

	(1)	(2)	(3)	(4)	(5)
	Dentist in past year	General doctor in past year	Mental health visit in past year	Medical specialist in past year	ED visit in past year
Panel A—DD model					
PSL mandate	−0.009	−0.013	0.020	−0.011	−0.061**
	(0.026)	(0.055)	(0.015)	(0.021)	(0.027)
Panel B—Event study					
1–3 years before PSL	−0.032	0.011	0.012	0.026	0.003
	(0.033)	(0.034)	(0.012)	(0.016)	(0.054)
<1 year before PSL	−0.008	−0.032	−0.008	0.010	−0.008
	(0.018)	(0.033)	(0.013)	(0.027)	(0.033)
1–3 years after PSL	−0.015	0.003	0.027**	−0.009	−0.054
	(0.033)	(0.057)	(0.014)	(0.028)	(0.040)
≥3 years after PSL	−0.015	−0.056	0.024	−0.055***	0.093**
	(0.032)	(0.036)	(0.019)	(0.017)	(0.047)
Panel C—DDD model					
PSL mandate × prob. (PSL)	0.083	0.118**	0.017	−0.025	0.010
	(0.064)	(0.049)	(0.038)	(0.037)	(0.116)
PSL mandate	−0.044	−0.062	0.013	−0.002	−0.063
	(0.036)	(0.060)	(0.019)	(0.033)	(0.053)
Prob. (PSL)	−0.310***	−0.216***	0.020*	−0.077***	0.173***
	(0.025)	(0.033)	(0.012)	(0.018)	(0.051)
Panel D—DDD event study					
1–3 years before PSL × prob. (PSL)	0.060	−0.043	−0.011	0.098*	0.037
	(0.048)	(0.057)	(0.018)	(0.052)	(0.074)
<1 year before PSL × prob. (PSL)	0.048	0.172**	−0.024	−0.031	0.120
	(0.110)	(0.008)	(0.022)	(0.087)	(0.128)
1–3 years after PSL × prob. (PSL)	−0.071	0.012	0.010	−0.072	−0.184*
	(0.095)	(0.069)	(0.036)	(0.076)	(0.111)
≥ 3 years after PSL × prob. (PSL)	0.103	−0.039	0.078	−0.023	0.078
	(0.082)	(0.129)	(0.059)	(0.070)	(0.209)
Mean of dependent variable	0.600	0.571	0.059	0.170	0.270
Observations	39,992	40,102	40,124	40,108	40,100

NOTE: * p<0.10; ** p<0.05; *** p<0.01. Paid sick leave (PSL) probability is determined using a logistic regression to estimate the probability of paid sick leave access conditional on demographic characteristics, income, and industry of employment. The interaction coefficient represents the effect of a PSL mandate for those gaining access to PSL. Analyses exclude the year immediately following the enactment of mandatory paid sick leave. Each regression includes demographic controls (sex, race, marital status, education, age, and health insurance coverage), state-level unemployment rates, county fixed effects, county-specific linear time trends, and a set of year-by-quarter indicators. Event study coefficients can be interpreted as the change compared to the prior period. The omitted category is >3 years before PSL. Standard errors (in parentheses) are clustered at the county level.
SOURCE: Authors' calculations.

Table 7 Estimates of the Effect of Paid Sick Leave Mandates on Health Behaviors

	(1) Days binge drinking in past year	(2) Cigarettes smoked in past month
Panel A—DD model		
PSL mandate	1.788	12.739
	(1.862)	(13.023)
Panel B—Event study		
1–3 years before PSL	−3.333	−1.186
	(2.541)	(9.018)
<1 year before PSL	−1.030	9.532
	(1.862)	(8.340)
1–3 years after PSL	0.967	9.082
	(1.887)	(15.321)
≥3 years after PSL	6.810***	−26.494***
	(0.817)	(4.827)
Panel C—DDD model		
PSL mandate × prob. (PSL)	7.556*	2.708
	(4.525)	(19.163)
PSL mandate	−1.078	12.536
	(2.414)	(17.620)
Prob. (PSL)	8.933***	65.324***
	(2.214)	(13.668)
Panel D—DDD event study		
1–3 years before PSL × prob. (PSL)	−0.697	−25.182*
	(1.829)	(13.339)
<1 year before PSL × prob. (PSL)	5.965	15.589
	(5.633)	(25.312)
1–3 years after PSL × prob. (PSL)	−0.802	11.370
	(6.806)	(46.852)
≥3 years after PSL × prob. (PSL)	5.982	−0.411
	(6.328)	(41.705)
Mean of dependent variable	8.477	50.524
Observations	34,886	35,602

NOTE: * $p<0.10$; ** $p<0.05$; *** $p<0.01$. Paid sick leave (PSL) probability is determined using a logistic regression to estimate the probability of paid sick leave access conditional on demographic characteristics, income, and industry of employment. The interaction coefficient represents the effect of a PSL mandate for those gaining access to PSL. Analyses exclude the year immediately following the enactment of mandatory paid sick leave. Each regression includes demographic controls (sex, race, marital status, education, age, and health insurance coverage), state-level unemployment rates, county and year fixed effects, county-specific linear time trends, and a set of year-by-quarter indicators. We also add state-level controls for beer prices, cigarette prices, and restrictions on smoking in bars, restaurants, and private workplaces. Event study coefficients can be interpreted as the change compared to the prior period. The omitted category is >3 years before PSL. Standard errors (in parentheses) are clustered at the county level.

SOURCE: Authors' calculations.

Table A.1 Municipal and State Paid Sick Leave Mandates, 2004–2014

Municipality or state	Effective date	Scope of coverage	Accrual period
San Francisco, CA	February 5, 2007	All workers	One hour for every 30 hours worked
Washington, DC	November 13, 2008	All workers except independent contractors, students, certain health care workers, certain unpaid volunteers, and casual babysitters	• Firms with 24 or fewer workers: 1 hour for every 87 hours worked • Firms with 25–99 workers: 1 hour for every 43 hours worked • Firms with 100 or more workers: 1 hour for every 37 hours worked
Connecticut	January 1, 2012	Hourly workers in the service sector working for firms with 50 or more employees	One hour for every 40 hours worked
Seattle, WA	September 1, 2012	Workers in firms with more than 4 employees completing more than 240 annual hours of work	• Firms with more than 4, but fewer than 250 workers: 1 hour for every 40 hours worked • Firms with more than 250 workers: 1 hour for every 30 hours worked
New York, NY	June 26, 2013	Workers in firms with more than 5 employees completing more than 80 annual hours of work with certain exemptions	One hour for every 30 hours worked

NOTE: We rely on information provided by the Work and Family Legal Center (2016) and the National Partnership for Women and Families (2016) for the information in this table. Portland, OR; Newark, NJ; and New Jersey City, NJ, passed a paid sick leave mandate on January 1, 2014; however, because we exclude the first year after the enactment of a paid sick leave mandate, we are unable to include the Portland mandate in our analyses.

Table A.2 Industry Codes Defining Retail and Service Employment

Industry description	CPS industry code (2014)	CPS industry code (2005–2013)
Auto parts stores	4690	4690
Furniture stores	4770	4770
Household appliance stores	4780	4780
Computer stores	4795	4790
Hardware stores	4880	4880
Lawn stores	4890	4890
Grocery stores	4970	4970
Specialty food stores	4980	4980
Beer, wine, and liquor stores	4990	4990
Pharmacies and drug stores	5070	5070
Health and personal care	5080	5080
Gasoline stations	5090	5090
Clothing stores	5170	5170
Shoe stores	5180	5180
Jewelry store	5190	5190
Sporting goods stores	5275	5270
Sewing stores	5280	5280
Music stores	5295	5290
Book stores	5370	5370
Department stores	5380	5380
General merchandise stores	5390	5390
Retail florists	5470	5470
Office supplies stores	5480	5480
Used merchandise stores	5490	5490
Gift shops	5570	5570
Miscellaneous retail stores	5580	5580
Restaurants and food service	8680	8680
Drinking places	8690	8690
Car washes	8780	8780
Barber shops	8970	8970
Beauty salons	8980	8980
Nail salons	8990	8990
Dry cleaning	9070	9070
Other personal services	9090	9090

NOTE: Industry codes are from the 2005–2014 Current Population Survey’s basic monthly files.